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It is quite unnecessary to dwell upon the recent events which have given a new interest to the subject of the zymotic as distinguished from the sporadic diseases. An unusually violent epidemic yellow fever, ravaging a part of the "outlying" possessions of this country, and decimating the inhabitants of some of the principal towns in the southern part of the United States; and at the same time the immediate approach of the third visitation of the malignant Cholera to Britain since 1830—are events which may well excite attention, even in the most thoughtless breast, to the laws by which it has pleased Providence to guide these still mysterious agents; and must have suggested to many the important practical reflection, that the study of Nature is adequate to confer great benefits on mankind, even when the powers which we seek to control are apparently beyond our reach, and are hardly susceptible of any modification from all the resources of our art. When the course and effects of a disease are beyond our power, we may still, by a simple induction of facts, ascertain its external causes; and if this knowledge of the laws of nature is allowed the influence which it ought to have on the councils of nations, we may be fully justified in asserting, that we do more for the prevention of sickness and suffering among mankind, by studying the mode of propagation of these diseases, even so far as yet known, and giving that advice by which they may be shunned, than we should do by the discovery of a new remedy, more powerful than any that is known in medicine.

When it is remembered that so late as the time of Sydenham, the greater part of the annual mortality in London resulted from four diseases (plague, small-pox, dysentery, and scurvy), which are still known, and still nearly as fatal as ever to those who are affected by them, but that the number who take these diseases in a given time in this country is now comparatively trifling, simply because their specific causes are known, and may be counteracted,—we have said enough to show, that this *second great object* of medical inquiry and observation is, at the present day, and in this country, a matter of peculiar interest and importance.

Neither can it be said that this department of our science has failed to attract attention, or that the *cupiditas veri videndi* applied to it has been a vain or unprofitable inquiry in the present age. The example, to which we shall afterwards refer, of the *diffuse* or erysipelatous inflammation, the knowledge of its peculiar effects, both local and general, and the different textures of the body which it may affect; and more especially, the careful induction of facts by which it has been put beyond doubt, that, whatever other sources it may have, it is the natural effect of the application to any part of the living body, deprived of its protecting cuticle, of a peculiar *cadaveric poison*, which has been long known to be frequently evolved during the decomposition—we are pretty sure only during a certain stage of the decomposition—of the human body; and the proof that this same poison is the immediate or exciting cause of one of the most distressing kinds of epidemic disease, the puerperal fever;—these constitute a body of information which, imperfect as we must admit it to be, is sufficient, in the great majority of cases in which that malady can present itself, to disarm it of its terrors; not by opposing the diseased action itself, but by enabling us to give advice, by which those who would otherwise be liable to its attacks may be effectually preserved from them. And we shall immediately show, that our information regarding the malaria exciting intermittent and remittent fevers, and likewise regarding the contagious poison exciting continued fever, in this climate, is sufficient, when circumstances admit of its being fairly applied and firmly acted on, to preserve from these diseases the great majority of those who must otherwise be their victims. At the risk, therefore, of "tiresome iteration," we shall resume this subject at some length, endeavouring now to fix the attention of our readers rather on what has been ascertained of the natural history of the specific *exciting* causes of some of the most important epidemics, than of the *predisposition* to them, of which we have recently treated.

Several of the principles which we may regard as established in regard to the Etiology of the epidemic and endemic diseases, are indeed of such importance, with a view to the great practical result of prevention of disease, that they may be said to possess the same value in that view as the contrivance called a *catch* has in the actions of a machine, being a point up to which the requisite actions may be depended on, and from which a new series of actions, designed for some special end, may safely and beneficially originate.

Indeed, so much is this the case, that we think it a serious fault in many of the observations which are continually given to the world on this subject by medical observers, that they do not fix with sufficient confidence and expression of certainty on certain principles which we may hold to be established by decisive evidence; and therefore they seem to leave the grounds of the advice which they give as ambiguous and vacillating, when in fact they may state these as invested with the certainty and precision of the exact sciences. We need not say that if there are principles on this subject, which we regard as put beyond farther controversy by the evidence already obtained in support of them, it is doubly incumbent on us, on that account, to reflect carefully on the grounds of those opinions, and be prepared to show that the facts have been accurately observed, and that our inferences have not gone beyond what strict logic will justify.

When we remember that the knowledge acquired by what we call the empirical observation of diseases, and of the effects of various influences, either on their accession or their decline, is greatly in advance of that which we possess regarding their pathology, or intimate nature, we may be prepared to expect that it can only be by observations of individual cases, repeated so frequently as to come under the denomination of Statistics, that we can ascertain the "universality of the facts," which we thus elevate to the rank of principles. It is by the "numerical method," therefore, that these principles must be established; and so much has been said and written in regard to that method of inquiry, that it is worth while to pause for a little, and endeavour to satisfy ourselves as to what may be expected, and what is not to be expected, from the applications of that method in medicine, and particularly in medical police.

First of all, a preliminary error, we believe, of very considerable importance, has been frequently committed in considering this subject, the more dangerous as it comes under the guise of political wisdom and prudence. It has been said that, with a view to the health of communities, as well as to other objects, specific regulations by law, founded on such knowledge as that of the remote causes of disease, among an intelligent people, enjoying a free constitution, are unnecessary or injurious, because such a people will always be disposed to consult their own interest in all their municipal regulations; and that this immediate object of all political communities, if not interfered with by forms or acts of government, may always be reckoned on with as much certainty as in the conduct of an individual; that in the case of at least a community enjoying a good constitutional form of government, true political wisdom consists simply in leaving the regulation of all matters affecting the public health to the good sense of the people, provided only that these shall be duly instructed, and left quite at liberty to follow their own interests.

The true and general answer to this we believe to be, that if "the true test of the excellence of a constitution is to be found," as stated by Professor Stewart,* "in the details of its municipal code," liberty and a good form of government are themselves valuable, with a view to the end of all governments, *ut cives feliciter vivant*, not as a means of good government, but only as a security against bad; and that, truly and essentially useful as we believe that security, in the end, to be, still the experience of mankind, which is more to be trusted than any political opinion, has clearly shown that, at least in the present state of public information, we have no security in the circumstance of political liberty, for regulations affecting the health of a people being either wisely framed, or carefully followed out. Not to mention the deficiencies which may be observed in the regulations for the education of the poor in this country, and again, for the preservation of our people from small pox by universal vaccination, as compared with the state of these matters in some of the despotic states on the Continent,—we may here observe, that in this country, and at the present day, it continually happens that streets are built, edifices of all kinds erected, even towns planned, laid out, and inhabited, with perfect neglect of the simplest precautions for securing ventilation or draining; or promoting, in the simplest ways, the purity of the atmosphere. We need give only two illustrations. The first is supplied by a letter published in the *Times* newspaper of September 30, 1853, describing the condition of a piece of ground in the vicinity of the Hackney-road, which is at the disposal of so benevolent and intelligent a body as the governors of St. Thomas's Hospital. Eight or nine years ago, we are informed—

"This land was let by the hospital to a speculative builder, for a long term, at a high rent, but without any conditions in reference to drainage. The consequence was, that a large number of houses was speedily built, and to every house was attached a small, badly constructed cesspool; the ground being very retentive, these cesspools are continually in a state of overflow, and the whole soil is completely saturated with the filthy overflowings, the stench from which is at times most sickening and disgusting. If any of the subscribers to the hospital will take the trouble to inspect the rows of houses on the right hand side of Fellow's-street, leading out of the Hackney-road, they will see a state of things which is a disgrace to a civilized country. The only wonder is, that typhus and cholera should ever be absent from a spot so carefully prepared for their reception."

The other case is the town of Merthyr Tydvil, which has been very rapidly extended of late years, in connexion with the iron trade in its vicinity, and which was allowed to attain the population of 37,000 souls, before any one thought of providing a drain for any of its houses; after which time, of course, the construction of drains became an object of much greater expense and difficulty than it would otherwise have been.

If farther illustration of this principle were wanted, we could easily produce it, by referring to the "fetid marshes of Montfauçon, at Paris," or the "Stable Nuisance," lately reported on—in the hope that a favourable opportunity for suppressing it is presented by the present alarm—by the medical profession in Glasgow.

We do not think we go too far in asserting that even in this country (and, *à fortiori*, in most other countries of the world) we can have no

* Life of Adam Smith.

security for the interests of the people in regard to health—especially the interests of the poorest of the people, whom we know to be the most liable to disease, especially to epidemic disease, and whose health, with a view to the general interests of the community, is therefore of the greatest importance—being adequately provided for, otherwise than by making them, under certain regulations, the care of the State, and extending to them the protection of the Law.

In every inquiry, directed with such views, regarding the efficiency of any alleged cause, either of the accession or abatement of a disease, the greatest difficulty will very generally be found to result from the circumstance simply and shortly stated by the late Mr. Playfair, in his 'Commentary on the works of Bacon'—the impossibility in these inquiries of commanding all the conditions of any experiment, or contrived observation, so as to leave out one after another of these conditions in each repetition of the observation, and have an *instantia crucis* as to the influence of any one of them on the result.

"The *instantia crucis*," he says, "is of such consequence in all matters of induction, that wherever it is unattainable,—a part of the conditions of every observation we may make being fixed by Nature and beyond our control,—there must necessarily be a great want of conclusive reasoning. This holds of *medicine* and of *political economy*. Making two observations exactly alike in every respect but one, is what the *instantia crucis* and the method of induction in general especially requires, but it is what in these sciences, for the reason now given, can hardly ever be accomplished. Men deceive themselves continually in such cases, and think they are reasoning on facts only, when they are in reality reasoning on a mixture of facts and hypotheses."

In most cases, the true and legitimate mode of overcoming this difficulty is simply, to multiply the observations—especially the comparative observations—made on cases to which any alleged cause, either of the accession or abatement of a disease, has been applied, distinguishing as accurately as possible those in which the other conditions of the observation have varied. Although in every one of these some peculiar unperceived conditions may no doubt exist, which are not found in others, and which may affect the result, yet, if the observation is repeated a sufficient number of times, and under all varieties of circumstances which *can* be perceived, it may confidently be expected that these *unperceived* or uncontrollable variations in its conditions—some of them favouring, and others opposing, the effect of the cause which is under trial—will ultimately destroy one another, and the efficacy or inefficacy of that cause, known to have been common to all the cases, will be made manifest.

But although the general principle of multiplication of cases, in order to exclude those antecedents of the event under consideration, which may have escaped our observation or been beyond our power, is that on which we must chiefly rely, in applying the method of induction in many of these inquiries—i. e., we must put our information in the form of *statistics*—yet it is easy to perceive, on a little reflection, that several fallacies must peculiarly embarrass all such applications in medicine. In stating these, we are so far from wishing to set aside this method of inquiry, that we shall very often find it to be only by further application of statistics, consequent on further reflection, and subdivision of the subject, that errors otherwise unavoidable may be corrected, and that we can proceed, in the way of induction or exclusion, until we find ourselves, in the phraseology which has lately become prevalent among some of our northern theologians, "shut up to certain conclusions." We set aside the doctrine, *Testimonia ponderanda sunt, non numeranda*, as distinctly inapplicable to many inquiries in medicine, but nevertheless maintain with confidence, that in all cases, *Testimonia ponderanda sunt antequam numeranda.*

I. We may first direct attention to the remarkable difference between such application of statistics to Etiology, and therefore to the prevention of diseases, and to Therapeutics or their treatment. In the former case, the importance of the practical rules which may be thus suggested is very apt to be *underrated*, because the practical result to which we look, on any trial of such rules, is merely *negative*—it is the nonappearance of disease in persons whom we suppose liable to it; and this result, although it may be perfectly well founded in statistics, is not matter of ocular demonstration, and very often makes little impression on the public. In the latter case, the probability is, that the efficacy of the measures under observation will be *overrated*, because the desired result is the *positive* one of recovery of patients; we know, that in almost every case, various causes besides that under trial have contributed to that result; in acute cases, especially, the salutary provisions of nature for the decline of diseases, or, as we may very often more correctly express it, the essentially temporary nature of the diseased action itself; in chronic cases, more remarkably, the unobserved agency of other external circumstances besides the remedy in question. Of the degree in which these causes have contributed to the fortunate event of any individual case, all candid and intelligent medical men will allow that it is very difficult to judge; and without judging of them, we can have no certain inference as to the power of any remedy.

It is, however, pretty generally admitted by medical men, that it is to inquiries in Etiology—i e., into the *causes* of disease and the *means of prevention*—that statistics are really most applicable; but we have not seen so general or satisfactory a statement as we think may be given, of the circumstances of this inquiry, which make it so much more open to the evidence of numbers, than the investigation of the power of remedies.

The questions to which the numerical method is to be applied, in the former case, are generally in reality much simpler and more general than in the latter. Many of the questions of this kind, indeed, even of those on which the rules of medical police or the most useful suggestions for the prevention of disease are founded, belong to that very general class, in which we are not required to inform ourselves of the nature of the diseases which certain influences produce, or fail to produce; the mere amount of sickness and of mortality is enough to establish the propositions in question. When we find that—the average annual mortality in this country being 1 in 45 or 46—the mortality in a particular town or district rises, in any one year, to 1 in 28, or even to 1 in 22; if we are sure that this mortality is fairly ascertained, and that the obvious source of fallacy from immigration and emigration is avoided;—or when we find, that in a particular trade the average duration of life, instead of being 40 or more, is under 30 years, we have the important, although very general principle established, that some of the peculiarities of the mode of life of those among whom such mortality is observed, must be peculiarly prolific of disease. On the other hand, when we have such a statistical statement as that given by the late Dr. Luscombe of the health of a division of the army in Spain, engaged, during a week of almost incessant rain, in an expedition to Arroyo de los Molinos, in which they outmarched and overthrew one of the most active divisions of the French army, passing two of the nights of the week in bivouac without fires, and find that, nevertheless, the number of sick in that division was less during that and the next week, than in any other equal period of the year,—we have in that simple statement evidence, such as nothing but statistics can furnish, of the efficacy of some cause, acting on the constitutions of those men, which must have counteracted the usual effect of cold, wet, and fatigue, in producing disease; and we can hardly conceive that any circumstances common to this large body of men can have had this effect, excepting those to which Dr. Luscombe ascribes it—viz., "exercise and mental excitement."

Even in this simplest case, however, of such inquiries, it has often happened, that the inference at first drawn has been greatly beyond what subsequent experience has justified; and the reason most generally has been, that the conditions of the observation (generally made with a view to the action of some special cause) have been thought to be simpler and more under command than they really were; and the observation has been supposed, therefore, to approach closely to the *instantia crucis*, and to warrant a specific conclusion as to the efficacy of a single cause; whereas farther examination and more minute subdivision of the subject have been necessary, before any statistical principle really of that character could be deduced from it.

Of this we may give an example from the writings of an author whom we highly respect, although we cannot doubt that in this and some other statements relative to the prevention of disease he has fallen into this error. Dr. Southwood Smith observes, in the course of discussions on the effects of air vitiated by decomposing animal or vegetable matter, that the districts of a town which are undrained will very generally be found much more liable to disease, and particularly to epidemic diseases, than those which are drained; from which he infers, that it is by the diffusion through the atmosphere of putrescent matters, which good draining would carry off, that these diseases are produced, or that the poison exciting them acquires what has been called an epidemic influence; and when this statement is taken along with the somewhat hasty assertions, to be found in several scientific works of late years, as to the intimate relation, if not identity, of the process of decomposition of an organized body after death, with the changes effected in a living body by the action of malaria or contagion,—it is not surprising that it should be regarded as an exposition of ascertained truths, very different from what have really been made out.

We have no doubt that the observation itself will be found very generally correct; but in regard to the inference, we must observe, *first*, that the condition of the inhabitants of the undrained parts of a town differs from that of the inhabitants of other parts, in many other circumstances besides the degree of vitiation of the air which they breathe; and, *secondly*, that the vitiation of the air which they breathe depends on many other causes, besides those which draining can remove; it depends on the construction of the houses, and of the streets, courts, or alleys which they inhabit, often such as to make ventilation impossible; it depends especially on the degree of crowding of their rooms, and very often likewise on their own habits: they are very generally the poorest of the artisans, many are, indeed, destitute; they are ill fed, ill clothed, ill lodged, almost always crowded together, and careless as to the use both of pure air and pure water, often inadequately protected from cold, often exposed to fatigue, often addicted to intemperance. It is a fact statistically proved, and more general than any other that has been ascertained in regard to the health of different portions of the human race, that among those who are most in want of the comforts of life, there is the greatest amount of sickness and mortality. In order to ascertain, therefore, by the method of induction, that the unhealthy condition of any poor district of a town, or that the prevalence of any epidemic disease there, is owing to defect of drainage, we must have the subject subdivided, and statistical evidence adduced on its subdivisions, excluding other peculiarities of the condition of the people there, and fixing attention on the results of the deficiency of draining only; which, so far as we know, has not yet been done in this country.

To show the necessity of such subdivision of this inquiry, in order to give to its results that precision which we may hope to acquire by them, we would beg to refer to a paper on the health of the different districts of Paris, published many years ago (in 1825*) by Villermè and Villot, but framed, as we conceive, more strictly on the method of induction or exclusion than any one we can mention—published in the view of maintaining any special doctrine as to the causes of disease,—in this country.

It appears from that paper, that on comparison of the bills of mortality in the different arrondissements of Paris for ten years consecutively, these different districts of the town preserved with remarkable uniformity the same relation to one another in regard to mortality; the average mortality of the whole city being about 1 in 32. the greatest mortality, that of the 10th arrondissement, being about 1 in 24, and the smallest, that of the 1st arrondissement, being about 1 in 40.

These physicians then attempted to ascertain whether this uniformly greater mortality could be ascribed to a denser population in the unhealthy districts—whether estimated by the number of houses to an acre, or by the number of inhabitants to a house; next, whether it could be ascribed to differences of soil, or to vicinity to or distance from the river, or, again, to the fetid marshes of Montfaucon. None of these conditions appeared to influence the average mortality of the districts in which they existed, simply for this reason, that when arranged according to the degree of any one of these conditions, the different districts were found to occupy very different positions from what they did in the order of healthiness already mentioned. But when the different districts were compared according to the numbers of their *pauvres*, i.e., of inhabitants who were excused from payment of taxes on account of certificates of destitution, they were found to arrange themselves, year after year, in the same order

* See Archives de Médecine for that year.

as when compared according to their mortality—the 1st arrondissement occupying the lowest place, and the 10th the highest—so that the examination of the records of the mortality in this city serves only to confirm the general principle long understood, and which is laid down by Mr. Chadwick, in his 'General Report on the Sanitary Condition of Britain,' that the probability of life in any class of men is greater, *ceteris paribus*, as their circumstances are more elevated above destitution, and less as they are in nearer proximity to that condition (pp. 155-157).

Some of the most striking of the results of his inquiry, in districts widely different in other respects, are contained in the following table of average age at death, of

	Gentry and their families.	Tradesmen and Artificers.	Labourers.
Derby	49	38	21
Truro	40	33	28
Manchester	38	20	17
Rutlandshire	52	41	38*
Bolton	34	23	18
Bethnal Green	45	26	18
Leeds	44	27	19
Liverpool	35	22	15

So much in regard to the questions as to medical police, and the prevention of disease on a large scale, which are so general and simple, that they do not imply any exercise of the power of distinguishing diseases, in order that they may be answered; we are concerned only with the number of cases of all diseases, and the number of deaths in a given time, as proportioned to a given population; and we wish it only, at present, to be observed, that it is undeniably by the simple process of multiplying cases carefully under the right heads, as belonging to certain districts, and occurring at certain times, and attending to *all* the circumstances of those districts—i. e., by the "numerical method," that the information on those points, which it is the object of our science to acquire, can be obtained; but that in doing so, much caution and circumspection are often requisite, to prevent our inference from extending beyond the data, and restrain the natural tendency of the mind to assign causes such as *we* deem adequate, for phenomena, which we profess only to be enumerating and arranging.

II. In the greater number of the inquiries which come under the head of Etiology, we require, indeed, the exercise of the power of diagnosis, to fix the titles of the diseases to which these inquiries relate, but we are spared all occasion for the more difficult exercise of judgment, which is required of us whenever we have to give an answer to a therapeutical question, as to the efficacy of any remedy in any disease. In almost every case, when such a question presents itself—hardly even excepting the case of a remedy alleged to possess such a specific power as quinine exerts over ague—it is not alleged that the virtue of the remedy will uniformly and unequivocally show itself; it is only said that *if* the remedy be given at the right time, and with the right precautions, the progress of the symptoms constituting the history of the disease may be expected to be modified—that symptoms which seem urgent will abate—

* This very small mortality may be partly referred to habitual emigration.

that symptoms to be expected will not appear; or that a tendency towards a particular mode of injurious or fatal termination, which had shown itself, will be altered. It is here obvious that the *judgment of the practitioner* on some of the most delicate questions in prognosis and in practice—as to the dangers to be apprehended, or the time and mode of administration of remedies in individual cases—is *one of the elements* of the question we have to decide; and when we reflect on this, we cannot be surprised to find that we can frequently satisfy ourselves much more completely, as to the alleged efficacy of a new remedy, by watching the progress of a single case of a well-known disease in which it is carefully administered, and the subsequent progress of the symptoms accurately noted, than by studying a number of tables, exhibiting the use which has been made of the remedy in a great number and variety of cases, where we have no security for similar equally careful observations having been made on it—sometimes where we are merely informed of averages that are struck, on a subsequent review of the cases, as to the period of the disease when the use of the remedy was begun, the length to which it was carried, the degree of certain symptoms thought to demand it, or the mortality from disease with or without its use.

We do not mean to deny that questions occur in Therapeutics, likewise, as to which large numbers of cases may be compared with advantage, and the "numerical method" applied; but we think we have said quite enough to show, that many sources of fallacy must necessarily embarrass the application of that method to all questions regarding the cause, or combination of causes, to which we should ascribe the abatement or decline of a disease; and that it is reasonable and right, therefore, for practitioners to build their opinions as to the powers of a remedy on observations of very different kinds, besides the mere enumeration, and statement of ultimate results, of the cases in which it is given; or, as it is shortly and justly expressed by a practical author, that in order to make up our minds as to any such question, it is better in general "*to watch than to count.*"

In order to enable us to judge how far any given disease may be ascribed to the influence of any external cause, the only strictly medical question before us is as to the *diagnosis* of the disease, preceded by such alleged cause; and as we rest nothing on the judgment of the practitioner touching the more difficult questions, in prognosis and practice, which we have stated, we may reasonably admit that merely by the force of numbers we may often have a body of evidence brought to bear on this question, to which it would be unreasonable to refuse assent.

III. In order to perceive this more distinctly, it is necessary to attend to one consideration which we think has not attracted so much attention as it deserves—viz., that in those inquiries into the external causes of diseases, the number of individuals to whom our contrived observations or experiments may easily be made to extend is often very much greater than it ever can be in therapeutics; and therefore, if these observations have been carefully made and accurately recorded, they will very soon acquire, by the mere force of numbers, the full force of the *instantia crucis.* When we wish to inquire, by the help of statistics, into the action of any remedy on a disease, our observations are necessarily limited to those persons in whom that disease exists; but when we are inquiring into its remote causes, it very often happens that a much greater number of persons, who remain unaffected with the disease, form, in fact, the most important part of the experiment; and, according to the principle already stated, therefore, the experiment soon becomes decisive, and the subject one of those to which we would apply the observation already made, and maintain that to refuse assent to the principle thus supported, is to mistake the nature of the evidence which the subject requires, and injuriously to retard the progress of the science.

This is most distinctly perceived when a disease previously unknown, or at least long absent, appears in any country or district, and the question is, what are the conditions that determine its appearance. The most important observation in such a case very frequently is, not only that a succession of cases of it occurs in a particular locality, or under particular conditions, but that it occurs then, or under those conditions *only;*—i. e., the great mass of the community, who were previously, and remain subsequently, free from the disease, are the most essential part of the experiment by which its mode of extension is ascertained.

A good example of this kind is given by Dr. Davy, in proof of the contagious property of Plague; which is the more important, as he states that this fact carried conviction to his mind, before uncertain from anything he had previously observed in the East, as to that contagious property. And we perfectly agree with Dr. Davy in the observation, that in such an inquiry, where a single case affords any such strong evidence, it is right to fix our attention exclusively on that one case, and on the extent of information which it conveys, and satisfy ourselves completely as to these points, before undertaking a more general investigation of the subject. This fact was simply, that on a vessel aboard which an undoubted case of plague had existed, being taken into quarantine at Constantinople, and two of the officers appointed for the purpose being sent aboard to examine the cargo, both these officers were seized within a few days with symptoms of the plague, of which one died in the lazaretto—the disease having been, for a considerable time previously, and being thereafter, unknown in Constantinople—i. e., in a population of 800,000 souls.*

Now when we attend to this fact, we think it must appear sufficiently obvious that what makes it decisive is not the mere circumstance of the case of plague having occurred aboard that vessel, and succeeded to others previously occurring there, but it is the circumstance of its having occurred there *only*, at that time. and for a considerable period before and after; and that when this is duly considered, it must be allowed to approach as nearly to the *instantia crucis*—i. e., to an observation made on *many* persons placed in similar circumstances, *in every respect but one*—as can reasonably be desired. For without inquiring into the circumstances of the remaining population of Constantinople, we are quite safe in asserting that among them there must have been great numbers, in all other respects equally liable to plague as these officers of police; but we know that there was no other exposed to the influence of this particular cause—intercourse with the sick; the efficacy of which it is our object to ascertain; and finding that no other was affected by the disease, we infer

* See Notes, &c., on the Ionian Islands and Malta, vol. ii. p. 333.

that the efficacy of that particular cause is established by the true method of induction or exclusion. In all such cases we have a large body of *negative evidence* statistically known, although often not formally stated, to support the *positive* evidence, of individual cases following one another in a particular succession. When this negative evidence is clear and extensive, a very few positive facts become decisive as to the operation of an individual cause, because the cases in which all previous conditions except that one have been alike, being almost infinitely numerous, the influence of that one cause is soon brought to the *experimentum crucis*. But when such negative evidence does not exist, to furnish a comparison with the positive evidence, a great deal of labour may be expended on the statistics of a question of this kind, without any practical result.

We submit the following cases for consideration at this time, with the single observation, that if the facts stated in them are, as we believe, carefully examined and truly represented, we consider them equally conclusive, on the very same principle, as to the fact of a certain contagious property being attached to the malignant Cholera of 1832 and 1849.

The first is, of course, known to us only from the report of the French authorities; as to the others, we have personal knowledge.

1. In 1833, the frigate *Melpomene* arrived at Toulon from Lisbon, at which latter place cholera was raging. The *Melpomene* had lost fifteen men before she started, and more than half the crew had been attacked during the voyage. On her arrival at Toulon, where not a single case of cholera existed, the cholera patients were taken into the lazaretto, where four galley-slave attendants, with an inspector, were sent to wait on them. Four ordinary attendants were also sent on board the frigate. One of the latter was immediately attacked, and died in eight hours. On the next day, two others, who likewise died. The fourth was also attacked, but escaped. Of the four *galley-slaves in the lazaretto*, two died on the second day, a third soon afterwards, and the inspector on the fifth. The disease did not spread beyond the precincts of the lazaretto, and *Toulon remained free from it for two years.**

2. In the month of January, 1832, when cholera was prevailing in Musselburgh, but no case had yet originated in Edinburgh or Leith, the whole population of these towns (above 150,000 people) was more completely under observation than it probably ever was before, or is likely to be again: in hourly expectation of the unknown pestilence, to which every one's attention was turned, medical men appointed to each of the police districts, to give assistance to all who had no medical attendants of their own; stations in each district open night and day, where reports were to be made and their assistance could be had, and all the medical men anxious to report their cases immediately on their occurrence. In these circumstances, it is quite certain that the first case which originated in Edinburgh or Leith was that of widow M'Millan, of which an account was published by Dr. Simpson, in his paper on the 'Contagious Propagation of Malignant Cholera,' in the 'Edinburgh Medical and Surgical Journal,' April, 1838; that this woman's son had been in Musselburgh and slept in a house where the cholera was, on the Monday; that on the Wednesday, having returned to Edinburgh, he was seized with vomiting

* Medical Times, vol. iii. new series, p. 515.

and purging of watery matters, cramps, and the other symptoms of cholera, so as immediately to excite the suspicion of his having been at Musselburgh, and so exposed to the poison, which was at the time denied, but afterwards confessed; that he was nursed and frequently rubbed by her in a small confined room on the Wednesday and Thursday, and recovered under treatment; that she herself, an aged and infirm woman, was never out of her own *close*, during that week, or many months previously; that she took the cholera on the Saturday, and died in ten hours; and that the disease originated in *no other inhabitant of Edinburgh or Leith for at least ten days thereafter*. When these points had been all satisfactorily established by examinations before the sheriff, it appeared to us, as it does still, that on the true principle of induction or exclusion—just as in the case quoted from Dr. Davy, and in that from Toulon—the evidence of this disease having a contagious property was conclusive; by which proposition we mean merely, that it may be excited by intercourse of the healthy with the sick, without pledging ourselves to any opinion on the mode of communication from the one to the other; and that farther statistical evidence than this one case, the only one out of 150,000 persons resident in Edinburgh and Leith, and not going to Musselburgh or to any other affected locality, in whom the disease appeared, during that time, is not necessary to establish that point.

3. In Dr. Simpson's paper, published in 1838, and in the 'Edinburgh Monthly Medical Journal' for the year 1849, several other instances are recorded of the importation of cholera into detached localities in Scotland, where the evidence of its extension by intercourse of the healthy with the sick is exactly of the same kind, and seems to us equally conclusive; we may mention, in particular, the case of the little town of Bathgate, containing nearly 3000 inhabitants, where four cases of cholera, all fatal, occurred in April, 1832, in strangers who had come from infected districts; two cases immediately after, one of them fatal, in women who acted *as nurses* there, and *no other case*, during the whole of that epidemic, in that town, or in any house or village within seven miles or more in any direction. (See 'Edinburgh Medical Journal,' 1838, p. 361.) Again, the case of the little town of Dollar, into which the disease was introduced on the 12th May, 1832, by a young woman a native of Dollar, but who resided and was taken ill at the Devon Iron-works, about four miles off, which had been severely affected. This young woman came to her mother's cottage in Dollar with the disease upon her, was sent back in a cart the next day to the iron-works, where she died that night (the 13th). Her mother, who had never been out of Dollar, was put into a house a little way out of town, previously prepared as a cholera hospital; there she was seized with the disease in the most unequivocal form on the 14th, and died on the 15th; and we can say with certainty, from happening to know that town well, that *no other* case of malignant cholera has ever occurred in the inhabitants of Dollar from that day to this. To the same purpose, we may mention, among the cases of importation recorded in the year 1849, the village of Dalmellington, in Ayrshire, where a man from Kilmarnock, a town then affected with the disease, fell ill on the 24th January, and died on the 25th; a woman who lodged in the same house with him, and who washed his clothes after his death,

took the disease on the 28th and died on the 29th, and *no other case* had occurred up to the month of June—i.e. above four months thereafter—when the account of these cases, by Dr. Cruickshank, was published in the 'Monthly Journal.' In like manner, we know that the first appearance of cholera, in the year 1849, in any part of Ireland, was in Belfast; that the first person affected there, and who had the disease upon him when he arrived there, came from one of the affected localities in Edinburgh (College-wynd); that he was taken into the workhouse there, and that after him there was a succession of 30 cases *in that workhouse, and there only;* not only before there were any others in Belfast, but *before there were any others in Ireland.**

The evidence here is precisely similar to that afforded by the case quoted from Dr. Davy, but it has been wholly misapprehended by such intelligent authors as the editors of the 'Edinburgh Medical and Surgical Journal,' who, after publishing the cases above quoted from Dr. Simpson, comment upon them as follows:—"Although the fact of succession in the attacks be established, it does not follow that this succession indicates the relation of cause and effect. We are not entitled to infer, when we establish the fact of succession of these two events, that we thereby establish the relation of causation."† This is certainly mistaking entirely the nature of the inference which is drawn from these cases, because we have already stated that what we rely on is, *not* the fact of a succession of cases *having occurred,* among those who had intercourse with the sick on each of these different occasions, but the fact of these successions of cases having occurred *among them only*, and for a great length of time, while great numbers of others, living in the same neighbourhood, have been similarly situated in every respect but that one, and remained unaffected—i. e. it is the *positive* evidence of a few facts supported by the *negative* evidence of a very great number; and this, as we maintain, affords evidence which, if we are to proceed on the principle of induction or exclusion, we *must* regard as decisive.

We regret to say, but it seems to us obvious, that this evidence is equally misapprehended in the following general statement made by the Board of Health; at least, if this statement is meant to apply to the case of Cholera:

"Persons who see disease only on a small scale, and who observe that within that small circle one attack is often followed by another in the same family, and that by another, and so on, naturally believe that the second case is caused by the first, and the third by the second, and thus refer the whole series of events to contagion. *It is on this description of evidence that the entire structure of quarantine is based.*" (Second Report on Quarantine, p. 132.)

This, we say, is asserting that we trust to the *positive* evidence of a few cases *only*, as determining whether or not a disease is contagious; whereas we have fully explained, that we trust to that positive evidence only when supported by a great body of *negative* evidence, distinctly *excluding* the operation of any other cause; and we maintain further, that when it is so supported, according to the true principles of induction as known since the time of Bacon, we are not only entitled, but *bound* to trust to it.

As to the poison which excites the cholera attaching itself to fomites, and

* See M'Cormac—Directions for Management of Cholera, p. 5. Additional evidence is furnished by the late outbreak in Arbroath.—See Chronicle.

† Loc. cit., p. 406.

therefore affecting peculiarly those who, without such precautions as the use of lotions or fumigations of chlorine, come into close contact with, or wash the clothes of persons who have had cholera, we may give the following further statistical evidence, just similar to that afforded by the cases above quoted, and, as we maintain, equally conclusive.

1. In the year 1832, the cholera prevailed much among the fishermen at the town of Wick, many of whom, at the time of the herring fishery, congregate there from various quarters. Among others, some sailors from the town of Banff took cholera at Wick, and died of it. A boat carrying their clothes soon after came across to Banff, and several women, relatives of the men, but who had not left Banff, were employed to wash these clothes. Three of these women took well-marked cholera immediately, one of them before leaving the boat where she had been thus employed, and one at least died of it; and we were assured by a very intelligent practitioner from Banff, that these were *the only cases* that occurred in the town of Banff, or, indeed, along the south coast of the Moray Firth, in that year, or from that year to the present.

2. The following case, the particulars of which are published in the 'Monthly Journal of Edinburgh,' for August, 1849, was particularly investigated by Dr. Robertson, who acted as physician to the Cholera Hospital, and as secretary to the committee appointed by the Edinburgh colleges to investigate cholera in that year.

On the 3rd of March, a woman belonging to Campbelltown, in Inverness-shire, at least 50 miles from any place where cholera existed, arrived there from Glasgow, bringing with her a box containing blankets and other clothes which had been used by her sister-in-law, who had died of cholera in Glasgow in the end of January. On the 14th of March she washed these blankets and other clothes, and poured the suds into a drain between her house and that of her next-door neighbours, a father and son, who were engaged in thatching their own house, and used for the purpose a quantity of clay lying close to this drain, besides coming into the woman's house and talking to her. On the 16th of March, both father and son were seized with cholera, and both died on the 17th. Between that day and the 1st of April there occurred in that village 39 more cases, and 12 more deaths; with very few exceptions, intercourse with persons previously affected could be traced in all these; but the decisive fact here is, that when the disease under those circumstances broke out at Campbelltown, it existed *nowhere else, nearer than Glasgow.* (Monthly Journal, &c., p. 1010.) Here, as in the other cases, we do not draw our inference merely from there having been a succession of cases *there*, but from there having been this succession of cases *there only*, and *then only*, in a district extending probably not less than 100 miles square, notwithstanding that the epidemic influence had existed in one part or another of Scotland since the previous October, and a very large number of negative observations therefore exist to support the positive one.

In connexion with this remarkable fact, of communication of the disease by the clothes of persons who have had cholera, we cannot help mentioning several cases, in which it appeared to us in the highest degree probable that it was communicated by the clothes of persons (as in the case of Campbelltown above quoted) who had been in contact with cholera cases, but had not themselves taken it, and in several of these cases never did take it. For example; when the disease was not indeed prevailing very generally, but spreading in several localities in Leith in 1848-9, the mother of one of the young medical men there had been confined to the house by a state of general feeble health for some months previously, and was in a state of great anxiety about her son, who was visiting cholera patients, often late at night, on which occasions she had repeatedly sat up long past her usual hour, to receive him. In these circumstances she took the disease unequivocally, and died of it; and we could hear of no other case in the street in which she lived, or, indeed, in that quarter of the town. Soon after, we went 9 miles from Edinburgh to visit a medical man who had taken cholera, and had died before we arrived. This gentleman had been in attendance on a poor woman (the first case seen in his village) who had been travelling from the south, and had taken cholera before entering the village. His sister, who lived with him, in feeble health, and anxious for his safety, had taken the disease two days before he did, and died some hours before him. A still more striking case was that of one of the first families severely affected in Edinburgh in October, 1848; a family of 8, living in a small room in New-street, Canongate, of whom 6 took the disease, and 4 died of it, between the Sunday and Wednesday. It was denied at first that this family had any communication with Newhaven (the place first affected in Scotland, and, as was believed at the time, in consequence of communication with ships from the Baltic), but one of the daughters afterwards admitted that she had walked through the village of Newhaven two evenings before the first of her sisters was affected, although she did not admit having gone into a house there. This girl was the third of her family that actually took the disease; but considering that the disease undoubtedly existed at that time in Newhaven, and that it had not then shown itself in more than *three or four houses in Great Britain since the year* 1833, we cannot consider her communication with Newhaven to have been only an accidental coincidence. And as it is universally admitted that the specific virus exciting puerperal fever has very often been communicated to patients by medical men, who were, and continued, in perfect health, there is nothing unreasonable in the supposition that cholera may be thus transmitted also.

Again, in regard to the affection of nurses who have, of course, close intercourse with patients in cholera, we can give the following statistical facts with perfect certainty:—1. That in the year 1832, at the time when the whole number affected in Edinburgh had amounted to 1 in 1200 of the population, the proportion of the nurses employed in the hospitals, who had taken the disease, was 1 in 5; and 2ndly, that during the epidemic of 1848-9, the number of nurses employed in the cholera hospital in Edinburgh was 18, of which number 5 took the disease, and 3 died of it. At the same time when this disease was going on in this small hospital, a much larger number of nurses (45), living in the Royal Infirmary, only a few paces distant, employed by the same managers and under the same matron and superintendant, paid, fed, and clothed in the same manner, were engaged in similar occupations, relieving one another in like manner, but tending on patients in other diseases (fevers among the rest) not in cholera; and among these, no case of cholera or resembling it occurred. In the Cholera Hospital at Glasgow, 84 nurses were employed, 9 took the disease, and 4 died of it. In all, of 102 nurses 14 were affected, and 7 died. Having the evidence of these statistical facts, on a comparatively small scale, to indicate that this disease may be communicated by intercourse of the healthy with the sick, we should next remember that on a large scale, as has been illustrated by Dr. Holland, Dr. Graves, and others, this disease has always been found to follow the *lines of human intercourse*, and particularly that two facts have been repeatedly observed in regard to it, on such a scale as to entitle us to call them statistical facts, and which seem to indicate that its exciting cause does not often extend to any great distance from living human bodies. 1. That it has been distinctly observed to make its way in opposition to the trade or monsoon winds, and 2, that it is never observed to make its way from one place to another more rapidly than human beings can travel.

When these different facts are duly considered, we maintain that we do not go further than statistical evidence will support us, in asserting that this disease may be propagated by the intercourse of the healthy with the sick, or with something that has been thrown off from the bodies of the sick; that to doubt the truth of this principle, is to betray an "unmanly want of confidence in the clear conclusions of human reason;" and that any regulations for checking the extension of the disease, which proceed on the supposition of its having *no contagious property*, must be held to be essentially faulty.

But agreeably to the principle above laid down, we must be careful not to carry this conclusion too far. It may seem a contradiction to say that a disease can be both contagious and non-contagious, but there is no inconsistency in saying, that the poison exciting a disease may both be propagated by contagion—i. e., by intercourse of the healthy with the sick, and likewise may, either through the atmosphere or in some other way, be diffused, to a certain extent, over the surface of the earth, independently of any such close intercourse. Various theories have been proposed to explain this. That which perhaps it is most important to keep in mind, because it agrees best with an important part of the phenomena of this and other diseases, is that which ascribes it to swarms of microscopic animalculæ, or vegetative germs which are taken into the bodies of persons exposed to them, multiply there, and excite the disease, but are capable of diffusing themselves through the air, and of attaching themselves to, and *rapidly multiplying on*, other matters likewise.* There is no decisive evidence for or against any such theory, and the microscopic evidence, so far as it goes, is clearly against this one; but we should not do justice to the applications of statistics to this subject, if we did not state shortly the evidence we have for the disease on some occasions—perhaps especially in certain places and in the hot climates—extending itself independently of any close intercourse of the healthy with the sick, and requiring, in order that its extension may be checked, other means besides that separation of the sick from the healthy, which is known to be so effectual in the case of continued fever or of plague.

* See Holland On the Hypothesis of Insect Life, &c., in Medical Notes and Reflections.

In asserting this to be the fact, we do not lay any stress on the merely negative fact often adduced on the subject, that many persons have close intercourse with the sick, and nevertheless escape the disease. This observation has, indeed, been very frequently made by all who have seen much of the disease in any climate; but when it is stated as proof that the disease has no contagious property, the simple answer is, that if it prove anything, it proves too much. Those who have had close intercourse with persons already sick of cholera and escaped, have likewise breathed the same air—they have generally inhabited the same place,—they have, in almost every case, been exposed to the same epidemic influence, *of whatever nature that may be,* and escaped. If their escape proves anything, therefore, it proves that we were mistaken in supposing that the disease belongs to the class of epidemics—i. e., that it depends *on any cause* which is of local and temporary existence only; which inference, being in opposition to the much more general statistical facts, of total absence of the disease from many millions of men and for long series of ages, and rapid occasional extension within narrow limits of time and space, must be held to be erroneous.

This negative fact, however, taken in connexion with such positive facts as have been stated on the other side, is important, as indicating one general principle in regard to the imperceptible cause of cholera—that it is liable to most remarkable variations in its influence on the human body on different occasions. This principle is not essentially different from what may be observed in regard to all epidemic diseases; indeed, if they were not liable, more or less, to such variations, they would cease to be epidemics; but the variations of effect are more sudden, more striking, and seem less capable of being reduced to any perceptible law, in the case of cholera, than of any other. Many facts are known, however, which show that they are mainly dependent on two principles already mentioned, and exemplified in the case of other epidemic diseases, although in a less extraordinary degree—viz., alterations of the intensity of the virus itself, and peculiarities of constitution, giving a predisposition to its influence in some persons much more than in others.

But in asserting that the cholera must have a mode of extending itself among mankind, independent of any close personal intercourse of the sick with the healthy, we do not trust to the negative observation above stated, but to this distinct positive observation, that in many instances, when the disease is prevailing extensively, the proportion of cases occurring in certain districts among those who have no known intercourse with the sick, is at least as great as among those who have close and continued intercourse; and further, that the disease appears in certain localities, affecting numerous detached individuals almost simultaneously, while not only neighbouring localities, but the attendants on the sick in these, remain unaffected.

Notwithstanding the distinct evidence above stated of the disease showing a contagious property on many occasions in Scotland, we have ourselves seen many instances, and been assured of many more, in which this immunity of the immediate attendants on the sick, and nearly simultaneous affection of many within a limited district, who had no direct communication with one another, has been strikingly exemplified. The late Dr. Reid, of St. Andrew's, whose accuracy of observation is well known, and who went from Edinburgh to Dumfries, and took charge of one of the districts of that town, at the time when it suffered more than any other town in Scotland from the epidemic cholera, in 1832, expressed himself distinctly as satisfied, as all the medical men in Dumfries at that time were, that those who had close intercourse with the sick at that time in that town, were not affected with the disease in a larger proportion than those who avoided such intercourse; and Dr. Blacklock, of Dumfries, who saw much of the disease when it prevailed again pretty extensively there during the winter of 1848, found that the same remark might be made upon it at that time. The same is stated by several of the medical men who have recorded their observations on cholera in the 'Edinburgh Monthly Journal' in the year 1849.

A more unequivocal statistical proof of this disease having a mode of epidemic extension independent of actual intercourse with the sick, is given by cases on record in India, in which it has affected severely large organized bodies of men in particular localities, and abruptly ceased on their being removed from these, without affecting peculiarly, sometimes without affecting at all, the attendants of the sick, or those who came in contact with the sick, in those other districts; so as to justify the expressive phrase employed by some of the Indian practitioners, that there are "tainted districts," varying in different seasons, to which the disease is confined.

Some of the cases of this kind given by Dr. Hamilton Bell seem to admit of no other interpretation—e. g., the case quoted from the Bengal Official Report, of a detachment of 90 men of the 26th Native Infantry, who halted, on their way to join the main body at Tangor, on the evening of the 11th May, 1818, after an ordinary march, on the banks of a small lake, under a fortress, but on open ground. About midnight the first man sickened, and died in half an hour, and by sunrise 20 men out of the 90 were affected. They were all carried forward in carts and doolies, but by 11 A.M. on the 12th, when they reached their ground, four were dead and two more moribund, and before the end of the week, *every man of this detachment* was in hospital with cholera, or at least some form of bowel complaint. These men were mixed promiscuously with the others who had been stationary at Tangor, but none of these last men were affected.* Here it may be doubted whether these 90 men who were thus affected nearly simultaneously, caught the disease on the banks of that lake, or whether they had imbibed it previously; but it can hardly be supposed that they infected one another, and it is certain that they did not infect others; so that the epidemic must have had a cause independent of contagion.

The most striking exemplification of this principle, of the diffusion of the disease being confined to districts, and often unconnected with intercourse of the sick and healthy, was in the case of the army of Lord Hastings, amounting (including camp-followers) to nearly 100,000 men, where—

"When the disease was at its height, and all business had given way to solicitude for the suffering, the army was moved, on the 13th November, 1817, and

* Bell on Cholera, p. 77.

after marching above 40 miles, and reaching the high and dry banks of the Bottah, halted on the 19th. The road, it is stated, was strewed with the dead and dying, and the ground of encampment presented the appearance of a field of battle. But here the army got rid of the pestilence, which ceased to be epidemic on the 22nd; so that this crowded camp, carrying thousands of sick along with it, by moving 40 miles in less than ten days, *shook off the disease*." (Bell on Cholera, p. 76.)

A case formerly related in this journal,* to the same purpose, seems free from all objection, and to be a statistical document as satisfactory as can be desired. In this case, one wing of a cavalry regiment just arrived from England, and in high health, ascended the Ganges from Calcutta (where there was no cholera at the time) in boats. At a certain period of the voyage, the troops arrived at a part of the country where cholera prevailed in the villages on the banks of the river, but with which they did not communicate. Here cases of cholera occurred; they were advised to push on rapidly, and after a few days, when they had passed the limits of the existence of the disease on the banks, it ceased to show itself in the boats. But what makes the case peculiarly conclusive is, that the other wing of the regiment followed afterwards by the same mode of conveyance, became "affected with the disease at the same point, and lost it again at the same point;" so that the limits of the tainted district seemed to be as clearly marked out by the voyage of the one detachment as of the other.

And when we state this evidence of the disease having a method of diffusing itself independent of any contact or close intercourse of the sick with the healthy, we should also remember that it is a general fact, in regard to its extension over the earth's surface, that, although following the great lines of human intercourse, it has made its way, not uniformly, indeed (as the case of the Island of St. Louis shows), but very generally, in spite of cordons and quarantine regulations. Indeed, any one who has observed its sudden and capricious outbreaks even on a small scale, in different parts of a large town or district of this climate, must be strongly impressed with the conviction that it must have some way of diffusing itself—whether through the atmosphere, as many suppose, or along the earth's surface, or under the earth (as some have conjectured), and of *multiplying itself at the points which it reaches*—which we can hardly conceive that any restrictions on human intercourse can surely or uniformly counteract.

But having stated the evidence which seems the most decisive on both sides of this disputed question, we may again ask, Why not admit that the disease can extend itself, and that we ought to be prepared with means of limiting its extension,—in two distinct ways;—one simple and easily understood, the other, more obscure, but equally ascertained and probably even more destructive? Is it not well ascertained, in regard to other diseases, that they may be excited and even spread epidemically, under certain circumstances, both by contagion or the more unequivocal process of inoculation, and independently of it? As to the erysipelas and the dysentery, we have seen cases which enable us to answer this question in the affirmative, to our own satisfaction, and from personal observation. As to the yellow fever, we have repeatedly said that this appears the most reasonable explanation of the facts, which were fully ascertained by Dr.

* Vol. ii. p. 70.

M'William, as having occurred at Boa Vista. And although we know nothing of the intimate nature of the influence producing the cholera, yet if we put together the facts which have been statistically ascertained on both sides of this disputed question of contagion, and others, perhaps equally important, which are admitted on both sides, have we not obtained an amount of knowledge relative to the laws which regulate its extension, —not, indeed, complete or satisfactory, but highly important, and from which practical rules will emerge, according to the circumstances of individual cases, more truly useful than specific directions founded on more limited views of its causes?

The peculiar virus which produces this disease is known to us only by its effects on the human body, just as electricity or galvanism, or even caloric, is known to us only by its effect on other bodies; and it is by statistical evidence, therefore, that we have acquired all the information we possess concerning it. But in this manner it is known to vary remarkably in its own intensity at different times and places, to be altered by certain agents, and to affect some persons much more than others. We know that it has hitherto been more intense and general in hot climates and seasons than in cold; that it has appeared on different occasions to be influenced by electricity, having abated considerably in effect after a thunderstorm; that it is influenced by moisture of the atmosphere, producing its effect most remarkably in low and moist situations; that it is influenced, like other epidemics, to a certain degree, and its effect increased, by vitiation of the atmosphere from other causes, such as decomposing organic matters. We know that there are certain towns and districts which show a peculiar, unexplained liability to it, being affected severely in different epidemics, as, e.g., Dumfries in Scotland; while others have shown as remarkable an exemption, such as Birmingham; and this fact may lead us to suspect that, besides mere elevation (which has, however, a greater effect), there is something, not yet ascertained, in particular soils, which favours the development of the disease; above everything else, we know that it is influenced, like other epidemics, and more rapidly than any other, by *time*, the disease being always most virulent when it first appears in any town or district, gradually abating in intensity, as every epidemic affecting either vegetables or animals sooner or later does, and ultimately dying out. We know, further, that it is aided remarkably in its effect on the human body by various concurrent causes, which may frequently be avoided,—by exposure to cold, wet, fatigue, imperfect nourishment, especially by disorder of the stomach or bowels, and by intoxication; we know that it takes effect most easily on those in a feeble state of health, remarkably on women during pregnancy and lactation, and in those in whom organic internal disease exists; and yet not on those in whom such disease exists as keeps up a febrile action—few persons affected with tubercular disease, at least of the lungs, falling victims to cholera. We know also, from such facts as have been stated, that, in this country at least, it arises frequently from the human body when affected by the disease; but that it may not only be conveyed, somehow, to a considerable distance from that origin, but *multiply and increase in energy* in certain situations when so removed from living bodies (in which respect it would not seem to differ from that peculiar matter, of low organization, which produces the aphthæ, nor from that cadaveric poison which excites the erysipelas, or the puerperal fever; and may probably, as we shall see that this last pretty certainly may, be counteracted by the use of chlorine); we know that it may attach itself to clothes and goods, and there multiply and increase in virulence. And to this we add a curious fact (although one which is known only by negative observations, and therefore not advanced with absolute confidence), that it does not attach itself to the *dead body*, at least to the body in a certain state of decomposition; for it is certain that the dissecting rooms in Edinburgh were supplied during the greater part of 1848-9, as they were in the year 1832, almost exclusively by cholera subjects, and in neither year was there a single case of the disease among the numerous students attending these rooms.

But what we think especially important to be observed is, that this virus, whatever its origin, shows a remarkable tendency to attach itself *to places*. There are *tainted districts*, in this country as well as in India, in which, even when epidemic, the disease is observed to almost exclusively prevail; and this leads to what we believe to be most important in the way of prevention. The strong representations made by the Board of Health in this country, of there being a peculiar liability to this disease in damp, filthy, and ill-drained situations, are, we believe, quite true, but they are certainly not the whole truth. The "tainted districts" which we have seen have been often less peculiar in their nature, and always much more *limited in extent*, than could have been supposed from these representations.

Thus, in the case of a family, one of the first affected in Scotland in 1849, several of whom were taken into the hospital in 1849, from New-street, Canongate, and in which there were 6 cases and 4 deaths within four days, the tainted district in which these cases appeared was a *single room*, on the *second* floor of a common stair, which leads to several winding passages and to *many* rooms, all ill-aired and dirty, and most of them having windows which open into a small court containing often receptacles of dung. But it is quite certain, that neither at that time nor since has any case occurred in any other of these rooms. Cholera appeared likewise at that time in Burt's Close, in the Grassmarket, where it might be supposed to be owing to the same circumstances of dirt and deficient ventilation, which had attracted notice when that close was on several occasions a "*nest of fevers*;" but on inquiry it appeared that all the cases of cholera were from a different tenement from any of those which had formerly furnished the fever cases. It was said, indeed, at the time, that the tainted district in this close was connected with the Greyfriars Churchyard, on which it is quite true that the windows of the rooms that were affected open; but it is equally true that the windows of two other suites of rooms in the *lower* stories of the same common stair open on that churchyard, and yet that in these there was not a single case of cholera. In this as in other cases as to cholera, as formerly in regard to fever, we have seen the tainted districts confined to the upper floors of lofty tenements, while the lower, notwithstanding dirt and defective drainage, have remained free from the disease. We have seen it prevail and spread on the very pinnacle of the Old Town of Edinburgh, the highest story of the highest houses on the Castle Hill, while the lower stories of the same were exempt. At the Water of Leith in 1832, and again in the spring of 1849, it appeared in one of the situations in which it might be expected, in a damp and dirty row of houses on the *left* bank of the river; but in several villages, almost exactly similarly situated, a little higher up on the same river, it never appeared; and when it broke out again in July 1849, at Water of Leith, the tainted district was confined to the *right* bank of the river; on examination of which it then appeared, that in point of dirt and defective drainage this portion of the village had long been at least as favourably circumstanced for the disease as the other, notwithstanding which it was perfectly unaffected, both in 1832 and in the main epidemic of 1849.

Another curious fact, which seems well ascertained in regard to this virus is, that those very limited districts in which it has once shown itself certainly continue liable to outbreaks of it for a considerable time, insomuch that if measures of purification are employed, and the disease does not reappear, there is a strong presumption of those measures having been really useful. We have seen more than one place where it reappeared in the same room where it had been two months before, the inhabitants having changed; and two places in which a few decided cases appeared as late as October, 1833, just a twelvemonth after it had existed in the same houses previously. In the town of Hawick, which suffered severely by epidemic cholera (imported, as was believed at the time, from Newcastle) in January and February, 1832, there was a sudden fresh burst of the disease in October of the same year, four cases occurring on the same day at four distinct parts of the town, but all of them in houses which had suffered before, eight months previously. And in this and other cases of fresh outbreaks of the disease, the agency of auxiliary or concurrent causes might be distinctly observed, all the persons now affected having been exposed to cold, wet, and fatigue on digging-up potatoes, on the day before their attack; and, in other cases, the occurrence of attacks of cholera in houses long previously affected was distinctly traced to intoxication.

From this tendency of the disease to localize itself *within narrow limits*, and at the same time, from the admitted agency of some such concurrent causes in exciting it, arises the obvious importance of the measure which was adopted in Edinburgh as early as February, 1832, and which has been recommended by the Board of Health in London during the present year, that of establishing Houses of Refuge, into which the persons from those tainted districts apparently the most liable to the disease, might be received, immediately on the violent attack, or the death or removal to hospital of the first cases, where those persons might be regularly fed, and preserved from cold, wet, and fatigue, and from the use of strong liquors, at least until the time was over when the most rapid successions of cases have been observed to occur, and until the rooms where the disease had broke out had been cleaned, fumigated with chlorine, and thoroughly aired. In the houses of refuge they would of course be kept under observation, and any attack of bowel complaint be immediately met by medical treatment. Of course this resource would only be availed of by a portion of the people in any such district, but we have known different cases where it was availed of by workmen in regular employment, with their families; and it would suggest similar measures to others having the means of taking such steps themselves; and we may conclude these observations on cholera with a brief statement of the results which followed these measures in Edinburgh, both in 1832 and 1849.

In the first three months of the epidemic of 1832 at Edinburgh (which was always very partial in its attacks), the number taken into these houses of refuge was 353 from about 70 houses in Edinburgh and the Water of Leith, 248 of them from this last village, all of them not only from tainted districts, but *tainted rooms*, in which decided cases, most generally deaths had occurred. Of these 353, 15 took the disease, and 7 died after removal to these houses; the remaining 346 escaped. How many of these would have been affected if they had remained in the tainted districts can, of course, only be conjectured; but certainly it was the general fact, that when the disease appeared in the houses of the poor, and no such measures were adopted, several cases, and even several deaths, occurred in almost every house, particularly at the beginning of the epidemic; and at the Water of Leith, where, in 1832, it broke out suddenly and violently, but where the removal of the people was most satisfactorily effected, we had the satisfaction of finding that only in one-fifth of the families where the disease showed itself was there a second case; and what was still more striking, there was only one family in that village in which two deaths occurred, and that was a family which had refused the offer of removal to the house of refuge. In the later period of that epidemic, we can only speak with certainty of what happened in a small district of the town, in which the disease broke out in seven different houses; the inmates of these, not affected within the first twenty-four hours, were all got into the refuge, and the disease went no farther in any one case.

During the last epidemic in Edinburgh, the same system was adopted, but not carried to the extent that was desirable, chiefly on this account, that the chief sufferers by this, as by all other epidemics, were the poor Irish wandering in search of work; many of whom, when offered a refuge for their families in a large building belonging to the city poorhouse, immediately took up the notion that they were to be sent back to Ireland, then so unfortunately suffering from famine, and not only refused to avail themselves of the offer, but sometimes even concealed the disease, rather than run that risk. Into that building at the city poorhouse, however, 196 persons, all from rooms where cholera had broke out, were admitted and retained during the residence of their relatives in the cholera hospital, or till their death, and until their rooms were cleaned. Of these only 5 took the disease, and 2 died while in the refuge; and we could not learn that any of these took the disease after returning home.

Again, a similar refuge was established at the poorhouse belonging to the West Church parish, into which, however, only 43 persons could be induced to go. Of these, one only was affected with cholera, immediately on admission.

Again, in Glasgow during the epidemic of spring 1849, 401 persons (not more, for the same reason as in Edinburgh) were admitted to the house of refuge established in Glasgow, of whom 19 took the disease and 5 died.

Thus, in all, we have a record on which we can depend, of above 1000 persons in these two cities, all of them from *rooms* in which the disease existed, taken into these houses of refuge, of whom 40 took the disease and 15 died, while 978 escaped with their lives. At Oxford, in 1849, the same expedient was tried with 70 persons, and no case appeared among them. Of the results to be expected in any district fitted for the diffusion of the disease, where no such means of removing the population exist, we may judge from the statement of Dr. Hamilton, of Falkirk, of the result of his observations in that town and neighbourhood, where about 400 cases occurred in the winter of 1848-9. He says that he had known 47 instances of two affected in a family, 23 of three in a family, 3 of four in a family, 6 of five in a family, 4 of six in a family, 2 of seven, and 1 of eight in a family; in all, therefore, 251 cases in 86 families; but wherever, he says, he could effect isolation and due ventilation, there was *no second attack* in a house. (See 'Edinburgh Monthly Journal,' May, 1849.)

The numbers above given are somewhat different from those published by the Board of Health, in their 'General Report on Cholera in 1848-9' (pp. 124 and seq.), but we have much satisfaction in observing that the practical conclusion of that Board, as to the importance of such houses of refuge when cholera becomes epidemic, being much greater than that of hospitals for the treatment of the disease, is quite in accordance with our own observations; and is supported by the statement, that "in 1691 inmates of the houses of refuge, of whom they had accounts, there were only 33 attacks and 10 deaths."

This measure of removing the still healthy inmates of a district that has been tainted by cholera, is, in fact, exactly similar to the plan usually adopted in India, of moving troops and moving the inhabitants of villages into more healthy situations, when cholera shows itself among them, and may be more easily carried into effect here, because, as we have stated, the tainted districts are generally so limited.

We trust it will not be supposed that we mean to say anything in disparagement of the important measures for maintaining, as far as possible, the purity of the air of towns and of populous districts, on which so much stress has been laid of late years, and which we believe to be one of the most essential means that can be adopted for improving the general health and strength of a population, and diminishing their tendency to this and other epidemics. But when *the specific poison of cholera is actually among us*, we are confident that the result of experience and of statistical observation is to show, that it must be expected to attack certain persons, and attach itself to certain places, for which we can assign no such reason; and that in addition to these general means for improving the general health, means ought to be taken to remove as many as possible of the unaffected inhabitants from the limited *tainted districts;* and likewise to treat the persons actually affected in these districts, and their clothes and bedding, just as in the case of typhus fever, on the belief that the disease may *probably*, although it will not *necessarily*, show more or less of a distinctly contagious property.

(*To be continued.*) *W. P. Alison.*

Review II.

Sammandrag af Officieller Rapporter om Cholerafarsoten i Sverge, ar 1850. Af Dr. Fr. Th. Berg.—*Stockholm*, 1851. P. A. Norstedt & Söner.
Analysis of the Official Reports on Cholera in Sweden, in the Year 1850. By Dr. Fr. Th. Berg.—*Stockholm.* 8vo, pp. 370.

A complete description of the progress of cholera in any portion of our globe, is as yet a desideratum in medical literature; but if the reports collected in 1848, 1849, and 1850 be compared with those obtained of its first terrible invasion, twenty years ago, it will be acknowledged that considerable progress has been made in accumulating evidence, and in examining the bearings thereof on the great questions of quarantine, contagion, and sanitary reform. Amid the hurry, tumult, and anxiety which pervaded all classes when the cholera first appeared in England, in 1831, it would have been difficult, even had we then possessed an efficient Board of Health, to have collected any well ascertained and connected series of facts relative to the mode of propagation of the disease. The invasion of the disorder was in many instances so rapid, and its consequences so terrible, that all attention was directed to escaping its immediate influence, while the time of those qualified to observe was wholly occupied with the treatment of individual cases, and no leisure was afforded to the exhausted practitioner for investigating accurately the mode in which the pestilence first broke out, or for elucidating the numerous questions regarding it which will directly suggest themselves to every well-informed mind. When cholera had ceased, and had passed on to ravage other countries, the contest between the contagionists and the non-contagionists gained fresh vigour, and a virulent controversy was the result; till, as years rolled on, and party spirit wore away, the facts that had then been accumulated were subjected to more sober and impartial analysis; and, on the second invasion of the malady, much more care seems to have been bestowed on the investigation of the all-important question of the contagious or non-contagious nature of the disease. On the side of the contagionists appeared the names of Drs. Copland, Watson, Graves of Dublin, and J. Y. Simpson of Edinburgh; and to the latter two eminent contributors to medical science we owe a series of well-ascertained facts and cases tending strongly to support their views. Some writers there were who seemed inclined towards a *juste milieu* on this much disputed question: they admitted that cholera might, under certain circumstances, be occasionally transported from one person to another, but they maintained that quite as often, and perhaps more frequently, it arose spontaneously, and, like influenza or other epidemics, did not depend for its progress on any contagious property. When the second invasion of cholera occurred, the influence of non-contagious opinions at head-quarters was made evident by the fact, that the disease was pronounced to be incommunicable from one person to another; and it is evident that it was regarded by the then existing Board of Health as a malady arising in a great measure from local influences. We acknowledge to have formerly held the opinion just alluded to; but the facts that forced themselves on our observation during the last invasion of the disorder,* brought with them such strong proofs of the contagious character of cholera, that our ideas underwent a total change, and the evidence that we have since collected and examined has tended only to confirm these impressions. We saw repeated instances where a previously healthy locality was infected by persons arriving from a distance, and from places where cholera then prevailed; we found that these individuals were sometimes apparently in good health when they arrived, or, perhaps, they already exhibited the premonitory symptoms of the malady; we met with them labouring under the disease in low lodging-houses, from whence the disorder spread to the other inmates of the same house or room, and we traced the malady from these lodging-houses to other localities, which, in their turn, became focuses of infection in previously healthy districts. We met with cases, too, which inclined us to believe that cholera could not only be transported by the persons, but likewise by the clothes and bedding of those labouring under the disease; and on some occasions the clothes &c. were carried to considerable distances before being opened out or used; yet they, too, seemed capable of spreading the disorder.

It has been constantly urged by the opponents of the contagion of cholera, that this disease pursues a certain steady course across a country or district, such as has been observed to occur in the case of an epidemic of influenza. We think that Dr. Graves has fairly exposed the utter fallacy of this opinion; he has shown that cholera varies most signally in the directions it pursues. From India, after its first appearance in 1817, cholera spread to Sumatra, China, Borneo, and the Islands of the Eastern Archipelago, while at the same time it radiated towards the north and west, following the great lines of communication towards the frontiers of Europe. As early as the year 1823, cholera was on the borders of the Russian empire in the vicinity of Astracan; but a strict quarantine was then enforced by the Russian government, and the pestilence, diverted from its famous "north-west course," spread in a south-westerly direction towards Arabia and Asia Minor, following the track of the caravans to Mecca, and causing a most frightful mortality in the holy city of the Prophet.†

Cholera appeared at St. Petersburgh in 1831, and was from Russia transported into Poland during the war of Polish independence. From Poland the pestilence advanced into Prussia, and traversed North Germany to Hamburgh; and in November, 1831, it broke out in Sunderland. Sweden and Norway were not affected with the disease till 1834, and yet, if cholera had kept true to its north-west course, it should have reached these countries in 1831, instead of turning to the south-west to ravage Poland and Germany. Following this supposed line, it should thence have extended itself to the northern parts of Scotland, to the Orkneys, Shetland, and Ferro Isles, and to the distant regions of Iceland, all of which countries, in a sanitary point of view, were eminently favourable to the progress of the epidemic. Yet all these northern islands escaped the disease, strict quarantine was enforced, and, though cases of cholera occurred on board of ships in Bressay Sound and elsewhere, the malady never reached the land.

* That is, the invasion of 1848-9. This review was written some months ago.—*Editor.*

† See Dr. Graves' Essay on the Progress of Asiatic Cholera, Dublin Journal of Medical Science, January 1840, p. 355.

We have stated that no complete report of the progress of cholera through any of the great kingdoms of Europe has as yet appeared; but the work now before us, though necessarily still imperfect, goes far to remove this reproach from the kingdom of Sweden. No exertion seems to have been spared to render the history of the pestilence complete; and the whole of the reports are investigated by Dr. Berg in a spirit of candour and impartiality, which imparts additional value to the facts that they contain. We find here, not only a history of the progress of the disorder through the various districts which it visited, but a detailed account of the preventive measures that were adopted, both on the sea-coast and in the towns and parishes of the interior of Sweden, with reports of the prevalence of other disorders, epidemic or contagious, before, during, or after the invasion of the pestilence; and also tables of the condition of the atmosphere &c. during the same period. The sanitary state of those provinces that escaped altogether is likewise recorded; and the comparative spread of the malady in 1834 is briefly noticed. A valuable table, too, is given, showing the various foreign ports trading with Sweden that were, from time to time, declared to be *infected*, *suspected*, or *free*, by the Swedish Board of Health; though it must not be forgotten that many ports were really suffering from cholera, to a considerable extent, before they were declared to be infected by the Swedish authorities.

The government regulations to prevent the introduction of cholera into Sweden were restricted chiefly to the sea-ports, and to the great lines of traffic through the interior of the country. The more inland frontiers, and those more remote from the great roads, seem not to have been subjected to any very strict supervision, as it was left to their respective authorities to determine whether quarantine should be enforced or not. The prudence of such an arrangement would have been very doubtful if the Swedish government had decided on regarding the malady as contagious; but as isolation was adopted by some parishes, and neglected or entirely repudiated by others, ample means for comparison of the two systems were in this way afforded. Quarantine on vessels arriving from infected ports seems to have been pretty rigidly enforced; at all, or nearly all, the quarantine stations along the coast, instances occurred of vessels arriving with cholera on board; but by strict seclusion from intercourse with the shore, the disease was in almost all instances arrested. These observations, however, apply only to the ports along the Baltic; the great commerce of Gothenburg, and the constant communication with Denmark, rendered the enforcement of quarantine very difficult, if not impossible, on the western coast of Sweden. In this last invasion of the disease, Stockholm and all the eastern ports but one escaped entirely, though, in 1848, the cholera had raged at Cronstadt, and along the Russian coast. In 1834, Stockholm and Gothenburg suffered most severely, while the town of Malmö, where the disease first showed itself in 1850, entirely escaped on the former occasion.*

Malmö, a town of 12,000 inhabitants, is situated on the south-western coast of Sweden, nearly opposite to Copenhagen. It lies well exposed to the sea breezes, but its situation is low, and there are some muddy ditches in the lowest parts of the town, which in hot weather exhale odours agreeable to none but to the olfactories of the anti-contagionists.

* In 1831 the port of Skänor, distant only a few miles from Malmö, was ravaged by cholera.

Malmö is one of the ports of Sweden which has the most frequent communication with Germany; here the steamboats from Lübeck and Travemünde land their passengers, and an almost constant intercourse is kept up with Copenhagen.

The general health of the town had been favourable during the preceding summer, which, until the month of July, had been remarkably cool, but with the commencement of that month hot weather set in, and continued till the end of August, when an unusually low temperature supervened, followed by heavy rains in September. In 1834, when Malmö escaped cholera altogether, a much higher temperature prevailed; and in those central parts of Sweden which remained free from cholera in 1850, the range of the thermometer was much higher than on the coast. We here quote Dr. Berg's own words on the subject.

"It appears, therefore, from the reports, that a high temperature prevailed all over Sweden during the dog-days (*rötmanaden*), and was succeeded by a sudden and unusual degree of cold, and that this change likewise occurred in those districts where the malady first appeared, but that central Sweden, which almost entirely escaped the cholera, was subjected to a greater degree of heat than the most southern provinces.

"It is the unanimous opinion of the medical practitioners of Malmö, that neither the sudden change from heat to cold, nor the cold, rainy, and stormy weather of the end of August and of the beginning of September, nor yet the milder temperature of October, produced any perceptible influence on the progress or intensity of the disease." (p. 58.)

The disorder seems to have broken out in Malmö about the 12th or 13th of August, 1850. Out of 12,981 inhabitants, 1138 were attacked, and 378 died, during the twelve weeks that the pestilence prevailed. The mode in which the malady was introduced does not seem to have been satisfactorily ascertained. A suspicious case occurred in the person of a teacher of languages, named Nordlin, as early as the 3rd of August; but the first instance where the disease was pronounced to be cholera, was observed in the person of a female, Maria Jönsson, who had attended Nordlin in his illness, and two or three days after (August 6th) was seized with well-marked symptoms of cholera, was carried to the hospital, and there died. On the 9th of August, the day that she was removed to the hospital, her child, a boy of two years and a half old, was attacked, and died the next day.

Although the origin of the disease in Malmö cannot with certainty be ascertained, still there are strong grounds for the belief, that the malady was brought by the steamboat *Malmö* from Lubeck, where cholera then prevailed. A son of the cook on board of the above-named steamer died of cholera an hour before the boat left for Malmö. No cases of cholera, however, occurred on board the steamer, nor was it proved that any communication took place between Nordlin or the others who were first affected and the vessel in question. The boat arrived at Malmö on the 27th of July, and two Custom-house officers, who had kept watch on board during the stay of the steamer in the harbour, were seized with cholera and died. We doubt much, however, if the infection in this case can be traced to the stay of these men on board the steamer, for they were not attacked with cholera till the 14th and 16th of August respectively. The disease, however, had already appeared in the dwelling of the Custom-house officer, Jacobson, on the 11th of August, where a female, Mamsell Wred, sickened, and died on the following day. On the 13th Jacobson's wife was attacked, and died on the same day. On the 14th Jacobson himself became affected with cholera, but he lingered till the 23rd. Two of his children sickened also on the 14th, and both died within 48 hours. On the 15th, 16th, and 17th, the two remaining children were seized with cholera, as also a servant-girl in the house, but all recovered. The other Custom-house officer, J. Hansson, was attacked on the 16th, and died that day; and on the following day another officer, C. Hansson, and his wife, fell victims to the malady. Both the Hanssons *had attended their friend Jacobson during his illness.* The child of Maria Jönsson which sickened on the 9th of August, as before stated, was carried to the workhouse and died there on the 10th, on which day the first case of cholera appeared in the above-named institution, when 29 cases rapidly followed, 25 of which were fatal. The disorder showed here, as elsewhere, a disposition to locate itself in certain houses, where almost every inhabitant was attacked. We observe, too, in the report given by Dr. Gräh, that out of those employed about the sick, many were attacked with cholera. Out of three physicians, two died; of eight male attendants thus affected, five were carried off, as were likewise five nurses, out of nine who suffered from cholera. The principal mortality occurred during the first three weeks, but it diminished when better sanitary measures were adopted in the hospital.

It must be confessed that the evidences of contagious propagation above adduced are not of the lightest character, but we are not inclined to deny the influence of other predisposing causes, as of poverty and uncleanliness, in favouring the progress of the malady.

The disease was in Malmö, as elsewhere, chiefly confined to the most wretched and insalubrious parts of the town, and especially to a low-lying quarter called "Bethlehem," inhabited by the labouring classes, and adjoining two swamps with a filthy ditch running through them, filled with stagnant water. In this quarter the mortality of those attacked was above 80 per cent., and from 8 to 19 cases occurred in many of the single houses or tenements. It was remarked that the workmen who provided their own meals suffered more than those whose food was prepared for them in the different establishments where they laboured. Those who had the comfort of a warm meal once or twice a day were less affected than the lower class of labourers, who lived chiefly on salted herrings, sour rye bread, and still sourer milk.

In the villages and hamlets around Malmö several cases of cholera were observed, almost all of which occurred in persons who a few days before had visited those sick of the disease in that town. Thus, in Hyllie parish, a widow died of cholera on the 21st of August, who on the 19th had visited her sick brother-in-law in Malmö. At the farm of Tagarp, half a Swedish mile NNE. of Malmö, a servant of the name of Nils Pehrsson was seized with cholera on the 4th of September, after having been in Malmö on the preceding day. This man recovered, as did also his friend, Nils Hansson, who had not been in Malmö, but had sat for some time with Pehrsson on the day of his return from the town, and who was likewise seized with the disorder the next day. In the village of Sunnana, three-quarters of a mile east from Malmö, Hans Tufveson was attacked with cholera on the 2nd of October, eight days after having been in that town. He recovered, but on the 10th October, both his daughters, aged respectively eight and nine years, were seized with the malady, and the youngest died on the 13th October. In almost all cases, the houses where such patients died were carefully secluded, and the disease did not spread farther.

In the district of Flädie, a mile and three-quarters (Swedish)* north of Malmö, there resided, in a small cottage near the sea shore, a peasant named Ake Andersson, with his wife and three children. On the 27th August the brother-in-law of this man came from Malmö, as his wife had been that day seized with cholera, and had been carried to the hospital. He himself, on reaching Andersson's cottage at Flädie, with his little girl, aged 4 years, laid himself down to rest in Andersson's bed, but rose from thence when the latter came in from his work. On getting-up he vomited, but ascribed this to some brandy which he had taken, and he remained during the night in the house. Next morning the authorities ordered him to return to Malmö with his daughter, but on the road thither the symptoms of cholera became more and more developed, and he expired that evening. Andersson himself had not been in Malmö for fourteen days before this; but he sickened of cholera on the 30th of August, and died on the 1st of September. Within a Swedish mile and a half of Malmö lies the town or city of Lund, containing about 6000 inhabitants. During the warm months of July and of the early part of August, numerous cases of diarrhœa occurred in Lund, but not more than are usually observed in hot seasons, succeeded by sudden lowering of the temperature. It was determined by the authorities of Lund, supported, we presume, by the professors of the University there, that the town should be closed to all persons from infected places on the 22nd August; and this regulation was strictly enforced until the 4th of November of the same year. Ten days' quarantine was imposed on all travellers from suspected localities, and on the 2nd of September all importation of goods or wares from Malmö was strictly prohibited. The great object of these strict regulations was to prevent the influx of the terrified fugitives from Malmö; the duty of watching the entrances of the city was performed by 700 or 800 of the inhabitants, day and night; and passengers arriving in carriages from Malmö were made to change horses outside of the city, and to pursue their journey without entering its precincts. The disease appears to have reached within half a Swedish mile of Lund on two sides, but no case of cholera occurred within the city, and as time wore on, the confidence of the inhabitants in their measures of precaution continued to increase. Other towns and villages, such as Ystadt and Trelleborg, adopted the like precautions, and with the same fortunate results. The district physician, Dr. Ström, reports that these measures of seclusion and quarantine towards suspected persons were in these places warmly seconded by the inhabitants, so that their doors were kept rigidly closed to the fugitives from Malmö; and he remarks, too, that wherever quarantine was adequately enforced, cholera did not appear, and the reliance of the inhabitants on its efficacy was never diminished.

* The Swedish mile is equal to 11,700 English yards, or 6·640 British statute miles.

Five and a half Swedish miles to the north of Malmö, and half a mile from Helsingborg, lies the fishing village of Raa, with 920 inhabitants. Relying on the fact of this village having escaped the cholera in 1834, the authorities of the place took no precautions to prevent daily communication with Malmö, and many of the fishermen, after having visited cholera patients in that town, and resided in infected houses, were allowed, without hindrance, to return to Raa. From the 18th of August to the 9th of October, 1850, 70 cases and 33 deaths occurred in this village. The disorder attained its greatest height about the fourth week, and its virulence was augmented rather than diminished during cold and windy weather. Here, as elsewhere, a succession of cases frequently occurred in a single dwelling-house.

The first case of cholera at Raa was observed in a fisherman of intemperate habits, who had been at Malmö on the 15th of August, but continued in perfect health up to the 19th, when severe symptoms of the disease set-in, and he died on the following day. Attempts were now made to shut up the dwelling of this man, but, as Dr. Stenkula, the reporter, informs us, crowds of curious peasants had flocked around his bed to witness his dying struggles. The well-known fact that this man had, by way of bravado, handled the bodies of persons dead of cholera in Malmö, no doubt increased the excitement in Raa regarding this case. On the 27th of August the two daughters, and on the 29th the widow, of this first victim, were attacked with cholera, but all subsequently recovered. David Johansson, aged 60, a fisherman of Raa, was seized with cholera on the 24th of August, and died on the 26th. His companion and attendant, Jon Eliason, died on the same day, his brother on the 2nd of September, and his brother's wife and daughter on the 5th of that month. The observations of Dr. Stenkula on these and some other cases are worthy of notice.

"As regards the contagious or non-contagious character of the disease, I have no hesitation in declaring, in opposition to the generality of the more recent authorities on the subject, that the cholera is essentially a miasmatic contagious disorder. It is true that here, as everywhere else in Sweden, diarrhœa, vomiting, and gastric disorders had occurred, but the appearance of this malady in a locality so healthy as Raa, introduced, as it certainly appears to have been, by communication with an already infected locality, is a fact so well established that it cannot be disposed of." (p. 82.)

Dr. Stenkula even maintains that the disease can be conveyed by individuals who themselves escape, and instances a case in Raa, where two children took the disorder from their father, after his return from Malmö, while the latter never exhibited any symptoms of the disease. He, however, fully agrees with the London and Christiania reports, in believing that all seclusion of healthy districts is unnecessary, save as respects ships arriving from infected ports. The advantages, however, of shutting up and watching the houses in which the disease may have broken out, and of subjecting the inmates of such houses to close observation and seclusion for a time, he thinks cannot be denied.

In 1834 the island of Gottland escaped entirely; in 1850 about half a dozen cases occurred there, and that only in one part of the island, in the village of Kapellshamn, at its north-eastern extremity. These cases do not seem to have been accurately observed, and consequently we cannot place absolute reliance on them, but it is a significant fact that a sloop from the infected port of Lübeck arrived at Kapellshamn, on the 4th of August, having during the voyage lost one of her crew, from well-marked cholera, while another man on board had taken the disease, but recovered. This death was falsely represented to the authorities of Gottland as having been caused by apoplexy, and after some delay, the crew were permitted to land. The first case of cholera occurred in the children of a tailor (Lindgren), who, on the 13th of August, visited the sloop, and took from thence some clothes home with him to repair. On the 16th two of his children, aged 13 and 9, were attacked and died, and his wife was likewise affected, but recovered.

From the coloured chart which accompanies Dr. Berg's Report, we see that the disease in Sweden did not spread after the manner of an influenza epidemic over the whole face of the country, but that it followed the great lines of communication into the interior, or else showed itself in detached localities upon the coast. Thus, 98 cases and 43 deaths are reported from the village of Rönneby, on the southern coast of Sweden, between Carlshamn and Karlskrona, but neither of these towns were affected, and the latter escaped also in 1834, when, as in 1850, the authorities of the place enforced the strictest quarantine regulations.

Pursuing our course northward, along the western coast of Sweden, we come to the small town of Falkenberg, where a few instances of death from cholera were observed. Nearer to Götheborg lies the district of Fjäräs, 24 English miles south of Götheborg, and here cholera seems to have been introduced by a sailor from that town, who left Götheborg on the 20th of September (the day on which cholera broke out there), and who was attacked with the disease on the 21st, and died on the 23rd. On the 25th, the wife of this man was likewise attacked, and died on the same day. The house was then shut up, and carefully guarded, and no further cases occurred. Other instances of the introduction of the disorder from Götheborg were subsequently observed, but in all cases the same precautionary measures were adopted, and with the like fortunate results. Peasants who had visited Götheborg, where cholera was then raging, returned home in apparently good health, but a day or two after, they were seized with cholera, which extended to those, and to those only, who had communication with them. The district physician, Dr. Carlsson, says—

"We found these precautionary measures (the closing and watching of infected houses) of great use, for the disease never spread out of houses thus secluded, and when it did show itself in the neighbourhood, it could be traced to those who had visited the infected localities. Here, therefore, the non-contagious character of the disease was not maintained, for those who died in this place of cholera, were exactly the individuals who had themselves visited the infected places, or had been in communication with persons just returned from these." (p. 121.)

At the mouth of the Götha Elv, the great channel of water communication across Sweden to Stockholm, lies the large town of Götheborg—a considerable town, at least for Sweden, for it contains 21,000 inhabitants. In 1834, this busy trading and manufacturing town was most severely visited by the pestilence, which carried off seventeen hundred of the inhabitants. In 1850, the cases were fewer, but the comparative mortality was not much less. 1316 were attacked, and 529 died. Of these 311 were males, and 218 females. Previous to the outbreak of the pestilence, the general health of the town is reported to have been good. When the sudden change from oppressively hot to cold and windy weather took place, in August, 1850, diarrhœa and colicky pains in the bowels became frequent, but these diminished remarkably before the first cases of cholera appeared, about the 22nd of September. Scarlet fever, which had prevailed more or less throughout the summer, was not in any way arrested or influenced by the cholera. Among the individuals attacked were 22 nurses, of whom 6 died. The disease attained its greatest intensity in the third week. Carefully-compiled tables are given in Dr. Berg's Report, exhibiting the range of temperature and the barometrical indications during the whole period that the disorder prevailed in the town, but they indicate no coincidences between the atmospheric vicissitudes and the progress of the pestilence. In Götheborg, as elsewhere, the disease prevailed chiefly in the poorest and the most densely populated quarters of the town, and the intemperate were its first victims, but at a later period even those of more orderly lives did not escape. In a town of such great commercial activity, and situated on the great highway from Western Europe to Stockholm and the east of Sweden, no measures of seclusion or quarantine could be adequately enforced. The interruption of trade by a strict quarantine, and the consequent loss of employment to thousands of the working classes, would have brought them to the brink of starvation, and would have rendered them ready victims to the pestilence, which sooner or later would have made its way into the city with the great crowd of travellers that could not be arrested or turned aside. The authorities of Götheborg did not even consider it possible to prevent the importation of goods from infected places into the town. Here, as in the large towns, the faculty were divided on the question of contagion or non-contagion, but though the exact mode in which the disease was introduced cannot be ascertained, it made its first appearance in those quarters of the town which had the most frequent communication with strangers, and with vessels arriving from foreign ports. It was in the streets along the Götha Elv that cholera first showed itself, among the boatmen and sailors employed about the shipping on the river. From thence it extended to the suburbs, and overstepping the boundaries of these, it broke out in various villages north and east and south of Götheborg. In most of these the disorder did not show itself till some days or even weeks had elapsed from the time it had established itself in the town, and in the majority of instances it was observed first in those persons who had recently visited Götheborg. But its chief intensity seems to have been exhibited in the parishes that border the Götha Elv from Götheborg to the Falls of Trollhätte. Along this great river there is a considerable population, almost all of whom are employed about the ships and steamboats that pass up and down towards the Great Wener lake and the interior of Sweden. We have not room to make extracts from the elaborate reports here presented to us on these districts, but their main features are the same as those we have before detailed regarding other parts of Sweden. The malady first showed itself in persons who had recently been in

Götheborg, or who, in other places, had been in communication with infected individuals, or at least had worked in the ships coming from Götheborg up the river. Again and again do we meet with instances where an individual returned from an infected spot to a previously healthy locality, and there first exhibiting the symptoms of the disease, communicated it to the other inhabitants of the district. In at least twenty or thirty cases the persons who were first seized with the disorder had been in Götheborg only a day or two before; they were in apparently perfect health when they returned home, but after the lapse of 48 hours, more or less, the symptoms of the malady appeared. It was to the other inmates of these houses, or to those who came from a distance and volunteered their services as attendants, that the cholera first spread.

Thus, in Thorsby parish, Anna Johannisdotter (aged 24), slept one night in Götheborg, in a bed from which the body of one of her relatives who had died of cholera had just been removed. On the following day (November 3) she was attacked with cholera on her way home, and died on the 6th. Only one case of cholera had previously been observed in this parish, and that on the 27th of September, in the person of a sailor who had been brought home sick from Götheborg. After Anna Johannisdotter had arrived at home, and before her death, her grandfather, Jöns Hero, aged 81, was seized with vomiting and diarrhœa, and died on the 8th of November. Anna Hero, his wife, also 81 years of age, died from the same symptoms on the 10th of November, and her case was certified to be one of Asiatic cholera, by the district physician who saw her. Andreas Bengtsson, a widower, of 70 years of age, had read prayers by the bedside of the first-named patient; he was attacked with cholera on the 8th, and died on the 10th of November. No comment on such an array of evidence is required. In this parish no precautionary measures were adopted; the local authorities declared "that the disease would spread no farther than God permitted, and that all seclusion was unavailing." Perhaps, too, another reason was more powerful than this pious fatalism—viz., that the prosperity of the inhabitants mainly depended on their intercourse with the shipping on the Götha Elv, and that any quarantine regulations would therefore deprive them of the means of subsistence.

Some of the best illustrations of the introduction of the pestilence may be found in the report of its progress through the various islands that stud the coast of Sweden.

On the 10th of November, a boatman, already labouring under cholera, returned from Götheborg to Lofoen in the *Skärgärd;* he recovered, but his wife and mother both took the disease and died. The persons who placed the deceased in their coffins carried the malady to another island, Brattö, and from thence it passed to the mainland adjoining. The parish of Ucklum remained free from cholera till the end of October, when a labourer, Nils Magnusson, returned home from the Saetter, or hill pastures at Ström, where several persons had already died of the disorder. Nils recovered, but four persons in his house took the disease, of whom two died. A young woman who attended on these last, sickened and died, and was shortly after followed by her father, who had nursed her in her illness. A labourer, who lived in the house of the last named person, died at a cottage about 6 English miles off, when the cottager next took the disease, and died on the same day. It is observed in the Report, that none but those who had communication with the infected suffered from cholera. No precautionary measures were adopted in this district, or in the greater part of the neighbourhood of Götheborg.

The island of Tjörn is separated from the mainland of Sweden by a narrow sound. Great alarm was felt by the inhabitants when the disease appeared at Götheborg, but their measures of precaution seem to have been lamentably deficient.

The reports of the district physician, Dr. Ossbahr, plainly show that the inhabitants of Tjörn really exposed themselves in every way to the pestilence, and that they were in such a condition, as regards their customs and their habits, as pre-eminently to favour the progress of cholera. The mortality in Tjörn seems to have much exceeded the usual average. Out of 51 cases 20 deaths are recorded.

In Aseby, where the disorder first appeared, the greater part of the inhabitants were attacked, and only three recovered. It was observed too on the other farms, that most of the cases occurred in one or two houses or families. The excessive mortality in Aseby is ascribed by Dr. Ossbahr to the circumstances that three families, which had before inhabited separate chambers, all, upon occasion of the first death from cholera, crowded themselves into a single room, wherein children and adults, the diseased and the healthy, continued to reside in the most extreme misery and filth. Moreover, numerous relatives from other farms flocked in to see them, and many of these becoming infected were carried to their own houses, and were the means of spreading the disease in other places. So excessive, however, was the fear of the pestilence among the people of Tjörn, that at first, continues Dr. Ossbahr—

"It was impossible for me to obtain attendants to wait upon the sick, except among their nearest relations. At length I succeeded in persuading a few, both men and women, to undertake this office; but, alas! after a few days, the best and the most active nurse, Anna Olsdotter, took the disease, and, still worse, she died!!

"When I first arrived on the island (November 30th), I earnestly entreated the authorities not to permit the healthy and the diseased to remain in the same chamber. This, however, was neglected, or no measures were taken to enforce obedience. During the first days of my residence in Tjörn, the doors of the infected houses were constantly closed to me, nor could I anywhere obtain the requisite attendance on the sick; but no inhabitant ever hesitated to attend the funerals of the cholera victims, where brandy, with camphor dissolved therein, was swallowed in immense quantities." (p. 194.)

As to the mode in which the disorder was introduced, Dr. Ossbahr's report is at variance with that of the local authorities. The latter assert that not one out of at least 100 individuals who visited Gothenborg, while the cholera raged in that town, were affected with the disease either there or on their return to Tjörn. Dr. Ossbahr tells us a very different story, without, however, directly contradicting the above assertion:

"From what I ascertained on Tjörn, a man of the name of Rutger Jonasson, a son of Jonas Pehrsson, in Aseby, had visited Götheborg on the 6th of November, and had there purchased various articles of clothing which had belonged to persons there dead of cholera. These clothes were made up into a bundle by Jonasson, and were brought by him direct to Aseby, where he placed the bundle in a chest, and allowed it to remain there for eight or ten days. One day, when a number of his relatives were assembled at Aseby, the bundle was taken out by Jonasson, with the remark that, 'the things had now lain-by so long that there could be no danger of infection,' and he accordingly offered the articles of clothing for sale. The clothes were handled and examined by those present, and purchased by some. The day after (November 16th), Jacob Christiansson, a man of 76 years of age, and one of those who had been present at Jonasson's house on the above occasion, was attacked with cholera, and one after another, all who had been there on the 15th, to the number of six individuals, fell victims to the disorder." (p. 195.)

The parish of Quille lies 30 or 40 English miles to the north of Tjörn, and no case of cholera had occurred in the intervening district. The disease here broke out on the 17th of November, shortly after the arrival of three boatmen from the infected district of Wenersborg.

In Lilla Edet, on the left bank of the Götha Elv, about 120 cases occurred, nearly one-half of which proved fatal. Here, as in other places, it was remarked that many persons whose bowels had been habitually constipated for years, became perfectly regular in their evacuations during the prevalence of cholera, but when the pestilence ceased, their bowels again became inactive.

Of four nurses employed at Lilla Edet, two were affected with cholera. The disease was perhaps more widely spread in this district, from the circumstance of its inhabitants being mainly employed on the "Ströms" Canal, which is cut through the rocks from the Wener Lake to the Gotha River, to avoid the Falls of Trollhätta. The amount of commerce on the Ströms Canal may be inferred from the fact, that from the 20th of September to the 29th of October, 1850, 562 vessels passed through the sluices at Trollhätta, and in many of these, in their voyage up the river from Götheborg, cholera had appeared. The people of Lilla Edet were therefore in constant communication with ships and with individuals coming from the infected district of Götheborg.

In the parish of Asbräcka, the first case of cholera was that of Peter Andersson, who had visited his brother, Andrew, in the parish of Fuxerna (8 or 10 English miles off), while the latter was labouring under the disease. Peter Andersson sickened in his brother's house, was brought home to his own cottage, and there died. Five more of his household took the disease, of whom three died. Of his whole family the widow alone survived.

At the Falls of Trollhätta there is a considerable population of 1400 to 1500 souls. Here 96 cases of cholera occurred with 43 deaths. The causes of this high mortality are best given in the words of the report:

"Unhealthy and crowded dwellings, want and intemperance, greatly increased the number of victims, and it was observed that the malady was peculiarly severe in those families where many individuals resided in one or two small rooms; while, when cholera did appear among the better classes, it seldom spread to the rest of the household, especially where the dwellings were large, airy, and well kept. Two old persons, man and wife, who lived about an English mile from Stafvered, but were not known to have had any communication with infected persons, were attacked with the disease almost at the same hour, and both soon died. A woman, aged 30, who attended them, took ill two days after at her own house, but gradually recovered; while her aged parents, residing in the same dwelling, fell victims to cholera after about a day's indisposition. A boy of 6 years of age lived also in the same room, but on the death of the old people and the illness of their daughter, he was sent home to his father, who resided in a wretched cottage at Stafvered. On the day of his arrival there he sickened and died; directly afterwards a girl in the same house was attacked and soon expired; and two children were likewise affected, but recovered. Many of the nurses who tended patients in some of the more remote and more wretched tenements, were carried off by cholera." (p. 219.)

The first case of cholera in Skepplands parish occurred in the person of Anders Jonasson, who, on the 29th of September, had visited Götheborg, to bring home his son Andrew, who was working there. On the 1st of October, this son was attacked with diarrhœa, vomiting, and cramps. In the night of the 3rd of October, the father was seized with the like symptoms, and died on the 4th, at two in the morning. Another son, Hans, sickened on the 6th, and died on the following day.

At Wenersborg, a few miles above the Falls of Trollhätta, the Götha Elv flows out of the great inland lake of the Wener-see. The tide of commerce from Götheborg, which has hitherto been confined to the limits of the Götha river, here spreads out to the various ports situated on this vast expanse of waters. One of these ports is the town of Amal, containing 1297 inhabitants, and situated on the northern shores of the Wener lake. Here 58 cases occurred, but only 12 deaths, which may be explained by the fact, that the disorder prevailed a good deal among the better classes.

On the 7th of October, a ship arrived from Götheborg, in which cholera had broke out four days after leaving that town. We cannot, however, trace the introduction of the disease to this vessel, or to the fact of one of the victims of the pestilence having been buried in the churchyard of Amal. On the 13th of October, the steam-boat, *Arvika,* arrived from Wenersborg (an infected port), and the passengers are then said to have been in good health, but on board of this boat were a number of workmen returning to their homes far up the country beyond Amal, and many of these men, as we shall presently have occasion to relate, carried the pestilence with them to their own distant dwellings. Between thirty and forty miles to the east of Götheborg lies the parish of Möne, where a few cholera cases occurred, while the intervening district for at least 20 miles enjoyed perfect immunity from the disease. The first case was that of Gustav Johansson, aged 25, and an intemperate man, who, on the 9th of November, returned from Götheborg, after having purchased there the clothes of some of the cholera victims. He was seized with cholera on the 11th, but recovered. On the 20th of November, his father, and shortly his mother and two young relatives, were attacked. During their illness they were attended for two days by a cottager's wife, Maria Andersdotter, who sickened in their house with the same symptoms, and being carried home to her own dwelling, died there on the 26th. A soldier's wife, Annicka Winberg, nursed the last-named patient for two days, and died of cholera on the same day. On the 28th, her husband was carried off by cholera, and on that day also his son, who, after attending his mother's funeral, had returned to his own dwelling at some distance, and there was seized with the usual symptoms. His wife, his child, and an old man who frequented his house, were likewise severely affected. A woman, Sarah Blix, who had come from a distance of two miles to wash and clothe the bodies of Annicka Winberg and others, sickened on the 27th of November at her own home. She was attended for two days by her daughter-in-law, who became also affected with cholera, but both eventually recovered.

In the next parish of Timmelhed, the following cases were observed. Johannes Andersson, aged 36, sickened on the 22nd November, after having visited Götheborg on the previous day. On the 29th, his wife sickened, and died after 18 hours' illness. The same evening, his mother-in-law, and also a child of a year old, were attacked, and both died on the following day. On the 2nd of December his daughter, Maja, was seized with cholera, and died on the 6th; and a girl in the house suffered from the same symptoms, but recovered. The house was now shut up and watched, and the disease did not spread farther.

We have now traced the course of the pestilence along the Götha river upwards from Götheborg, to where it debouches from the Wener lake. Some of the traffic from Götheborg passes into the interior of Sweden, to the east of the Wener lake, by the Götha canal, which connects that lake with the Wettern-see. The Götha canal joins the Wener-see at Sjötorp. From the 20th of September to the 9th of October, 36 vessels passed the sluices of the canal here, and proceeded to the eastward. All these vessels were from the infected districts of Götheborg or Wenersborg, and few or no precautions against the importation of the disease seem to have been taken. The first case occurred on the 9th of October, in the person of a labourer in the dockyards at Sjötorp. He died after 14 hours' illness; his brother, who attended him, sickened on the same day, and died on the 11th. The brother-in-law of the first-named victim attended the post-mortem of his relative, and took ill immediately after, and died on the 12th. A few hours after, his daughter was attacked, and she died on the 13th. The individuals who attended on these patients all suffered more or less from sickness and diarrhœa, but the disease did not spread farther. The clothes &c. of the dead were carefully fumigated, and the house was avoided by the neighbours.

Cholera also appeared in an isolated spot in Wanga parish, about 15 E. miles due south of Lidkjöping, on the Wener-see. Here 46 cases occurred, with 18 deaths. The first case was that of Gustaf Gabrielsson, who had been to Götheborg on the 12th of November to sell poultry. He was taken ill of cholera on the 17th, but eventually recovered. A soldier, aged 33, who had conversed with this man in the open air, after his return, but had not visited his house, took the disease on the 22nd of November, and before the 28th of that month two of this man's children died of cholera, while another child and his wife were severely affected. On the 27th of November, the nurse who attended the above was attacked, and died on the 4th of December. During her illness, five of her children took the disorder, of whom three died. On the 2nd of December, the nurse who had succeeded the above in attendance on the soldier's family, sickened, and on the 4th of that month two of her children took the disease, and one only recovered. The village where these cases were observed was, as regards its sanitary condition, eminently favourable to the progress of cholera. Large dunghills were accumulated around the cottages, the inhabitants were crowded together in dark and filthy chambers, to which fresh air never found access; while their diet was little, if at all, superior to that of the Irish peasantry. They seem, too, to have been a rude and uncultivated race, for the Report goes on to state, that one afternoon the peasants forcibly entered the hospital, with the view of expelling all the inmates, and this for the sole reason that their maintenance occasioned a heavy expenditure to the parish!

On the north side of the Wener lake, on a promontory in the district of Näs, lies the parish of Eskilsaetter. Into this isolated district cholera seems to have been introduced by the yacht *Anna Maria*, which sailed from Lidkjöping on the 14th of October, after having taken on board several labourers, who had come thither from the infected districts on the Götha-Elv. During the voyage across the Wener lake, two of the passengers became affected with diarrhœa and vomiting; and on the same afternoon, the skipper of the vessel, Nils Olsson, was seized with the same symptoms, and died on the 17th, on an uninhabited island off the coast. On the 19th of October, another of the crew, Gustav Carlsson, was similarly affected, but he so far recovered as to be able to reach his own home at Getterud in Eskilsaetter parish, where he was visited on the 20th by Dr. Ekegreen, who pronounced his case to be one of genuine Asiatic cholera. This man recovered, but on the 21st, his mother, who had attended upon him, was seized with cholera, and died on the morning of the 23rd. A young woman, who had likewise acted as a nurse to Carlsson, was affected with the usual symptoms, but recovered. The clothes of the captain of the yacht were taken to his home at Gaperhult, and shortly after, a young woman in the house where they had been deposited was seized with cholera. A fresh crew was now put on board of the ill-fated vessel, and she sailed from thence towards Carlstadt; but on the 2nd of November, the new captain died of well-developed cholera. The clothes of Nils Olsson at Gaperhult were now carefully fumigated, and the house was closed; the same was done at Getterud, and the disease did not spread farther. Dr. Ekegreen remarks—

> "Whatever differences of opinion there may have been regarding the efficacy of measures for *shutting out* (*utestängande*) the cholera, all seem to be agreed as to the advantage of *shutting in* (*innestängande*) the disease when it has made its appearance in a house, whether in a village or in the country." (p. 296.)

To the north of the Wener lake, and thirty or forty English miles inland from Amal, lies the secluded district of Fryxdahl. About thirty workmen from this locality had been for several months employed in the construction of the new prisons at Wenersborg, above Trollhätta. The first cases of cholera in Wenersborg were reported on the 10th of October; and on the 13th of that month, thirty-three of these men embarked in the steam-boat *Arvika* to return to their homes. On the 15th, they landed at the village of Arvika, and then separated into parties to make their way home. One of these men, Hakan Nilsson, was attacked on the succeeding day, and died at a hamlet on the road. His brother, who lived at a considerable distance, hearing of his illness, hastened to see him, but Hakan Nilsson was dead before he arrived. The brother immediately returned to his own home, and died there on the night of the 19th, of cholera. The clothes of Nilsson were carefully fumigated, and the malady did not spread. A larger party of these labourers took a route to the eastward of Arvika, and crossing a considerable tract of country, rowed over the Fryxen lake to Ostra Emtervik, and there separated to go to their different dwellings. Three of this party were now attacked with cholera—viz., Jöns Olsson on the 17th, Jan Jonsson on the 19th, and Olof Jonsson on the 20th. The two first died, each after 24 hours' illness; the last recovered. All these men lived in separate cottages, at a considerable distance from each other. On the 22nd, Mans Ersson, who inhabited the same room as the above-named Olof Jonsson, was attacked with cholera, and expired after five hours of suffering. This man had not, for a long period, left his home, and had had no communication with any infected locality or person, excepting with Olof Jonsson. The same night, Olof Olsson, dwelling at Södra As, sickened of cholera, but recovered. He had been foreman of the workmen at the prisons at Wenersborg, and had also visited Jöns Olsson when the latter was taken ill. On the 23rd, Jan Nilsson, another of the workmen from Wenersborg, was seized with cholera. On the 24th, Sven Pettersson and his wife, residing at a little distance from the hamlet of Lerbratarne, where the above cases occurred, both sickened of cholera, and these individuals had, a day or two before, visited the people at the above-named hamlet. On the 25th, Brita Olsdotter, widow of Mans Ersson before mentioned, was attacked with cholera, as was likewise Olof Jansson in Ostmanby, whose son had been one of the workmen at Wenersborg, and had returned with them, but was not himself affected. The village of Lerbratarne was now carefully secluded, and no further cases appeared.

We have now examined the details presented to us of the progress of cholera through the south-western part of Sweden in 1850, and shall next briefly call the attention of our readers to the able *resumé* given by Dr. Berg of its ravages, as compared with those of the pestilence of 1834, in the same kingdom. The extreme points reached by the cholera in 1850, extended in Sweden from 55° to 60° north latitude, and from 29° to 34° longitude east from Ferro. Within this area, however, only a few localities were affected, and many of these lay at considerable distances from others. On the eastern side of Sweden, cholera appeared only in one spot—viz., at the port of Döderhultsvik, from which, however, it did not spread into the surrounding country. No conclusions can, we think, be drawn from the geological character of the infected district, though Malmö and the island of Gottland differ materially in this respect from the general gneiss formation of the rest of Sweden.

In 1834 the pestilence advanced on the eastern coast nearly 30 Swedish miles farther to the north, while the western districts, and Götheborg among the rest, suffered severely. From Götheborg, in 1834, the malady spread in a north-east direction as it did in 1850, following the course of traffic along the Götha river up to the Great Wener lake. Besides the capital Stockholm, eighteen other provinces were affected in 1834; but Malmö and Gottland then escaped. Götheborg was ravaged on both occasions; but in 1850 Stockholm remained free from cholera. In 1834 the disease lasted from the 26th of July to the end of December; in 1850 it continued its ravages for nearly five months, or from the beginning of August to the end of December. The greatest spread of the pestilence in 1850 took place during the cold and rough weather which prevailed from the 17th of September to the 17th of October, a period when the diarrhœas and dysenteries incident to the dog-days had almost entirely subsided. In 1834 the mortality from cholera in Sweden was 12,637; in 1850 it amounted to only 1761 deaths. In 1834 more than 12,000 of the above-mentioned deaths occurred in the months of August and September; in 1850, there was a mortality of 422 in these two months, and of 1309 in October, November, and December. Dr. Berg thinks that this great difference in the relative period of mortality in these two years may be accounted for by the fact, that Götheborg, the centre of trade and on the great highway to the rest of Sweden, was one of the first localities affected when cholera appeared in 1834; while in 1850, it lingered long in Malmö, an outlying spot with but little trade.

Of the 4410 cases of cholera in 1850, 2647 were in the towns, and 425 in divers villages and hamlets; while 1338 occurred in the country. Among these last, we have included about 80 cases among the sailors in vessels on the Götha river, where the greatest comparative mortality was observed. In 1834, at least 25,000 cases of cholera were reported in Sweden. It is evident, therefore, that the pestilence of 1850 was much less fatal and less widely spread, the comparative mortality being only 39 per cent. to 50 per cent. in 1834. In 1850, there sickened of cholera 2207 males, of whom 975 died; and of females 1684, of whom there died 745. In 1834, the deaths among females were nearly 100 above those of the other sex. In 1850, the pestilence raged chiefly among the labouring classes, especially among those denominated free labourers (*fria arbetare*), who are not lodged and boarded at the various houses or establishments where they work. Very few of the higher, or even of the middle classes of society, were affected, while in 1834 many of the better ranks fell victims to it.

Dr. Berg next presents us with some observations on the assertion made by the English Board of Health in 1850 in reference to the cholera of 1848-49—viz.,

> "That in every European town where cholera appeared, it gave warning of its approach by the increased prevalence of zymotic diseases, such as influenza, scarlet fever, dysentery, and diarrhœa, and that the last-named, as a premonitory symptom, invariably prevailed extensively before cholera broke out."

On this question, Dr. Berg remarks that agues and small-pox were extremely prevalent in Sweden in 1847 and 1848, that the former of these maladies spread over the whole country at that time to an unprecedented degree, but that in 1849 its limits were much narrowed, and in 1850 only a few isolated cases occurred. The progress of small-pox was not in the slightest degree altered or arrested by the cholera; scarlet fever, of a mild character, prevailed in several districts, but did not increase before the appearance of the pestilence, while gastric nervous fever (typhus) rarely occurred, and never became epidemic. Indeed, the health of the whole country previous to August 1850, was for four years remarkably good. Diarrhœa, dysentery,* and cholerine, appeared annually in summer and in autumn, and were perhaps, indeed, more abundant in 1850 than in the three previous years; but they were not in any way confined to those districts where cholera shortly afterwards broke out. Influenza did not show itself till 1851, when cholera had entirely ceased.

Dr. Berg therefore comes to the conclusion, that

"It was not possible in 1850 to foretel in Sweden the near approach of cholera from the augmented prevalence or mortality of the so-called zymotic diseases, and that if it be true that cholera is always preceded by an increase of these maladies, and perhaps forms a species of culminating point, or is a product of such culmination of any zymotic disorder, it would be labour in vain to seek how the disease arose, or how it was propagated."

The prevalence of diarrhœa and of dysentery in Sweden during hot weather, has been proved by the carefully kept statistical reports of that kingdom, and which extend back for nearly a century, to be dependent upon atmospheric changes; and the frequency of the above-named disorders during the hot summers of 1834 and 1850 does not appear to be in any way connected with the appearance of cholera in those years. Summers quite as hot, atmospheric vicissitudes quite as great, have repeatedly occurred, and have almost always been followed or accompanied by a great increase of cases of diarrhœa and dysentery, and yet no cholera has been produced by these. Indeed, with the exception of the town of Malmö, where the first cases of cholera were observed during the prevalence of a predisposition to diarrhœa and cholerine, that predisposition, which had manifested itself so generally throughout in the hot summer months, had ceased and almost entirely disappeared for weeks, nay in some instances for months, before cholera broke out. In many places, too, where the pestilence numbered many victims, no diarrhœa, dysentery, or cholerine had been observed during the preceding twelve months; and again these disorders prevailed with great severity in other districts which entirely escaped the ravages of the pestilence.

At Wenersberg, Kongelf, and elsewhere, diarrhœa became frequent about a week before cholera fairly broke out, but this disorder is by Dr. Berg regarded as the result of the pestilence itself. It is true that in most zymotic disorders the first cases in any locality are generally the most severe, but, on the other hand, where the invasion of a terrible pestilence is feared, the earliest cases are too often carefully concealed, especially if the quarantine regulations have been infringed by those who are thus affected. During the prevalence of cholera, diarrhœa was very frequent, and if not checked early, it often ran on into well-marked cholera. In Sweden, as elsewhere, the well-known premonitory diarrhœa was not wanting in individual cases, while in other instances, cholera with all its terrible train of symptoms was developed almost at once.

Dr. Berg approaches the all-important question of the mode in which cholera is propagated, with becoming caution. It is evident that our Swedish colleague cannot divest himself of a certain degree of awe on entering the lists against so imposing a body as that of the English Board

* Dysentery is, in some provinces, as has been shown by Dr. Huss, almost endemic, and occasionally it becomes extremely severe and epidemic. Thus in the Dales province (Dalarne) dysentery was remarkably prevalent in 1838, but this province has always escaped the ravages of the cholera.

of Health. Still the facts elicited from these Reports appear to him (and we own to ourselves also) of so convincing a character, that he does not hesitate, after sundry apologies for his boldness, to place himself in opposition to the opinions expressed in the recent Report of the Board of Health in England, and which has been more than once considered in the pages of this journal. A brief *resumé* of Dr. Berg's opinions on this subject is all that we can here present to our readers; a full translation of this, the most valuable part of the Report, would lead us beyond the limits of the space assigned to us. Dr. Berg has divided this portion of the subject into several distinct heads.

1. The appearance and spread of the pestilence on board ships coming from infected ports.

2. Its influence on the officers &c. at the different quarantine stations on the coast.

3. The mode in which the malady spread along the chief lines of communication throughout the country.

4. The number of *first* cases in previously healthy localities, proved to have been in communication with infected districts or persons.

5. The influence of infected persons on their friends, relatives, and attendants.

To each of these we now invite the brief attention of our readers.

1. *The appearance and spread of the pestilence on board ships coming from infected ports.*

Many of these vessels had been several days at sea before cholera broke out on board, and the malady spread from one to another of the crew and carried off many victims. These ships were then sailing on the Baltic in different directions, and it might be urged that they by chance had encountered a current of air charged with cholera miasm, but it is strange that other vessels, which had not communicated with Lubeck, Stralsund, and other towns where cholera was then raging, did not encounter this supposed poisoned current, or at all events passed through it in safety.

2. Of the physicians, servants, and officers, attached to the different quarantine stations on the coasts, eight sickened of cholera, and two died.

3. *The mode in which the disorder spread along the great lines of traffic.*

When cholera broke out, about the 22nd of September, 1850, in the large town of Götheborg, no signs of the pestilence had appeared in the surrounding villages on any side; but after a longer or a shorter period, the malady radiated from Götheborg as a centre, to almost all those parishes, villages, and hamlets which were in constant communication with the town. It was observed, however, that cholera adhered to what has been so often remarked upon, yet never explained by the anti-contagionists—a pre-eminently erratic course. At a single bound, and before any of the intervening parishes were affected, it passed to Lilla Edet, 25 miles up the Gotha river, the great channel of communication with the interior of Sweden. Here it showed itself first on the 28th of September, in the person of a sailor who had that day arrived from Götheborg, and from this man the disease spread to other persons in the village. Almost all the parishes around Götheborg were, as before said, sooner or later infected, but in the most irregular manner; the pestilence would at one time break out in a parish lying some miles to the north, and then, a week after, a single commune to the southward of the town became, in its turn, infected. In the very next parish, perhaps, no case of cholera occurred till ten days or a fortnight later, and we consider this erratic property of the pestilence as one of the strongest proofs of its being transmitted by individual influence, and of its not partaking of the nature of an atmospheric or telluric miasm.

4. Of the 80 communes or parishes that were visited by cholera in 1850, not less than 50 reported that the first cases of cholera occurring within their boundaries were observed in individuals who had either themselves visited infected places, or who received visits from such localities. The parishes around Götheborg took for the most part few or no precautions, but kept up their daily intercourse with the town, and we accordingly find by the appended map, that scarcely a single parish in that district escaped being sooner or later visited by the disease.

5. The influence of the affected persons on their attendants and relatives has been already several times alluded to, but upon this part of the subject we prefer to give a faithful transcript of Dr. Berg's own opinions:

"The reports that have been laid before us appear not only to establish the fact that the visit of a healthy person to an infected locality may produce in such an individual an attack of cholera, either immediately or not till some days after he has returned to his yet healthy residence, but also, 1, that in spite of the salutary and purifying influence which the sea air and the sea breezes may exercise upon a ship which has left an infected port, cholera may break out on board of such a vessel at any time during at least fourteen days after she has been at sea.* We must either admit that in such a case the disease has been caught in the infected port, and has lain dormant for a time in the system of its victims, the premonitory diarrhœa, the first symptom often of the malady having been overlooked or concealed, or else that the ship itself has become the receptacle of a store of miasm while sojourning in port, and which pestilential vapour or miasm breaking forth, at length affects the crew with cholera."

It has, however, rarely happened that many of a ship's crew were seized at once; the disorder has crept from individual to individual in a fashion eminently favourable to the doctrine of contagion.

"2. That after individuals affected with cholera have been removed from such ships into a quarantine hospital, the disease has broken out among the officers and servants of the previously healthy establishment, and it is therefore hardly possible to disbelieve, in such cases, the influence of personal contact in spreading the disease.

"3. That it repeatedly happened, that when a previously healthy individual had had communication with an infected person or locality, and had returned to his own home, where no cholera had hitherto appeared, he has sickened of the pestilence, and that the next victims have been his nearest relatives and friends, and not only those whose condition and habits of life were favourable to the reception of the disease, but even persons living in comfortable circumstances, and that the disease spread again from these new foci. While it is acknowledged that in such cases fear, affliction, and bodily exertion may have a certain influence, yet the effects of personal contact with the sick appear undeniable, unless it can be proved that such depressing influences will of themselves produce an attack of cholera at times when the disease does not prevail in the country.

* It would be very interesting to obtain some positive data as to the period of *incubation* in cholera as compared with other zymotic diseases. As regards measles, the researches of Dr. Panum in Feroe have, we think, fairly proved that the period of incubation for that disorder is from fourteen to sixteen days.

"We must therefore come to the conclusion, not only that cholera can be thus propagated from one individual to another, but that this is really the mode by which it is spread over the country.

"While we admit that possibly cholera *may* be generated in other countries, and even in Sweden, by local influences alone, the facts elicited in the examination of these reports are, we think, of such force, as to overbalance entirely the negative results obtained as to the origin of the disease in Götheborg, Rönneby, and Döderhultsvik, in favour of the origin of cholera in these places from a local cause." (p. 340.)

The fact, that hundreds of persons visited those sick of cholera and yet escaped the disease, is estimated by Dr. Berg at its true value. After remarking that the same is known to occur in all infectious diseases, he admits that something more than mere personal contact, or the inhaling of the miasm from the sick, is required to produce the disorder; that there is some predisposition necessary in the case of the recipient individual, without which he may escape altogether unharmed. What this condition of the system is, we cannot at present define, but we are well convinced that want and misery, uncleanly dwellings and habits, a foul and stagnant atmosphere, and, above all, perhaps, the abuse of spirituous liquors, with its long train of accompanying evils, are causes that most powerfully predispose individuals to receive infection. It is against these, then, that our energies must be directed, in order to mitigate their influence by a well-conducted sanitary reform. We do not mean by this, the boasted activity of well-meaning but mistaken poor-law guardians and magistrates, who, when roused into activity by the near approach of pestilence, enter upon a crusade against the noisome drains, sewers, and privies of our towns; and having perfected their work by whitewashing the walls of each filthy alley, believe that they have done all that is requisite to repel the invading foe;—meanwhile, the lodging-houses, of which the walls have just been cleansed, are permitted to be crowded night and day with the pallid, half-starved victims of intemperance and disease—fit food for any pestilence that may appear; and should cholera be introduced among these, scarcely an individual, as we have ourselves witnessed, will escape the contagion. Without the separation of the sick from the healthy, or at least, unless an abundance of fresh air is permitted to circulate around the beds of the affected persons, cholera will be propagated through contagion in these lodging-houses every time that it visits our shores. Should we, then, have recourse to quarantine regulations for the protection of our coasts? *We think not.* Quarantine, in a country so dependent for its prosperity upon its foreign trade, is, in our opinion, a greater evil than the cholera itself. To be of any avail, such a quarantine involves an almost absolute cessation of intercourse with infected countries; no loophole must be left whereby the disease might creep in amongst us. Is there any one who believes that this is practicable? is there any person who will maintain that by the most stringent penal enactments we can effectually isolate England from the rest of the world? And if we cannot accomplish this, then our imperfect measures of restriction will be infinitely more prejudicial than no quarantine regulations at all. Starvation and misery among the working classes, the inevitable results of the closure of our ports, would create a tenfold predisposition to the disease; and should cholera then be wafted to our shores, twenty victims would fall before it for one that would have died, if the trade of our great commercial country had continued unchecked. In small communities, as in the isolated parishes and thinly scattered villages of Sweden, such prophylactic measures may be, and undoubtedly have been, available, as they have likewise proved efficacious in our northern islands, where, although in a sanitary point of view every circumstance favoured the progress of cholera, yet a strict quarantine enforced on the few vessels that visited their shores, effectually protected these islands from the pestilence. Since, then, we cannot hope to exclude the pestilence altogether, let us oppose every obstacle to its progress, and mitigate the severity of its ravages by effective sanitary measures. On the advantages of sanitary reform all parties are agreed—all acknowledge the necessity of good drainage in our cities, and the protective benefits of cleanliness, temperance, and good food; but the great strongholds of disease—the very foci of infection—our low lodging-houses in the towns, villages, and hamlets, have been too often overlooked; and while deceiving the public by an outward show of cleanliness, their whitewashed walls have concealed a charnel-house within.

Of the care and attention bestowed by Dr. Berg in the preparation of this able Report, it is impossible to speak too highly; and it would form an excellent model to be followed in the documents to be issued by any future Board of Health.

Edward Charlton.

Review III.

What to Observe at the Bedside and after Death in Medical Cases. Published under the authority of the London Medical Society of Observation.—*London*, 1853.

The little book before us (a convenient pocket volume) enumerates in a very satisfactory manner all the points of importance which are to be taken into consideration in the examination of medical cases, as well at the bedside as after death. It appears that the members of the Society of Observation felt the necessity of performing their examinations, and the arrangement of the symptoms and after-death appearances, according to a certain plan, by which they might be enabled more easily to compare the various results and inferences, and to classify them for statistical purposes. To this end the Society adopted a form which had been framed by Dr. Walshe, and modified by a committee. As it appeared desirable that a fixed form should be used, not only in the limited circle of the Society, but, as much as possible, in the whole profession, the publication of the little volume was resorted to. The Society deserve our best thanks, as their task has been fulfilled with equal zeal and circumspection. Such a work is very useful in many respects, among which the following points deserve particularly to be mentioned:

1. It promotes indirectly the *progress of science.* Though it may contain nothing materially new, yet it is evident that through cases which are elaborated according to the plan laid down in it, science must be more advanced than through others described without a certain plan. Cases given according to a fixed form can be much more easily compared and, for statistical purposes, digested; besides this, the cases elaborated

according to 'What to Observe,' will be as complete as possible; we shall not miss some one or other important point, as so frequently happens in the generality of cases, in which, too often, the very circumstances required for the purpose of drawing inferences are entirely omitted.

2. The work is a very useful one for *practice.* In simple cases the medical practitioner will perhaps not want to look into 'What to Observe,' but he will derive from it great benefit in obscure cases with difficult diagnosis, such as are constantly met with. Frequently it happens, in such circumstances, that even the most experienced and circumspect observer overlooks some one or other point of importance, which does not lie on the surface. Thus it may happen that he completely misunderstands the case. Whoever, under such circumstances, performs a thorough examination, according to this little book, cannot easily neglect any symptom of consequence.

3. It offers much *instruction.* We will not say that it is agreeable and amusing to read 'What to Observe;' on the contrary, its contents are, by the nature of the object, necessarily rather tedious. But in spite of this, we do not hesitate most earnestly to recommend the attentive and *frequently-repeated* study of all its chapters; the younger as well as the elder members of the profession will find it an excellent mode of increasing their medical knowledge. They will not only derive benefit from it for the examination of obscure cases, as already said, but, what is even more important, they will also be always reminded by its study that in the investigation of any disease many things are to be taken into consideration. Every single case, even the apparently most simple one, is more or less frequently very complicated. By constantly being mindful of this, the physician is best protected against the danger of compressing the infinite variety of cases met with in practice into the deficient classes of an unalterable medical system, or of thinking it sufficient to give the diagnosis of a case by one single and necessarily insufficient word, such as "Rheumatism, "Gout," "Pneumonia," "Gastritis."

The arrangement of 'What to Observe' is the following. It is divided into two parts. The first relates to the living patient, the second to the dead body.

Part I. *Clinical examination of a patient.*—Personal description and physiological peculiarities. Prèvious history. Condition of the patient at the time of observation; generalities; condition of the skin and its appendages; of the organs of locomotion, digestion, respiration; circulation (with the state of the blood); the lymphatic, the uropoetic system (with urine); the organs of generation; the encephalon, with its coverings and appendages; the spinal cord; the organs of the senses; the nerves; the vascular glands. Progress of the case—diet, regimen, external and internal remedies—phenomena of death.

Part II. *Examination of the body after death.*—At first, points to be ascertained and noted prior to commencing an examination. Then, points to be noted *during* an examination—generalities, skin, organs of locomotion, &c., in the same order as just mentioned for the clinical examination. In an *appendix,* particular attention is called to the several objects under the following heads: Condition of the mucous membranes; of the serous membranes; redness; serosity; purulent-looking fluid;

lymph and other exudations: adhesions; abscess; fistulæ; mortification; gangrene-sphacelus; ulcers; perforations; wounds; cicatrices; tumours; cysts; cancer and cancerous-looking matter; tubercles, or tubercular-like bodies; microscopical characters.

The whole account is so complete, that no essential point is wanting, only a few trifling statements might have been added. If we therefore, in the following remarks, mention some one or other subject which we should have annexed, or managed otherwise, we do it not with the purpose of showing a deficiency, but merely with the view that so useful and thankworthy a work may, in a future edition, be free, as much as possible, even of the most unimportant omissions and wants.

To the physiological peculiarities (p. 2) the reviewer thinks desirable to append also a *chemical* history of the individual—i. e., an account of the metamorphosis of matter. As every individual displays certain peculiarities concerning size, weight, &c., so also there are peculiarities in the chemical changes of tissue, in the quantity of the daily excretion of fæces, of urine, of the quantity of solid matter daily contained in the urine, of the various substances composing it—as water, urea, uric acid, salts, &c. The knowledge of these chemical peculiarities is of great importance for the proper understanding of diseases. As, for instance, an individual usually forming or excreting a large quantity of urea, or usually passing through the kidneys a considerable part of the water daily excreted from the body, will bear much worse an affection of the kidneys than another person whose daily excretion of urea is smaller, or in whom a greater proportion of water is excreted through the skin. It cannot be denied that such a chemical exposition of a case is learned with difficulty, therefore those few medical men who recognise the necessity of the investigation draw back during practice, on account of the labour it requires. But as now many of the methods of examination are so much simplified that they may be easily practised, we think that this subject ought not to be omitted in the future edition of a work which treats of the various ways of examining a patient.

Ad No. 115. Physical examination of *the liver.*—It is possible only in few cases accurately to explore the extent of dulness *posteriorly;* but we miss the much more important exploration of the vertical diameter of the dulness in the anterior median line,* which, of course, cannot be performed without examining at the same time for the size and position of the heart.

Ad 184. *Arteries.*—The rigidity of arteries (thickening of the walls with or without deposition of calcareous matter), a point of great importance as the most frequent source of apoplexia sanguinea, appears to be alluded to by "artery tortuous," but we should wish to have it more distinctly mentioned, as it is by the generality of medical men very little noticed, though it must be of considerable significance in the prognosis and treatment.

Ad 220. *Urine.*—In 'What to Observe,' we find it recommended to

* Line from the incisura manubrii sterni to the point of the processus xiphoideus and symphisis pubis. See Chronicle—"Conradi's Paper on the Examination of the Size," &c.

record the quantity of urine by *weight* (in ounces).* This appears to us to be unnecessarily laborious and troublesome. It is much more simple to *measure* the urine and note it by cubic centimetres. We can always do this at the bedside without any loss of time, by making use of graduated urinals. If, at the same time (and it never should be neglected), the specific gravity is taken, the absolute weight may be calculated very easily. Under the head of the *chemical examination* of urine, it might have been mentioned that a quantitative analysis, reduced to 1000 parts, is of very little use, and that this little stands in no proportion to the time employed in it. It is necessary to give always the period during which the urine has been excerned, and to reduce the results, not to 1000 parts, but to one hour or 24 hours.

Ad 452, 476.—In the examination of the blood of the dead body, which is best taken from the cavities of the heart, we should wish to draw attention to some points not mentioned. 1. Does the fluid part of the blood, after having been taken out (and placed in a glass), afterwards coagulate or not? 2. Does the blood develope ammonia or sulphuretted hydrogen? To answer this question, a glass stick moistened with hydrochloric acid is held over a glass half filled with the blood (white clouds indicate the development of ammonia). A paper, humected by a solution of acetate of lead, is by means of a cork placed over the blood. Should there be a development of sulphuretted hydrogen, the paper will be blackened after a short time. 3. Under the head of the microscopical examination, we should have taken notice of a method for the approximative estimation of the quantity of the colourless corpuscles (lymph corpuscles) contained in the blood, and of the necessity of an exploration for the accidental admixture of other elements of the body (coagula, caudated cells, cancer cells, &c.).

We should further wish that allusion should be made to the indispensability of a chemical examination of the various organs of the body (under the head of the lungs, the liver, the spleen, the kidneys, &c.) It is true that in most cases it is not easy to fulfil this requirement, but the reviewer has succeeded already now so far in the simplification of the methods for several examinations of that kind, that they ought not to be neglected in any good clinical institution or medical school; and it is to be hoped that this will soon lead to the cultivation of other simple and exact methods. We beg to add a few of the methods which we lately have adopted.

Examination of an organ for the quantity of blood or better of hæmatine contained in it.—We cut the organ (liver, spleen, kidney, &c.) in several pieces, or, if possible, we rub it in pieces, wash it out with water and squeeze it out. The red fluid obtained in this way is collated through a piece of linen, and, as much as possible, freed from solid particles; after this the whole of the fluid is measured. In a carefully measured part of it, we can by means of a scale for the hæmatine,† within a few minutes' time calculate the quantity of hæmatine, and through this indirectly that of blood in the organ in question.

* Professor Vogel has been misled here by a slight inaccuracy in 'What to Observe.' Strictly speaking, the expression should have been "fluid-ounces," which would have implied measurement. As it is, however, always understood in England, that in speaking of an ounce of fluid we refer to measure and not to weight, the inaccuracy, so far as this country is concerned, is of little consequence. It is very likely, however, to mislead foreigners.—*Editor.*

† Archiv des Vereins f. gemeinschaft. Arbeiten. Heft ii. p. 195.

It requires not more time or trouble to ascertain the quantity of *chlorine*. The organ is prepared as just described, but washed out with *distilled* water instead of common water. To a carefully measured quantity of this fluid, nitric acid (free from chlorine) is added in a surplus, and afterwards as much of a nitrate of silver test solution ("*titrirte Loesung*," i.e., a solution of any test, in this case of nitrate of silver in a certain proportion, by which we know the exact quantity of nitrate of silver contained in a certain measure of the "test solution"), as is sufficient to precipitate all the chlorine contained in the fluid. During the process of adding the test liquid, a part of the fluid mixed with this liquid is filtered; this filtered fluid is examined, partly by nitrate of silver, partly by chloride of sodium, in order to detect whether too little or too much of the test liquid has been added. If the rather dirty part of the rubbing in pieces and washing out can be done by an inferior assistant (a male attendant of the patients, &c.), both the one and the other method will require only a few minutes. Both methods may, of course, as well be used to determine the quantity of hæmatine and chlorine contained in the blood furnished by venesection, &c., and both methods, when properly executed, are sufficiently exact.

After all we have said, we can only highly recommend 'What to Observe,' and must sincerely wish that it may be frequently perused and consulted by every medical man. But we cannot help remarking, at the same time, that we consider the work only as an introduction, as the precursor of a greater, more extensive one, which we trust may soon appear, as we are certainly much in need of it. 'What to Observe' is only an enumeration or a register of *what* we are to observe at the bedside and on the dead body, but which does not profess to mention *how* this is to be done. It is presumed in it, that those who read it are well acquainted with the necessary methods of examination, and are to be reminded only of what they know already. In a few instances only the authors make an exception, as in the method of weighing the liver, heart, lungs, &c., by stating that the gall-bladder is to be attached to the liver, but emptied; that the weight of the lungs is to be taken "after tying the vessels, with trachea and bronchi attached;" that the heart is supposed to be taken out with the venæ cavæ cut through close to the auricle, the aorta and pulmonary arteries just above the free edges of the valves, &c. &c. They are quite right in doing so, as it is impossible to compare the results except the process of weighing has been performed according to the same rules. But the reviewer thinks that an accurate statement of *how* to perform certain processes ought to have been given also in other places. Thus, for instance, how to ascertain the *temperature* (65), which cannot be properly done without paying attention to certain precautions (Traube); how to take the *weight of a body* (56), which ought to be performed either without clothes, or with taking off the weight of the cloth from the entire weight; with an empty stomach, and after the urine and fæces have been emitted. In treating of the physical emanations of the lungs, &c., the authors request that the circumference of the chest be measured during both deep and medium inspiration and expiration; but it would appear to us necessary to add to this, that the circumference is *always* to be measured by taking it during the deepest inspiration and expiration, and putting down the medium of the two; this is the more desirable, as we daily remark mistakes in the description of cases, where the circumference is given without attention being paid to the state of expansion in which the thorax was. We should have further wished that the standard number might have been mentioned in all cases where numbers are in question, in order that those who examine a case might find at once, in consulting the book, whether the number they have found is below or above the average. As, for instance, the average of the weight and size of the liver, the spleen, the kidneys, the heart; the average quantity of urine, &c. &c. It is true that most of these average numbers are yet to be found, and it is to be hoped that the work before us will lead soon to this result, by giving an impulse to such investigations. But for the present, also, an only preliminary communication of such numbers would have been very valuable, and would, no doubt, have led to more frequent examinations, as many practitioners are more inclined for them, if they at once find, by doing so, whether their case, in the one or the other way, deviates from the standard. The reviewer has, for many years, been accustomed to perform such explorations, in order to promote individual medical statistics, and begs to subjoin some of the numbers he has found. He will give only the average numbers, such as may serve in some way as the standard for the practice, adding the outlines of the method by which they are found. Though he has extended his inquiries over the whole of the body, yet he will mention here only those points on which he has the most numerous (in general, several hundred) observations. He begs also to remark, that he gives here only the numbers he has found himself, without comparing them with those of other observers (Reid, Sibson, &c.), as this would lead too far for the present purpose. The weight is expressed in *grammes;* the measure in *centimeters* and cubic centimeters (CCM).

Liver.—Average weight (the gall-bladder being emptied), with the exception of cases of considerable enlargement or shrinking:

Adult men (average of 100 cases) .	1590 grms.	= 56	ozs. *av.*
„ women (average of 40 cases) .	1360 „	48	„

Size of the liver, as found by percussion in the living body. The numbers are the average of several hundred observations of adult male individuals. For women they are not quite so high. They express the whole diameters, from superior border, where it is covered by the lung, downwards to the extreme end of the thin margin (superficial and deepseated dulness together.)

Vertical Diameters:

1. In the *linea axillaris* (from the middle of the axilla to the anterior extremity of the 11th rib)	12 CM.	($4\frac{2}{3}$ in.)
2. In the *linea mammalis* (vertical line below the nipple)	12 „	„
3. In the *linea mediana* (the superior margin meets with the heart)	6 „	($2\frac{1}{3}$ „)

Horizontal Diameter:

4. The liver extends to the left beyond the linea mediana . . . . 6 CM. ($2\frac{1}{3}$ in.)

(Though these numbers may not be perfectly accurate, yet they are sufficiently correct for the practical purpose, and are easily borne in mind.)

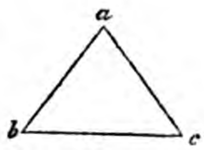
Spleen.—Average weight in adults:

Male (73 cases)	232 grms. =	$8\frac{1}{5}$ ozs. *av.*
Female (25 cases)	181 „	$6\frac{1}{4}$ „

Average diameter of many hundred observations in male living individuals (in the female slightly smaller), and with an empty stomach, as the spleen becomes considerably larger during the act of digestion, some hours after dinner.

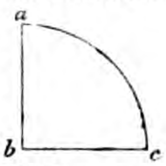
Vertical diameter in the linea axillaris (superficial and deep-seated dulness together) . . .	6-8 CM. ($2\frac{1}{3}$-3 in)
Horizontal diameter (the spleen extends beyond the linea axillaris to the right)	3-4 „ ($1\frac{1}{6}$-$1\frac{1}{2}$ „)

Heart.—Average size and position in adult men, as found by percussion:



Deep-seated dulness (*Herzdaempfung*).—In woman, on account of the mammæ, the exploration is often impossible. The following numbers are the average of several hundred cases. The shape represents a triangle with three sides and three corners.

Superior corner (*a*) at the 3rd costal cartilage, or 3rd intercostal space to the left of the sternum. *Right* corner (*b*), in the height of the 5th, more rarely of the 6th costal cartilage, slightly to the right of the right sternal margin, about 3-5 CM. ($1\frac{1}{6}$-$1\frac{3}{4}$ inches) from the linea mediana. *Left* corner (*c*), 5th intercostal space or 6th rib, 10 to 12 CM. (4-$4\frac{2}{3}$ inches) to the left of the linea mediana.—*Length of the sides. Left* side (*ac* = length of the heart) 12-14 CM. ($4\frac{2}{3}$-$5\frac{1}{2}$ inches.) *Inferior* side (*bc* = breadth of the heart) 13-16 CM. ($5\frac{1}{8}$-$6\frac{1}{3}$ inches.) The exploration of the *right* side (*ab*) has seldom a practical interest.



Superficial dulness (*Herzleerheit*), i.e., that part of the heart which is not covered by the lungs (smaller and disappearing in cases of emphysema pulmonum, larger in cases of hypertrophy of the heart, and of considerable exudation into the pericardium), presents likewise a triangular form. *Superior* corner (*a*), 4-5 left costal cartilage. *Right* corner (*b*), basis of the processus niphoideus. *Left* corner (*c*), 5th intercostal space or 6th rib, 6-8 CM. ($2\frac{1}{3}$-3 inches) from the median line.—*Position and length of sides. Right* side (*ab*), in general corresponding with the right margin of the sternum. *Left* side (*ac*) 6-8 CM. ($2\frac{1}{3}$-3 inches). *Inferior* side (*bc*), 7-9 CM. ($2\frac{4}{5}$-$3\frac{1}{2}$ inches).

Quantitative observations about the URINE.—*In adult men:*

Average *quantity* of 24 hours .	1500 to 1600	CCM.	= $52\frac{3}{4}$ to 56	ozs.
„ „ 1 hour. .	60 „ 70	„	$2\frac{1}{8}$ „ $2\frac{3}{4}$	„
1000 grms. of a man excrete in 1 hour	1	„		
100 centim. of height of body are proportionate in 1 hour to about . .	40	„		

Average specific *gravity,* 1020.

Urea, by the method of *Liebig* (nitrate of mercury):

Average of 24 hours	36 grms.	= 556	grains.
„ 1 hour	1·5 „	= 23·166	„

Chlorine, either by the method of *Liebig* (nitrate of mercury), or by a test solution of *nitrate of silver* (as described above):

Average of 24 hours	10 grms.	= 154·440	grains.
„ 1 hour	0·4 „	= 6·077	„

Free acid (reduced to oxalic acid = O + 3 aq.) by a test solution of *ammonia* or *potass. caust.*:

Average of 24 hours	2·2 grms.	= 33·970	grains.
„ 1 hour	0·1 „	= 1·5444	„

Phosphoric acid, by *Liebig's* and *Breed's* method, with ferr. chlorid. and soda acet. (test solution):

Average of 24 hours	4·3 grms.	= 66·7	grains.
„ 1 hour	0·18 „	= 2·779	„

Sulphuric acid, by test solution of *chlor. of barium:*

Average of 24 hours	2·0 grms.	= 30·888	grains.
„ 1 hour	0·09 „	= 1·389	„

In taking leave of the volume before us, we must once more express a most sincere wish that the short communications we have ventured to add may lead to further inquiries in this important and interesting field.

J. Vogel (Giessen).

REVIEW IV.

Nouvelle Fonction de Foie, considéré comme organe producteur de Matière Sucrée chez l'Homme et les Animaux. Par M. CLAUDE BERNARD.—*Paris,* 1853. pp. 92.

New Function of the Liver, considered as the formative organ of Saccharine Matter in Man and Animals. By CLAUDE BERNARD.

AMONG those who have more recently aspired to advance the science of physiology by experimental inquiry, the name of Claude Bernard is honourably distinguished. He has investigated with patience and ingenuity various problems in the phenomena of life at once difficult and important; and although in some instances his generalizations have been advanced hastily, and in others his experiments have been inconclusive, still, since the days of Magendie, France has produced no physiologist who has more earnestly laboured to elucidate some of the most abstruse functions of the animal economy, or who has accomplished more brilliant discoveries by his researches. To give a succinct account of these discoveries is our object in the present review. We have abstained, except occasionally, from criticism, nor have we thought it necessary to test the validity of everything M. Bernard has advanced. Our chief endeavour has been to place before the reader a clear statement of his labours, and we must leave others to decide on the value of his experiments, and to confirm or refute his inductions. The title of his latest work is placed at the head of the review, but we propose to deal in succession with all his numerous papers, and trust to have omitted from our list none that contain either new or interesting information. Long an assistant of Magendie's in his experimental inquiries, Bernard has derived from that able professor the ready tact by which alone experiments on living animals can be successfully conducted, and the penetration without which it is impossible to interpret aright the results that such inquiries afford. Like most French physiologists, however, he performs vivisections with unscrupulous freedom; and if this method of inquiry has led him to some of his most splendid triumphs, it must be confessed that in many instances he has resorted to it without sufficient necessity, and has thereby exposed a noble science to reproach.

1. *Inquiries into the Nervous System.*—An inquiry into the function of the spinal accessory,* which obtained for its author the prize of experimental physiology from the Academy, is among the earliest of Bernard's productions. A detailed account of the experiments which he performed, and the conclusions to which he was led, while investigating the subject, appeared first in the fourth and fifth volumes of the 'Archives Gén. de Méd.,' and has since been separately published in the form of a thesis.† It is, however, unnecessary to enter at length into his views respecting the function of this nerve, because they have been elaborately criticized by able men, and are embodied in all recent text-books on physiology.

Not long after the publication of these researches, Bernard wrote a very interesting paper on the impairment of taste which sometimes accompanies paralysis of the portio dura.‡ He relates four cases in illustration of this singular defection, in each of which the individual affected was unable to distinguish with natural facility the contact of sapid matters, such as quina, or of acids, as the citric. These substances were perceived readily and naturally enough on the sound half of the tongue, but on the affected side they seemed to be tasted with slowness and difficulty, and did not produce the vigorous sapid impression which they impart under healthy conditions to the nerves of taste. The appearance of the tongue was unaltered, its mucous membrane being moist, and as sensitive as ever to the influence of common irritation. The loss of function was always limited to the two anterior thirds of the affected half, and was more or less marked in proportion as the other symptoms of facial palsy attained a greater or less severity. The chorda tympani being the only medium of communication between the facial and gustatory nerves, this impairment of taste was attributed to a cessation of its influence; and Bernard found, while endeavouring to elucidate the subject by experiment, that division of the chorda tympani in the internal ear of animals was followed by a partial loss of

* Arch. Gén. de Méd., vols. iv. & v. pp. 397 *et seq.*

† Récherches Experimentales sur les fonctions du Nerf Spinal du accessoire de Willis. Par Cl. Bernard. Paris, 1851.

‡ Arch. Gén. de Méd., vol. vi. 1844.

taste, similar to that which he had observed in the human subject as a consequence of disease. If, therefore, there is impairment of taste, conjointly with paralysis of one side of the face, it may be reasonably inferred, either that the facial and chorda tympani are both implicated in a common affection, or that the facial is paralyzed above the origin of the chorda tympani. Bernard relates two cases in which cadaveric examination proved the justice of this conclusion. In both there had been facial paralysis, with partial loss of taste in the same side of the tongue, and in both the chorda tympani was involved in the affection. In one of these examples, indeed, the internal ear was the centre of a scrofulous suppuration, which had fairly destroyed the chorda tympani and a considerable portion of the facial.

Bernard accounts for this curious phenomenon by supposing that the papillæ are mobile structures, which elongate and contract under the influence of the chorda tympani, and are thus enabled to absorb sapid matters with great rapidity, so that when this nerve is paralyzed, they are no longer capable of adequately discharging their functions.

Against this hypothesis it may be urged, that the distribution of the chorda tympani to the mucous membrane of the tongue, is without anatomical foundation; that the existence of muscular fibres in the papillæ, capable of rapid contraction, and therefore organic, has never been demonstrated; lastly, that it is difficult to understand how the papillary movements, credited by Bernard, can in any way facilitate the perception of impressions by the nerves of taste.

The existence of sensibility in the anterior roots of the spinal nerves, to which Magendie first directed attention, has been since observed by Bernard.* If the spinal cord of a vigorous, well-nourished dog is exposed, it will be found that pain is given when the anterior roots are pinched; and if one of these roots be divided, pain invariably follows irritation of the peripheral extremity, while no effect is produced when it is applied to the central. This sensibility can be no longer excited upon division of the corresponding posterior root. In one remarkable instance, however, Bernard found that it was not completely destroyed, till he had divided the posterior root of the spinal-nerve above. It appears essential to the manifestation of this peculiar sensibility, that the animal should be vigorous and healthy, and that the experiment should be conducted without loss of blood or serious injury. Abstinence from food diminishes it, and long abstinence makes it disappear entirely. Magendie noticed that its existence could not be demonstrated if the animals had lost much blood, or were previously in an anæmic state. It does not, moreover, appear directly after the exposure of the spinal cord, but the animal must be left quiet for a little time, till the immediate effects of the operation have subsided, and the exposed structures have partially regained their natural warmth. These precautions are important; for from inattention thereto, the existence of the recurrent sensibility has been denied by physiologists of high reputation and among others by Longet. Under the administration of ether, the recurrent sensibility departs before that proper to the posterior roots; and it returns later than the sensibility in the posterior roots, and simultaneously with the peripheral or cutaneous

* Comptes Rendus, 1847, vol. xxv. p. 104.

sensibility of the body when the anæsthesia is passing away. If, then, the posterior root of a spinal nerve be divided, pain is excited by irritation of the central extremity; and if the anterior root be divided, sensibility is manifested when its distal extremity is irritated, provided no injury has been inflicted on the posterior root, which sensibility is destroyed by division of the posterior root, and disappears when the animal becomes exhausted.

During the course of last year, Bernard laid before the French Academy* some remarkable observations, resulting from experiments made by him on the sympathetic system. Having divided the sympathetic in the neck of a rabbit, he noticed an elevation of temperature in the tissues on that side of the head, ranging between 5° and 7° Fahrenheit. When contrasted with the uninjured side, this increase of heat was plainly perceptible by the hand, and admitted of accurate measurement by the introduction of the bulb of a thermometer within the nares, or into the external auditory meatus. While, however, this development of heat was most evident on the side of the neck where the sympathetic had been divided, and least so in the opposite corresponding region, where it had been left uninjured, the whole body shared, to a certain extent, in its production, and exhibited evidences of a temperature exceeding the natural standard. Indeed, not much difference was apparent between the warmth of the abdomen and rectum, and that of the side of the head which had undergone mutilation; while, however, the mercury rose to 72° Fah. on the affected side, it was raised only to 68° or 70° by the uninjured one. Nor was this elevation a transitory phenomenon; it persisted with remarkable steadiness till the animals were killed; and even after death, Bernard found the side of the neck on which the experiment had been practised, the last part of the body to lose its vital heat,—the last, in fact, to die. No signs of inflammation or of any other disorder exhibited themselves, to which this augmentation of temperature might be reasonably referred; for although, in the first instance, it was accompanied by increased vigour of the circulation and vascular turgescence, yet these conditions subsided in a few days, while the temperature itself showed no symptoms of diminution.

Some singular results were obtained by exposing rabbits thus mutilated to extremes of heat and cold, the details of which are given in the 'Comptes Rendus.'

2. *Inquiries into the Digestive System.*—We pass from this abstract of the views and experiments of Bernard on the nervous system, to consider his researches respecting the digestion and assimilation of food, the function of the liver, and the organs which participate in sanguification.

One of the most important consequences of these discoveries has been the establishment of the doctrine that animals, like vegetables, are endowed with the power of transforming one ternary principle into another, and also of changing quaternary principles into ternary, by eliminating nitrogen from the former, and converting them into sugar and fat; in short, that the power of chemical combination, as well as of chemical destruction, has been conferred alike on animals and vegetables. Other observers have contributed in no small degree to the recognition of this important truth;

* Comptes Rendus, vol. xxxiv. p. 471. Premier Semestre, Fevrier, 1852.

but the inquiries instituted by Bernard, with reference to the formation of sugar in the liver, have resulted in proving beyond a doubt that such transformation is a constant and habitual process of the animal economy, and that evidences of its operation may be detected in almost all vertebrate, and in a large number of invertebrate animals.

As Bernard has made out some interesting particulars respecting the secretions of the different salivary glands, a cursory review of this subject will, perhaps, form an appropriate prelude to his experiments on digestion and the formation of blood.

The salivary glands,* though analogous in structure, secrete fluids which present dissimilar qualities, and seem destined for dissimilar purposes. The saliva formed by the sublingual gland is tenacious and viscid, and is discharged most freely when mastication is completed, and when the first act of deglutition is on the point of accomplishment, the performance of which it facilitates by covering the arches of the palate and the alimentary mass with a slippery fluid, which enables the food to slide on more easily to the œsophagus. The parotid secretion is limpid, and without viscidity. It flows freely during mastication, and apparently serves the purpose of dissolving some of the more simple constituents of food, and also of softening the whole mass, for the quantity discharged is proportionate to the dryness or humidity of the provision. The fluid formed by the submaxillary gland holds a middle place between the parotid and sublingual secretions, being neither so limpid as the former, nor so tenacious as the latter. Bernard assigns to it the property of communicating sapid impressions to the nerves of taste, on the following grounds. If vinegar is poured down the throat of a mammalian animal in whom the salivary glands have previously been exposed, the submaxillary secretion alone is discharged freely; and if the horizontal portion of the lingual nerve is divided, and the central extremity irritated, a reflex stimulus is the result, which produces a free flow of submaxillary saliva. But the more profuse flow of saliva from the submaxillary than from other glands, when a pungent fluid is placed on the tongue, or irritation applied to the divided gustatory nerve, proves only that the submaxillary is more under the control of reflected or direct nervous influence than the other salivary glands—a circumstance every way compatible with the revelations of anatomy. It proves nothing at all respecting the peculiar power assigned to this secretion of being the exclusive minister to the possession of taste—a property which it is not likely to possess exclusively, and which, at all events, must be vouched for by other and better experiments before its truth can be accepted in physiology. Bernard found that by boiling a portion of either of the salivary glands, the peculiar qualities of the secretion which it furnished were imparted to water. Boiling a portion of the parotid gave a limpid fluid, endowed with all the properties of saliva; and the same process applied to the sublingual gland, produced the viscid secretion which it naturally affords. In this way he was enabled to demonstrate that the labial and buccal glands furnished a tenacious secretion resembling that of the sublingual; and fishes and reptiles, who possess neither cryptiform nor conglomerate salivary glands, have their mouths furnished with a mucous membrane that seems to compensate for the want of those organs,

* Comptes Rendus, Fevrier, 1852, op. cit. Arch. Gén. de Méd., tom. xxviii. 1852, p. 350.

for when boiled it communicates a viscid property to water, like the labial or buccal glands of a mammalian animal.

While, however, these secretions exhibit different physical qualities, they are similar in chemical composition, and few substances thrown into the blood escape from that fluid by their intervention. Neither sugar nor salts of iron, nor the ferrocyanide of potassium, could be detected by Bernard in the saliva, after they had been injected into the blood. The sugar which has been found in diabetic sputa is not present in the saliva, but in the bronchial mucus expectorated with it. On the other hand, some salts, such as bromides and iodurets, passed so rapidly into the saliva, as to be able to draw with them salts which, under other circumstances, were not eliminated by this secretion. In this exercise of elective elimination the saliva resembles the gastric juice, urine, and other secretions, each of which fluids is invariably found to contain certain mineral salts, to the exclusion of others. Some later experiments of Bernard seem to show that iodine and its compounds appear in the saliva almost immediately after their entrance into the blood. The same substances show themselves in the bile and urine, after a longer interval; but when all evidence of their presence in these secretions have passed away, they are yet eliminated for some time by the tears, the saliva, and the pancreatic juice. On the other hand, sugar, the ferrocyanides of potassium, and the salts of iron, are excreted by the bile and urine with great rapidity, but never appear in the saliva or pancreatic juice.

The office of the saliva is regarded by Bernard as merely mechanical.* It acts, according to him, like so much water moistening the food, and thus facilitating mastication and deglutition. He performed some experiments on horses—which were barbarous, because quite unnecessary—to prove that deglutition is accomplished with far less facility when the supply of saliva is diminished or cut off. It is, however, unnecessary to enter at length into the influence of the saliva on food, because Mialhe and Bouchardat and Sandras have clearly shown that it possesses the power of transforming boiled starch into dextrine, and to a slight extent exercises that power; and Bernard admits that the saliva can effect this change, in common with many other fluids, but says that the food passes into the stomach too rapidly for it to take place under natural circumstances.

Proceeding with the physiology of digestion, and with the investigation of the secretions by which it is accomplished, we are brought to the review of a very able series of papers communicated to the Academy of Sciences by Bernard and Barreswil, in the years 1844 and 45.† As, however, most text-books of physiology have *embodied* the substance of these observations in their pages, it will be unnecessary to repeat information already sufficiently known, and we shall therefore confine ourselves to particulars which have been hitherto unnoticed, or not noticed sufficiently, in systematic works. The experiments of Bernard and Barreswil, with reference to the quality of food, and the facility which it presents for assimilation, have furnished results alike interesting and instructive. They were pur-

* Arch. Gén. de Méd., tom. xiii. p. 1.

† Comptes Rendus, vol. xviii. p. 783; vol. xix. p. 284; vol. xxi. p. 38. Arch. d'Anatomie, &c., published with the Arch. Gén. de Méd., 1849, volume xi., *Digestion*, par Cl. Bernard.

sued as follows: The substance to be tested was first dissolved in gastric juice collected through a fistula in the stomach, and then injected slowly into the blood of the jugular vein. The urine was subsequently examined, and if the injected material was found therein, it was considered unassimilable; but if the urine manifested no traces of its presence, it was inferred that the injected solution had been appropriated by the blood for the purposes of nutrition. Aqueous solutions of grape-sugar and albumen appeared unchanged in the urine, but when the same substances were dissolved in gastric juice, and then injected, they were assimilated, no traces of their presence in the urine being discovered. Some carefully conducted experiments on dogs seemed to show that sugar and albumen were assimilable, but that gelatine was not.

Bernard and Barreswil repeated these experiments on themselves, and with the same result. Each of them took, when fasting, a quantity of sugar. of albumen, and of gelatine, on separate occasions, and neither could detect sugar or albumen in the urine, but never failed to detect gelatine. As it appeared evident from these results that the gastric juice not only dissolved the constituents of food, but at the same time adapted them for assimilation by the blood; the next object of inquiry was the manner in which this adaptation was accomplished. They were led to believe, from a series of ingenious experiments, that the acidity of the gastric juice was due to the presence of lactic and phosphoric acids: but as subsequent analyses have thrown considerable doubt on the accuracy of this view, it is unnecessary to detail the researches on which it was based. Afterwards they satisfied themselves that there is an organic principle possessing great digestive powers, common to the saliva, gastric, and pancreatic juices, whose mode of action on the constituents of food is determined by the reaction of the medium in which it is placed. If the gastric juice is rendered alkaline, it loses the power of digesting albumen, and acquires that of transforming starch into dextrine and sugar; while the saliva and pancreatic juice, when rendered acid, cease to exert any influence on boiled starch, but act with energy on albumen.

Todd and Bowman, however, assert that they have been unable to find that gastric juice made alkalescent by the addition of an alkaline salt, acquires the faculty of effecting chemical transformations in starch; and probably the whole hypothesis rests on insufficient data. The following experiment of Bernard seems to show that the proper secretion of the stomach takes place only in the pyloric third of that organ. A solution of cyanide of potassium was injected into one of the external jugular veins of a dog, and into the other was thrown a solution of the proto-sulphate of iron. The salts passed out from the blood into the gastric juice, and gave clear evidence of their presence in that secretion by the formation of prussian blue. The colouring of the mucous membrane was, however, confined to the pyloric portion of the stomach, in which spot it made its appearance twenty or thirty minutes before it was observed in any other secretion.*

Blondlot discovered that the albuminous principles of food, if left sufficiently long in contact with the gastric juice, were by that fluid perfectly digested; and if injected into the blood, were assimilated. Bernard

* American Journal of Medical Sciences, October, 1851.

repeated the experiments, and confirmed their results.* Both of these physiologists, however, perceived clearly that observations on the changes effected in the food by the gastric juice out of the body, afforded no exact criterion of the office it discharged under natural conditions within the body. It was at once evident that the length of time required for the completion of artificial digestion by the gastric juice alone, offered a strong argument against the belief that any part of the digestive act was completed in the stomach. By subsequent experiments, Bernard convinced himself that the albuminous principles left the stomach changed, but imperfectly digested. The whole alimentary mass passes into the duodenum subdivided and broken up, the soluble and watery parts having been absorbed by the capillaries of the stomach, and the albuminous principles being modified, though as yet unfit for assimilation. In the duodenum and jejunum, the final processes of digestion are accomplished. The bile, pancreatic juice, and the secretions of Brunner's glands, form an intestinal fluid, which acts with energy on all the articles of food, completing the digestion of albuminous materials, transforming starch into glucose and emulsifying fat.

The experiments of Bernard which relate to the influence exercised on the functions of the stomach by the pneumogastric nerves, are certainly open to objections, and perhaps many of the phenomena observed as consequences of their division might, as Dr. Reid believed, be produced by any severe shock, and would therefore disappear as soon as the effects of that shock had subsided. Bernard, without doubt, killed his animals too quickly after having divided the vagi, for the immediate consequences of so severe an operation to have passed away. Still, making allowance for this, and acknowledging that in some instances the gastric secretion has been restored, that the formation of chyme has recommenced, and nutrition been carried on to all appearance as perfectly as before, yet it seems impossible to deny that an operation, which was followed almost at once† by complete paralysis of the powers of the stomach, which arrested in an instant its peculiar secretion, and converted it, as far as regards the digestion of food, into a lifeless pouch, must have deprived that organ of an influence closely connected with the proper performance of its functions.

The discovery of the changes which oleaginous matters undergo when mingled with the secretion of the pancreas, forms one of the most interesting and important results of Bernard's physiological researches. In an article, the dimensions of which are necessarily limited, it is neither possible nor desirable to detail the experiments and investigations by which the power of emulsifying fat was found to belong especially to the pancreatic juice. Nor, indeed, is such an account needed; for the works of physiology in general use have made the more important of these investigations accessible to every inquirer. But inasmuch as experiments conducted by able hands throw doubts on the entire accuracy of Bernard's conclusions, and unequivocally demonstrate that the inferences he deduced are more sweeping than the results of further inquiry will support, it becomes necessary to glance briefly at the salient features of the question, in order to form a just decision respecting them.

* Arch. d'Anatomie, op. cit. American Journal of Medical Sciences, op. cit.

† Comptes Rendus, 1844, vol. xviii. p. 995. American Journal, op. cit. Arch. Gén. de Méd., 1844, vol. v. p. 235.

The pancreas, according to Bernard,* secretes a viscid, transparent, and colourless fluid, which exudes during digestion, in large pearly drops, and becomes frothy by agitation. It is formed only during digestion, both Bernard and Frerichs having found the pancreatic duct dry when there was no food in the stomach. Its reaction, according to Magendie and Bernard, is constantly alkaline. On the application of heat it coagulates almost entirely, and is transformed into a white concrete substance. A similar change is effected by the admixture of small quantities of the strong acids, of alcohol, turpentine, and a solution of the metallic salts. Weak acids, such as lactic and acetic, do not coagulate the pancreatic juice. The coagulum thus produced preserves its properties when dried, and communicates them by solution to water. At a temperature of about 100° Fah., oil, butter, or fat, are instantly emulsified, and converted into a white, creamy liquid, resembling chyle, on the addition of a sufficient quantity of pancreatic juice. When placed in a medium, where the thermometer ranges between 40° and 50° Fah., healthy pancreatic juice may be preserved for some days, without manifesting any alteration beyond an increased viscidity; when, however, the temperature of the surrounding medium is raised to 105° or 110°, the secretion in a few hours becomes decomposed, evolving an unpleasant odour, exhibiting a cloudy deposit, and losing the power of coagulating on the application of heat. In the midst of summer this change takes place in a few minutes, so that it is necessary to keep the animal, from which the pancreatic juice is collected, cool, as well as the caoutchouc bladder in which it is received. Division into molecules is not the only modification effected in fat by the instrumentality of the pancreatic juice; for after some hours, the emulsion formed by the mixture of an alkaline secretion with neutral fatty matters, and which is therefore itself alkaline, becomes acid, in consequence of chemical changes, by which the emulsion is converted into glycerine and a fatty acid. This conversion, however, is the result of decomposition, and never takes place, as Frerichs remarks, in the digestive canal, because the other components of the intestinal fluid—such as the bile and gastric juice—interfere to prevent it. It is urged by Dr. Donaldson, that Bernard never maintained that the emulsified fatty matters of food underwent any chemical transformation within the digestive canal, but that he was always disposed to regard this process as the result of decomposition. But whatever may have been the impression on Dr. Donaldson's mind, Bernard clearly seems to believe that the chemical changes which slowly follow the admixture of fatty matters with the pancreatic secretion, are as much dependent on the peculiar influence of that fluid as the emulsification that precedes them. His language is explicit on the point. He states repeatedly that the pancreatic juice first emulsifies the fat, and then converts it into glycerine and a fatty acid; and he nowhere mentions that the latter stages of transformation occur only when the emulsion has been exposed to air, and are the results of a decomposition to which natural digestion offers no similarity. He evidently regards them as changes which the emulsified fat undergoes, as well in natural digestion as in that produced artificially. This, however, is an undoubted error. The experiments

* Comptes Rendus, 1848, vol. xxviii. p. 259 *et seq.* Arch. Gén. de Méd., vol. xix. p. 60. L'Union Méd., op. cit.

of Lenz, Bidder and Schmidt* have placed it beyond question, that the chemical transformation described by Bernard does not naturally occur within the body.

After having fed cats upon butter, they found neither in the small intestines, nor in the chyle, nor yet in the blood, the smallest trace of butyric acid. They next tied the duodenum half-way between the pylorus and the orifice of the bile duct, and immediately afterwards injected into the portion below the ligature some melted butter, which they took care to throw in above the orifice of the biliary and pancreatic duct. After six hours the small intestines contained certainly some butyric acid; and on performing the experiment afresh, having in addition tied the bile duct, the same formation of butyric acid took place. It is therefore evident that decomposition and formation of fatty acids are hindered under the natural conditions of digestion by the gastric juice which is mingled with the chyme.

Bernard recognises two descriptions of pancreatic juice—a normal secretion and a morbid one—the former exhibits the properties and characters previously mentioned; the morbid variety is discharged from the pancreas when that organ has undergone inflammation, however slight, and if the operation for procuring the juice is improperly performed, or prolonged from accidental circumstances, the healthy pancreatic secretion is never obtained. The altered fluid differs considerably from the healthy. It is much less viscid, has a saline and slightly nauseous taste, appears thin and watery, and coagulates feebly and imperfectly on the application of heat or strong acids; it is without action on fat, and rapidly decomposes. The alteration in quality to which the pancreatic secretion is liable, is especially insisted on by Bernard, and should never be overlooked, as it might otherwise become a source of discrepancy and confusion in the subsequent experiments of physiologists. The characters ascribed to the pancreatic juice by Lehmann are certainly those of the morbid secretion, as he himself acknowledges; consequently the assertion of Frerichs that pancreatic juice does not form an emulsion when shaken in a tube with oil, melted butter, or fat, but sinks separately to the bottom, like other fluids, is valueless, because his experiments were performed with the morbid secretion, which Bernard himself states has no action on fat. The manner in which Bernard obtains the pancreatic juice is as follows. An incision is made in the right hypochondrium of a dog, below the free margin of the costal cartilages, so that the operator is enabled to draw out the duodenum, and part of the pancreas. The pancreatic duct is then isolated as quickly as possible, and opened by a fine pair of scissors. A small silver tube is placed in the aperture thus made, and secured in its position by a thread. The duodenum and pancreas are then returned into the abdomen, and the external wound is closed by sutures, care being taken to leave the rim of the silver tube slightly projecting, so that the pancreatic secretion may be discharged externally. After the alkalescence of the first drops of the exuding fluid have been ascertained, a small bladder of caoutchouc is attached to the end of the tube in which the secretion is collected. Bernard consequently obtains the pancreatic juice before it has reached the

* Lehmann: Phys. Chemie, Zweiter Band, p. 110. Lenz: De adipis concoct. et absorptione.

intestine, and we think his operation preferable to that of Frerichs, being less likely to excite inflammation of the pancreas, and alteration of its secretion.

The proceeding adopted by Frerichs, and recommended by Lehmann, is conducted as follows. An incision from two to three inches long is made in the linea alba, and the descending portion of the duodenum is then laid open; a ligature must be placed round the bile duct, before it pierces the intestinal wall, and a small silver canula is then passed into the pancreatic duct from the intestinal tube, through which the secretion of the pancreas may be obtained. Now this operation is open to serious objections. It inflicts more injury than that practised by Bernard; and is therefore more likely to create disturbance. The mucous membrane of the duodenum, no longer lubricated by the bile or pancreatic juice, and irritated by the presence of a foreign body, suffers inflammation, and the pancreas itself falls into a similar state, either from sympathy, or by direct propagation along the duct. It is by no means unlikely that gastric juice, or intestinal mucus may find its way down the tube, and disorder both the pancreas and its secretion. Moreover, the fluid obtained by this method must be collected with difficulty, and is likely to be impure. For these reasons the characters assigned by Lehmann to the pancreatic juice must be received with doubt; nor can any importance be attached to the experiments performed by Frerichs with the secretion obtained by his own method, because that secretion was almost certainly either morbid or impure. Frerichs, Bidder, and Schmidt have, however, proved that the pancreatic juice is not the only fluid by which fat is emulsified during digestion. They tied the pancreatic duct in cats, and then deprived the animals of food for twelve or twenty-four hours, so that the intestines might be free from pancreatic juice; they then fed these animals with milk, rich fatty food, or butter, and killed them after six or eight hours. After having repeated such experiments several times, they invariably found the lacteals injected with white chyle, and the receptaculum chyli full of the same fluid. Frerichs cut the intestinal tube of a cat in half, and having injected olive oil, he tied the open extremity of each portion; on killing the animal, the lacteals in the upper half were full of milky chyle, while those in the lower half were much less injected. Frerichs, therefore, concludes that the emulsification of fatty food is mainly effected by the joint action of the bile and pancreatic juice. The experiment with the rabbit, so much relied on by Bernard, and confirmed by Todd and Bowman, is delusive. Bernard having observed that the pancreatic duct in rabbits opened twenty-five centimeters below the orifice of the bile duct, gave to one of these animals a quantity of oleaginous food, and subsequently a meal of carrots, and killed it after six hours. When the intestines were opened, it was at once apparent that white chyle was not present above the orifice of the pancreatic duct, for the lacteals were not at all perceptible above this point, while below it they were distended with a milky injection. This experiment was repeated several times by Bidder and Schmidt, who found that the conclusions based upon it by Bernard were erroneous. For if the rabbit was killed two hours after feeding, the lacteals between the pylorus and orifice of the pancreatic duct were filled with chyle; but if four hours were suffered to elapse before death, the lacteals for some distance below the pylorus were found uninjected; while after six hours, white chyle had disappeared from all the lacteals between the pylorus and pancreatic duct; and after eight hours, had receded twenty or thirty centimeters below that duct itself; clearly showing that the absence of chyle from the lacteals above the orifice of the pancreatic duct, was referable to the time which had elapsed from the reception of food to the death of the animal, and not to the influence of the pancreatic juice. Further experiments are requisite to decide the amount of power possessed by the secretion of the pancreas, and the other intestinal juices separately, to modify or transform fat; at present, the only conclusion to be drawn from the conflicting statements of distinguished physiologists is, that the pancreatic juice takes an important share in emulsifying oleaginous food, but that the bile and intestinal juices are themselves endowed with a similar power—a power that is greatly enhanced by the mixture of these various secretions. But, considering that a large portion of the animal kingdom eat little or no fat, and that the herbivora have, as Valentin remarks, a pancreas larger than the carnivora, it can hardly be supposed that the influence exerted by the pancreatic secretion over fat, is by any means its most important function. This fluid seems rather destined to act throughout the animal kingdom on the unazotized constituents of food, transforming starch into sugar, in the herbivora; assisting in the emulsification of fat in the carnivora; and discharging a combined function in man and other omnivorous animals.

The lacteals, says Bernard, absorb only the oleaginous principles of food which have been previously emulsified. The other constituents of chyle are independent of the digested matters in the intestinal tube, and resemble the components of the lymph in character and formation. Chyle, in short, is looked upon by Bernard as lymph holding in suspension emulsified fat.* The experiments of Brodie, Tiedemann and Gmelin, Bouchardat and Sandras, point to the same conclusion; but it is only necessary in this place to refer to those adduced by Bernard in support of his opinion. Sugar injected in large quantities into the stomachs of dogs, cats, and rabbits, was invariably detected with ease in the portal blood, but never in the chyle. The reason for this is as follows: It is essential for the assimilation of all saccharine principles that the sugar they contain should be transformed into glucose, or diabetic sugar. Cane-sugar injected in solution into the jugular vein of a rabbit is eliminated by the kidneys; grape-sugar, on the contrary, remains in the blood, is there assimilated, and transformed into other compounds. Now, the conversion of cane-sugar into glucose and other compounds is, according to Bernard, effected by the liver; and hence the necessity that all saccharine principles should be absorbed by the intestinal bloodvessels, as otherwise they would pass unmodified into the circulation, and be excreted as useless to the economy. A solution of the albumen of eggs injected into the jugular vein of a dog or rabbit, appears after a short time in the urine; but if a similar solution be thrown into a branch of the portal vein, the urine undergoes no change in its composition—proving that an alteration is effected in the albumen during its transit through the liver, adapting it for the nutrition of the tissues. Bernard, therefore, maintains that the albuminous principles of food

* Comptes Rendus, vol. xxxi. p. 798. L'Union Médicale, 1850, op. cit.

are absorbed wholly by the bloodvessels. But this supposition is not only unproved, but opposed to many important facts. For, granting that vitelline albumen is incapable of being assimilated before undergoing modification, and granting, also, what is by no means clear, that this modification can be effected entirely by the liver, it still requires to be shown that there are no other organs capable of producing a similar alteration. It is certain that the chyle contains three times as much albumen as the lymph, and equally certain that the imperfect albumen present in the lacteals is developed and matured before it mingles with the blood. If, then, we acknowledge, in the immatured albumen, a power of self-development, or if we consider that the change it undergoes is accomplished by the instrumentality of the lymphatic glands, it is difficult to understand how we can disbelieve that the same substance possesses also a power of self-modification, or that the lymphatic glands are unable to adapt it to the nutrition of the tissues. If the lacteals do not possess the power of absorbing albumen, whence is the large quantity of albumen present in the chyle derived? and if the lacteals are capable of absorbing albumen from the interstices of the tissues, why not also from the digested aliments? Many animals, moreover, eat no fat, and consequently do not form white chyle; but shall we deny to such animals the existence of a lacteal system, because in them that system is precluded from carrying an oleaginous emulsion? Does it not rather appear that the least important constituent of chyle is its molecular base, and that compensation is made for the absence of this constituent from the chyle of herbivorous animals, by an increased activity of the hepatic functions? As regards the conversion of quaternary or other ternary compounds into fat, that it is quite unimportant whether the fat is taken up by bloodvessels, or lacteals, is evident, from the circumstance observed by Bernard, of the portal blood always containing as much fat as the contents of the thoracic duct. Cocks, pigeons, and sparrow-hawks, when fed on butter or fat, and killed during active digestion, exhibit no appearance of white chyle in the lacteals, but an abundance of emulsified fat in the portal blood; so that there are really good grounds for believing that the oleaginous constituent of chyle is an element incidental, but not essential, to its composition.

The discovery of the formation of sugar by the liver constitutes the brightest of Bernard's physiological achievements; and it is impossible to estimate too highly the zeal with which he pursued his researches, or the sagacity he displayed in interpreting their results. Pathological phenomena first drew his attention to the subject. It appeared to him a remarkable circumstance that diabetic patients, while restricted most absolutely to azotized food, should yet continue to pass large quantities of sugar with their urine.* To ascertain whence this sugar was derived, and by what means it was formed, he commenced a series of experiments; and after two years of laborious investigation, in which he was greatly assisted by M. Barreswil, he conclusively demonstrated that in a very large proportion of animals the liver is constantly forming sugar out of the azotized or unazotized substances furnished to it by the portal blood. Some idea may be formed of the extent of Bernard's investigations by the number of animals which he succeeded in convincing himself possessed the property

* Arch. Gén. de Méd. 1848, vol. xviii. p. 303.

of forming sugar at the liver. They are as follow: among the mammalia generally; in all birds; in a large number of fishes, both osseous and cartilaginous; in the pulmonary gasteropoda and acephalous mollusks. Sugar was also found in the crustacean decapoda; but Bernard is not inclined to ascribe any importance to this circumstance, because in animals so low in the scale, the apparatus of nutrition undergoes considerable modifications. Bernard commenced the inquiry by experimenting on two dogs.

A bitch was killed seven hours after having fed heartily on mutton and the bones of poultry. Blood collected from the cavities of the heart, and which had stood for an hour and a half, furnished an opaline, milky serum, which, when tested, was found to contain sugar. Not the smallest evidence of the presence of sugar in the intestinal canal could be obtained, nor were there any indications of its presence in the urine. The animal was killed while digesting actively.

In the next experiment, a full-grown and well-conditioned dog was left completely without nourishment for two days, and then put to death. Blood from the cavities of the heart afforded serum containing sugar, while not the smallest trace of such formation was perceptible in the stomach or intestinal canal. It was thus clearly demonstrated that the blood contained sugar independently of the nature of the food, or the changes accomplished in digestion; and the question to be determined was the source from which this sugar was derived. To solve this problem, Bernard undertook a second series of experiments.

A full-grown and healthy dog was killed during active digestion, seven hours after having fed heartily on meat and bones. The abdomen was immediately opened, when the digestive organs were seen to be turgid with blood, and the lacteals filled with white chyle. Blood was collected from the portal vein, near the spot where it receives the splenic, and also from the cavities of the heart: some chyle was obtained from the thoracic duct, the contents of the stomach and small intestines were carefully separated, and these various products were severally tested for sugar. There was none in the chyle; not a trace in the chyme, either from the stomach or intestines; but a large quantity was yielded by the serum of the portal blood, and a less amount by the serum of the blood from the right cardiac cavities.

Second experiment: A full-grown dog was killed, after having been kept entirely without sustenance for three days. On inspecting the contents of the abdomen, the digestive organs were found pale and anæmic, and the stomach and intestines in a contracted state. The lacteals were filled with transparent chyle. The serum of the portal blood betrayed distinct evidences of sugar, which was, however, less abundantly present than in the previous experiment. The blood on the right side of the heart also contained sugar, but the chyle not a trace.

Similar experiments, repeatedly made, invariably confirmed these results; but still it appeared improbable that the walls of the portal vein should possess the power of forming sugar, and if not, from whence was the sugar in the portal blood derived? Bernard, believing that one of the great agents in effecting the portal circulation was the compression exerted by the abdominal walls, thought it not improbable, that when that compression was withdrawn, a reflux of blood from the liver into the portal system took place, whereby substances became mingled with the portal blood, otherwise foreign to its composition. The justness of this conjecture was established conclusively by the following experiment:—A dog, while in full digestion of animal food, was killed by section of the medulla oblongata. The abdomen was immediately opened, and ligatures were placed with all possible speed on the veins emanating from the small intestines, not far from their origin—viz., on the splenic vein, some centimeters distant from the spleen, on the pancreatic veins, and on the portal vein, before its entrance into the liver. From all the channels thus obstructed, blood was collected and carefully tested. The food in the intestinal tube was also submitted to examination. No evidences of the presence of sugar could be detected in the blood taken from the various branches of the portal vein, or in the contents of the digestive canal; but when an aperture was made in the portal vein, on the hepatic side of the ligature, the blood regurgitating from the liver furnished evidences of abundance of sugar. Proofs, moreover, of saccharine formation were yielded by the tissue of the liver itself, while examination of the pancreas, spleen, and mesenteric glands afforded no such testimony. Hence it was concluded, that the sugar found on previous occasions in the portal blood, arrived there in consequence of regurgitation from the hepatic veins of the liver—a circumstance dependent on the sudden withdrawal of the pressure by which the abdominal circulation is in great part effected. In order, therefore, to isolate the sugar as much as possible at the place of its production, it became obviously necessary to tie the portal vein close to the liver, immediately after the division of the abdominal wall; and in all subsequent experiments, this practice was invariably adopted.

The following steps are necessary in order to discover the presence of sugar in the hepatic tissue or the blood.* If, says Bernard, a portion of the fresh liver of an animal be broken-up in a mortar, and then boiled a few instants with a small quantity of water, the decoction presents an opaline aspect, and exhibits all the characters of a saccharine solution. The serum of recent blood from the hepatic veins or right cavities of the heart, when tried by appropriate tests, afford as decisive proofs of the presence of sugar as the hepatic tissue itself. The presence of sugar in the fluids or tissue submitted to examination, may be proved by the establishment of fermentation, or by reduction of the oxides of silver or copper; but a combination called the "solution of Barreswil" constitutes an accurate and ready test of its presence in any fluid mixture. This is composed by dissolving four scruples of potash, and the same quantity of crystallized carbonate of soda, five scruples of bitartrate of potash, and three of sulphate of copper, in a pint of distilled water; the whole must be heated to boiling, and then filtered. A few drops added to the strained decoction of a solid tissue, or to a fluid containing grape-sugar, will produce, on the application of a spirit-lamp, the reddish yellow colour which shows the formation of the sub-oxide of copper. Sometimes, however, the presence of organic compounds in animal solutions interferes with the action of the test; it is therefore advisable, when blood or a decoction of hepatic tissue is to be examined, to add first a small quantity of acetate of lead, and

* L'Union Médicale, op. cit.

then filter the fluid, and afterwards, especially when experimenting on the products of herbivorous animals, to throw down the carbonates in the organic solution by the addition of a small quantity of sulphuric acid, and then, after having again filtered, the test of Barreswil may be applied. The hydrated oxide of copper is reduced, and the sugar is transformed into glucic and paraglucic acids. All the efforts of Bernard to isolate this animal sugar in a crystalline form were unsuccessful. The addition of alcohol, followed by gradual evaporation, yielded only a thick syrup, and never a crystallized residue. Bernard is disposed to ascribe this absence of crystallizable power to the presence of salts, and especially of chloride of sodium.

In all similar investigations, it is of the utmost importance that the albuminous principles should be separated from the animal solution by the addition of alcohol, and by subsequent evaporation, because, in consequence of their presence, the sugar undergoes rapid destruction, and quickly disappears.

Although the saccharine compound thus yielded by animal juices exhibits, in many respects, a behaviour identical with glucose, it yet manifests characters peculiar to itself, which seem indispensable to its special office in the economy. Bernard calls it diabetic sugar, to which he has always found it exactly analogous. Magendie proved that a small quantity only of glucose could be injected into the blood without being discharged in the urine; whereas five times as much diabetic sugar was injected into the blood without affecting the urine. Astonished at the discovery he had made, and hesitating as to the interpretation of facts, which seemed likely to subvert the received notions of physiological chemistry, Bernard submitted the results of his experiments to Dumas, and repeated them before him. The formation of sugar at the liver, both during digestion and abstinence, irrespective of animal or vegetable food, was demonstrated as unequivocally as before. Dumas, however, suggested that the liver might be endowed with the power of storing-up in its tissue the products of previous saccharine or starchy food, and surrendering the same to the blood by slow instalments. It seemed to him that an adequate explanation of the presence of sugar might be thus afforded, without supposing it to have originated in the continual transformation of other ternary or quaternary compounds. But the result of the following experiment is strongly opposed to that hypothesis. A grown dog was kept without food for eight days, and was then nourished by meat alone for eleven days, at the expiration of which he was killed. Sugar was found to exist abundantly both in the blood of the right ventricle and in the tissue of the liver.

Division of the pneumogastric nerves immediately arrests the formation of sugar in the liver. Diseases which exhaust nervous energy are followed by the same result, for which reason it is never found in the human liver, except after sudden death; and even in the last stages of diabetes, during the exhaustion that precedes death, sugar ceases to appear in the urine. A diabetic patient of Andral's was subject to attacks of diarrhœa, during the prevalence of which the urine ceased to contain sugar. Bernard, however, obtained upwards of five drachms of sugar from the liver of an executed criminal, and forty-seven grains from the liver of an ox. He has also collected a large amount from a diabetic patient, who died somewhat quickly from another disease; and has detected it in the hepatic tissue of two individuals who perished suddenly, one from a gun-shot wound, and the other from disease of the heart. While pursuing his experiments on the formation of sugar, Bernard discovered, accidentally, that puncture of the floor of the fourth ventricle of the rabbit, with a finely-pointed instrument, was followed in a very few minutes by the appearance of sugar in the urine, and by symptoms of uneasiness and nervous depression. The puncture was made in the middle of the calamus scriptorius, just between the filamentous radicles of the auditory and those of the pneumogastric nerves. Subsequent researches have shown that this is not the only part of the nervous system whose irritation causes an increase of the quantity of sugar in the blood. Puncture of the olivary bodies produces the same effect even more strikingly; and Bernard is able to estimate with great exactitude the degree of diabetes that will ensue, according to the amount of irritation inflicted on the nervous centres. Irritation, however, when roughly made, and involving much lesion of the nervous substance, not only fails to augment the quantity of sugar in the blood, but deprives the liver for a time of the power of forming it. To be successful, the irritation must be made delicately, with a rather finely-pointed instrument. The manner in which it influences the liver is unknown. Bernard was disposed at first to believe that the stimulus being conveyed by the fibres of the pneumogastric nerve, excited the hepatic tissue to a more energetic discharge of its functions; but the discovery that irritation of the olivary bodies was followed by glycosuria, even after secretion of the vagi, forced him to relinquish this view. At present he is rather inclined to regard the sympathetic as the agent of transmission; and Dr. Donaldson states,* that a case of diabetes is on record, in which the sympathetic nerve was observed to be four times as large as natural below the diaphragm. It is, however, impossible to attach much importance to a solitary statement of this nature; and it is not at all likely that such an enlargement of the sympathetic is habitual in diabetes, as it could scarcely have been overlooked in the numerous post-mortems made upon patients who have died of this disease.

The sugar formed at the liver is destroyed, says Bernard, at the lungs, and is not to be found under natural circumstances in the blood of the left ventricle. This statement requires confirmation, as it does not appear to rest on refined analyses. But if the whole of the sugar does not disappear while traversing the pulmonary bloodvessels, that the greatest part of it does so is unquestionable. What then becomes of it? How is it decomposed? Bernard conjectures that after leaving the liver it undergoes a gradual fermentation, and becomes transformed into water and lactic acid, and then into carbonic acid, which is exhaled at the lungs. The source of this fermentation is unknown; Bernard believes it to be an organic principle, though he has hitherto failed to isolate it. Both Bernard and Bouchardat agree that Mialhe certainly erred in maintaining that the alkalinity of the blood was alone sufficient to accomplish the destruction of the sugar; and that in diabetes the sugar is not destroyed because the blood is acid, in consequence of the suppression of the cutaneous transpiration. But the alkaline reaction of the blood, though

* American Journal of Medical Sciences, July 1851, p. 38.

necessary to the decomposition of the sugar, is not of itself competent to accomplish that change; and the blood is neither acid in diabetes, nor after the suppression of the cutaneous transpiration. The manner in which Bernard disposes of the sugar is not altogether satisfactory, for on considering the subject, we shall find that a large number of animals form sugar by their livers; and it has been ascertained by Bernard, that the most actively-breathing animals, such as birds and mammalia, form it in the greatest abundance; while it exists in a far less proportion in the blood of the reptilia, and that no traces of its presence can be discovered in some fishes.* There is evidence enough, and evidence, too, of a striking character, to prove that the formation of sugar by the liver furnishes one of the conditions necessary to the proper performance of respiration. For example,† if artificial breathing be kept up in a decapitated animal, the production of sugar in the liver goes on; and if the lungs are inflated with air mingled with some irritant vapour, such as chlorine, the sugar appears in the urine, and the animal becomes diabetic after death. Moreover, it seems not a little significant that immediately before the blood is sent to the lungs for oxygenation, it is joined by a compound which, after oxygenation, exists in it no longer. It seems therefore reasonable to believe that the sugar is destroyed at the lungs, in order to minister to the functions of respiration, and to the maintenance of animal heat; and it is probably decomposed into water and lactic acid, the acid passing off in combination with soda, while the water transudes the walls of the pulmonary capillaries, to dissolve the oxygen without, by which means the gases concerned in respiration are placed in conditions favourable to their mutual diffusion. This is, however, a pure hypothesis, and it is obvious that many new researches must be instituted, and many new facts brought to light, before we can hope to elucidate the depths of so difficult a subject.

Besides the functions already ascribed to it, the liver forms fat during digestion, and discharges it into the blood.‡ This fat is similar to butter, or the fat of milk; and Bernard thinks that this constituent of milk is in all probability derived principally from the liver, for he found that it was formed much more abundantly by that organ in females than in males. The phenomena of fatty degeneration in muscle, the production of adipocire after death, as noticed by Mr. Paget, and the remarkable experiments of Wagner, go far to prove that nitrogenous tissues may part with their nitrogen, and be transformed into fat: and after having conceded to the liver the power of forming sugar from nitrogenous compounds, it is nothing astonishing that it should possess also a similar power of forming fat. Fat and sugar seem, indeed, in a certain degree, to bear a definite relation to one another; the herbivora forming more of the former, and the carnivora more of the latter. Bernard considers that the hepatic fat differs from that of the chyle, in not being resolved into molecules, in being combined with an azotised substance, and in the long resistance which it offers to the action of ether. It passes through the lungs, being only partially destroyed in their capillaries, is found in the left cavities of the heart, and

* Ch. Robin and Verdeil.

† See an article by Dr. Max. Vernois (Arch. Gén. de Méd. Troisième Série, vol. i. 1853), analyzed in No. xxiv. p. 551.

‡ L'Union Méd. 1850, op. cit.

being carried onwards with the arterial blood, is lost in the capillaries of the systemic circulation. No traces of fat can be discovered in the venous blood during health, except in the space intervening between the liver and the lungs. In some diseases, however, fatty particles have been observed in the venous blood, and Bernard believes that an excessive formation of this constituent at the liver may produce fatty diabetes or chylous urine. Further information on this interesting subject may be obtained from Dr. Donaldson's articles in the American journal, before referred to; and from the writings of Liebig, Lehmann, Combe, and Chambers.

Bernard asserts that the blood of the hepatic veins contains more fibrine, and fibrine too of a better quality, than the portal blood, which, according to his belief, is produced by the liver. He seems, however, as Dr. Carpenter remarks, to have overlooked entirely the fibrine furnished by the blood of the hepatic artery; and in opposition to his statements, Lehmann maintains that there is really less fibrine in the hepatic venous blood than in the portal, and that the error of supposing the contrary has arisen from inattention to the augmented number of blood-corpuscles in the blood of the hepatic veins, and from confounding the globuline they furnish with fibrine.

3. *Hepatico-renal circulation.*—It is well known that during digestion materials are received into the blood which profoundly influence its composition, and as some of these, if allowed at once to circulate, might be injurious to life, and others would exist in excess, and become on that account pernicious, the whole of the blood thus altered is received into special channels, which breaking up into a capillary network, submit the returning fluid to the action of the liver, where certain principles are wholly eliminated, and others altered in quality, before it is allowed to pass into the circulation, and minister to the nutrition of the tissues.

The portal circulation is carried on, as we have already seen,* in a great measure, by the pressure of the abdominal muscles; but inasmuch as it presents a capillary system at each extremity, and as both systems are simultaneously called upon to discharge active duties, the intestinal capillaries receiving supplies from the digested aliment, while the liver is exercising its peculiar powers on the blood thus replenished, it is manifest that congestion of the whole system, hepatic engorgement, and stagnation of the portal blood, would repeatedly ensue, unless some provision existed for diverting the blood from the portal vein before its entrance into the liver, and so affording relief to the tributaries of that vessel during the congestion consequent on the reception and assimilation of fresh nutritive materials. Bernard asserts that this provision is supplied by the presence of vessels which establish a direct communication between the portal vein before it enters the liver, and the vena cava ascending to the diaphragm. He has as yet only succeeded in demonstrating the existence of these anastomoses in the horse, in which, he says, they may be easily discovered, both by injection thrown into the portal vein, which distends them, and also by the passage of air from the portal vein into the inferior cava, through these very channels. The blood thus transmitted from the portal vein to the inferior cava, charged with new principles, is disposed

* Arch. Gén de Méd., vol. xxiii. p. 300. L'Union Méd., op. cit.

of in a singular manner. There exists in that portion of the inferior cava which lies behind and is below the orifices of the hepatic veins, a muscular coat of considerable thickness, the contractions of which cause the cava and renal veins to pulsate during digestion, the pulsations not being synchronous with those of the heart. In a rabbit whose abdomen is opened during active digestion, these movements are readily perceived: and in the horse, dissection of the vena cava inferior places the existence of a muscular tunic beyond dispute. But, in addition to this muscularity, the inferior cava of the horse presents two valves, attached to its wall immediately below the orifices of the renal veins. Now, the consequences of this arrangement are as follow:—During digestion the liver becomes congested, the portal blood regurgitates, and would stagnate but for the existence of channels enabling it to pass into the inferior cava below the orifices of the hepatic veins. The blood thus diverted is not permitted at once to mingle with the general circulation before being submitted to glandular action. The muscular coat of the inferior cava contracts, and greatly diminishes its channel; the impeded blood is thus thrown backward on that ascending from the limbs, but the valves below the orifices of the renal veins prohibit further regurgitation, and it is compelled to flow off right and left by the renal veins to the kidneys, which eliminate from it such materials as are excessive and pernicious; and so the urina cibi is constituted. Meanwhile, the order of the circulation is interrupted by the arrest of the blood ascending from the lower limbs, in consequence of the closure of the valves below the renal veins; but this disturbance is provided for by the existence of the venæ azygos, which receive the impeded blood, and convey it to the superior cava.

Such are the views of Bernard respecting the "hepatico-renal circulation."* He adduces three notable experiments to support them. In the first, cyanide of potassium, mixed with carbonate of soda, was introduced into the stomach of a rabbit, and in ten minutes the urine exhibited the characteristic blue, on the addition of a few drops of solution of acetate of iron. At the expiration of half an hour the animal was killed, and blood collected from both jugulars and both renal veins; the serum from the former furnished scarcely a trace of the presence of the salt, while that from the renal vessels contained a large quantity. A strong blue colour was also produced by the application of a solution of iron to the cut surface of the kidney; while a similar proceeding was followed only by a faint tinge when applied to other organs.

In the second experiment, a solution of cyanide of potassium, in the proportion of 20 parts of the salt to 100 of water, was thrown into the mesenteric vein of a rabbit. The urine in a few minutes contained a large quantity of the salt, but the animal suffered no inconvenience from its presence in the portal blood. When, however, a solution containing two parts of the salt to 100 of water was thrown into the jugular vein, the animal died in a few minutes, before the slightest trace of the poison could be detected in the urine.

A solution of lactate of iron was injected into the subcutaneous cellular tissue on the back of a rabbit, and a solution of prussiate of potash into the cellular tissue of the thigh of the same animal. In a few minutes a

* American Journal of Medical Sciences, July, op. cit.

blue colour was manifested at the spot occupied by the solution of iron. But when the prussiate of potash was administered by the stomach, and the solution of lactate of iron placed, as before, under the skin of the back, no blue colour was developed in that situation; and while the urine was found by tests to contain a large quantity of the salt, the serum of the jugular vein manifested but slight traces of its presence.

These experiments prove, according to the author, that the urine formed during digestion is secreted from venous blood; and thus, too, the rapid appearance in that secretion of materials received into the stomach (a phenomenon which astonished Sir Everard Home) may be accounted for. Remembering, moreover, that a large number of animals, such as the reptilia, secrete their urine from venous blood, there is no difficulty in believing that, under certain circumstances, mammalia may form theirs in a similar manner. At the same time, the evidence for the "hepatico-renal circulation" is incomplete, and an extended series of anatomical researches are requisite to demonstrate in various animals the existence of the three conditions essential to its establishment—viz., direct anastomoses between the vena porta and inferior cava; muscularity of the latter below the entrance of the hepatic blood; and valves below the orifices of the renal veins. The absence of one of these conditions is sufficient to render the hepatico-renal circulation impossible.

4. *The elimination of urea.* — In the thirteenth volume of the 'Archives Gén. de Médecine,' 4th series, will be found an interesting communication by Bernard and Barreswil, on the elimination of urea from the blood after extirpation of the kidneys.* It appeared to them a singular circumstance that whenever the kidneys of an animal had been extirpated, a period of time, varying from twenty-four to forty-eight hours, always elapsed before the blood yielded on analysis any traces of urea; and it immediately became a question in what way the urea escaped from the system during this interval. To ascertain this point, a series of experiments on dogs was instituted. The kidneys of these animals were first extirpated, and the blood and intestinal contents were subsequently carefully tested. Two of the dogs perished quickly after the mutilation, one from peritonitis, and the other from suffocation. In neither of these could any urea be detected in the blood. The gastric fluids liberated, on the addition of caustic potash, a suffocating ammoniacal odour; the intestinal fluid and bile, when similarly treated, disengaged also large quantities of ammonia. In the other experiments, the animals survived longer, and died, finally, in a state of considerable exhaustion. It was observed, in these instances, that the gastric juice was much increased in quantity, and was secreted without intermission both fasting and during digestion. Large quantities of ammoniacal vapour were liberated from it by the addition of caustic potash. No urea could be detected in the blood till the animal had become weak and exhausted, and the quantity of gastric juice secreted had undergone a considerable diminution in consequence.

Bernard and Barreswil conclude, from these experiments, that after the kidneys of an animal have been extirpated, the urea is eliminated by the secretions of the intestinal tube, and chiefly by the gastric juice, in the form

* Sur les Voies d'Elimination de l'Urée après l'Extirpation des Reins: Bernard et Barreswil, Avril, 1847, Arch. Gén. de Méd., Quatrième Série, p. 449.

of an ammoniacal salt; and that no urea can be detected in the blood till, from progressive diminution of the vital powers, the intestinal fluids become more and more diminished in quantity, and thus the supplementary channels for the separation of urea are cut off. Retention of urea, they argue, is not a direct result of suppression of urine, but rather a consequence of the loss of vigour which follows it; for if the vital powers remain active, the urea escapes from the blood by other portals, when those provided for its natural elimination are closed. True it is, that in no instance was urea, as such, detected in the intestinal contents; but this circumstance is accounted for by the authors, on the supposition that it is decomposed by the acid of the stomach, and transformed into lactate or phosphate of ammonia, and in proof of this, they state that urea, when introduced into the stomach, is always changed into a salt of ammonia before it can be absorbed.

5. *The curara poison*—Before concluding, a passing reference may be permitted to the discoveries of Bernard and Pelouze,* respecting the action of the curara poison, and the manner in which it enters the system. So singular are the facts which have been ascertained respecting this poison, that they have been widely published in medical periodicals, and have even found their way into popular literature; it will be unnecessary, therefore, in this place, to do more than refer to the points which relate especially to its mode of action and absorption into the system.

The curara, according to Humboldt, is the aqueous extract of a plant belonging to the order Logonaceæ, prepared by the natives inhabiting the forests which skirt the high Orinoco, the Rio Negro, and the Amazon. It is affirmed by M. Goudet, that the Indians of Messaya are accustomed (during its composition) to mingle with it some drops of poison obtained from the most venomous serpents. Its toxic action closely resembles that of the poison of the viper, for it may be taken with impunity into the stomach, whereas a speedy death invariably follows its injection into the blood, or its introduction into any of the tissues of the body. Under its influence an animal falls suddenly dead without uttering a cry, or scarcely even moving a muscle. If, for example, it be placed beneath the skin of a bird, the animal flies away, and in a few seconds falls dead without a movement or a cry. It has been remarked by Bernard and Pelouze, that death is constantly attended by phenomena that indicate a complete extinction of the functions of the nervous system. A minute after death the nerves are flaccid, like those of an animal which has long expired; no reflex movements can be produced; the blood is always black, and frequently it coagulates with slowness and difficulty, and does not redden on exposure to air.

Yet this deadly poison may be taken with impunity into the stomach or intestines. Mucous surfaces prevent its absorption, and if an endosmometer be made of the gastric mucous membrane of a recently killed animal, so that the epithelial surface look outwards, it will be found that endosmosis will ensue between a saccharine solution within the tube and an aqueous solution of the curara without; but that none of the poison will pass through with the endosmotic current into the interior

* Comptes Rendus, Deuxième Semestre, 1850. Récherches sur le curare, Bernard et Pelouze. Arch. Gén. de Méd., 1850, vol. xxiv. p. 300.

of the instrument. It must not, however, be supposed that the poison decomposes in the stomach, for it remains perfectly unchanged by admixture with the gastric juice, saliva, bile, or pancreatic fluid; but it cannot pass through the mucous membrane to enter the circulation. The lining membrane of the air-cells alone forms an exception to this rule; through it the poison passes without obstruction, and quickly produces death. But this exception is more apparent than real; for it will be remembered that in the air-vesicles the respiratory mucous membrane has lost its characters, and is reduced to a simple layer of fine and almost structureless tissue, devoid of epithelium. These curious facts with reference to the action of the curara may not be easy of explanation, but they prove that imbibition through an epithelial covering is, to a certain extent, an elective act.

With the exception of two papers on 'Organic Combinations,' contributed by Bernard to the Arch. Gén. de Méd., 6th Vol., 4th series, we have analyzed more or less completely all the important productions of his pen, and we have omitted to notice the articles whose title has been mentioned, because much of their subject matter is a repetition of former statements, and because the experiments related by the author are too few for any satisfactory conclusions to be deduced from their results.

The progress of medicine and surgery is essentially dependent on the sure advancement of physiological science. Experiment alone affords but too often a deceptive and uncertain light for the exposure of error and the establishment of truth, and they who, animated by the recital of Bernard's successes, may aspire to imitate his career, must unite skill in the conduct of experimental research to the industry, penetration, and judgment which have enabled him to achieve discoveries that reflect lustre on his name.

Harvey Ludlow.

Review V.

1. *A Bill intituled 'An Act to amend an Act passed in the Ninth Year of Her Majesty, for the Regulation of the Care and Treatment of Lunatics.'*
2. *A Bill intituled 'An Act to consolidate and amend the Laws for the Provision and Regulation of Lunatic Asylums for Counties and Boroughs, and for the Maintenance and Care of Pauper Lunatics, in England.'*
3. *A Bill intituled 'An Act for the Regulation of Proceedings under Commissions of Lunacy, and the Consolidation and Amendment of the Acts respecting Lunatics and their Estates.'*

(*Continued from No.* 24, *p.* 434.)

Since the advancing intelligence or scepticism of mankind repudiated the belief in demoniac possession, and insanity began to be recognised as a disease, it has, until recent times, been considered as a disorder of our intellectual nature alone. One great authority decided that it was right reasoning upon wrong premises; others described it as the result of mistaking the ideas of conception for those of sensation—a species of unbridled imagination; and Erskine pronounced that its essence consisted in delusion.

This last idea has taken so deep a root in the legal mind, that many of our judges are understood to entertain the strongest aversion to extend the privileges of insanity beyond the range of this delusion test—or say, rather, this delusive test—and to consider that to every man who does not believe that his head is an empty saucepan, that he is the fourth person of the Trinity, or some such nonsense, must be allowed the rights and the responsibilities of sanity. Consequently, when a cross-examining barrister, in obedience to "instructions," girds himself to the task of weakening, by sophistry or ridicule, the effect of some damaging medical evidence, his first question is, "Will you have the goodness to define what you mean by the word delusion?" If medical witnesses were of our opinion, they would never use the term, and refer lawyers inquisitive about it to their Becks' and their Taylors.'

The late Dr. Prichard, whose illustrious name recently adorned the Lunacy Commission, perceived that these narrow definitions did not square with the undoubted facts of experience; that many insane persons had no delusions; that they reasoned well upon right premises, and that in them the essence of the thing was perverted emotion: hence he established the existence of Moral Insanity.*

These opinions, which gained ready assent among those who had daily and hourly opportunities of observing the phenomena of diseased mind, did not find credence among the profound intellects of the legal profession—intellects among whom the reason of the thing is paramount, and the motive generally goes for nothing.

A lawyer practising much in lunacy lately told us that the judges had made up their minds not to endure any more nonsense about moral insanity. If so, perhaps they are right, for in some instances it is undoubtedly a knotty point and a difficult matter to distinguish between a man afflicted with this form of disease and the moral condition of a degraded human being, born and nurtured in the lowest depths of ignorance, infamy, and vice. The public have not yet learned to distinguish between the two; and as an example and terror to evil-doers, one of these hung by the neck at the end of a rope will answer quite as well as the other.

It may happen, at no distant period, that the public, from whom in free countries all authority is derived, and whose convictions slowly but surely modify judicial opinion, will become enlightened and decided in its convictions in this matter, and render necessary an amelioration of the common law thereupon.

Sir James Mackintosh observes—

"An abhorrence of crimes, especially of those which indicate an absence of benevolence as well as regard to justice, is peculiarly strong, because well framed penal laws being the lasting declaration of the moral indignation of many generations of mankind, exceedingly strengthen the same feeling in every individual, *as*

* It may be doubted whether Dr. Prichard did more than find a better name for a condition already well observed and accurately recorded. Not to mention Pinel's "manie sans delire," Dr. Thomas Arnold, of Leicester, who published in 1782, says: "I call that *impulsive insanity* in which the patient is *impelled* to do or say what is highly imprudent, improper, unreasonable, impertinent, ridiculous, or absurd, without sufficient, or with very slight, or with no apparent, cause." He makes of this impulsive insanity a species with four varieties, and he quotes an admirable delineation of the same from Dr. Monro's remarks on Dr. Battie's Treatise on Madness.

long as they remain in unison with the age and country for which they are destined, and, indeed, whenever the laws do not so much deviate from the habitual feelings *as to produce a struggle between law and sentiment,* in which it is hard to say on which side success is the most deplorable."

The contention between law and sentiment thus sketched by the hand of a master, appears at the present time to exist in this country, on the responsibility of those morally or partially insane. The law not only rejects from its estimate of insanity the greater portion of the mental faculties, but refuses to recognise degrees in responsibility arising from this source—so that an offender must either be entirely and intellectually insane, or altogether criminal. The growing intelligence of the people is rapidly teaching them that there are infinite grades of responsibility, and that justice demands there should be corresponding grades of penal treatment. Public opinion is not *en rapport* with the common law on these matters, consequently complaints are heard, on the one side, that whenever the slightest doubt exists of a culprit's perfect sanity, juries cannot be brought to convict; and on the other side, assertions are heard, that the criminal law, on all that relates to lunacy and partial responsibility, is antiquated and barbarous; that whereas in nature every thing and every quality is shaded off into its corresponding opposite, at the bar of assize, as at the bar of God, there are but two classes, the sheep and the goats, and that without the divine omniscience to distinguish between the two.

The existence of moral insanity is for medical witnesses a simple question of fact; they observe it in practice, and they bear testimony to these observations in courts; they cannot there, like the toxicologists, exhibit any process of reduction, therefore with the enunciation of their skilled knowledge their responsibilities end, except so far as the duties of good citizenship will make them endeavour to procure modifications of the law where it is wrong or defective.

In seeking for the explanation of legal opinion on this subject, that of the pleaders is worthy of consideration only because from among them the judges are appointed; any mental bias acquired by long habit cannot be suddenly got rid of, by transferring in the evening of life the person who entertains it from the bar to the bench—if the child is father to the man, the barrister is father to the judge. For the influences of the profession on the mind of the barrister we appeal to Archbishop Whately, who, after speaking of a great Amsterdam corn-merchant who had never seen a field of wheat growing, and who would have been greatly at a loss in the *cultivation* of corn, although he had been in a certain way long *conversant about corn,* proceeds—

"Nearly similar is the experience of a practised lawyer (supposing him to be nothing more) in a case of legislation, because he has long been conversant about *law,* the unreflecting attach great weight to his judgment; whereas his constant habits of fixing his thoughts on what the law is, and withdrawing it from the irrelevant question of what the law *ought to be*—his careful observance of a multitude of rules (which afford the more scope for the display of his skill, in proportion as they are arbitrary, unreasonable, and unaccountable), with a studied indifference as to (that which is foreign to his business) the convenience or inconvenience of those rules, may be expected to operate unfavourably on his judgment in questions of legislation, and are likely to counterbalance the advantages of his superior knowledge even on such points as do bear on the question." (Rhetoric, Part 2, chap. 3.)

Again:

"The barrister having to plead *various* causes, is called upon to extenuate to-day what he aggravated yesterday—to attach more and less weight at different times to the same kind of evidence—to impugn and to enforce the same principles, according as the interests of his clients may require. But this very circumstance must evidently have a tendency, which ought to be sedulously guarded against, to alienate the mind from the investigation of truth. Bishop Butler observes and laments, that it is very common for men to have a curiosity to know what is *said*, but no curiosity to know what is *true*. ...... A judge, or any one whose business it is to ascertain truth, is to decide according to the *preponderance* of the reasons; but the pleader's business is merely to set forth as forcibly as he can those on his own side; and if he thinks that the habitual practice of this has no tendency to generate in him, morally, any indifference, or intellectually any incompetency, in respect of the ascertainment of truth—if he consider himself quite safe from any such danger—I should say then that he is in very great danger." (Ibid.)

Briefly to make the obvious application of these generalities to the matter in question. A barrister who may be thoroughly conversant with lunacy as it exists in the decisions of the courts, may remain utterly ignorant of it as it exists in the great field of nature; he may never have seen or have conversed with a single lunatic, except under the restraint of judicial formalities: he may know what all the rules and precedents on the subject are, but for want of comparing legal descriptions with the real entities they describe, he may remain perfectly ignorant of what they ought to be. Add to this the professional "tendency to generate moral indifference and intellectual incompetency in respect of the ascertainment of truth," and we need experience little surprise that the law made by men trained in this school is, on the difficult and intricate subject of insanity, at variance with the requirements of science.

Another important element of the opinions on insanity entertained among lawyers, may be found in the influence which the ethical system of Mr. Bentham exercises on the profession. The biography of an eminent law lord recently dead informs us that he had diligently sat at the feet of the great utilitarian philosopher, and that he regulated his actions and opinions by the precepts there obtained. Perhaps the high places of the law contain few other disciples so devoted and sincere, and may contain many who would earnestly repudiate the jurisprudential doctrines of the sect. Notwithstanding which, the ethics of Bentham exercise an influence over the minds of persons who would repel the imputation of being Benthamites on matters of law procedure and reform. According to these ethics, nothing is absolutely right or wrong, moral or immoral, except as it is, or is not, a matter of utility, of profit or loss; virtue is a sum well proved, vice is a mistake, and conscience, which "makes cowards of us all," is but an equation. It is no wonder, therefore, that men believing in a philosophy which developes all the natural and healthy emotions of the human mind from the operation of the reasoning faculties, should feel themselves restrained from allowing the possibility of the secondary and dependent faculties becoming perverted and diseased, while their origin and cause remained healthy. This would be to acknowledge that the healthy action of the reason could, in the same direction, produce two results—namely, a healthy and also a diseased condition of the emotions, which is absurd. The ethical system of utilitarianism would appear, therefore, imperatively to forbid the supposition that moral insanity can exist without previous intellectual disease. Thus the fundamental obstacle in the legal mind to the admission of a just and comprehensive estimate of insanity, may present itself, not in the shape of half a dozen judicial dicta, more or less inconsistent with well authenticated facts and with each other, but in the philosophical leaven of utilitarianism which imbues the profession, and upon which a true mental pathology can never be engrafted.

It ought not to be difficult to succeed in explaining chemical changes, according to the most recent scientific knowledge, to men whose notions lingered in the regions of phlogiston, because opinions concerning physical phenomena easily give way to sounder opinions properly substantiated. But those which respect the noumena of the world of thought and emotion, retain their hold with the tenacity of opinions founded, not upon the testimony of others, but upon that of the monitorial witness which every man carries within him.

Mr. Bentham himself appears to have recognised this difficulty. If his system of utility were fully carried out, it would apparently demand that questions of this difficult nature should be decided upon the testimony of experiments, but he draws a distinction between physical and moral improbability which eludes this necessity.

"The degree of distrust produced in the mind of a judge by the improbability of the alleged fact, when that improbability is of the physical kind, as above, will depend upon the confidence he has on his own knowledge respecting the powers and order of nature, so far as the particular fact in question is concerned. If he have any doubt, he will do well to have recourse to scientific evidence—to call in the opinion of such persons as, by their professional situation or reputation, are pointed out to him as being peculiarly well informed in relation to matters of that sort.

"Concerning moral improbability as above described, every man acting in the situation of a judge will naturally consider himself as competent to pronounce. A man on these occasions looks into his own mind, and asks, as if it were of himself, whether it be probable, or possible, that in the circumstances in which the person in question is stated by the evidence as entertaining such and such perceptions, conceptions, intentions, wishes, and the like, it could have happened in such circumstances to himself, to have entertained any such perceptions, conceptions, intentions, wishes, and the like." (Bentham's works, vol. vi. p. 153.)

Now questions relating to insanity being "confined to such facts as have their place in the human mind," would, according to this distinction, be withdrawn from the sphere of scientific evidence, and referred to a comparison with the mental operations of the judge. No doubt such comparisons are involuntarily made by all men, and are the source of many judgments and of more sympathies and antipathies; this mode, however, of arriving at truth, is liable to fearful mistakes, and is ill calculated to subserve the ends of impassive and even-handed justice. But it may be asked, Is the evidence of medical witnesses all that could be desired? is it always lucid and simple, bearing the impress and the authority of skill and experience? above all, do medical experts never disagree, and thus mutually neutralize the force of each other's testimony?

Those conversant with lunacy trials will readily make for themselves the humiliating answer. The occasions for that answer are not far to seek. In the first place, the great mass of practitioners feel themselves, year by year, further removed from opportunities of acquiring an efficient practical knowledge of the subject. The legislature imperatively commands all pauper lunatics to be forwarded without delay to the county asylums; so that a medical man in extensive practice cannot, in the bulk of the population, see more than the commencement of insanity cases, without subjecting himself to heavy penalties. Even when insanity occurs among his wealthier patients, and he does not feel himself under this compulsion, well knowing that the probabilities of cure will be small so long as the patient remains at home, in the midst of the influences which have occasioned the malady, he urgently and disinterestedly counsels removal to an asylum. The improved treatment prevailing in most asylums, and the enlightenment of the public mind on these matters, concur to support his counsels. From these circumstances, it happens that the generality of medical men in practice see very little of the phenomena and treatment of insanity; so little, indeed, do they see, that when the overt act of some neglected lunatic occurs to place one of them in the witness-box, he may feel as much need to grind up for the occasion from Taylor and other text-books as the barristers themselves. Nor is it surprising that the latter gentlemen should have the advantage at this kind of work, seeing that they are in the constant habit of thus getting-up knowledge sufficient for their purposes at short notice, and upon every variety of subject; and that the practised word-fencer must ever beat the unpractised at word-fencing was recognised by Plato in his dialogue between the sophister and Socrates.

This removal of lunacy practice from private medical practice can in nowise be compensated for by a little talk about insanity during the hurried years of medical studentship, or by information derivable from a course of lectures at Hanwell, and a course round the wards of the same institution repeated some half score of times, during which the prevailing sentiment among the pupils may be conveyed in that proverb which consigns the most tardy to the enemy of the human race.

In the annual report presented last year by the medical officers of Bethlem, it is stated—

> "There have been latterly a few pupils in attendance, but their main object, with a few exceptions, appears to be, the obtaining a certificate of competence to superintend a county asylum, or some fleeting object of the day; any steady pursuit of a real knowledge of the nature and treatment of insanity is a rare circumstance, and scarcely to be expected among the multifarious objects which necessarily distract the attention of medical students."

With this pretence of instruction, and with no subsequent practice, it may come to happen that witnesses unconnected with the specialty may seldom have reasons for speaking with authority on lunacy; or if any, they may be sounding rather than sound, like those of the gentleman who undertook to teach the German language from his knowledge of the German flute.

But if modern legislation and medical practice tend in this manner to concentrate the knowledge of insanity in a few experts, who make its nature and phenomena their especial study, do these experts always entertain opinions conceived with that unanimity which exalted knowledge should engender? Are the physicians of Hanwell, Bethlem, St. Luke's, and other like institutions, never to be found ranged in the affrays of law against each other, like the Homeric Olympians, mingling in the vulgar feuds of mortals? Alas! human reason is fallible, and even the bench of bishops do not agree on all subjects. There is, however, extraneous to this fallibility, a cause which leads the opinions of skilful and experienced men to diverge from each other; a cause which enters into active operation when the real balance of probabilities approaches nearly to an equipoise, when the matter in dispute is almost a point—a point, however, which may loom large through the haze of conflicting interests. We allude to the manner in which cases are *got-up* by the attorneys. The depreciating opinion of lawyers is well known, that on nice points, if a dozen medical witnesses can be induced to take one view of the question, thirteen can readily be placed in opposition to them who will take the other view. We admit that this allegation is, to some extent, founded upon the realities of experience; we deny that it is dishonourable to the profession. The same array of conflicting opinions may be marshalled on scientific questions of any description not medical, provided they are intricate, disputed, and of balancing probabilities. Thus it has ever been, and thus it must ever be, so long as questions of this nature continue to exist, and the minds of men present their usual varieties of capacity and bias. If discredit attaches anywhere, it attaches to the form of legal procedure, which adopts such clumsy expedients for the investigation of truth and the administration of justice.

Suppose a case in which the main question at issue is the sanity or insanity of an individual; the first step is the appointment, by chance, interest, or merit, of attorneys—for the prosecution and defence, if the case be a criminal one—for the plaintiff and defendant, if it be a civil action. Suppose, further, that the case is not one in which the facts will necessarily carry conviction to the minds of the judge and jury, but will leave full scope to the ability of the attorneys in procuring favourable scientific evidence, and to the skill of the barristers in extolling or depreciating such evidence as may tell for or against the interests of their clients. How do the attorneys proceed to enlist under their respective banners the opinions of disinterested medical men? Why, they ascertain what men of repute are known to entertain general opinions leaning towards stringent or modified responsibility, towards a restricted or relaxed application of the term insanity, and the like. If these gentlemen do not already rank in the highest grade of the profession, and have therefore more desire to gain credit than fear of losing it, so much the better for the lawyer's purpose. These men they call upon, and place before them the facts and arguments bearing on the case, in a manner as favourable to their clients as it is possible to do without palpable departure from the truth. Medical witnesses thus for the first time see the case through the medium of an attorney's spectacles, which are tinged, but certainly not with a neutral tint. They may commence with a hesitating and qualified assent, but they will be fortunate and highly praiseworthy if they permanently repel and subdue every trace of desire that the opinions to which they assent should prevail. If such desire should creep in, it will make them in a commensurate degree partisans. A man who is able on such occasions to resist all approach to partisanship is more or less than human, for partisanship, like laughing, cooking, and the other peculiarities characteristic of the human race, is a part of his nature.

In courts of law, partisanship is rampant; it is the very essence of the barrister's calling. The probability of its existence in the jury-box is recognised by the provisions for challenging, and by allowing the prejudices of foreigners to be represented there when the prisoner is an alien. Even judges cannot be said to be free from it, although what may be considered as a mere frailty in others may become a high crime in them.

"Naturam furcâ expelles, tamen usque recurret."

They cannot see a prepossessing or a repulsive expression in the countenance of a prisoner, without experiencing on that account a feeling of favour or disfavour towards him. But the expression of such feeling, though not unknown, is highly reprehensible, because it would prejudice the jury and prejudge the prisoner's guilt.

Under the existing system of criminal procedure, the conduct of the prosecution and that of the defence are often equally unscrupulous. Can nothing save our courts from the scandal of a system, which is removed but a few years in time, and a still less degree in spirit, from the practice of rewarding with blood-money witnesses on whose evidence a criminal was hung!

For the purpose of ascertaining truth, it appears to us that the duty of setting forth the argument with impartiality and justice, to "nothing extenuate nor set down aught in malice," is as weighty, as important, and as dignified as that of forming a judgment upon the argument and apportioning the penalty.

Why, therefore, are there not public prosecutors?—officials responsible for the dispassionate discharge of those duties which at present devolve upon attorneys, with whom the administration of justice is secondary in importance to the honour and prestige of success!—officials who would supersede the worn-out machinery of the grand jury, and impart certainty, unity, impartiality, and dignity, to those portions of criminal-law procedure which at present are too often distinguished by the opposite qualities!

With public prosecutors, scientific evidence would still be sought, but would no longer be *got-up* as at present. Scientific opinion would no longer be warped at the outset by partial and interested statements; the honest and dignified pursuit of truth and justice on the side of the prosecution, would enforce the same spirit upon the defence; and on questions of medical or other science, skilled witnesses would be called, not to gain a cause, but to elucidate the truth.

But in civil suits involving the question of insanity, and in inquisitions, the appointment of public prosecutors would not be operative; and in these processes, the *getting-up* of scientific evidence is pursued in a manner still more unscrupulous than in criminal-law procedure; the same agencies are even more actively at work, warping opinion and enlisting evidence.

Let it be granted that the attorneys only do their duty. These difficulties appear inseparable from the practice of admitting opinions as evidence, of permitting men to testify not only to their sensations and knowledge, but also to their convictions and judgments. Such mental operations are not evidential, but judicial; and we are compelled to the belief that persons exercising them are out of place in the witness-box, and that the only efficient remedy is to change their position. This may be effected in one of two ways: either by separating the question of insanity from that of guilt, by leaving the latter to be tried in the ordinary manner, and by impannelling a jury of experts to try the former; or, secondly, by adopting a hint from the practice of the Admiralty court, and calling scientific aid to *assist* the judge. Who can doubt, that in questions of salvage, and collision, and barratry, and others involving the art and science of navigation, skilled opinions could be ranged on either side with as much facility and with as neutralizing an effect as in the more intricate questions of insanity, were such opinions called in to assist the plaintiffs and defendants, and not to assist the court! But this stultification of evidence is avoided by calling in skilful and experienced mariners, the Brethren of the Trinity Corporation, not as partisans to assist the plaintiff or defendant, but as *amici curiæ*, to assist the court with their opinions and judgment.

We are convinced that if a similar procedure were adopted in all lunacy trials and inquisitions, the decisions and awards come to by its means would be more satisfactory to the contending parties and to the public, would better promote the ends of justice, would more effectually sustain the honour of the legal and medical professions, and be found in every respect vastly superior to the present one.

From this digressive excursion into the realms of law, we return to those of psychology. Dr. Prichard, as cautious as he was learned and experienced, satisfied himself with recording the facts he had observed in the occurrence of moral without intellectual insanity: as he twisted no especial theory out of these facts, there is none to examine: had he formed one, it would have resolved itself into a part of the larger question to which we must now direct our attention. Those of our readers who have had opportunities of observation will not require that Dr. Prichard's statements should be verified by examples; those who have been less fortunate must refer to his treatise on insanity. As for the lawyers, we must now reluctantly leave them behind; like tail hounds, they are boggling on the cold scent of the last check, while we have had a smart burst and have come to another; they will never come up again unless they run to our cry.

All medical men of experience now acknowledge the occasional existence of mental disease without disorder of the intellectual faculties. The problem now claiming attention is a more advanced and extensive one: namely, whether, with certain admitted and well marked exceptions, insanity does not invariably commence with and consist in emotional disturbance. The exceptions include those cases which by some writers are designated Symptomatic Insanity, and arise from recognised physical causes: from drunkenness, gout, fever, phrenitis, apoplexy, epilepsy, blows on the head, insolation, parturition, old age, &c. We believe that, except in these cases, convincing arguments can be adduced to prove that *insanity is always in the first instance emotional;* that intellectual disturbance is always secondary; and that Dr. Prichard's cases were apparently exceptional, because in them the secondary part of the disease did not occur, from the unusual force of a conservative tendency in the intellectual faculties. Any hesitation we may feel in adopting this theory arises not from any deficiency of argumentative proof, but because in reconciling so many inconsistencies and in smoothing so many difficulties, it appears to favour the dominion of that idol of the tribe which leads us to expect and to require an amount of uniformity which does not exist in the operations of nature.

M. Guislain has recognised some portion of this theory, though he has missed the whole breadth of its truth. He has unnecessarily limited his view to the operation of the painful emotions, and has therefore persuaded himself that mental aberration is but a state of phrenalgia. He may possibly be right, on the ground that pleasurable emotions must become painful from their intensity, before they can produce so serious a result as mental disease. Being tickled to death is said not to be an easy or agreeable mode of dissolution; and by blunting the nervous sensibilities, chloroform might have saved that luckless wight who

"Died of a rose in aromatic pain."

It is to be regretted that M. Guislain should thus have been diverted from a train of ratiocination directly tending to an enlarged emotional theory, by subtile distinctions between pleasure and pain; distinctions which almost lead him to disbelieve the existence of the former. He expresses his concurrence with that accomplished psychologist, M. Brierre de Boismont, who thus sums up his long experience on the causation of insanity:

"Eh bien, ce que nous avons vu et entendu depuis trente ans, nous donne la conviction inébranlable que la souffrance morale est le lot de l'humanité. Quand la statistique, que nous apprécions à sa juste valeur, nous accablerait de ses chiffres, nous ne pourrions nous empecher de dire: ils souffrent; s'ils le nient, ils trompent sciemment. Le bonheur n'a pas d'enseigne."

We do not ourselves concur in the Rasselas sentiment which estimates the lot of mankind as one of inevitable moral suffering. Undoubtedly the life of man is one of effort, of contest against evil, or of craven submission to it. But so far as we have learned to appreciate the struggle, it is one which, on the whole, elicits and confers far more of happiness than of misery. We entertain a firm conviction that not only in the world at large, but even in the wards of lunatic asylums, the amount of moral enjoyment vastly preponderates over that of moral misery.

The instances of insanity caused by sudden and great success, or elevation to high fortunes, are too numerous and too well authenticated either to be explained away or denied. We cannot, therefore, subscribe either to the major or the minor of the argument which refers the causation of insanity to moral pain alone.

In addition to the above objections is the impossibility of defining for purposes of scientific exactness, the meaning of pain and pleasure. What is pleasure to one man becomes pain to another, or to the same man at another time. King John tells Constance that she is in love with grief; and in reply to Rosalind's taunt, "They say you are a melancholy fellow," Jaques says, "I am so; I do love it better than laughing."

In Devonshire, invalids are popularly said to *enjoy bad health*. Although this expression may be only a vulgar periphrasis, and not intended to convey the meaning of pleasure derived from bodily disease, it may nevertheless sometimes express a fact. The sensations produced by psora are described by writers of authority as rather agreeable than otherwise; and we have read of patients subject to recurrent mania, who looked forward to the excitement of the paroxysm with lively anticipations of pleasure. Certainly there is no accounting for tastes, and the man who can draw a scientific line between pleasure and pain will have something to bequeath to posterity. It will not, however, be by placarding the words *bonheur* and *malheur* in large capitals, after the manner of M. Guislain, that this achievement will be accomplished, and the *mélange* of good and evil to be found in this mortal life be analyzed for the purposes of psychological inquiry. To import the question of pleasure or pain into an inquiry on the causation of insanity, appears to be a gratuitous complication of a subject sufficiently difficult in itself.

Eschewing, therefore. the phrenalgic theory as unnecessarily narrow and involved, by what kind of reasoning may we expect to found our theory of insanity upon the broad basis of emotion in general? The arguments adducible for such a purpose might be arranged in two divisions, according as they belong to ethics proper, or to mental pathology. The former would embrace the whole controversy on utilitarianism, on selfish or unselfish motives to action, and cannot, therefore, be introduced in this place. Being convinced that it is impossible to explain the nature of shame, remorse, justice, moral approbation and disapprobation, by the calculations of advantage and disadvantage, we adopt the unselfish theory. We feel assured that "mankind demands of its heroes some other merit than that of a sagacious merchant;" that civil law is not the measure of innocence and crime, or theologic law that of virtue and vice; that innate principles of duty and of right are implanted in the human soul; and that in these principles, and in the varied play of the emotive faculties, is to be found the true key of human action.

That reason, as reason, can never be a motive to action, is thus succinctly demonstrated by Sir James Mackintosh:

"An emotion has not necessarily anything in common with a perception but that they are both states of mind. We perceive exactly the same qualities in the taste of coffee when we may dislike it, as afterwards when we come to like it. In other words, the perception remains the same when the sensation of pain is changed into the opposite sensation of pleasure.......We can easily imagine a percipient and thinking being without a capacity of receiving pleasure or pain. Such a being might perceive what we do; if we could conceive him to reason, he might reason justly; and if he were to judge at all, there seems no reason why he should not judge truly. But what could induce such a being to *will* or to *act?* It seems evident that his existence could only be a state of passive contemplation. Reason, as reason, can never be a motive to action. It is only when we superadd to such a being, sensibility or the capacity of emotion or sentiment, or (what in corporeal cases is called sensation) of desire and aversion, that we introduce him into the world of action. We then clearly discern, that when the conclusion of a process of reasoning presents to his mind an object of desire, or the means of obtaining it, a motive of action begins to operate; and reason may then, but not till then, have a powerful though indirect influence on the conduct. Let any argument to dissuade a man from immorality be employed, and the issue of it will always appear to be an appeal to a feeling. You prove that drunkenness will probably ruin health—no position founded on experience is more certain; most persons with whom you reason must be as much convinced of it as you are. But your hope of success depends on the drunkard's fear of ill health; and he may always silence your argument by telling you that he loves wine more than he dreads sickness."

The conclusion is obvious. If our reasoning faculties, when in a sound and healthy condition, cannot conduct to action, still less will they be capable of doing so when they are enfeebled by disease.

The pathological argument rests upon the facts well known to physicians, that the causes of insanity are of a nature producing in the first place emotional changes only, either by the sudden and violent agitation of the passions,

> "When all the heartstrings like wild horses pull
> The heart asunder,"

or by the long-continued influence of circumstances operating more insidiously upon the mind, and producing an habitual state of abnormal feeling.

A man was never yet either reasoned into insanity, or reasoned out of it. The delusion-test may with propriety be upheld by our judges, highly intellectual as they are, and full of dialectic power; but should the test of insanity ever become a part of statute law, it is inconceivable that the large experience of our houses of parliament will permit them to allow it to rest upon any kind or amount of bad reasoning.

The larger portion of the treatment of insanity consists in what is emphatically denominated moral treatment, in restoring the equipoise of the emotions, in repressing the monster passion which swallows up the rest, and in renewing the activity and vigour of the little passions which have been thus unceremoniously dealt with.

No sane man would attempt to reason away the erroneous opinions of the insane; even lunatics of asylum experience come to recognise the hopeless nature of such a task, and respect the delusions of others, although they may be antagonistic to their own.

In the prodromic period of the disorder the emotions are always perverted, while the reason remains intact. In the period of convalescence, the return of correct judgments is an uncertain and fallacious indication of cure, so long as the emotions remain, even in a slight degree, perverted from their normal condition; but immediately the latter are put straight, the cure may be considered complete. Lastly, and chiefly, with the exceptions above indicated, there is no description of insanity which, if traced to its source, will not be found either to consist in perverted emotion, or to emanate from that origin. Such will insanity ever be found by those who diligently investigate its origin, although by secondary disorders of the intellectual faculties it may and usually does become so transformed and disguised, that its essential nature is subsequently not easy of recognition.

In endeavouring to ascertain the nature of insanity by observing and determining the effects of certain causes, the progress of investigation is impeded, by the difficulty of meeting with cases in which the causes are single and simple. In such an investigation, great difficulty arises from the multiplicity of causes, and in a still greater degree from the intermixture of effects. Again, much perplexity is experienced in the observation of effects; for, as the centre of sensation feels not when it is wounded, so the centre of attention can scarcely attend to itself. Few men are able to appreciate the noumena occurring within themselves, and M. Cousin rightly observes that there are not more Des Cartes in the world than Newtons. Were this not so, the almost incredible blindness which left men for more than fifty-six centuries in ignorance of the circulation of the blood, would be as nothing compared with the self-ignorance which left it to David Hartley to make the noble discovery of the simple law under which our passions and affections are formed. When we endeavour to observe the effects of moral agents, not in ourselves but in others, the cloud is still darker. What doth it profit to the end of this inquiry, if, according to the author of 'The Vestiges,' "man in the mass is a mathematical problem," if, "when seen in the individual, he continues to be an enigma"? Suppose it known that one of every six hundred and fifty Frenchmen will commit a crime once a year. This knowledge in itself will not advance us one step towards ascertaining or removing the motives of crime in the enigmatical unit. Keble truly says—

> "Not e'en the tenderest heart, and next our own,
> Knows half the reasons why we smile or sigh.
> Each in his hidden sphere of joy or woe
> Our hermit spirits dwell."

It is true that a madman may often be what Horace says of a drunkard, "pellucidior vitro;" but this species of insane candour is far from being trustworthy, and the difficulty of accurately ascertaining motives, and of observing the operation of pathematic agencies, remains one of the most serious perplexities of the practical psychologist.

In the endeavour to ascertain the antecedents of an attack of insanity, we are particularly liable to be misled by the interested statements of friends and relations, who disguise or deny circumstances which might be thought discreditable to the patient or to themselves. Even when a true history of the case is obtained, its aspect may be very fallacious, as the following well-contrasted examples may serve to illustrate. A seaman returning from the coast with prize money was hocussed, robbed, and immediately became insane. A young Protestant female, on a visit to a Catholic family, came under the influence of the priest, and became a convert: much unpleasant discussion took place in consequence with the members of her own family, and in little more than a year she became insane. In the first case, there appeared to exist the obvious physical cause of poisoned blood; in the second, a no less obvious moral cause. Nothing, however, could be further from the truth, as subsequent knowledge of these cases proved. In the first, the cause was moral, arising from grief; in the second, the cause was physical, arising from semi-starvation and watching during Lent.

For aught we know to the contrary, injuries to the head, poisoned blood, and other similar causes, are capable of producing insanity in any human being; but moral causes, on the contrary, appear to be efficient only when the mind is in a condition favourable to the reception of the morbid influence. When this predisposing condition exists, trivial causes become influential and effective; when it does not exist, men have for lengthened periods been subjected to every kind, degree, and combination of mental suffering, without lapsing from the healthy states of vigorous resistance or of patient endurance. The nature of this predisposing condition appears to consist in abnormal motivity or impressibility of some emotion or set of emotions, combined with a weak and deficient power of will. Too much weight cannot be attached to the recognition of this state of mind, with the view either to the prevention of insanity by a properly adapted education, or with a view to its prodromic treatment.

In investigations concerning mind, it must never be forgotten that a cause, however simple in its nature, does not produce simple and unchanging effects. Geology shows us that the rain-drops of a passing shower may leave their impression upon the soft sand, to remain unchanging and unchanged through countless ages. A like natural phenomenon has often decided the issue of battles, the fate of kings and of nations, has prevented famines, has influenced and is influencing the destiny of the human race. It is said that a rainy day will spoil the best or the worst Parisian revolution. The former example of causation finds no type in the world of mind; the latter is continually represented there. How far did the petty theft of a piece of ribbon influence the life and opinions of Rousseau, and through them the opinions and destiny of the French nation? In mental dynamics, a cause not only produces a train of effects incalculable, from the ever-varying nature of the existence acted upon, but even when simple the cause is oftentimes one of an accumulating sort: like that illustrated by Mr. Mill in the increasing heat of the summer as the days become longer, and the nights during which the heat can be radiated back become shorter, so that the heat accumulates and becomes greater after the solstice than at it—the funded heat of August, as it is called by De Quincy. This description of cause is frequently observed operating in the production of insanity, in unchecked desires and emotions habitually excessive, which, gaining strength with indulgence, eventually overcome all antagonistic and balancing faculties. Of this kind are many causes not strictly moral, but acting upon the emotions; as poverty causing mental distress and incapacity to struggle against it by industry, and consequently greater poverty.

The ever-changing, increasing, and indefinite operation of causes producing insanity, is a strong reason for supposing that their power is exerted upon the emotional part of man's nature. Whatever operates upon the reason produces a definite effect. The intellect cannot refuse either assent or dissent, qualified or entire, to any and every argument or theorem propounded to it. The dialectic function is impassive as machinery, and there is little doubt, if it were possible to submit the premises, that logical engines could be constructed as efficient in their work as Mr. Babbage's celebrated calculating machine, or Mr. Clarke's ingenious instrument for grinding Latin hexameters. The logical engine which every man possesses often does its work badly enough, but whatever the results may be, they are definite and precise.

"Rage seized him as these contrasting pictures rose before his view. He walked to and fro in disorder, striving to re-collect his thoughts, and reduce himself from the passions of the human heart into *the mere mechanism of calculating intellect.*"*

This intellectual faculty forms an opinion general or particular—erroneous or correct—there it stops; and according to the materials submitted, the quantity and quality of the work done, a man becomes full or empty, stupid or wise, a dolt or a Newton. A man may be an idiot from congenital deficiency of this faculty, or may become demented from decay of it; he may be crotchety from its imperfection, but it appears to us impossible that any condition of the reasoning faculty can produce madness. A medical witness was asked in a lunacy trial whether, if a man believed that a person could see through a three-foot brick wall, such a belief would constitute a delusion, i. e., a criterion of insanity. The unfortunate witness having replied in the affirmative, was of course trotted through an amusing array of consequences, terminating in the melancholy and distressing insanity of all believers in mesmerism. Now, whatever may be thought of the powers of observation and ratiocination displayed by these gentlemen, it must be admitted, that to call them insane is most erroneous; because, with the exception of getting angry when one presumes to argue with them, their opinions do not touch their motives, and consequently do not lead to action. Like the necessitarians, they act in opposition to their opinions. The discovery of clairvoyance has not stopped the printing of one newspaper, or lightened one mail-bag; the electric telegraph is as much used as if the stupendous discovery of the snail telegraph had never been made; and notwithstanding the newly perceived translucency of three-foot brick walls, we doubt whether the fact has caused any alteration in the thickness of party walls, or any other architectural arrangements of the most enthusiastic disciple. When a mesmerist judge seriously endeavours to supersede witnesses, counsellors, and jury, by employing sensitive clairvoyants to discover the truth, we think it not altogether improbable that, out of court at least, such proceedings will be considered rather mad.

In 1787, a Dr. Elliot was considered insane, because he anticipated some of the scientific opinions of Sir W. Herschel. He was tried at the Old Bailey for firing a pistol with intent, &c., at a Miss Boydell, thereby burning her clothing and contusing her shoulder. The jury could not find that there was a ball in the pistol, and on this acquitted him. Sir David Brewster, in a note to 'Ferguson's Astronomy,' says:

"The friends of the Doctor maintained that he was insane, and called several witnesses to establish this point. Among these was Dr. Simmons, who declared that Dr. Elliot had, for some months before, shown a fondness for *the most extravagant opinions;* and in particular, he had sent to him a letter on the light of the celestial bodies, to be communicated to the Royal Society. This letter confirmed Dr. Simmons in the belief that this unhappy man was under the influence of this mental derangement; and as a proof of the correctness of this opinion, he directed the attention of the court to a passage of the letter, in which Dr. Elliot states 'that the light of the sun proceeds from a dense and universal aurora, which may afford ample light to the inhabitants of the sun beneath, and yet be at such a distance aloft as not to annoy them. No objection,' says he, 'ariseth to this great luminary being inhabited; vegetation may obtain there as well as with us. There may be water and dry land, hills and dales, rain and fair weather; and as the

* My Novel. Bulwer.

light, so the season must be eternal, consequently it may easily be perceived to be by far the most blissful habitation of the whole system.' "*

Our good friends the Mesmerists may, if they think proper, make use of this illustration, and enjoy the fond anticipation that before 1887, a greater name than Elliot may have justice done to it, and that opinions which are now sneered at as illustrative of delusion, will come to be considered as inspirations of the highest genius.

But suppose Dr. Elliot's opinion, instead of a scientific, had been a really foolish one; that instead of a solar, it had taken a lunar direction, and he had thought the moon was made of green cheese, or that, like the wise men of Gotham, he could fish it out of a pond with a net. It does not appear that even such opinions could have induced him to fire a pistol at Miss Boydell, to burn her petticoats and bruise her shoulder: the intellectual mistake could have produced no desire leading to such an act —in other words, the overt act could not have been traced to or connected with the delusion.

Let us now proceed to examine cases in which the insane conduct may be thought to be obviously traceable to and dependent upon delusive opinions.

It was argued that Miss Nottidge was insane, because she believed that Mr. Prince was God; but in Germany a considerable sect of thinkers believed that God exists in every man, and that every man is God as far as he goes; and Spinoza believed that God was matter: therefore, if opinion constitutes madness, and Miss Nottidge was, on that account, insane, *à fortiori*, all these metaphysicians must have been so.

But the opinions of the latter touched not upon motive and action— they scarcely influenced the conduct of life; at most they might possibly tend to keep their advocates out of churches, but could certainly never lead them into asylums. On the other hand, the opinions of the lady probably originated in, and were certainly most intimately connected with, the fondest and deepest emotions of the heart. For them she sacrificed the ties of maternal and sisterly affection, the opinion of the world, and all that she possessed; she left all to follow him. Perhaps her opinions were not more irrational than those of the philosophers, but her emotions were deeply implicated: she entertained towards the creature those *sentiments* which are due only to the awful majesty of the Creator. In such cases, the true test of insanity must be sought for, not in deluded opinion, but in perverted emotion. They supply, therefore, an additional argument in favour of the emotional theory.

Having adduced in support of this theory arguments at least sufficient to establish its claim to a fair and deliberate investigation, we leave it to be tested by time and deductive inquiry. We ask not, and would decline to accept for it, more than an impartial scrutiny, believing that too ready acquiescence in the truth of propositions of this nature is not less inimical to the advancement of knowledge, than dogmatic assertions unsupported by argumentative proof.

Before these pages are presented to the public, the Lunacy Bills which have suggested them will probably have become the law of the land. Their principal, and almost their sole merit, consists in condensation

* American Journal of Insanity, July, 1852; taken from Notes and Queries.

and simplification of the existing statutes. Their demerits of omission are considerable. Many important and pressing questions, such as the treatment of criminal lunatics, are entirely passed over. The visitation of asylums, private and public, is to be left on its present footing—although, as regards the former, the noble chairman of the Commissioners in Lunacy has expressed his opinion in the Lords, that nothing could be more defective and unsatisfactory; and as regards the latter, the utmost diversity of practice exists in different counties—a diversity of practice so contrasted, that the existence of error at one or both extremes is unavoidably impressed upon the conviction.

In some asylums the actual work of visitation has lapsed from the main body of the visitors, and has become the privilege and the glory of some two or three members, who, from a natural and well-founded anxiety concerning the treatment of lunatics, constitute themselves into an hebdomadal board, and become what their own capabilities or those of some adroit matron may be able to develop. Surely some reasonable medium might be devised and enforced by enactment, which might rescue the important duty of asylum visitation from degenerating into a formal sham, or developing with exuberant vitality into a mischievous source of excitement to the patients.

Another subject on which the greatest diversity and laxity of practice exists in different county asylums, is the manner of apportioning the expenditure between the maintenance fund and the county rate. In several counties a building and repair fund has been established, from moneys strictly and legally, perhaps, belonging to the maintenance fund: the county rates have thus been altogether relieved from the repair charges to which they are liable under the 26th section of the present act. At other asylums all expenditure for the repair of the building, for painting, glazing, and furnishing, is charged on the county rate. In some instances, where the main efforts of the visitors and the superintendents appear directed to the acquisition of credit for extreme economy of management, the most extraordinary items of expenditure are thus charged. Charges for bedding, clothing, attendants' salaries, under the title of artisans' wages, are thus transferred from the poor rate to the county rate. In one county even the chaplain's salary is thus classed with building repairs, and in another the county rate is saddled with the maintenance of the steward-superintendent's pony, though no efforts are made to prevent the medical officer from becoming thoroughly *footsore*. Thus, by cooking the accounts, the maintenance charge is kept at a figure astonishingly low. So great is the diversity which exists in the manner of keeping these public accounts, that no fair comparison can be made between the maintenance charges of one asylum and those of another, without an elaborate analysis and rearrangement of all the items of expenditure.

An amended Lunatic Asylum Act might reasonably have been expected to rectify these irregularities: yet so far is this from being the case, that in the new bill not only is no attempt made to do so, but even that strange ambiguity of expression is allowed to remain, "shall be claimed to be *leviable*, &c." (*sec.* 70), which leaves Visitors at liberty to puzzle out, if they can, whether the rent-charge on patients from non-contributing boroughs should be carried to the maintenance fund, or to the relief of the county rate. Even the visitors of two county asylums in the same county have adopted opposite opinions on this point; one board having decided to send this considerable sum to the county treasurer, and the other having passed it to the maintenance account.

Having shown that no attempt has been made to place the question of lunacy on a more philosophical basis, and that even on the important subjects of asylum visitation and expenditure, the most glaring practical defects of the present system are left without amendment, we have no inducement to extend our criticisms to details. We look on the Bills somewhat as a Portuguese fundholder might look upon a new arrangement of the interesting debt of that country—as an advance, namely, towards simplification of arrangement, but as holding forth little prospect of any important results of a beneficial nature.

That they indicate no greater approximation than has hitherto been made between the Law and the Theory of Insanity, can perhaps scarcely with justice be urged as an objection against them, inasmuch as their operation lies out of the pathway of those circumstances which render most apparent the antagonism of true mental science with the lunacy practice in law.

During a recent visit which we paid to that "Cave of Despair," the criminal ward at the Royal Hospital of Bethlem, we were informed that the Government had strictly prohibited any alteration or reform therein. We received this announcement with much satisfaction, as an indication that this department of lunacy treatment was likely to be put on an entirely new footing; that Government believed the affair to be utterly bad, and beyond amendment, and contemplated a revolution therein, in preference to reform.

When new arrangements for criminal lunatics are attempted, if they are not dictated by true psychopathic science, manifest failure will scarcely be avoided. But if in such new arrangements the Law and Theory of Insanity are made to submit to a satisfactory amalgamation, such arrangements may form a *point d'appui* from whence other important reforms of a similar nature will be possible.

That the antiquated barbarisms of our common-law dogmata on the subject of insanity can much longer possess the slightest weight of authority, we do not believe. When the members of our own profession are giving to the world such works as Sir Henry Holland's 'Chapters on Mental Physiology,' and Professor Carpenter's remarkable section, 'On the Functions of the Nervous System'—a chapter which we do not hesitate to characterize as the most profound treatise on the subject which the world has seen—it will be impossible for any length of time to retain lunacy laws founded even a century ago, when physiology was in its infancy; to say nothing of those which took their origin at a time when the nature of insanity was a theological question, and its treatment was confined to the exorcist and the priest.

John Charles Bucknill.

Review VI.

Traité de Chimie Anatomique et Physiologique Normale et Pathologique, &c. Par Charles Robin, M.D., et F. Verdeil, M.D., &c.—*Paris,* 1853. Tomes I., II., & III.

Treatise of Anatomical and Physiological Chemistry, Normal and Pathological; or, of the Immediate Principles, normal or morbid, which constitute the body of Man and of the Mammiferæ, &c. By Ch. Robin and F. Verdeil.

We have endeavoured, in a former number of this journal, to give an account, extracted from Messrs. Robin and Verdeil's elaborate treatise, of the 'Immediate Principles which constitute Healthy Human Urine;' we shall now proceed to consider the contents of this work in a more general point of view, insisting upon a few topics, which, from their novelty, or from their classification, deserve particular notice.

Messrs. Robin and Verdeil's book was written with the intention of giving to physicians and anatomists a complete account of the intimate or molecular structure of organic substances in their three fundamental states, liquid, semi-solid, and solid; or, in other words, the authors have endeavoured to describe the various substances or immediate principles which, by their molecular aggregation, constitute organic substances. They do not therefore treat of the organized matter itself, but of its constituents.

Their work is divided into three volumes. The 1st is entirely devoted to general considerations of the 'Immediate Principles;' the 2nd and the 3rd present a systematic view of these substances.

The general feature of this treatise is the systematic classification of the subject. It was impossible that in an undertaking of such magnitude, the authors should have overlooked the importance of a proper classification, but there is a peculiar philosophical arrangement of the facts described, with the deductions drawn from them, which enables the reader to be at once acquainted with the whole of the subject, and to judge for himself of the accuracy of the author's conclusions.

M. Robin's writings all reveal a most systematic mind; from this characteristic tendency, so visible in his classification of general anatomy,* he possessed peculiar talents for the composition of a treatise on anatomical chemistry. M. Robin is an anatomist, he has prosecuted that science into its minute details, but he could not have undertaken to write alone a treatise so intimately connected with chemistry; it was therefore indispensable that this work should be the result of the united efforts of both an anatomist and a chemist, and M. Verdeil happened to be possessed of the very acquirements M. Robin stood in need of.

M. Verdeil is a chemist and a physician; his investigations have therefore been directed to chemical subjects more or less connected with physiology and anatomy. His researches into the composition of the blood, the constituents of the lungs, into the nature of animal and vegetable colouring principles, and more recently into vegetable humus, with Mr. Risler, suffice to show that physiology as well as chemistry is already considerably indebted to this author. We may also observe, that while

* See Tableaux d'Anatomie Générale, par Ch. Robin.

he possesses a remarkable power of generalizing facts, and establishing theories on their mutual relations, this philosophical tendency is accompanied by a correct judgment and a clear understanding; thus imparting to his share of the treatise we are about to review a considerable degree of originality. This tendency, however, is decidedly reprehensible when appearing in too great excess, and it is of the utmost importance to guard against inconsiderate and hasty generalizations, not only because the facts upon which a theory is founded may be afterwards discovered to be false, but also from the difficulty of drawing correct conclusions even from well established facts.

To conclude these preliminary observations, we may add, that both M. Robin and M. Verdeil appear to us to err in two directions; on the one hand by an unnecessary and indiscriminate desire to divide and subdivide the subject, and on the other by an inordinate tendency to construct hasty theories upon apparently well established facts. The authors, both from the new views they have brought forward upon the subject, and from their scientific and philosophical method of exposition, have created a new school, the peculiar character of which is complete independence. They consider that the methods of investigation in physiological chemistry have been hitherto misunderstood, and their task is to supply this deficiency.

At the commencement of the first volume, the reader is at once struck by the length of the introduction or *prolégomènes.* With the difficult task before them of stating new views, and opinions generally at variance with those advocated by the present standard writers, the authors had not only to introduce the subject to the reader, but also to refute such existing notions as were opposed to their theories. It was therefore of the utmost importance that the first part of the work should be devoted to an elaborate explanation of what they understood by anatomical and pathological chemistry.

After having carefully traced the boundaries of *anatomy, physiology,* and *chemistry,* and compared these sciences with each other, they conclude that chemistry has hitherto encroached upon anatomy, inasmuch as that part of the former science which is designated animal chemistry decidedly belongs to the latter. This view is discussed by the authors with the greatest minuteness; and every argument which might be adduced against their opinion is carefully weighed and refuted. Considering, therefore, animal chemistry as a part of anatomy and physiology, it was necessary to appropriate animal chemistry to anatomy, or, in other words, to show how the definite chemical compounds extracted from the animal solid and fluid substance constitute part of their anatomical elements. In anatomy, by means of the scalpel and forceps, we reduce tissues into their elements, such as fibres, nervous tubes, &c.; but these fibres or nervous tubes are not, strictly speaking, anatomical elements, for they contain other constituents quite as important to their existence as fibres or nervous tubes. For instance, if we treat the muscular fibres with water, this extract will yield definite chemical compounds, such as creatine, creatinine, &c.; these substances are anatomical elements of the muscular tissue as well as the muscular fibres or nervous tubes; only instead of the knife and forceps, we made use of water, alcohol, &c. To these definite chemical compounds, constituting anatomical elements, the authors have given the name of

Immediate Principles. They therefore define the immediate principles of the animal system, *the final products, solid, fluid, or gaseous, resulting from a well-conducted anatomical analysis of the various humours and anatomical elements, susceptible of no further division without an alteration of their chemical nature.*

The first volume is devoted to general considerations on the characters of these substances. Nearly all of them crystallize; and this constitutes one of their most important properties, since, by the mere inspection of the crystals under the microscope, we can recognise their nature at once, and be certain of their existing in a pure state. No doubt the necessity of obtaining crystallized substances constitutes one of the greatest difficulties met with in the study of the immediate principles; but this obstacle is easily overcome, if we make ourselves previously acquainted with the peculiar manipulations required in such investigations. Crystallized immediate principles may assume different forms, according to circumstances. Thus, common salt, for instance, when crystallized from urine by very slow evaporation, assumes the form of large, six-sided, perfectly transparent cubes; when obtained by the evaporation of alcoholic extracts, it generally assumes the shape of octohedra, or solids derived from that type; while, when prepared from ethereal solutions, the crystals resemble prisms with pyramidal extremities. The authors insist on the importance to the inquirer of making himself perfectly acquainted with the various forms of crystals of every immediate principle, which M. Robin has admirably illustrated in the atlas annexed to Messrs. Robin and Verdeil's treatise.

In regard to the methods employed in the preparation of the immediate principles, we must endeavour to make aqueous, alcoholic, and ethereal extracts, and to obtain the substances they contain by concentrating these extracts at a low temperature. The principles will generally crystallize at different degrees of concentration, and may be collected for their microscopical examination. It is impossible to give in a few words a general method for obtaining the immediate principles in a crystalline form; but the above, which is the most simple, ought always to be preferred, if possible. It proves, however, ineffectual in a great variety of cases.

It is much to be regretted that the mode of investigation we have just described does not allow us to make a quantitative determination of the immediate principles, as it is impossible to separate by crystallization the whole of a substance when mixed up in a solution with many others. In this case, therefore, we must have recourse to chemical analysis, and for this purpose a certain amount of chemical knowledge is quite indispensable. Thus, we can easily obtain crystals of phosphate of soda from the urine, but this will not give the quantity of phosphate of soda contained in it. It will therefore be necessary to precipitate the phosphoric acid by ammonia and sulphate of magnesia. But this operation, unfortunately, cannot lead to any correct results, as it will be impossible by such means to prevent the phosphoric acid existing in other forms as an immediate principle of the urine, from precipitating.

We have, therefore, no method for obtaining a correct quantitative analysis of the immediate principles. Besides the mode of crystallization of the two principles, the authors give several other methods for recognising and testing the nature of those substances.

Under the head of *Properties upon which depends the distinction between the different kinds of immediate principles*, the authors consider—1st, *The numerical or mathematical properties;* 2nd, *The physical characters;* the former comprising the volume and form, or crystallography, of the above principles, and the latter, their consistence, elasticity, specific gravity, &c., and finally their optical properties, such as their colour, their power of refracting and polarizing light, &c. The authors observe that the index or angle of refraction would be a very good mode of distinguishing the nature of the immediate principles, were it not that the minute volume of the crystals renders any operation of the kind utterly impracticable. The polarization of light by the crystals of certain immediate principles and not by others, affords another and more efficient mode of distinguishing these substances from each other. The limits of this notice, and the peculiar nature of the subject, prevent us entering into the optical details necessary for describing the polarizing apparatus by means of which this property may be ascertained, and we are forced to refer the reader to the original work, in which every circumstance connected with this part of the subject is minutely examined.

The authors consider, in the next place, the chemical characters by which the immediate principles may be tested. These characters include—

1st. *The chemical action of physical agents*, or the chemical action of changes of temperature upon the immediate principles, the theory of pyrogenic bodies, &c. 2nd. The chemical action resulting from the contact of bodies with each other, or the theory of solution and of combination. Finally, they proceed to consider the mechanical mode by which we can ascertain the specific characters of the different kinds of immediate principles, namely, the *microscope*. M. Robin having already published a treatise on the microscope and its uses, the authors refer the reader to his book for a description of that instrument. In the present work they insist more particularly upon the mode of preparing objects for the microscope, upon the different magnifying power to be used according to circumstances, upon the processes to be employed in the examination of the preparations, and likewise on the use of reagents under the microscope, the measurement of the angles and size of crystals, and the modes of delineating them.

The use of chemical reagents under the microscope being, in our opinion, of the greatest practical importance, we regret to see only one page devoted to this interesting subject. We have often observed a drop of ether added to a solution, viewed under the microscope, cause an instant crystallization; in the same manner, the action of nitric acid upon the smallest quantity of urea will produce on the microscope glass a crystallization of nitrate of urea. Common salt and oxalate of lime may also be distinguished under the microscope by the dissolving power of water on the former, and traces of carbonic acid can be detected by the addition of hydrochloric or nitric acid to the solution on the microscope glass. We cannot agree with the authors, when they tell us that the use of reagents on crystals placed under the microscope seldom offers satisfactory results; for we have found such means of incalculable value in a great number of cases. In the latter part of the first volume, our authors take an historical view of the study of the immediate principles. This portion of their work deserves especial notice, as it affords them an opportunity of reviewing and criticizing the various works connected with the subject they are treating, in order to advocate their own views.

Van Helmont was the first who, though admitting only one principle, *water*, extracted the solid substances it contained. He made us acquainted with the carbonic acid and carburetted hydrogen of intestinal gases, and showed the latter substance's property of burning; he also obtained from the blood an alkaline substance; and in the part of his work entitled *De Lithiasi seu ortu Calculi*, he endeavours to resolve urine into its constituent elements. Nicholas Lefévre, in 1660, admits that every organic substance is formed of five distinct principles—water, mercury, sulphur or oil, salt, and earth. At the same period Boyle showed that the principles extracted from the blood by fire were not true principles; he also observed that the vegetable juices are reddened by acids, and become green by the action of alkalies. Barbatus is the first who observed that blood is coagulable by heat, although he does not admit that the coagulable substance is a principle of the blood. Otto Tachenius considered an acid as the agent of every disease, and made some investigations into the nature of perspiration, in which he found salts similar to those extracted from urine, and which he calls urinary or microcosmic salts.

In 1682, Papin, by means of his steam digester, succeeded in extracting from bones, gelatine, salts, empyreumatic oils, and volatile alkali (carbonate of ammonia). In 1684, Robert Boyle published his researches. This writer studied blood physically and chemically, and endeavoured to extract its principles. He observes that the fixed salt of blood turns slightly green-blue vegetable colour, precipitates under the form of a white powder silver dissolved in aquafortis, and has a slight taste of sea-salt.

These analytical researches were naturally very imperfect, as distillation by a great heat was at that period the only known process. In 1771, Rouel the younger began his researches on blood, milk, and urine, showing a decided improvement in the methods of analysis he employed. He evaporated his solutions instead of distilling them, and treated the residue with various dissolving agents, as alcohol; the first, he considered an extractive matter insoluble in alcohol, and soluble in water, as a principle of urine. By the same method of investigation, he extracted from milk chloride of potassium, and a sugar which he compared to sugar-candy. In his comparative analysis of the blood of man, calf, ox, horse, sheep, pig, &c., he constantly detected the presence of the *natrum* or mineral alkali (soda and its carbonate), and showed that the amount of salts in the blood varies according to the different species of animals.

In 1775, Scheele's researches, and especially the improvement in his method of investigation, greatly enlarged the sphere of our chemical knowledge: among his numerous and important discoveries, we may record that of cholesterine, which death prevented his publishing. Fourcroy described it afterwards.

Guyton Morveau, in 1782, published the first treatise on chemical nomenclature. He admits several radicals or elements, as oxygen, so named by Lavoisier in 1778, who established also the existence of sul-ur and several metals.

In 1789, Fourcroy pointed out the importance of extracting the elements ubstances without having recourse to chemical decomposition. This chemist showed the spontaneous coagulation of blood, and the albuminous principle of urine. He found urine to contain mineral salts, and substances analogous to aqueous extracts; he also discovered peculiar principles in that excretion, such as the lithates; and finally established the importance of distinguishing those substances which have been since called *immediate principles,* or, in other words, which are separated immediately, without any alterations of the organized matter. These principles are:—1. The extracts, or extractive matters. 2. The saccharine matters, less abundant in animals than in plants. 3. Mucilages. 4. Fixed oils. 5. Volatile oils. 6. Resins, more frequently met with in plants than in animals. 7. Albuminous substances, or those which solidify by heat; their proportion in animals being greater than in vegetables. 8. The fibrous substances, analogous to the gluten of flour. 9. Soda, lime, potash, and the phosphoric, muriatic, oxalic, malic, benzoic, lactic, sacholactic, lithic, prussic, and boracic acids. It is important not to overlook the above enumeration, as Fourcroy is the first chemist who began a systematic study of the immediate principles. In 1801, he published his elaborate treatise on *philosophical, meteorological, mineral, vegetable, animal, pharmaceutical chemistry, and chemistry applied to manufactures and economy.*

Thenard published his treatise on chemistry between 1813 and 1816. He admits Fourcroy's system of classification, but does not add much to our previous knowledge of the immediate principles.

The treatise on chemistry published by Berzelius soon after Thenard's, contains a more minute subdivision of the subject. This celebrated chemist describes the constitution of the organic immediate principles, and gives a clear insight into the phenomena of fermentation and of putrefaction. We find, however, nothing very new in this treatise on the subject of the immediate principles.

M. Chevreuil was the first who applied the notion of *species* to the study of chemistry, and thus greatly contributed to increase our knowledge of the immediate principles. In 1823, he published his researches on the animal fatty matters; and in the following year, his work on chemical analysis. These remarkable productions show a spirit of investigation which, avoiding *all* minute and unimportant details, has ardently in view great and general results. Chevreuil describes the *elementary* and *immediate* analysis as two distinct processes, and gives rules for the separation of the immediate principles from each other without altering their composition. He also first adopted the name these substances now bear.

Hunefeld, in 1826, in his treatise on 'Physiological Chemistry,' was the first who examined the formation of the immediate principles, and showed that those substances derive their origin from peculiar parts of the economy, and under peculiar conditions. He advises, for the extraction of organic substances, to use alternately the method by incineration, and that by which we obtain precipitates with alkalies and acids.

In 1835, M. Dumas, in his theory of organic substances, endeavoured to fix the exact boundaries between mineral and organic chemistry, and considered the examination of fibrin, starch, &c., as belonging to physiology. In 1837, he established with Liebig the theory of the compound radicals, in which they regard *mineral chemistry* as including the various bodies resulting from the direct combination of elements properly so called, and *organic* chemistry as including the different chemical species formed by compound substances acting as elements.

In 1838, Mulder published his theory on the protein compounds. His formula of protein is $C_{40}H_{31}N_5O_{12}$, while that of Liebig is $C_{48}H_{36}N_6O_{17}$, and that of Dumas, $C_{48}H_{35}N_6O_{17}$. Mulder's researches have proved very useful, from their having shown the presence of sulphur as an important constituent of certain organic substances.

Between 1841 and 1842, Liebig published his work on 'Organic Chemistry applied to Animal Physiology and to Pathology.' Liebig examines more particularly the chemical phenomena which take place in the economy in connexion with the production of heat, digestion, secretion, and respiration. He considers especially the immediate principles in their dynamical condition, without having previously duly insisted upon their individual nature and properties. His work is essentially chemical, as is proved by that part of the book devoted to the study of the metamorphoses of organic tissues, in the chapter intituled 'Development of the Metamorphoses by means of Chemical Equations.'

The chemists of this period explain the formation of immediate principles by the metamorphosis of substances varying from the proteic radical by containing more or less oxygen, each proportion of oxygen taken up by the organism producing a proportional amount of heat. By the absorption of oxygen, the living parts are destroyed, and eliminated in the state of inorganic combinations; all the oxygen thus absorbed by the respiration is not entirely used to effect this metamorphosis, a part of it being employed to convert into a gaseous form certain substances no longer useful to the system. From this combustion is derived the heat peculiar to the living organism.

"There exists," observes Liebig, "an intimate connexion between the conditions necessary for the development of animal heat and those required for the production of mechanical phenomena; if there is an increase of the former, the latter will increase in a like proportion." Liebig considers animal life to be generated by the reciprocal action of two forces, one of which would produce an increase, or make up for the waste, the other would cause a decrease, or destruction of matter. The increase is effected by the vital power, while the destruction results from the chemical action of oxygen.

"The oxygen dissolved in the arterial blood, combining with the various principles incapable of resisting its chemical action, generates the temperature necessary for the production of the vital phenomena."

A morbid principle is a substance, or merely any mechanical cause, which destroys the equilibrium between the waste and the supply. Any diminution in the resistance of the living parts against the cause of waste becomes a want of power to resist the oxygen of the atmosphere.

The above are the facts more or less connected with the study of the immediate principles, contained in Liebig's work on organic chemistry applied to *physiology*. The same ideas are reproduced in his treatise on *organic chemistry;* but, however high our respect for Liebig, we are bound to say, that several distinguished chemists have expressed doubts as to the entire correctness of views, which, from their extreme simplicity, have naturally found a great number of ardent supporters. For the arguments adduced against them we must refer the reader to Messrs. Robin and Verdeil's treatise; our limited space unfortunately prevents us from dwelling at greater length on this part of our subject.

In 1841 and 1842, Lehmann published the first edition of his interesting work on 'Physiológical Chemistry.' This author, in his introduction, considers chemistry as a means of explaining physiological and pathological phenomena; and is quite opposed to the employment of chemical hypotheses in the study of organized bodies. We agree with Lehmann so far; but, unfortunately, he has not attained the object he had in view. The method he has employed explains this deficiency. He admits two kinds of chemistry, *mineral* and *organic;* and, moreover, a physiological chemistry which contains the science which he calls zoochemistry. He observes, "The fundamental principles of physiological chemistry must be searched for in general in organic chemistry. Zoochemistry is intimately connected with the science of physiological chemistry, and must be regarded as a most powerful means for its development." But in order to make zoochemistry the basis of physiological chemistry, each of its principles ought to be considered, not only in its exclusively chemical nature, but also in its general relations with the animal organism and its constituents. Accordingly, Lehmann admits an organic chemistry, or physiological chemistry, the basis of which is the science of zoochemistry; but where this organic or physiological chemistry begins, and where it ends, he is quite unable to determine. Thus he is unavoidably compelled to give an undue importance to the chemical point of view, and to adopt a general method very similar to that of the other authors who treat of this subject. Besides this, there is, in our opinion, a defective arrangement of the subject, as Lehmann does not describe, except for the inorganic principles, the constituents of the animal body when considered in the state in which they exist in the organism. Thus, for instance, *the lactates, the hippurates,* &c., are not described by the author as constituents of the body, but only as lactic, hippuric acids, &c.

When treating of the albuminous substances, Lehmann admits the protein theory, and describes several bodies which Robin and Verdeil do not consider as immediate principles. The third volume of his treatise, entitled *Histochemistry,* is devoted to the study of tissues, each chapter beginning by a short description of the several tissues, and then entering into their chemical examination, with its chemical study.

Between 1844 and 1851, Mulder published his essay on 'Physiological Chemistry.' He begins where Lehmann leaves off—namely, at the chemical molecular forces. Mulder's book is a treatise on general anatomy, in which the anatomical elements, the tissues, the immediate principles, and the humours, are described principally in a chemical point of view, and with a thorough knowledge of the importance of separating the groups of nitrogenized substances from the other constituents of the body.

In 1846, M. Dumas published his treatise of 'Chemistry applied to Physiology and Pathology.' Dumas admits the combustion of the substances assimilated by the body before they are eliminated from the organism. He believes that by that process the greatest portion of the fibrin, albumen, casein, gluten, gelatine, along with the fatty matters, are consumed, and that it is only the excess of those substances which is assimilated.

The last work which our authors mention in the historical part of their treatise, is the new Letters of Liebig, published in 1851. Liebig does not, however, refer in any way to the boundaries of the science he treats of, and his views are still essentially chemical. Since this time, other important works have been published, to which we shall not refer at present.

To give the reader an insight into Messrs. Robin and Verdeil's classification of the immediate principles, we have condensed the two last volumes of their treatise into the two synoptical tables which accompany this review.

VOL. II.

BOOK 2.—*Of the Immediate Principles considered individually.*

- *1st Class.* Mineral or Inorganic Immediate Principles.
  - I. General characters of the immediate principles.
  - II. Their characters considered according to sex, age, race, species, and morbid state.
  - III. Origin, formation, and exit from the system, of immediate principles of the 1st class.
  - IV. Functions of immediate principles of the 1st class in the organism.
    - *1st Division.*
      - I. Oxygen.
        - In the blood. In the lungs.
        - State in which it exists in the organism.
        - Pathological condition.
        - Exit of oxygen from the system.
        - Function of oxygen in the system.
      - II. Hydrogen.
      - III. Nitrogen.
        - In the blood.
        - In the lungs.
        - Its state in the system.
        - Origin and exit.
        - Functions.
      - IV. Carbonic acid.
        - In blood, lungs, intestines.
        - Its state and functions.
        - Extraction.
      - V. Carburetted hydrogen.
      - VI. Sulphuretted hydrogen.
      - VII. Hyposulphate of ammonia.
      - VIII. Water.
        - Volume and weight.
        - Origin and exit.
        - Function.
        - Extraction.
    - *2nd Division.* Mineral or inorganic.
      - Chloride of sodium; chloride of potassium, fluoride of calcium; hydrochlorate of ammonia; carbonate of ammonia; bicarbonate of ammonia; carbonate of lime; bicarbonate of lime; carbonate of magnesia; carbonate of soda; bicarbonate of soda, carbonate of potash; bicarbonate of potash; sulphate of soda; sulphate of potash; basic or neutral phosphate of lime; acid phosphate of lime; phosphate of magnesia; ammoniaco-magnesian phosphate; neutral phosphate of soda; acid phosphate of soda; phosphate of potash.
- *2nd Class.* Organic Immediate Principles.
  - I. Mathematical characters.
  - II. Physical characters.
  - III. Chemical characters.
  - IV. Organoleptical characters.
  - V. Variation of the immediate principles of that class, according to sex, age, race, species, and morbid condition.
  - Origin and conditions of the formation of the immediate principles of that class, and of their exit or destruction.
    - *1st Division.* Acid and saline principles.
      - Lactic acid; lactate of soda; lactate of potash; lactate of lime; oxalate of lime; uric acid; neutral urate of soda; acid urate of soda; urate of potash; urate of ammonia; urate of lime; urate of magnesia; hippuric acid; hippurate of soda, of potash, and of lime; inosate of potash; pneumic acid; glycocholate of soda; taurocholate of soda; hyocholate of soda; lythofellic acid.
    - *2nd Division.* Neutral principles and nitrogenized animal alkaloids.
      - Creatine.
      - Creatinine.
      - Urea.
      - Chlorosodate of urea.
      - Cystine.
      - Allantoine.
    - *3rd Division.* Saccharine and neutral non-azotized principles.
      - Sugar of liver.
      - Sugar of milk.

From a glance at the annexed table, it will be perceived that in this portion of the work the immediate principles are described individually, and with the utmost minuteness.

The method employed by the authors for the description of these substances is the following:

1. They consider the name of each immediate principle.
2. The terms synonymous with each name.
3. The definition of each principle.
4. They determine the place of each principle in the organism.
5. They state, in certain cases, the mass or volume of the immediate principle in relation to the volume of the body.
6. In several instances they mention the form which the principle assumes in the organism.
7. The period of its existence in the body.

These characters the authors term numerical or mathematical characters (*caractères d'ordre mathematique*).

8. They consider the gaseous, solid, semi-solid, or fluid state of each immediate principle.
9. Their weight, and also their comparative weight in relation to that of the body.

The above are the physical characters.

10. The properties of certain immediate principles may depend upon their being in a state of solution. Accordingly, the authors next examine the character of these substances in relation to this new condition.
11. The authors describe, as often as they consider it necessary, the nature of the chemical reactions of the immediate principles, in the economy, under the influence of physical agents, and when acted upon by chemical reagents.
12. They consider, in a few cases, the organoleptical characters, or the actions on our senses of certain immediate principles, such as that of common salt on the taste.
13. They describe the organic properties of each immediate principle, or the conditions upon which depend their physiological functions.
14. The variations of the immediate principle, according to sex, age, race, &c.
15. The physical and chemical phenomena which attend the entrance of the immediate principles into the body, and their exit.
16. The methods for the extraction of the various immediate principles are described with the greatest care and minuteness.
17. Lastly, a short historical notice accompanies the description of each immediate principle.

By this systematical method of proceeding, the authors are enabled to give a complete account of every immediate principle. In regard to the methods for their extraction, the authors not only describe with great precision the processes used by others, but also make us acquainted with several new methods, by which M. Verdeil was enabled to discover several principles hitherto unknown.

The second class of immediate principles, or the crystallizable organic principles, are next considered: the authors, according to their usual mode of proceeding, first enter into a few general remarks on the subject. They observe that the only organic character these principles possess is to concur in the formation of organized substances; they play only a secondary though indispensable part in their constitution. In regard to their formation, the greater number of these principles consist of the chemical elements of tissues, which require to assume that form for their elimination from the system. For this process, some are decomposed in the body, and pass into another state. In the embryo, for example, a part of the sugar contained in the liver is secreted by the kidneys as soon as it is formed, and is conveyed into the amniotic fluid, where it may be detected until the last period of embryonic life. (Bernard.) It also often happens that the above principles are eliminated from the body as soon as they are formed; and this may explain how it happens that they exist often in such small quantities as constituents of the urine.

In regard to the formation of the immediate principles of this class, in the system, no doubt they exert on each other a peculiar influence. This very mysterious action has been compared to the peculiar properties of spongy platinum, or ferments, which some authors have termed *catalytic* action. There are, however, certain chemical phenomena constantly going on in the organism which we are more able to judge of; as, when lactic, pneumic, uric, or hippuric acids seize upon the bases of certain saline principles of a mineral origin—a class of phenomena which, no doubt, greatly contribute to the production of animal heat.

The first division of the second class contains the *acid* and *saline organic immediate principles*, which are successively enumerated in the adjoining table. They are all of them salts or acids which burn generally without taking fire, and are insoluble or nearly insoluble in ether. These characters distinguish them from the fats, the fatty acids, and the soaps.

With the exception of lactic and pneumic acids, and inosate of potash, the amount of these principles is inconsiderable, except in the excrementitious secretions, such as urine, in the morbid products, and in the excremento-recrementitious secretions, as the bile. Their proportion in other parts of the body is very trifling.

The third division of the second class of immediate principles is devoted to the description of those that are saccharine or non-nitrogenized. A few observations on the subject will, perhaps, not be devoid of interest. The authors comprehend under this head the various immediate principles of the second class which take fire and burn with a flame, emitting a smell of burnt sugar (caramel). They are soluble in water, and possess the property of being converted by the presence of ferments, or nitrogenized substances, into lactic acid, or into alcohol and carbonic acid, according to the nature of the action which takes place.

Animals contain two kinds of sugars—the sugar of liver, or diabetic sugar, and the sugar of milk. Vegetables yield other kinds of sugar. *Grape sugar*, as far as we can tell from its chemical analysis, is perfectly analogous to that of the liver.

Sugar of the Liver.—This principle is a normal constituent of the liver, of the blood of the sub-hepatic veins, and of that part of the vena cava which is above the latter vessel, of the blood in the right side of the ···rt, and of the pulmonary arteries: none can be detected in the vena portæ, except when grape sugar is taken with the food; sugar is therefore secreted by the liver. M. Claude Bernard, to whom we owe this important discovery, found that during the period of digestion the liver produces a much larger amount of sugar than during the period of fasting; so that the arterial blood of an animal in full digestion yielded sugar, though none could be detected in the arterial system when the animal was fasting. Sugar is also found in the amniotic fluid and in the allantoine of the cow of four or five months old, and in those of the sheep from six weeks to two months old. It is sometimes wanting during the last weeks of the intra-uterine life. Thus, Claude Bernard failed to detect sugar in the amniotic fluid of the fœtuses of cows from six months and a half to seven months old; although he traced the existence of this principle in their urine.

In certain morbid states, as in the disease called *diabetes*, large quantities of sugar are found in the urine. Claude Bernard has also detected it in the saliva, the kidneys, the serosity of the pericardium, and in the spermatic fluid of a diabetic dog, but failed in extracting it from the substance of the central nervous system, the pancreas, and the spleen. The serosity contained in blisters and in vomited matters, according to M. Bernard, yielded sugar. Some sugar was found also in the acid perspiration of a diabetic patient.

We have no precise data in regard to the amount of sugar contained in the body; it must be very considerable in man, birds, dogs, pigs, horses, and rabbits. There is much less in the reptilia, and Bernard did not find any in the liver of the eel and of the snake. It exists in general in the body in a free state, but may also be combined with common salt.

The glucose, or sugar of the body, may either be conveyed into the system ready formed, or else be derived from the following sources:

1st. The various principles of the body may themselves yield sugar; it is not yet known whether these principles are organic or fatty. Bernard has shown that the livers of dogs or cats fed for four, five, or six months upon nothing but meat, bones, and the fatty matter which this food contained, yielded sugar, whilst none was found in the portal vein of these animals.

2nd. The cane sugar entering the portal vein by endomosis, disappears, and is transformed into grape sugar in the liver.

3rd. Glucose, or grape sugar, may enter the liver when taken into the body as food; in that case only can its presence be detected in the portal vein. Traces of glucose also evidently pass into the blood during the digestion of cooked amylaceous substances, for the chyme from the stomach to the cæcum always contains traces of that substance. A considerable part of the amylaceous matters taken up with the food pass in the intestines into the state of dextrine, which is probably one of the materials from which the sugar is elaborated by the liver.

Bernard has shown that the secretion of sugar by the liver is under the direct control of the nervous system. If the pneumogastric nerves be cut in the region of the neck, the production of sugar very soon ceases. If the extremity of the nerves connected with the lungs and liver be artificially irritated, no change occurs; but if the upper extremity be excited, the consequence is a reflex action through the spinal cord, which, following the spinal nerves, reacts upon the liver and again causes the production of sugar. This influence may be compared with the irritation in the lungs transmitted by the pneumogastric nerves—during the inspiration of chloroform, gas, or ether, for instance—when the amount of sugar produced by the liver is sensibly increased.

If, instead of irritating the lungs, we puncture the fourth ventricle of the brain or the olivary bodies, an increased action of the liver takes place, producing an excess of sugar, which is excreted by the kidneys. This excessive production of sugar, which can be obtained by an artificial irritation of the lungs, or of the superior extremity of the pneumogastric nerves when cut, or, finally, by the puncturing of the fourth ventricle of the brain and olivary bodies, may also be observed to occur in the human body under certain morbid conditions.

Accordingly Messrs. Robin and Verdeil consider diabetes to be owing to various affections of the lungs, or perhaps also to some disease of the medulla oblongata.

Some experiments very recently performed by Dr. Harley, at Paris, and communicated to the Société de Biologie, appear to show that the nervous reflex action, upon which depends the secretion of sugar by the liver, is generated in the liver itself, by the stimulating power of the blood of the portal vein upon the hepatic branches of the pneumogastric nerves. Dr. Harley increased the exciitng influence of the blood of the vena portæ by injecting into that vein alcohol, sulphuric ether, chloroform, or ammonia, and observed, that two or three hours after the operation, the dogs experimented upon voided sugar in their urine, and remained diabetic from two or three hours to two or three days. The presence of sugar in the urine of those animals was detected by means of the double tartrate of potash and copper, and also by fermentation, so that no doubt exists as to Dr. Harley's results.

These experiments throw a new light upon the pathology of diabetes, as in a great number of cases this disease is brought on by excesses in alcoholic liquors; its nature is, however, so complex, that no serious attempt can be made to account for all its symptoms, until we have become more thoroughly acquainted with the nature and action of the intestinal secretions.

The sugar constantly produced in the organism by the liver is destroyed by the lungs, its amount in the left side of the heart being hardly perceptible, except during the height of digestion. Our authors conclude, from Bouchardat's experiments, that the sugar is transformed into lactic acid. We have ourselves observed that it is easily destroyed by a current of chlorine gas, or by heating its solution with acid phosphate of soda procured from human urine by direct crystallization. This circumstance may tend to throw some light upon the disappearance of the sugar from the action of the lungs, without having recourse to a process of oxidation. We hope the experiments we are now making on this part of the subject may lead to some useful results.

In order to show the presence of sugar in the liver, it is necessary to make a decoction of the tissue of that organ, previously minced with care. This solution, filtered through calico, precipitates the reduced oxide of copper from the double tartrate of potash and of copper, and readily undergoes fermentation. The amount of sugar is quite sufficient for its detection by M. Soleil's saccharimeter. When, on one occasion, we were endeavouring to discolour the solution, by means of a current of chlorine gas, for the purpose of examining it through that instrument, we observed that the sugar had completely disappeared, and that the solution, now become perfectly colourless, no longer acted upon the polariscope, and did not reduce the oxide of copper of Bernard's and Barreswil's fluid. The above is by far the best test for ascertaining the presence of sugar in the human body. Another method for detecting grape sugar in the dissolved state is described by M. Maumené, who moistens a piece of flannel or calico with a solution of chloride of tin, and dries it. If, in this state, it be dipped in a solution containing the slightest trace of sugar, again dried, and subsequently heated over a piece of red-hot charcoal, it immediately turns to a reddish brown, without charring, the colour being more or less intense according to the amount of sugar present. We have found this test of great value in a multitude of cases where the urine of dogs was the subject of our experiments.

In regard to the best mode for ascertaining the quantity of sugar contained in a solution, we must not forget to mention the *polariscope* or *saccharimeter*, which enables the observer, day by day, to follow the variations in the quantity of sugar contained in diabetic urine. The instrument is easy to manage after a little practice, and by means of animal charcoal the urine can always be obtained sufficiently clear for the experiment.

VOL. III.

BOOK 2 (*continued*).

2nd Class (continued). *4th Division.* Fatty Principles, or Fatty Acids and Soaps.	I. Mathematical characters of fatty immediate principles of the animal economy. II. Physical characters of the fatty principles. III. Chemical characters, &c. IV. Organoleptical characters. V. Organic characters. Variation of fatty principles according to age, sex, &c. Origin, formation, and exit of fatty immediate principles.	Cholesterine. Seroline. Oleic acid. Margaric acid. Stearic acid. Oleate of soda. Margarate of soda. Stearate of soda. Caproate of potash, soda, and other alkaline salts of volatile fatty acids. Oleine. Margarine. Stearine. Elaierine. Steaerine. Cetine.

3rd Class. Of the Organic Substances, or Vegetable Immediate Principles.	I. Mathematical characters. II. Physical characters. III. Chemical characters. IV. Organoleptical characters. V. Organic characters. Their variation according to age, sex, &c. Origin, formation, and destruction of the immediate principles of that class. Their functions in the organism.	*1st Division.* Organic substances.	Fibrin. Albumen. Albuminose. Casein. Pancreatine. Muscosine. Ptyaline.
		2nd Division. Solid or Semi-solid Organic substances.	Globuline. Crystalline. Muculine. Elasticine. Cartilageine. Ostein. Keratine.
		3rd Division. Organic Colouring matters.	Hematine. Biliverdine. Melanine. Urrosacine.

BOOK 3.—*The Accidental Immediate Principles.*

BOOK 4.—*Principles imperfectly determined, or doubtful, and Substances not Immediate Principles.*

1st Section. Immediate Principles existing probably or certainly, but not perfectly determined.		
	Probable Principles of the 1st Class	Silica.
	Probable Principles of the 2nd Class	Acetate of soda. Leucine. Peculiar salts of dog's urine. Xanthine. Hypoxanthine. Lienine. First peculiar acid of human urine. Second peculiar acid of human urine. Hæmatoidine. Butyrine. Caprine, or Caprinim—Caproine, or Capronine—Capryline. Butyroleine, or Butrelaine. Hyrcine. Phocine. Phosphuretted fats of cerebral substance. Cerebric acid, or cerebrate of soda.
	Probable Principles of the 3rd Class	Neurine. Synovine. Lacrymine. Spermatine. Organic substances peculiar to dropsical fluids. Paralbumine. Pyine.

2nd Section.—Definite chemical compounds whose existence as immediate principles is doubtful.

3rd Section.—A few immediate principles, chemical elements, or simple bodies, whose actual combined state is unknown or commonly overlooked.

4th Section.—Natural and artificial chemical compounds which are not immediate principles.

5th Section.—Substances which have erroneously been called immediate principles, being merely mixtures, or products of decomposition, or even neither chemical compounds nor mixtures.

By referring to the annexed tables, it will be observed that the first part of the third volume is devoted to the description of the fatty immediate principles. These the authors define "neutral, acid, or saline substances, soluble in ether and alcohol, insoluble, or very sparingly soluble, in water, and burning with a flame evolving carbon free from ammonia or other nitrogenized products."

Our authors next describe the condition in which the fatty matters exist as immediate principles in the various parts of the body, and enter into a minute microscopical examination of the fatty globules contained in milk. These globules are observed to be perfectly spherical, when derived from animals which yield a soft butter, as in the case of human milk; they are, on the contrary, in general polyphedral in cow's milk, the butter of which is of a more solid consistence. These globules are semisolid, or nearly solid, in cow's milk, which is to be expected, as they contain 68 per cent. of margarine, 30 per cent. of oleine, and 2 per cent. of butyrine. According to our authors, the formation of butter depends merely upon the aggregation of these globules. They have previously been supposed to be surrounded by a peculiar membrane or envelope—but

Messrs. Robin and Verdeil consider this as a mistake, on account of the peculiar appearance of the spot caused by the pressing of fatty matters between two plates of glass.

Globules of fat are found in many other fluids, as in the prostatic and spermatic secretions. Their number is more considerable in the former than in the latter. Saliva, the synovial secretion, the mucus from the nasal passages, and the bile, also contain fatty principles which assume the form of spherical globules. Normal urine, and especially the morbid secretion, also occasionally contains globules of fat, presenting a soft fluid consistence, and a yellow colour deeper than that of milk. M. Rayer has observed, that if the urine be left undisturbed, these oily globules will rise to the surface with the ammoniaco-magnesian phosphates and the urates, which salts may be detected under the microscope. In some cases urine contains a sufficient amount of fatty matters to yield a considerable quantity of them to ether; the liquid then assumes a milky, opalescent nature, similar to that of chyle, and from thence its name of *chylous* urine.

This phenomenon has repeatedly been observed in hot countries, where urine generally contains also globules of blood and albumen. On two occasions the blood from a subject passing chylous urine was white. The addition of acetic acid to urine in this morbid state has never detected the presence of casein. The cases in which large drops of oil have been seen floating upon urine are very rare, and have only occurred once or twice after death arising from the fumes of burning charcoal; in these cases the blood found in the head, trunk, and extremities, contained masses of fluid fat.

The corpus luteum yields a very large proportion of fatty matters. The internal membrane of the Graafian vesicle contains, in its normal state, a few fatty granules; soon after the displacement of the ovum these granules increase in number and in volume, and assume the appearance of drops of oil. These are mingled with the amorphous, transparent, granular substance of the internal membrane. This circumstance prevents the minute drops of oil from congregating together, but as soon as they are pressed under the microscope glasses they escape and run into large drops.

The crystalline lens also yields a fluid substance of a pale rose-colour, soluble in ether, and which presents apparently the character of a fluid fat; but its precise nature has not yet been investigated.

In regard to the formation of the fat in the body, it may result either from the fatty ingesta, from a metamorphosis of the saccharine or amylaceous food, or from the nitrogenized food. We are unable to explain by what process nitrogenized ingesta can yield fat; the phenomenon is probably similar to that which accompanies the production of sugar in the liver, from nitrogenized food. In this case the conversion appears to take place in the liver; this, at least, is the opinion advocated by M. Claude Bernard, in the 'Gazette Médicale' of 1849. Liebig expresses a similar idea, but adds that this opinion still requires to be justified by experiment.

With respect to the extraction of the fatty principles from the tissues or solid parts of the animal body, the substance, previously minced, must be triturated in boiling water. The fatty matters will float on the surface, and collect into a hard mass on cooling. This mass is generally formed of stearine, margarine, and oleine. To separate these three principles, a sample of the fat is dissolved in boiling absolute alcohol; on cooling, crystals of stearine will appear first, and a few minutes afterwards the margarine also crystallizes; by means of the microscope we can readily distinguish these two substances one from the other. The oleine does not crystallize, but adheres to the crystals of stearine or of margarine, and may be obtained by pressing the crystalline mass in filtering paper, and afterwards treating that paper with ether.

In many cases this method of analysis, when used as a test, will prove sufficient. It is often, however, more convenient to transform the fats into soap by means of potash, and to decompose this soap by hydrochloric or sulphuric acid, when the fatty acids will float on the surface of the liquid, and solidify on cooling. This mass, dissolved in boiling alcohol, will yield crystals of stearic or margaric acid, which may be more easily recognised than stearine and margarine. We have often had opportunities of witnessing the crystallization of the neutral fats and their fatty acids, and have repeatedly observed the difficulty of obtaining them when they are mixed with a large proportion of oleine or of oleic acid. In certain cases, moreover, it is of the utmost difficulty to distinguish under the microscope stearic from margaric acid, and then we are obliged to have recourse to their fusing point: stearic acid fuses at 75°, and margaric acid at 56° Cent. A mixture of the two, fuses at temperatures proportionate to the amount of each acid present. Gottlieb has given a table of the fusibility of these mixtures, which will probably turn out to be of great practical use.

In order to extract fatty matters dissolved in animal liquids, the residue from the fluid, evaporated to dryness over the water-bath, may be treated with ether, and this solution mixed with alcohol, or concentrated at the temperature of the atmosphere, to induce the crystallization of the fats. This method, however, fails in every case where the fat is mixed with other principles also soluble in alcohol and ether. To obviate this difficulty, it is advisable to treat the solution with lime or sulphate of lime, which precipitates the fats, in some cases by an apparently mechanical action, and in others, by the formation of a soap of lime. This lime precipitate, collected on a filter, then washed and dried, will yield to ether or alcohol the fatty substances it contains. When a soap of lime is formed, it must be treated first with a mineral acid and then with ether; it is by this mode that we have ourselves succeeded in detecting the presence of free fatty acids in the blood.

Much more might be added on the study and extraction of the fatty animal matters, were it not that the limits of this notice prevent us from dwelling at greater length upon this part of the subject.

On the immediate principles of the third class.—Organic substances, or coagulable principles.

These substances the authors define:

"Fluid bodies, having the property of coagulating by heat at about 50° or 75° Cent., and also by the action of reagents; or semi-solid and solid bodies susceptible of softening, not crystallizable or volatile, or without undergoing decomposition; of an indefinite or undetermined elementary chemical composition; burning with little flame; evolving ammoniacal empyreumatic products having a sour smell; and finally, leaving a bulky, bright, porous charcoal, difficult to incinerate."

By referring to the adjoining table, the reader will at once make himself acquainted with the principles of this class. From the definition of these immediate principles, in which the authors avoid alluding to proteic compounds, we infer that they are not advocates of the proteic theory. This theory has been so much discussed of late, that we do not deem it necessary to enlarge upon it at present; the more so that recent investigations upon the albuminous compounds tend to show that it is no longer tenable.

Among the various properties of albumen, there are one or two more peculiar than the rest, which we shall now proceed to notice. The way in which albumen gelatinizes with strong acetic acid has been thoroughly investigated by Lieberkühn. An interesting reaction with acetic acid is the following. If white of egg be mixed with twice its volume of distilled water, and filtered through calico, in order to separate the precipitated mucus, a clear fluid is obtained, which does not coagulate on the application of heat, but merely turns opalescent. By the addition of acetic acid to the clear solution of albumen, no change occurs; but if a drop of that acid be added to some of the fluid after it has been boiled, coagulation instantly ensues. An excess of acetic acid redissolves the coagulum. Dr. E. A. Parkes has made the following observation on the coagulating property of albuminous fluids—viz., that if a solution of albumen in water be boiled with acetic acid, no change occurs; but as soon as a solution of common salt is added to the mixture, the liquid coagulates, and an excess of salt does not redissolve the coagulum. We have frequently repeated this experiment, and have found it an excellent method for testing the presence of albumen in albuminous urine. On mixing a very small quantity of albumen of the egg with healthy urine, we have also satisfactorily detected this substance by the above-mentioned test.

Coagulated albumen is insoluble in distilled water, either cold or hot. It dissolves, however, in that liquid, if its temperature be raised to a sufficient height above the boiling point in a closed tube, with a proportional increase of pressure. The experiment is made in the following manner :—Heat is applied by means of an alcohol or gas lamp, placed under a copper cylindrical air-bath, to which is adapted a movable cover, in which are two round apertures, one for a thermometer, and the other to allow for the expansion of the air. Under the cover of this air-bath are one or two hooks, to which is suspended an hermetically-sealed glass tube containing a mixture of coagulated albumen and water. Care must be taken that the tube does not touch the sides of the cylinder. The temperature of this case is to be raised to between 150° and 200° Centigrade, and at the end of four or five hours the albumen contained in the tube will be found entirely dissolved. In this state it has lost its property of coagulating by heat, though it is still precipitated by reagents. We are quite at a loss to explain the rationale of this experiment.

The third class of immediate principles includes the colouring or coloured organic substances.*

We have already noticed, in a preceding number of this journal, Dr. Harley's mode of extracting the colouring matter of urine, and shall proceed at once to describe the method employed by M. Verdeil for the

* See the Tables.

extraction of *hæmatine*, or the colouring principle of blood. This fluid is first coagulated by heat, and the pressed coagulum afterwards boiled with alcohol, and mixed with a few drops of carbonate of soda, for the purpose of increasing its alkaline reaction. The alcohol, which has assumed an intense red colour, is then filtered, mixed with milk of lime, or pounded lime, and again boiled until it has become entirely discoloured. The precipitate, which has now acquired a green colour, is collected on a filter, in order to be treated with hydrochloric acid. The result of the operation is a thick red mass, which is dried on a filter, and introduced into a glass flask. This substance, treated with a little ether, yields to this fluid its fats, together with a little colouring matter. When entirely free from fat, the colouring matter is dissolved by boiling alcohol, and as soon as this alcohol has become cold, it is mixed with ether. Various substances dissolved in the alcohol are now precipitated, and the filtered mixture of alcohol and ether contains the pure colouring matter of the blood. This is distilled and mixed with a certain amount of water, when the colouring matter precipitates in the form of a brownish-black powder, which must be thoroughly washed with water.

The hæmatine obtained by this process is perfectly free from fat, and entirely soluble in ether and in boiling alcohol; it differs therefore from that obtained by Lecanu by another process, and which he found to be insoluble in alcohol and in ether.

M. Verdeil employs a similar method for extracting the colouring matter of the bile, or *biliverdine*. Messrs. Verdeil and Harley have obtained *melanine*, or black pigment, from a melanotic tumour by the action of dilute aqua potassæ, a black powder being left behind which yielded by incineration more than 1 per cent. of oxide of iron mixed with traces of other salts. Indeed, M. Verdeil has observed that colouring matters are constantly combined with iron. Their state in this combination must be very peculiar, as they become entirely soluble in ether.

The latter part of the third volume is devoted to the description of the accidental immediate principles, and those of doubtful existence. It opens to the investigator a wide field of inquiry, which, carefully followed up, can hardly fail leading to interesting discoveries. This part of the treatise is moreover valuable, as it shows the mistakes fallen into by many chemists, who have too hastily and prematurely assigned the rank of immediate principles to substances which a more thorough investigation has proved to have no claim whatever to be considered as belonging to this important portion of organized matter.

William Marcet.

Review VII.

1. *De la Prostitution dans la Ville d'Alger depuis la Conquête.* Par E. A. Duchesne, Chevalier de la Légion d'Honneur, Docteur en Médecine, &c.—*Paris,* 1853. pp. 231.

Prostitution in the City of Algiers since the Conquest. By E. A. Duchesne.

2. *Die Berliner Syphilisfrage.* Von Dr. S. Neumann, Vorsitzendem des arztlichen Comités des Berliner Gesundheitspflegevereins.—*Berlin,* 1852. pp. 74.

The Berlin Syphilis Question. By Dr. S. Neumann.

3. *Die Prostitution in Berlin, u. s. w.* Von Dr. Fr. J. Behrend.—*Erlangen,* 1850. 8vo.

Prostitution in Berlin, &c. By Dr. Fr. J. Behrend.

Many, on turning over these pages, we feel assured, will ask, to what purpose so lengthy a review of a subject rarely brought before the British medical public? Why occupy so much space with an analysis of works, the chief interest of which may appear to be local? We feel, then, in some degree called on to preface this article with a declaration of the opinion, that few subjects have such intimate moral and physical relations with the well-being of man, and demand more earnestly the attention of the physician and the statesman.

Prostitution appears to have existed from time immemorial, and has had for many centuries a disease connected with it, whose baneful effects are not limited to an individual, to a family, or to a generation; effects which are readily propagated, widely spread, yet with such difficulty eradicated, that they are too often the indirect causes of destroyed physical and mental health, of premature death, of lunacy, idiocy, and suicide.

There are but few diseases which have passed through so searching an investigation, or whose treatment has formed the subject of so much debate, yet to the possibility of the prevention of which, attention has been in this country so little directed. Some unaccountable oversight or equally inexplicable dislike, has, with rare exceptions, caused even medical men to avoid dealing publicly and boldly with this subject. The necessity of removal of physical nuisances has with some difficulty been forced on our attention; while moral filth is allowed to reproduce itself to such a degree, that its progress must be checked, or generation after generation will suffer for our apathy.

The practice of prostitution both produces and propagates disease, and it is on this plea we solicit the attention of the profession to the subject of this review. We endeavour neither to excuse nor defend prostitution, but as to our profession is entrusted the duty of attending to the physical well-being of man, we are obliged to take man as we find him, and endeavour to obviate, as best we may, the effects of a cause that theologians and legislators have failed to remove. We therefore purpose using the space allotted to us, in discussing the justifiability, and demonstrating the means used on the Continent for limiting the propagation of syphilis.

In order to limit the extension of syphilis, we must either prevent illicit intercourse, or use means for immediately discovering when the disease exists. There are but few whose ignorance of the organization of man is so great as to permit them to indulge in the utopian belief, that it is possible, by Acts of Parliament, to prevent the illicit intercourse of the sexes. However, the experiment has been tried. In Denmark, for example, such enactments as the following have been made:—[In the Danish Code, under Chris. V., Book vi., Chap. 13, Art. 30.] Any males found in brothels, &c., are ordered, for the first offence, to be punished with eight days' imprisonment; for the second offence, with double that punishment. Any woman found in a brothel is ordered to be flogged, or confined in the Spinning-house. By Art. 5, it is enacted that the owner or keeper of a brothel shall be flogged, and sent out of the province in which he resides. By magisterial proclamation, dated Copenhagen, 27th July, 1728, it was enacted, "that no soldier or sailor should have in his house or service an unmarried woman;" and by royal order of 23rd November, 1725, no publican was allowed to have more than one female at his bar, and she must have attained the age of twenty-four years. These, and a long list of other enactments, of so stringent a character, that they must have interfered with the liberty of not only women suspected of prostitution, but also the community generally, were enforced by order dated 4th April, 1809, and again in August 29th, 1829. Is Copenhagen, then, a model moral city?—does prostitution exist in the kingdom of Denmark? It not only exists, but with the foregoing regulations still on its statute books, prostitution is legally tolerated, and the extension of syphilis is provided against in a manner that will be hereafter shown.

But let it be for a moment supposed that it were possible to enact and enforce measures that would effectually prevent public or open prostitution; as a sequence, private or clandestine prostitution, a far more demoralizing evil, would be proportionately increased. All who have thought seriously on our social system must have been surprised to find how large a mass of illegalities or of immoralities are overlooked, in order to keep society together. Man's weaknesses or faults cease to be seriously dangerous to society as soon as they are known. The more secretly crime is accomplished, the greater the danger; and any enactment that tends to necessitate secrecy, endangers society more than noon-day crime.

With the admission, then, that it is impracticable, and perhaps even not desirable, to prevent public prostitution, it appears to us to be the duty of the profession to recommend, and of the government to legalize, measures whereby this necessary evil can be kept within certain limits, and by which the origin of disease can be discovered, and its propagation prevented. Disease of this class cannot be detected without an examination, which cannot be enforced without a control over those whose habits dispose them to infection; and to carry a control into effect, a registration of prostitutes is necessary. It may be objected, that the legislature cannot interfere with the personal liberty of any subject, so long as the individual does not act contrary to the laws of the realm, and thereby endanger the well-being of society. In this, as in many other cases, society may be endangered without the breach of any existing statute; were it not so, our judicial system would have arrived at perfection; and if it be admitted that prostitution belongs to this category, there can be no doubt of the necessity for an enactment against it. The details of a control over prostitution need not form the subject of separate articles of a bill, any more than the Commissioners of Sewers require a new clause for each clearage. The object would be completely accomplished by its being enacted that prostitution, meaning thereby the demanding or receiving of money for sexual intercourse, is a criminal act; and that as a punishment, the individual shall be placed under the control and *surveillance* of a commission, and the commission be authorized to make such arrangements as may be considered necessary for the public safety. It would be premature to enter more fully into a description of the system of control that might be adopted in this country; suffice it that we can, when called on, bring forward a plan whereby the civil liberty of the woman may in a great degree be preserved, and the public be at the same time protected.

Finally, taking our stand on the high principle, that whatever is likely to conduce to the happiness of man deserves to be heard by man, we enter on our description of what has been done on the Continent to prevent the propagation of syphilis.

The control of Prostitution in Berlin.—Under the chief of police and by the advice of Dr. Behrend, there has been formed a commission for moral police (*die Kommission für Sitten-Polizei*), consisting of—1st, The chief of police as president; 2nd, The medical counsellor of the central police board; 3rd, The chief physician for sanitary police; 4th, The chief physician for moral and humane police, under whose immediate direction comes whatever relates to prostitution, &c.; 5th, Ten physicians, to each of whom a part of the city is assigned, whose duties are to examine the women living in the brothels in their districts, and to attend in rotation every day for two hours at the office of the commission, in order to examine the women who present themselves; they are also called by the police to all cases of severe accidents, violent deaths, &c., and attend the police gratis; 6th, Four surgeons to assist the physicians.

This commission has divided prostitution in Berlin into two kinds, the tolerated or public, the non-tolerated or secret, and to the first of these we will now direct attention.

Tolerated or Public Prostitution.—Whenever any one desires to open a house for the reception of prostitutes, application is made at the office of the commission for a copy of the 'Request,' which is to be filled up, signed, and returned to the office; and as this document contains some of the most important regulations, we give a complete translation of it:*

"I request from Commission for Moral Police, permission to let in No. —, in — street, furnished rooms to women who live by prostitution. If this request be granted, I hereby bind myself to fulfil the following conditions:

"1. I shall consider this permission as a concession which the commission can at any moment withdraw or modify, without my having the right to inquire their reasons for so doing.

"2. I will not admit any woman into this house without having received, for her in particular, the official form of permission from the commission; nor will I

* These and following regulations are not admitted into general circulation, and we are indebted to the kindness of Dr. Behrend for copies of them.

allow any other persons excepting the women for whom I have received such permission to live therein; and if I act otherwise I shall pay to the commission 5*l*.

"3. I promise not to have any other than women servants, and not to employ as a servant any one who has not attained forty years of age, under a fine of 7*l*. 10*s*.

"4. I promise not to allow any woman or any man under twenty years of age to enter this house, under a fine of 7*l*. 10*s*.

"5. In the aforesaid house there shall be no noise or tumult whereby the neighbourhood may be inconvenienced; if I have given rise to such noise, or if it appears that, in the event of its being caused by others, I did not do everything in my power to prevent the same, I shall pay a fine of from 15*s*. to 15*l*., besides remunerating in full for all damages that may have been made during the tumult.

"6. I promise not to keep any spirituous drinks in this house, nor to allow any to be brought into it, nor to suffer any dancing or music therein, under a fine of from 15*s*. to 7*l*. 10*s*.

"7. I promise that the street door shall be kept shut during the day and night, and if it be at any time found open I will pay a fine of 15*s*. to 30*s*.

"8. I promise that the windows shall be left and retained in the condition which is ordered and approved of by the commission; and I will pay a fine of from 15*s*. to 30*s*. for every arbitrary alteration or neglect of these arrangements.

"9. I promise not to make any alteration in the interior or exterior of this house, without previously acquainting the commission and obtaining their permission to make it, under a fine of 15*s*. to 7*l*. 10*s*.

"10. I promise that none of the women who live in this house shall appear at the street-door, nor in any public garden or other place of public amusement, nor in any dancing-rooms nor public walk; and if one or more of them are seen in any of these places, whether they be there with or without my knowledge, I will pay a fine of 15*s*. to 30*s*.

"11. I promise that none of these women shall go on a journey out of the city, or on any party of pleasure, without having previously obtained the permission of the commission, and its being made as they direct; under a fine of 15*s*. to 7*l*. 10*s*.

"12. I engage, out of the agreement that is made between me and these women, to provide them with lodging, board, attendance, and clothing, all of which shall be subject to the inspection of the commission, whom I will inform of all changes made in these respects; under a fine of 15*s*. to 60*s*.

"13. I promise to have a list of prices printed, a copy of which I will give the commission; and in case of my demanding or receiving more than is therein stated, I will pay a fine of 15*s*. to 60*s*.

"14. I will not allow any of these women to incur debt for more than three pounds, under a fine of 30*s*. to 60*s*.

"15. I promise not to use any bodily punishment with these women, nor to confine nor use any violence towards them, under a fine of from 15*s*. to 7*l*. 10*s*.

"16. I promise not to allow any one to enter this house from one o'clock at midnight until the morning, under a fine of 15*s*. to 60s.

"17. I promise that the women shall live in all respects with, and have every right contained in, the 'Book of Regulations;'* that they shall preserve the greatest personal cleanliness, and if any of them become sick, I will immediately inform the attending physician, as well as the commission; I will especially direct my attention to the discovery of syphilitic disease and of scabies in these women; and, should either come to my knowledge, I will immediately inform the attending physician and the commission; further, I will not in such a case allow any one to visit the woman until she be examined by the physician or removed to an hospital. For any transgression of these points I will pay a fine of from 15*s*. to 15*l*., in addition to which, I will defray the expenses of any one who may have thereby become diseased.

"18. I shall inform the commission if any of these women become pregnant; and if I omit to do so, I will pay a fine of from 15*l*. to 30*l*.

* See p. 119 for these regulations.

"19. I promise that the examination of the women and of the house can be made at any hour of the day or night by the commission, the attending physician, or police officers; that I will in every way facilitate the making of these examinations, and provide for the physician the prescribed instruments, vessels, &c. For every omission, or even neglect in these respects, I will pay a fine of 15*s*. to 60*s*.

"20. I promise to obtain from each of the women living in this house, with the exception of servants, from six to nine shillings per month,* and pay the amount half-yearly to the chief fund of the police; should any woman refuse or neglect to pay this monthly subscription, I engage to pay the same, considering her as my debtor.

"21. This is to the effect, that the monthly subscription entitles the women, when affected with syphilis, to free treatment and support in hospital, and that the owner of the house has no claim on this money.

"22. I promise that, in case any of these women are ill of any other than venereal disease, if they become pregnant, &c., I will provide them with medical attendance and support, or the commission can deduct the expenses from the security money.

"23. On the granting of this request I will pay once and for ever, to the chief fund of the police, the sum of fifteen pounds, and will not under any circumstances demand that this money be returned to me; with this one exception, that, within a half-year from the granting of this request, I be obliged from unforeseen and unavoidable circumstances to give up the permission.

"24. In order to secure the payment of the fines, I promise, within three days from the granting of this request, to deposit in the chief fund of the police the sum of forty-five pounds, as security which is to be returned on the conditions contained in clause number 23, or in the event of my giving up this house and acting towards the women as directed, of which I will give the commission at least three weeks' notice. For this I shall not seek to have this 45*l*. returned to me, if I retain one or more of these women, and for them I shall submit to the regulations of the commission.

"25. All the above-mentioned fines &c. are completely independent of the legal punishments for offences and crimes; I am amenable to the common laws against secret prostitution, against public prostitution, imposition, secret delivery, the production of abortion, &c.; and should I, for any offence or crime, suffer legal punishment, I shall consider it as just, if the commission withdraw their permission. Further, if I thrice wilfully break the regulations of this contract, or act in direct opposition to the orders of the commission, they have the right not only to withdraw this permission, but I hereby forfeit all claim to the security money, which is, in that case, to be used for the purposes of inspection and cure.

"26. I promise to submit to the opinion of the commission on all points connected with this contract, and in case that I consider myself aggrieved by that decision, I submit to the jurisdiction of the chief of police, whose judgment shall be final; if after that I have recourse to the civil law, I thereby lose the right of retaining the permission.

"27. The commission has the right of receiving all fines incurred under the regulations of this contract, without having recourse to the usual forms of law; and I engage to raise the security money to its original amount within three days after it has been reduced by the deduction from it of the fines.

"Finally, in the event of my failing to fulfil the last condition, I hereby forfeit all claim to the forty-five pounds security."

It may not be out of place to offer here a few remarks on this form of the request. It will be observed, that these conditions are offered to the commission, for though the form has been drawn up under their direction, still it is a proposition offered to and not emanating from them, which, if acceded to, they can at any moment withdraw, and thereby close

* This sum is not stated in the original, but it varies between the amounts given above.

the house; while the 4th clause prevents boys or very young girls from being admitted. The 6th clause is not put in force, as beer is sold in all these houses, and there is but little difficulty in obtaining brandy and other spirits; still it is a useful regulation, as it enables the commission to punish the owner of the house if any one is proved to have become drunk on his premises; the restriction regarding dancing and music is never enforced. The regulation respecting the closing of the street door, clause 7th, is so strictly acted up to, that it is at all times necessary to ring to obtain admission, and on entering, it is immediately closed, as it matters not by whom it has been left open or even unlatched, if found so the owner is fined, and it is one of the duties of the police to enforce this regulation. With respect to the windows, clause 8th, the lower sash is firmly screwed into the frame so that it cannot be opened, while a wire-gauze screen extends half-way up the window, and renders it impossible for those within to be seen from the outside; the upper sash can be opened to ventilate the room, and the shutters are closed when there are lights within. The value of these and following regulations must be apparent; we therefore pass on to the 14th clause, in which the owner of the house promises not to allow any of these women to incur debts above three pounds, but, unfortunately, the small fine of 30*s.* to 60*s.* completely fails to ensure the fulfilment of this promise.

It may appear strange, that we should attach the greatest importance to the most stringent enforcement of this promise, for the public, who generally consider, or, at least, only see the one great fault in these women, often conclude, that as they are understood to have lost their character they must as a consequence have no character, except it be a bad one. Some fear, and others wish not, to look deeper than their external demeanour, lest perchance they may discover that these women are not altogether prostitute, while the majority, even of our own profession, find it much easier to allow a part of their fellow-creatures to live and sicken and die under the unrelenting scowl of society, than to study a question which society appears to have proscribed. For our part, we fully anticipate, and are prepared to meet, the opposition that any attempt to establish a system of control and protection for prostitutes will excite; but if we can prove by statistics and probabilities, that it has been, and will be attended with benefit, opposition in this as in every other discussion that has truth for its basis, will only tend to excite argument, which will, we confidently believe, establish the necessity of the system, and inevitably ensure it support.

While visiting these houses, with our kind and truly humane friend Dr. Behrend, we have asked these women why they do not seek some honourable occupation, as they can at any moment leave the house, no matter how heavily they may be in debt? [see regulation No. 1, page 121,] and the invariable answer was, "I must first pay my debts." But the debts increase, and prostitutes do not understand insolvent debtors' courts, and therefore languish on from year to year until all hope of recovery is lost. Hence, any system that has for one of its objects the reformation of these women, should enact, that it be a high, very high offence to give them credit, for as long as they can incur debt so long will they remain prostitutes, even under the most favourable system of control. But to return:

clause the 18th obliges the owner of the house, and the woman is equally bound by regulation No. 14, page 121, to inform the commission and the attending physician when pregnancy occurs. In such cases the speculum is not used after the third month, and at the expiration of the seventh she is obliged to leave the house; but as she will not be received into hospital until the termination of the eighth month, how is she to live in the interim? For this, as the state has no provision, *her fellow prostitutes come to her aid;* a voluntary subscription is made in every brothel in Berlin, and the money thus collected is given to her; on it she lives until her admission to hospital, it helps or supports her while nursing her child, or pays for its being nursed, while she returns to her wretched life to pay off old debts, and help others in her turn. With this redeeming feature in her fallen nature, it cannot justly be said that the trail of the serpent has utterly effaced all traces of the beauty and nobility of the woman's heart.

Clause No. 19 has the effect of obliging the owner to preserve complete order, and the women are most scrupulously cleanly. It is provided in the 23rd clause, that the applicant for the permission shall pay fifteen pounds to the chief fund of the police, and he is informed that the entire of this money is to be given to the institution for repentant females, while the interest of the forty-five pounds, lodged as security, is also given to the institution. Thus, the first act of one about to open a brothel is to denounce his mode of life by helping to support an asylum for repentants. It is provided in clause 25, that if the owner of the house be found guilty of any offence against the common law, he shall be doubly punished, first by the civil courts, and secondly by the permission being withdrawn, or by losing, in some cases, the security. Lastly, in order to prevent any disagreements that may arise between the owner of the house and the commission, or between the former and the visitors or the women, from furnishing interesting (?) reports for the daily press, the 26th clause endows the commission with the power of judging all complaints, and a final appeal can be made to the chief of police.

If the request is granted, the applicant gets a copy of a book printed by order of the commission, containing regulations for his conduct; but as many of these are similar to those already given in the form of request, we shall notice only the additional rules.

Regulations for the Person who is allowed to provide Lodging and Board for Prostitutes.

"4th regulation.—He shall not allow billiards, cards, or any other game to be played in his house.

"5th regulation.—A printed list of prices must be hung in each room, and the commission must be furnished with a copy. (We give the following as a specimen of these tariffs:—Entrance, 6*d.*, for which a cup of coffee is given; coffee, per cup, 4*d.*; use of a room for fifteen minutes, 3*s.*; for thirty minutes, 5*s.*; for one hour, 9*s.*; and these prices include the company of one of the women for the time stated.) If there is a higher demand made from a visitor than that stated in the tariff, on his reporting it to the commission, the owner of the house is fined in accordance to clause 13 of his contract.

"6th regulation.—The agreement between the owner and each of the women must be written, one copy to be kept by the owner of the house, a second to be given to the woman, and a third left with the commission. (These agreements are generally to the effect that the owner gets two-thirds of what moneys she gets in conformity with the tariff, and for this he provides her with lodging, board, clothing, and attendance.)

"13th regulation.—That the owner of the house must provide an examination table of a certain form, two or three specula, several pounds of chloride of lime, and for each woman, besides bed and body linen, he must furnish a washing-stand, &c., a vaginal syringe, and two or three sponges. (Each woman must have a separate bedroom, so that there cannot be more women than there are sleeping apartments in these houses; this most sanitary arrangement, though not in the 'Book of Regulations,' is invariably enforced by the commission.)

"14th regulation.—If it be necessary that a woman take outdoor exercise, or if she goes out on business, she must be modestly dressed, and the owner of the house shall provide a trustworthy man to accompany her and see that she does not stand in the street or remain out longer than is necessary for her health or business.

"15th regulation.—If a woman wishes and determines to leave her unlawful course of life, the owner dare not make any attempt to dissuade her from so doing, nor dare he prevent her, as soon as he is acquainted with her desire, not even if she be his debtor. (The commission further enforce, that if she be entirely unprovided with proper clothes, he must furnish her with a suit such as is worn by servants, and send her, at his expense, to her native city, no matter how remote it may be.)

"16th regulation.—If a woman wishes to leave this house, in order to continue her debauched life elsewhere, she must first have fulfilled the conditions in the written agreement existing between her and the owner, or she may leave in accordance with a new agreement voluntarily made by him; but of this the commission must get notice.

"17th regulation.—It is expected that the owner of the house will assist the commission in their efforts to bring these women back to an honourable course of life, that he will endeavour to prevent secret prostitution, and to trace syphilis to its origin. (It must be evident that this regulation is only useful inasmuch as it expresses the objects for which the commission has been constituted.)"

The request having then been granted, and the owner provided with a copy of these regulations, the house is finally inspected, and if the arrangements are approved of, permission is given to open it.

A woman wishing to enter such a tolerated brothel must apply to the commission, with proof of her having attained the age of twenty, and being free from debt, as it is the endeavour of the commission to ensure that no one is obliged to have recourse to prostitution to free herself from debt. The regulations are read to her, she is informed that if she enters a brothel, it becomes for her a kind of prison,—in which she must submit to the regulations she has just heard; and such arguments as the nature of the particular case suggests are used to induce her to change her resolution. In the event of her adhering to her wish, she is examined, and if found healthy, her name, age, residence, birth-place, and personal appearance, are noted, so that she can be readily identified; she then obtains the written permission to enter such a brothel, and is given a book containing the following regulations, in which her name is written in full.

"*Rules for the women who have not been induced by the most urgent persuasions to leave their debauched course of life, and are therefore placed under legal inspection.*

"1st. The person can at any time leave the house in which, by permission of the commission, she obtains board and lodging, as soon as she has the earnest intention of leading a lawful and honourable course of life: nothing can oblige her to remain, neither obligations nor debts to the owner or to any other person, and in such a case she shall have the necessary protection and assistance from the commission or from the civil police of the district in which she lives.

"2nd. If a woman wishes to leave a house in order to continue her debauched life in another, she can only do so after having fulfilled the terms of the written agreement between her and the owner, or by his permission; the commission reserve to themselves the right of making exceptions to this rule, in the event of the woman desiring to leave on account of her having been bodily ill-used by the owner, or for other important and well-founded reasons; but of all such changes the commission must get notice.

"3rd. If a woman obtain the aid of the commission, and after leaving a brothel under pretence of following an honourable course of life, devote herself to secret prostitution, she shall be confined for three months in the House of Correction, and at the expiration of that period, she shall be detained there until she desires and obtains an honest employment, be given into the charge of her family, or sent to her native city.

"4th. The police of each district shall, from time to time, inquire if the women have any cause of complaint, which they or the attending physician will receive and communicate to the commission.

"5th. The woman is hereby seriously cautioned against entering into much debt, as she is responsible for all liabilities, and is thereby brought into a state of dependence which greatly increases the difficulties of her reformation.

"6th. The owner of the house must be obeyed in all that refers to the carrying out the regulations of the commission relative to the order and decorum of his house; the women must not appear at the street door or at the windows, nor attempt to attract the passers-by with words, gestures, &c.; and should any woman act contrary to these regulations, she shall, for the first offence, be punished with imprisonment for three days on bread and water, and for each repetition of the offence, with eight or more days.

"7th. That they shall not appear in the streets, or in any place of public amusement, under a penalty of three days' imprisonment.

"8th. That any necessary out-of-door exercise must be made in conformity with regulation 14th, p. 120; and she incurs imprisonment by any breach of these directions.

"9th. She shall not practise any deception or extortion against those who visit her, for which, as well as for theft, procuration, reception of stolen goods, fraud, &c., she shall be punished with more than usual severity.

"10th. That she shall preserve the greatest personal cleanliness; that during each menstrual period she shall not allow any one to visit her; that if she be in any way ill, has any swelling, ulcer, discharge, &c., she shall immediately inform the owner of the house and the attending physician. In the event of her acting contrary to this regulation, and thereby assisting in the extension of disease, she shall be sent to the workhouse for from six to twelve months.

"11th & 12th. In these regulations, she is ordered to pay particular attention to the detection of gonorrhœa, syphilis, and scabies, and is referred to the last pages of the book, where there are instructions for the detection of these diseases in both sexes, and also for the discovery of pregnancy.

"13th. This rule is a repetition of Clauses 20 and 21, p. 117.

"14th. If she suspects or knows that she is pregnant, and does not inform the owner of the house and the attending physician, she shall be most severely punished, according to the law against the concealment of pregnancy.

"15th. After each menstrual period, she shall take a bath, or wash the entire body; and after every coitus she shall wash, and inject a solution of chloride of lime; the syringes, sponges, solution, &c., will be provided by the owner of the house.

"16th. She must submit to, and on no account be absent from, the ordinary examinations of the visiting physician, nor from any extraordinary examinations, which can be made as frequently as, and at whatever time, the commission may direct.

"17th & 18th rules are to the effect, that the commission will act as arbitrators between the women and the owner, but that, so long as they remain in this house, they are under the control of the commission, whose regulations if they violate or refuse to obey, they shall be placed in confinement."

Having entered a tolerated brothel, the prostitute is visited twice every week by the attending physician, on which occasions the examinations are thus conducted. A woman enters the room used for the examination, gives the book of the regulations, in which her name is written, to the physician, who examines her hands for scabies, then the mouth and pharynx; lastly, the vagina – invariably, excepting during the periods of pregnancy, with the speculum: her book remains with the physician. Another enters, and thus the examination is continued; finally, the physician counts the books, to see if he has examined all the women in the house, and writes his report. This is forwarded the same day, to the chief physician; and if any one has been reported diseased, she is immediately sent to hospital, which she cannot leave until a notice of her being perfectly cured has been sent to the commission.

Such are the regulations for tolerated prostitution, and the commission has most humanely enacted, that no debt or obligation can retain the women in the house as soon as they intend to enter on a proper course of life; but they do not leave, except in rare cases, and often return to their old habits; to prevent which, the third of these last regulations has been instituted. We are, however, disposed to believe, that punishment and reformation stand much less frequently in the relation of cause to effect than is generally supposed. Punishment may prevent a repetition of the act, from fear of the consequences; but in the majority of cases, the ingenuity is taxed to discover how the crime can be repeated without detection: and if this supposition be applied to cases such as the present, it will become highly probable that the most lenient measures would be most effectual in reforming the immoral tendencies of these women. True it is, that in all efforts at reform, we are often obliged to argue and work as it were backwards, directing our energies to prevent the effects, in the hope of being thus indirectly enabled to remove, or at least oppose, obstacles to the action of the cause; but it would be probably more effectual if we directed more attention than has been hitherto done to discover and remove the causes of crime. The caution in Rule 5, p. 121, against their entering into too much debt, on account of its placing obstacles in the way of their reformation, can have no effect on those who do not desire such a change. It matters not how anxious a woman may be, either to reform her life or avoid the payments of her debts, she will not venture to leave as long as she is in debt to the owner, as she is well aware, that although she can leave to-day, he can prosecute her to-morrow for debts incurred in his house. How can she meet his demand? Her most direct, easiest, and habitual way of obtaining money, is now interdicted, under penalty of a long imprisonment, as laid down in Rule 3, p. 121. The commission cannot pay her debts, or they would be holding out a premium for crime; thus she is, in point of fact, obliged to remain until she has paid her debts. There is but one means for avoiding this evil, namely, by declaring the women non-responsible for debts incurred while living in such a house; forbidding the owner, under a heavy penalty, from giving them any credit; and enforcing that all clothes worn by the women shall be provided for them gratuitously by the owner of the house. Under this or some other regulation, by which it would be rendered impossible for prostitutes to obtain credit, we might reasonably hope for the reformation of some, and the moral improvement of many.

We shall next consider—

Non-tolerated or Clandestine Prostitution.

The class of women who come under this division are those who live separately in their own apartments, and correspond to "*Les Filles Isolées*" of Paris. After having reported and brought under the control of the commission the most notorious of these women, the civil police proceed in their search, and if a woman is observed to be frequently in the street at night, dressed in a manner very disproportionate to her station in life, should information or other circumstances cause her to be suspected, an inquiry is made into her occupation, means of subsistence, those who visit her, with whom she associates, her general character, &c.; a report is then laid before the commission, and if they consider there is good reason to suspect her of living by secret prostitution, she is brought to the office of the commission and examined; if found in health, her name, age, residence, &c., are entered in the Red Book, or book of the suspected, and she is cautioned that the police know her mode of life, and, if she does not alter, she will be placed under the commission. If she comes a second time under the notice of the police, for drunkenness, &c., or if, on the first examination, she is found diseased, after being sent to hospital she is enrolled on the Black Book, or book of control, which contains her personal description and history, with the reasons for her being inscribed. She must now attend to be examined, once a week, at the office of the commission; and if she fails to be present at the appointed time, she incurs imprisonment from one day to four weeks, according to the frequence of the offence. These examinations, made by the physician that day on duty, in the presence of the chief physician, who has to countersign all orders to hospital, &c., are conducted in the following manner. On entering the waiting-room, each woman is given, by the police officer in attendance, a small book, in which her name, residence, birth-place, age, religion, size, height, complexion, colour of her hair, eyes, &c., are inserted; with this she enters the inner room, gives the book to the physician, who proceeds with the examination as stated at p. 122; he then marks her book, each leaf of which is similar to the following:

DATE.	RESULT.	SIGNATURE OF PHYSICIAN.
April 17.	H. (for healthy.)	A. B.

On leaving, she returns the book to the policeman, who, seeing it marked "healthy," allows her to depart. Meanwhile, another woman has been examined, found diseased, and her book is thus filled up:

DATE.	RESULT.	SIGNATURE OF PHYSICIAN.
April 17.	S. (for Syphilis,) or G. (for Gonorrhœa,) or Sc. (for Scabies.)	A. B.

On giving the book thus signed to the policeman, he informs her that she must go to hospital; and at the termination of the examinations, all those who have been reported diseased are conducted there under the care of one of the officers of the commission. As the women return their books to the policeman, he marks them as having attended in the registry; and should one be found to be absent, she is, on the same day, arrested and placed in confinement. If one of these women be ill, she must send information to that effect to the office; she is visited the same day by the physician of that district, and sent to hospital; but if she has feigned illness, she is forthwith arrested; and this has been so strictly acted upon, that the attendance is almost invariably regular.

Such are the regulations for public and secret prostitution in the city of Berlin. We have given a detailed, and, we believe, complete account of this system, as, after a careful study of the subject, and having seen the systems of Austria. Belgium, France, &c., in operation, we found it to be the most efficient and humane.

It may not be uninteresting here to mention the relative number of prostitutes in Berlin.

In 1849, the population of Berlin was	423,902
The male population over 16 years of age was	134,772
The number of military (not included in the above) . .	19,030
Total males	153,802
The number of tolerated brothels is now	20
The number of prostitutes in these brothels	225
The number of "non-tolerated" prostitutes under the superintendence of the police	540

If we say, then, that there are 765 prostitutes actually known to the police, this gives one prostitute to every 201 males (including the military). As, however, the total number of clandestine prostitutes is not yet known to the police, the relative proportion of prostitutes is somewhat more than this.

It is at all times exceedingly difficult to demonstrate the effects of a control such as that we are discussing, as it has been hitherto found impossible to determine accurately the amount of syphilis that exists among the mixed and migratory inhabitants of capitals. We have fortunately been favoured with a statistic of the cases of syphilis among the troops in Berlin, which fulfils all that is necessary to secure the correctness of conclusions deduced from it. It must be observed, that the number of soldiers is fixed, and that the same corps are permanently on duty in Berlin; further, these statistics have not been collected for the purpose of proving or disproving the efficacy of the system of control, as the registry of the Military Hospital has afforded the required data.

"*Report.*—To the Royal Commission for Moral Police in Berlin.

"In answer to the letter of the royal commission, dated April 30th, 1853, I have to report that, among other things, we have observed, during the last few years, a remarkable diminution of syphilis among the garrison. While in the year 1849 there were 1423 cases of syphilis among the troops,

In	1850	there occurred	670 cases.
	1851	,,	526 ,,
	1852	,,	332 ,,
In the first quarter of	1853	,,	59 ,,

Also, in respect to intensity, the disease forms a most favourable contrast with that of former years. In my opinion, the above numerical proportion furnishes the most sufficient proof of the utility of the existing sanitary regulations.

"Dr. Stumpf,

"Berlin, May 3rd, 1853. Chief Physician to the Garde de Corps."

There has been, then, a diminution of 753 cases, or more than half, on the first year, when the examination of the tolerated prostitutes or those living in brothels was made regularly; a diminution of 144 on the second year, of 194 on the third, or that of 1852, in the February of which year began the regulations for non-tolerated prostitutes; and calculating of 1853 according to its first quarter, we get a diminution of 96 cases for that year.

Between the 1423 cases that occurred in the year 1849, and the 332 that presented themselves in 1852, we have the enormous difference of 1091 cases, and we are justified in calculating, that had there been no control for prostitution the frequency of syphilization would not have diminished hence, in 1850, 1851, and 1852 there would have been 4269 cases, whereas under the control there have occurred only 1528: therefore there has thereby been saved from infection, no fewer than 2741 soldiers during a period of three years; yet a complete control has only existed for eleven months.

Let us turn for a moment to the disease in women, and we shall find, that during February, 1852, this being the first month of the inspection over the non-tolerated, about 38 women were examined every week, and the cases of syphilis amounted to 29 per cent. per month; while in April, 1853, about 540 women were examined weekly, and the amount of syphilis had fallen to 5 per cent. per month.

With facts before us such as these, the beneficial effects and direct humane tendency of a control over and examination of prostitutes, is no longer theoretical or problematical; it has been found to protect the women from the ill treatment that they almost invariably more or less suffer from the owners of brothels in Britain, while it facilitates their reformation, and at the same time protects the public health.

We postpone until the next number an epitome of the systems of control used in Austria, Belgium, France, &c.; suffice it to observe, that syphilis is among the British troops *the most frequent of all diseases,* about 180 cases occurring annually among every 1000 soldiers.

T. S. Holland.

(*To be continued.*)

Review VIII.

1. *Untersuchungen über Thierische Electricität.* Von Emil Du Bois-Reymond.—*Berlin,* 1848-9.

Researches in Animal Electricity. By E. Du Bois-Reymond.

2. *On Animal Electricity, being an Abstract of the Discoveries of Emil Du Bois-Reymond, &c. Edited by* H. Bence Jones, M.D.—*London,* 1852.

In the following review we have endeavoured to give a perfectly simple, but at the same time faithful, account of the rise, progress, and present condition of our knowledge of Animal Electricity. If any one should think that we have descended to the enumeration of facts well known and universally admitted in the scientific world, we need only reply that our review is not intended only for those who have kept pace with the progress of investigation, but also for those whose busy lives have not permitted them to acquire accurate information on this important subject. Commencing with the simplest facts, we shall rise to the more complex, and refusing to enter into controversy, we shall endeavour to indicate the exact point to which accurate observation has reached, and beyond which it is now endeavouring to spread.

Before entering upon the subject, a few preliminary remarks on the electric current and the instruments applied to its investigation, will not, perhaps, be deemed superfluous by some of our readers.

If a strip of copper and a strip of zinc be both immersed in a glass of water, nothing remarkable occurs as long as the two metals do not touch each other; the moment they come into contact, however, an evolution of gas will be observed, which evolution is attributed to the passage of an *electric current.*

The term "current" is suggested by analogy, and is really meant to express a process regarding the real nature of which we know very little. In conceiving of and reasoning upon electrical phenomena, a physical image appears to be demanded by the intellect; and in the case before us this image is a fluid in a state of motion.

Philosophers are still disunited as regards the origin of this fluid; indeed, the question has given rise to two distinct national creeds in England and Germany. Referring to the example cited at the commencement, the Germans believe that the origin of the electric current is at the place where the two metals touch each other. It is an experimental fact, that when copper and zinc are brought into contact, and afterwards separated, the zinc is found feebly charged with positive electricity, and the copper with negative electricity. "At the place of contact of zinc and copper," says the German, "a certain force exists (the electro-motive force) which decomposes the neutral fluid of these bodies, collects the negative fluid upon the copper, and the positive fluid upon the zinc; and if both metals be united by a conducting liquid, the two electricities will pass through it and decompose the liquid in their passage, thus giving rise to the chemical phenomena of the electric current."

In England, on the contrary, the general belief is, that the source of action is at the place where the liquid in the glass comes into contact with the zinc; that the current is, in fact, a consequence of the chemical relations between the zinc and the constituents of the fluid. The former theory is called the theory of contact, and was first promulgated by Volta; the latter theory, which finds in Professor Faraday its most powerful advocate, is called the chemical theory.

The partisans of both theories make use of the term "current;" but this implies a fluid flowing in one determinate direction,—down hill, for instance, in the case of water,—and here the first imperfection of the analogy between electricity and ponderable fluids presents itself. In the case of the former, the positive fluid passes from the zinc, through the liquid, towards the copper; but the negative fluid passes from the copper, through the liquid, towards the zinc, so that in reality we have two currents instead of one. To avoid confusion, however, it has been agreed upon to call that direction in which the *positive* electricity flows *the direction of the current.* Hence in our example the direction of the current is from the zinc, through the fluid, to the copper, and from the copper, across the place of contact, to the zinc, the circuit thus traversed being that which is usually called the voltaic circuit.

If, instead of uniting the copper and zinc directly, we interpose a wire between them, the action will proceed as before—instead of crossing immediately from one metal to the other, the current will traverse the interposed metallic conductor.

If the interposed wire be of suitable thickness, no apparent change will be produced in it by the passage of the current. How, then, do we know that a current is really traversing such a wire?

1. The evolution of gas in the vessel in which the strips of metal are immersed gives us intelligence on this head; if the wire be cut across, the evolution instantly ceases.

2. If the ends of the severed wire be united by a very thin wire, and the development of electricity be strong enough,—which may be secured by using several glasses instead of one,—the thin wire may be heated to redness, to whiteness, and even melted, while the thicker wire, whose ends it unites, is apparently unchanged. But the power which thus affects the thin wire is transmitted through the thick one; and this is the power to which we give the name of the electric current.

3. Suppose the conducting wire to lie north and south, and a common compass-box containing a magnetic needle to be placed underneath it; before the circuit is established the needle will be parallel to the wire; the moment, however, the circuit is established, the needle will be diverted from its parallelism and will set itself across the wire. The north-pole will point in a certain direction. If the direction of the current be reversed, the north-pole will cross the wire and point on the other side. There is, in fact, a fixed relation between the direction of the needle and

that of the current; and we possess in this instrument the most ready and valuable means of establishing the existence of the voltaic current, its strength and direction. Suppose the right arm stretched along the wire, underneath which the needle is placed, with the palm of the open hand turned downwards, and conceive the direction of the current in the wire to be the same as that of the arterial blood in the arm—viz., from the shoulder to the fingers—then the north end of the needle will point in the direction of the thumb. Preserving the wire between the hand and the needle, as in the case just described, and the palm always turned towards the wire, no matter what the position of the needle may be, whether above the wire or below it, or in a lateral position, the thumb will always indicate the direction in which the north-pole of the needle will point. By means of this little artifice, we can at once infer the direction of a current from its action upon a freely suspended magnetic needle.

Bearing this simple rule in mind, we see that if, instead of being placed beneath the wire, the needle is suspended above it, the direction of the north-pole will be different. If in the former case it was westerly, it will now be easterly. Hence, if the needle be placed between two equal currents flowing in the same direction, the two currents will exactly neutralize each other, and the needle will remain undeflected; but if the currents flow in *opposite* directions, then it is easy to see that both pull the needle in the same direction, and an increased deflection is the consequence.

The single current, however, may produce the same effect. If the conducting wire be coiled into a vertical circle, and the needle be placed within the latter, then the direction of the current in the lower part of the circumference is opposed to that in the upper, and the action is the same as that of two distinct currents in opposite directions. If, instead of being coiled *once* round the needle, it be coiled several times, the various coils being so insulated that the current cannot cross directly from one to the other, but must make the circuit of all of them, then the actions of the coils add themselves together; and, by this multiplication, a very feeble current may be made to produce a very sensible effect. On this simple principle Schweigger based the construction of the multiplying galvanometer, an instrument of indispensable utility in the investigation of feeble electric currents.

In these cases there are two forces acting upon the needle; the magnetic force of the earth tends to set it north and south, while the tendency of the current is to set it east and west, and it will approach the one or the other position according as the one or the other force is predominant. The action of the current, therefore, can only render itself evident by overcoming to a certain extent the action of the earth; and if the latter action could be removed, the effect of the current would be so much the greater. Amperè was the first to propose, and Nobili to enact, a method by which this is effected. It consists simply in taking two magnetic needles of equal strength, turning the north end of one towards the south end of the other, and connecting both by means of a rigid rod. If the needles be of the same strength and perfectly parallel, it is evident that the system is completely freed from the magnetism of the earth, which attracts and repels one end of the system with equal force at the same time, and thus neutralizes its own action. In practice, however, we are not able to place the needles perfectly parallel to each other, and the consequence is, that the system retains a slight directive action, and takes up a certain position, from which, however, the slightest force can move it. Electric currents of an infinitesimal character, multiplied in the manner already described, and brought to bear upon such a system, are at once rendered capable of observation.

It might at first sight be supposed that the greater the number of coils, the greater ought to be the action produced. In certain cases, however, there is a speedy limit to their profitable increase. True, a certain amount of action proceeds from each coil and operates upon the needle; but it must be borne in mind that each additional coil increases the resistance offered to the passage of the current, so that a point is at length attained where any increase of the number of coils would, by increasing the resistance, become positively injurious. Let us suppose the case of a current which has already passed through several miles of wire; the addition of another hundred yards will affect it very slightly, whereas the same addition to a current which has already overcome but a slight resistance may produce a very great diminution. Now the resistance of the human body is equal to that of a copper wire one millimetre thick and many miles in length, and hence, in the investigation of a current which has passed through the human body, or been generated in it, we may employ with advantage a galvanometer with an immense number of coils. A clear apprehension of this fact induced Du Bois-Reymond to construct a galvanometer containing 5584 yards, or upwards of 3 miles, of copper wire; while the thermo-multipliers of Melloni, which are used to investigate currents generated with little resistance, rarely contain more than two or three hundred feet.

In the year 1790, a lady of Bologna, possibly to lend the cheerful encouragement of her presence to his scientific labours, chanced to be in the laboratory of her husband. A skinned frog lay upon a table near to which an electric machine was in action. Once, at the moment when a spark was taken from the conductor of the machine, the frog happened to be touched by a scalpel, and the quick eye of the lady was the first to observe a spasmodic movement of the dead limbs. We feel a pleasure in assigning to the lady an honour which the best evidence on the subject declares to be her due. She drew her husband's attention to the astonishing fact. The experiment was repeated, and it was found that whenever a spark was drawn from the machine the same convulsive motion exhibited itself. At this time all men's eyes were eagerly directed towards the phenomena of vitality, and, as may be readily supposed, the discovery of a dead animal restored to temporary life by electricity created a most profound sensation. From this moment the frog was a doomed animal; the experiment was repeated everywhere, and the world rejoiced in the possession of a fact which seemed to promise the control of the very principle of life itself.

Such was the opinion of Galvani, in whose laboratory the wonderful discovery had been made. Having satisfied himself as to the efficacy of machine-electricity in producing the phenomenon, his next effort was to ascertain whether the electricity of the atmosphere could produce a similar effect. He prepared a frog, and attaching it to a *copper* hook, hung it upon an *iron* railing near his laboratory. Having watched for some time, and observing no sign of electric action, he moved the animal, but in doing so the very spasmodic action which he sought exhibited itself. He soon discovered the condition of its production—that every time the moist body of the frog touched the iron rail, a motion of the limbs was the consequence. He took the frog into the laboratory, and substituting for the copper hook and iron rail a metallic arc, found that he could produce the convulsions at will. It was only necessary to place one end of the arc in contact with a nerve, or with the spinal column, and to cause the other end to touch one of the muscles of the leg, to produce a sudden contraction of the latter. A significant fact was observed in these experiments. If the arc was composed of a single metal, the convulsions were feeble, but when one half of the arc was of a metal different from the other, the contractions were strong.

To understand the exact import and relation of these two experiments, it will be necessary to call to mind the principal laws of electric action. We know that if two glass rods, or two sticks of sealing-wax, be rubbed together with a woollen cloth and suitably suspended, one glass rod will repel the other, and one stick of sealing-wax will repel the other, but the rubbed sealing-wax will attract the rubbed glass, and *vice versâ*. This action is expressed in the fundamental law, that electricities of the same kind repel each other, while electricities of opposite kinds attract each other. For the sake of reference we will give the two electricities their conventional names, calling that developed by friction on the surface of the glass rod *positive*, and that on the surface of the sealing-wax *negative*. The foregoing law of action would then be expressed by saying that positive electricity repels positive, and negative repels negative; but that positive attracts negative, and negative attracts positive.

At the commencement of treatises on electricity, we usually find the attraction of light bodies by rubbed amber or sealing-wax alluded to. These actions, though introduced thus early, are by no means elementary. We shall find the image of a fluid very useful here. All bodies are supposed to possess electricity in definite quantity, both negative and positive, but as long as these two fluids are exactly equal in amount, they neutralize each other's action, and the body is what we should call unelectrified. If either fluid be in excess, we have an electrified body. In the cases of amber and sealing-wax the act of rubbing disturbs the balance of the fluids, so that an excess of negative fluid is left upon the wax, while an exactly equal amount of positive escapes by the rubber. A substance thus electrified possesses the power of attracting light bodies; —but how? These bodies, it must be remembered, possess their share of neutral fluid. The approach of an electrified body decomposes this neutral fluid, repelling that which is of the same name as itself, and attracting its opposite. Thus, when a stick of sealing-wax is presented to a pith ball, the surface of the ball nearest to the wax will be covered with positive electricity, while the surface most distant from the ball will be covered with negative electricity; and in virtue of the greater proximity of the unlike electricity, the ball is attracted. Before the body can be attracted it must be electrified; this is the primary act of the wax, (called induction,) from which attraction follows as a consequence.

We are now in a condition to understand the precise import of Galvani's first experiment. If a man stand upon an insulating stool with his face towards the (say positively) charged conductor of an electric machine, his face and breast, although in no immediate contact with the machine, will become charged with the opposite electricity, while his back will be charged with the same fluid as that upon the conductor. If his back be connected with the earth by a wire, the positive fluid will pass away to the earth, while the negative in front is held fast by the attraction of the conductor. While things are in this state, suppose the conductor to be suddenly removed, or suddenly discharged, by a second person. The natural condition of the man will be instantly restored, the positive fluid will rush back from the earth to combine with the negative, from which it was forcibly separated by the conductor, and this sudden re-union of the fluids is accompanied by a shock to which Lord Mahon, the discoverer and elucidator of the fact, gave the name of the *back stroke.* If we suppose a frog to take the place of the man, and the wire to be superseded by the scalpel of the student, we have the conditions of the first famous experiment of Galvani before us, the only difference being that in the latter case the contractions which accompany the shock were observed upon a dead body instead of a living one.

Far otherwise was it, however, with the second experiment of Galvani. This was a development of electricity altogether new. This is the seedling which, cast upon the mind of Volta, produced the miraculous fruits of which the world now reaps the benefit. People attribute scientific progress to chance. Arago attributes the discovery of the pile to the accident of a lady being ordered frog-broth; but this is an incomplete expression of the facts of the case. Such chances are ever present, but, like the microscopic seeds which float through the atmosphere, they perish without a proper soil. Volta at first shared the general astonishment in observing the dead limbs revivified; but he soon perceived the significance of the fact that the contractions were strongest when the conductor which united nerve and muscle was composed of two metals. Pondering upon this, he was led to reject the notion that Galvani had discovered the principle of life. "Here," said he, "we have no new force, but simply the old electricity developed in a new way—namely, by heterogeneous contact." The thought occurred to him of substituting his tongue for the frog. He placed one metal underneath his tongue and the other upon it, and, standing before a looking-glass, he brought the metals into contact. He expected to see the tongue quiver like the frog, but instead of it he remarked that peculiar taste with which everybody is now acquainted. He found that the taste was a continuous phenomenon, and hence he inferred that the electrical development must be different from that exhibited by the instantaneous action of an electric machine. He published his views, and was strenuously opposed by Galvani. The world, indeed, at first frowned upon the man who threatened to rob it of its acquired treasure. But this opposition served only to drive Volta to a deeper discussion of the subject; his was the true way of conquering a scientific foe, not by wordy warfare, but by fresh discovery; he worked until he earned the right of exclaiming, with triumphant scorn, "I don't need your frog—give me two metals and a moist rag, and I will produce your animal electricity. Your frog is nothing but a moist conductor, and is in this respect inferior to my wet rag." He had made the discovery which immortalized his name.

The Voltaic pile was constructed a year after the death of Galvani, who was thus spared the bitterness of witnessing the complete overthrow of his theory. The splendour of the new discovery dazzled the world's eyes, and perhaps prevented it from estimating the real force and value of many of Galvani's experiments. Volta, as before remarked, urged that the convulsions were due to the passage of ordinary electricity, derived from the contact of heterogeneous metals. Galvani, a man of great ingenuity, replied by an experiment wherein a single metal only was used to connect nerve and muscle. Volta retorted that the so-called homogeneous metal was not homogeneous, and that the slightest change of the surface could cause the apparently simple arc to act as if it were duplex; he showed that the immersion of one end of the arc in boiling water was sufficient to effect this. Galvani at length succeeded in producing convulsions without the intervention of any metal whatever, by causing nerve and muscle to touch directly, or by connecting them with an animal tissue. For a time Volta seemed willing to grant the existence of an electricity peculiar to the animal, though much more feeble than that developed by heterogeneous contact; he soon, however, relapsed into his old scepticism, and referred the last result of Galvani to the heterogeneous contact of the tissues themselves. In this belief he was confirmed by the discovery of the pile, which lent such a prestige to his name, that an oracular value was attached to his opinion. He bore down all antagonists. Perhaps, as in the case of some of Newton's views, the grandeur of his truth gave a currency to his error, when animal electricity was banished from the realms of science.

Alexander von Humboldt was a guest of Volta's at the time Galvani succeeded in producing contractions without the aid of metals, and, not satisfied with Volta's explanation, he undertook the repetition and extension of Galvani's experiments. He was then scarcely thirty years of age; this investigation, therefore, is one of the earliest blossoms of that genius which has since ripened into such renown. His experiments convinced him that, besides the source of electricity contended for by Volta, there was another peculiar to the animal itself. He reasons as follows:— "I skinned a frog, and prepared it so that the trunk and limbs were connected by the ischiatic nerves alone. When the red flesh of the limb was bent back, so as to gently touch the ischiatic nerves, violent muscular motions were excited. Here, then, were only two substances, nerve and muscle, organically united, brought into contact. (Volta had at first contended for the necessity of three substances, at least.) The excitation could not be attributed to mechanical pressure, for all remained motionless when the ischiatic nerve was shaken with a bit of muscle, with sealing wax, wood, and other substances. The unnatural bending of the leg may, however, be urged against the validity of the experiment. I therefore quit this uncertain way of working, and proceed to other methods. I took the two limbs of a frog which possessed a high degree of excitability, prepared the crural nerves speedily, and laid the latter, together with the whole extremity, upon a well-dried plate of glass. To an insulating handle I attached four or five cubic inches of fresh flesh, and brought it simultaneously into contact with the crural nerve and the muscle of the thigh. Strong contractions ensued. When nerve and muscle were touched by two separate pieces of flesh, no motion was observed, until these two pieces were themselves brought into contact. The above experiments were also successfully repeated with several other land and water frogs, with the small rana arborea, the lacertus agilis, and the common mouse. Here, then, were only two heterogeneous substances, nerve and muscle, in contact, and hence the idea of referring these remarkable phenomena to Volta's theory of disturbed electric equilibrium through the contact of at least three substances, must be abandoned."

The following is a summary of Humboldt's results. Strong muscular contractions were obtained:

1. When the leg of an animal was bent back against the ischiatic nerves, both being organically connected.
2. When the crural nerve and its muscle were connected by a fragment cut from the same nerve.
3. When a connexion was established between two parts of the same nerve, by means of some animal tissue.

These results were published in 1797; Galvani died in 1798; at the end of 1799 Volta discovered the pile, and for nearly thirty years silenced the supporters of animal electricity.

In 1820 Œrsted discovered that an electric current deflected a magnetic needle in the manner we have already described. Shortly after Schweigger, of Halle, acting on the suggestion of Poggendorf, multiplied the effects of feeble currents by coiling the wire several times round the needle. In 1825 Nobili imparted an unexpected delicacy to the instrument, by introducing, instead of a single needle, an astatic pair, whereby the action of the earth was nullified, and the needle left free to obey the slightest exterior impulse. The first use he made of his improved instrument was to examine with it the electric currents supposed to be developed in the nerves of animals, but without result. In the course of inquiry he was led to repeat the old experiment of Galvani, where contractions were produced without the intervention of metals. The spinal column of a prepared frog was suffered to dip into a vessel of salt and water, and the feet of the animal into another vessel; on connecting the two vessels by a piece of moist cotton-wick, contractions were exhibited. The thought occurred to Nobili that the current which produced these contractions might be detected by his galvanometer; he introduced the instrument into the circuit, but to his disappointment, although the frog was convulsed, the needle of the instrument stood still. His jealousy was excited; he had imagined that his instrument could not be surpassed, in point of electroscopic delicacy, but here he found it cast entirely into the shade. He attacked the matter once more, improved the galvanometer, and finally succeeded in obtaining a deflection of ten, twenty, and even thirty degrees from the current of the frog. This is the man who first showed the applicability of the galvanometer to researches of this nature. Nobili's success did not convince him that the current was produced by the vital actions of the animal. The opinion of Volta may have had some influence upon him, and his own failure to obtain currents from the nerves, served, perhaps, to confirm his scepticism. Five years previously Seebeck, of Berlin, had discovered thermo-electricity; he had found that if two bars of different metals be soldered together, and the free ends connected by a conducting wire, on heating the place of junction of the bars, an electric current is developed. Nobili referred the current which produced the convulsions of the frog to a thermo-electric origin, and made some ingenious experiments in confirmation of this idea. A difference of temperature, he contended, was established by the quick cooling of the nerves, on account of their comparatively small size, and this difference, where nerve and muscle were connected, gave rise to a thermo-electric current.

Nobili's conclusion had some effect in retarding the progress of inquiry on animal electricity. His theory was accepted by many eminent men, until at length its insufficiency was generally admitted to be proved by an investigator, whose name has ever since been closely associated with the progress of the science. Matteucci showed that the current could be produced under circumstances where all idea of a difference of cooling was confessedly excluded. It was only necessary to immerse the legs of the whole frog, deprived of its skin, in one vessel of water, and to cause the head or back to touch the water in a second vessel; when both vessels were connected by a moist conductor, convulsions were obtained. Passing over the earlier memoirs of M. Matteucci, we will converge our attention upon a valuable paper published by him in the 'Annales de Chimie et de Physique,' for 1842, and from which we may infer the precise state of the question at this period. The memoir is preceded by a brief description of the methods of experiment. To the two ends of the wire of a galvanometer two plates of platinum were soldered, and these plates were caused to dip into two small glasses, containing a solution of sea-salt. Precautions were taken to cause the same amount of metallic surface to be in contact with the fluid, for a variation of the surface would, with a sensitive galvanometer, inevitably produce a current. The frog was prepared in the ordinary manner, it was cut across about the middle of the spinal column, deprived of its skin, the entrails and the bones of the pelvis were removed, until the animal was reduced to a morsel of spine united to the limbs by the nerves alone. The bit of spine and a portion of the nerves were plunged into one of the small glasses before alluded to, and the legs were caused to dip into the liquid of the other glass; the two thighs thus formed a bridge across the space which separated the two vessels. On establishing the connexion, the needle of the galvanometer was always observed to move, and with a frog of average vigour the current produced a deflection of four or five degrees. The direction of the current, in the frog itself, was always from the muscle to the nerve, or from the feet of the animal towards its head. The discovery of this fact is due to Nobili, who named the current "la corrente propria della rana," rendered in the paper before us, "le courant propre de la grenouille." With a very active frog a deflection of eight or ten degrees was obtained by M. Matteucci, and when his needle was more perfectly astatic, the deflection amounted even to fifteen or twenty degrees. The removal of the pelvis is by no means necessary to the production of the current; the effect is stronger if it be permitted to remain.

Our readers, doubtless, remember the arrangement of Volta's *couronne des tasses.* If a rod of zinc be soldered to a rod of copper, end to end, and the bar thus formed bent into an arc, so that one end may dip into a glass of salt and water, and the other end into a second glass, containing the same liquid, and if a number of such vessels be so connected that each glass shall contain the zinc of one arc and the copper of the succeeding one, then on uniting the two extreme glasses of the series by a conductor we obtain an electric current. M. Matteucci arranged a number of frogs in a similar manner, causing the feet of each frog to dip into one glass and its bit of spine into the next; and in this way he obtained increased effects. Dispensing with the glasses, he placed a number of frogs upon the same insulating surface, so that the nerves of one touched the limbs of the succeeding one; on connecting the ends of the series an increased current was observed. When one frog produced a deflection of four or five degrees, three or four, arranged in this way, produced a deflection of fifteen to twenty degrees. This is an important result. It might be argued that the current observed in the case of a single frog is due to the chemical action of the salt and water upon the animal parts immersed in it, but here we find that while the parts immersed, and consequently the chemical action, remain the same, the interposition of a greater number of frogs gives a greatly increased current. But if the current were due to the cause just mentioned, the interposition of more frogs would simply increase the resistance of the circuit, and thus enfeeble the current, instead of increasing it. The frog itself was also employed, instead of the galvanometer, for the detection of this current. In these experiments it was convenient to have a considerable length of nerve; hence the limb of a frog was prepared after the manner of Nobili, the thighs being cut away, and the leg alone permitted to remain, with a long filament of nerve attached to it. The pile of frogs being arranged as already described, instead of connecting the two extreme glasses by the wire of the galvanometer, they were brought sufficiently near to each other to be united by the long nerve of the prepared limb; the end of this nerve was caused to dip into one glass, and another point of the same nerve into the other. The moment the circuit was established or interrupted, the limb was convulsed. The direction of the current is also indicated by the limb with tolerable certainty. If the limbs contract on the closing of the circuit, and do not contract on the circuit being broken, the current, in the nerve, flows from the origin of the nerve towards its ramifications. If the contraction takes place when the circuit is interrupted, and not when it is established, the direction is the reverse.

In the course of the inquiry, M. Matteucci was led to experiment upon the legs of frogs only. He united a number of legs, so that the tendon of each lay against the severed extremity of the succeeding one; a current of equal strength with that produced by the same number of entire frogs was obtained, the direction in the leg being from the foot upwards. Cutting the thighs of the animals across, and arranging them so that the exterior muscle of one piece was in contact with the interior muscle of the succeeding one, a current was obtained which was constantly directed from the internal surface to the external one. The effect produced was greater than when an equal number of frogs were employed. From this important experiment, in connexion with the preceding, M. Matteucci infers the existence of two currents, one the current proper of the frog, and the other a current directed from the interior of the muscle to its surface. He also observed that the frog-current is enfeebled during convulsion, though later experiments led him to doubt this result.

M. Matteucci next operated upon warm-blooded animals. The reason why the frog is chiefly used for these experiments is known to be, that it preserves its vitality long after death; in warm-blooded animals the vitality soon ceases, and with it all electric action. M. Matteucci succeeded in producing contractions in the limb of a rabbit, quite similar to those produced in the experiments of Galvani and Humboldt. The nerve was separated from the thigh, raised by a glass rod, and suffered to fall upon the muscle of the leg; contractions followed. He also obtained a current on wounding an animal, and dipping one of the terminal plates of the galvanometer into the wound, while the other plate was placed upon the surface of the wounded muscle. The current was constantly directed, in the animal, from the bottom of the wound to the surface of the muscle. Finding, however, some serious irregularities when the terminal plate was brought into direct contact with the animal, he resorted to the method of piles, and arranging his cups like Volta's *couronne*, he prepared a number of pigeons' thighs, and placed them so that in the fluid of each cup the muscle of one thigh and the leg belonging to the succeeding one were plunged. Dispensing with the cups, he composed other piles, where the animal parts were brought into direct contact; piles were also constructed in which the nerve of the thigh was simply caused to touch the tendon of the leg. Other birds, smaller than the pigeons, were made use of, and rabbits were also examined. The result is, that a current was always exhibited by the galvanometer, which was directed, in the animal, from the nerve or the interior mass of the muscle to the external surface.

In a notice at the end of the memoir of which we have just given a digest, M. Matteucci communicates the following important observation. A frog was prepared after the manner of Galvani, with its lumbar nerves exposed. The leg and the long nerve which passed from it to the vertebral column were taken from another frog, and the leg was so placed that its attached nerve rested against the thigh of the frog prepared according to Galvani's method. The lumbar nerves of the latter were connected with a voltaic element; the passage of the current through the nerves caused the limbs connected with them to contract, and at the same moment *the leg whose nerve simply rested upon the thigh, as aforesaid, contracted also.* When, instead of making use of a voltaic current, the nerves were mechanically excited, so as to produce contractions, the prepared limb was also convulsed. The contractions of the muscle of a rabbit were also found to produce a sympathetic contraction in the limb of the frog. It is a significant fact that, although the muscle on which the nerve rested might be moved mechanically, no contraction of the prepared limb followed; a real muscular contraction was alone able to produce a motion of the neighbouring leg. We shall have occasion to return to this subject.

M. Matteucci deduces a number of general conclusions from his interesting paper, of which the following chiefly concern us:

1st. That in the frog and in warm-blooded animals an electric current is exhibited, when the interior of a muscular mass is connected by a conducting arc with its external surface.

2nd. That the nerve belonging to a muscular mass, and all the cerebral system, perform the office of the interior of the muscle through which the nerve is distributed.

3rd. That the current is directed in the animal from the interior of the muscle to its surface or to its tendon.

5th. In the case of the frog a current (*le courant propre*) is obtained on connecting the muscles or tendons of the fore-leg with the muscles or nerves of the thigh; this current is directed in the animal from the leg to the thigh or nerve.

7th. It remains to be explained, and to the anatomist, perhaps, must be referred the solution of this question, how, in the case of the frog, the muscles of the leg, and particularly the tendons by which they are terminated, play the same part in the production of the *courant propre*, as the interior of the muscle, or the nerves distributed through the muscles, in the case of warm-blooded animals.*

In January, 1843, a month or two after the appearance of M. Matteucci's memoir, a remarkable paper bearing the following title was published in Poggendorff's Annalen :—' Preliminary abstract of an Investigation on the so-called Frog-current, and on Electric Fishes; by Emil du Bois-Reymond.' The author does not describe in detail his methods of experiment, but he announces in distinct terms a law which casts a flood of light over the complicated phenomena which M. Matteucci was the first to observe. The law, which is as simple in its expression as it is embracing in its application, may be stated as follows:—" *When any point of the longitudinal section of a muscle is connected by a conductor with any point of the transverse section, an electric current is established, which is directed, in the muscle, from the transverse to the longitudinal section.*" Let us apply this law to the 7th conclusion of M. Matteucci, where he refers to the anatomist the solution of the entangled problem of the *courant propre*. Connecting the tendon with the muscle of the thigh, we have a current. Now the transverse section of the muscular fibres abuts against the tendon; the latter is a conductor, and hence when we connect the tendon with the thigh, we, in point of fact, connect the transverse section of a muscle with the longitudinal section. By removing the tendon, we simplify the connecting arc, and the current is observed as before. We thus arrive at the important conclusion, that the frog-current, instead of having a distinct individuality assigned to it, ranges itself naturally under the general law of muscular currents, and thus the difficulty which it presented receives the most complete solution.

* A sense of justice to M. Matteucci induces me to mention another paper of his published in 1843, subsequently, and without doubt altogether independent of that of Du Bois-Reymond. This paper describes the effects of poisoning, of temperature, and various other circumstances, upon the strength of the muscular current, and is replete with interest. The experiments appear to be judiciously varied, the reasoning is clear, objections are stated with frankness, and met with ability.—J. T.

The experiments on which the law of the muscular current is based were made on the frog, on pigeons, on rabbits, on the water-crab, and on lizards. The electro-motive action of the nerves, concerning which we had been hitherto in total darkness, is also stated in the same paper. The action is precisely the same as that of the muscles; if the transverse and longitudinal section of a nerve be connected, we have a current in the same direction as in the case of muscles, and differing from the muscular current only in the fact of its being feebler. We do not hesitate to express our opinion that M. Matteucci's method of experimenting with piles of muscles, possesses advantages peculiar to itself; it certainly enabled him to exhibit an increased action, and this fact conducts at once to the important inference that the current cannot be due to the action of the liquid in which the extremities of the pile were immersed. If we seek the elementary cause of the phenomena, however, we cannot resort to such piles; they by no means represent the matter under its simplest form, and the consequence is, that the law of action deduced from them by M. Matteucci would, under certain circumstances, totally fail. In all the experiments of the latter philosopher on wounded animals, we have no indication of the *direction* in which the incision was made, nor does the slightest importance appear to be attached to this capital condition. If, however, we arrive at the interior of a muscle by an incision parallel to the fibres, the law affirmed by M. Matteucci, that a current is always obtained on connecting the interior and the exterior of a muscle, breaks down. Again,—the tendon, as before stated, is the moist conductor against which the ends of the muscular fibres abut. Let us suppose the incision to be made parallel to the fibres; then, a point of the interior muscular mass being connected with the tendon, or with the natural bases of the fibres, would show a current from the external surface to the internal; which is directly opposed to the requirements of the law of Matteucci. In arguing thus, we take it for granted that Du Bois-Reymond's statement of the law is correct, and our object is simply to show that his statement differs essentially from the statement of M. Matteucci. We take Du Bois's law for granted, because we are not aware that it has been denied by M. Matteucci, and it further comes to us recommended by the authority of a committee of the Academy of Sciences. The point at issue is, not whether the current observed by Du Bois be the same current as that previously observed by M. Matteucci, for this is indisputably the case, but whether the law affirmed by Du Bois be the same as the law affirmed by M. Matteucci, which is indisputably not the case. M. Matteucci experimented with limbs, and portions of limbs, and obtained results confessedly important, but Du Bois shows us that not only the separate muscles which compose these limbs, but the separate fibres which compose these muscles, are the real seats of the electro-motive action.

In later experiments, the author last mentioned expanded the law of action above expressed, and proved, that to obtain a current it was not absolutely necessary to connect the longitudinal and transverse sections; that, under certain circumstances, on uniting two points of the longitudinal section, or two points of the transverse section, a current, though much feebler than that resulting from the connexion of the two different sections, is obtained. Let a cylindrical piece of muscle be imagined, the fibres of which are parallel to the axis of the cylinder; suppose the length of the cylinder to be bisected, and call the point of bisection *a*, then, if two points at opposite sides of *a*, and equally distant from it, be connected, we have no current; but if the distances from *a* be unequal we have a current. This is the case when the point *a* itself is connected with any other point of the cylindrical surface. In like manner two points of the transverse section, equally distant from the axis of the cylinder, on being connected produce no current; but if one point be more distant from the axis than the other, a current is obtained.

Du Bois also places the fact beyond doubt, that if a muscle be tetanized, its current undergoes a remarkable diminution, while the convulsions last. Let a gastrocnemius muscle be laid upon the terminals of the galvanometer, so that the muscular current is shown by the deflection; let the long nerve attached to the gastrocnemius be irritated by a series of electric shocks, so as to throw the muscle into convulsions; the needle instantly descends, and sometimes passes to the negative side of zero. A hasty reasoner would infer that a current in the contrary direction had been excited in the muscle; but this is not necessarily the case. Let an ordinary voltameter be introduced for a few seconds into a common voltaic circuit, the liquid within the instrument will be decomposed, oxygen will discharge itself on one platina plate, and hydrogen on the other. If the circuit be now interrupted, and the two plates of the voltameter speedily united, we obtain a secondary current, of brief duration, and in a direction opposed to the primary one. On this fact (commonly called polarization) the well known pile of Ritter is based. Exactly the same takes place in the case now under consideration. The muscular current first causes the polarization of the platinum plates at the end of the galvanometer wire, and when the original current has been enfeebled by tetanus, the secondary current, due to polarization, comes into visible play and produces a negative deflection.

We are now in a condition to take up the discussion of the remarkable fact communicated by M. Matteucci—that when the nerve of a prepared limb is laid against the muscles of a frog, prepared according to Galvani's method, on causing the latter to contract, the prepared limb contracts also. To this action M. Matteucci has given the name of *induced contraction.* Its cause he has failed as yet to discover; but his view may be in some measure inferred from the name he has bestowed upon it. The fact appears to us to be one of the most interesting yet discovered in the domain of animal electricity. Du Bois-Reymond accounts for it as follows:—If the transverse and longitudinal section of a muscle be in any way connected by the nerve of the prepared limb, a current will proceed through the said nerve, from the latter section to the former. This current announces itself by the contraction of the muscle of the prepared limb on first making the contact. The contractions cease when the current is fairly established in the nerve, and on breaking the circuit they are again observed. But it is not on the closing or the breaking of the circuit alone that contractions are produced; every sudden fluctuation of the current traversing the nerve is accompanied by contractions. Applying this to the case before us, we find that the current of the muscle against which the nerve of the prepared limb rests, circulates through the said nerve. When the muscle is tetanized, this current is diminished at each convulsive effort, and its fluctuations are answered by corresponding contractions of the prepared limb. The extreme beauty of this explanation cannot fail to strike the reader. In one way alone can it be impaired, and that is by denying that the nerve touches two portions of the muscle in the manner above described; this is exactly what is done by M. Matteucci. Not having seen the experiment, we are unable to offer an independent opinion; but we may be permitted to refer to the decision of the Paris Academy, which is "that the above fundamental fact furnishes a direct explanation of the induced contraction of M. Matteucci."

Hitherto we have abstained from mentioning the manner in which the galvanometer was used in these experiments. It would never do to bring the ends of the galvanometer wire into direct contact with the animal parts; the wires are in reality terminated by plates of carefully purified platinum, which dip each into a suitable porcelain vessel containing a saturated solution of common salt. If the space between the vessels be bridged over by an electro-motor of any kind—an arc of zinc and copper, for example, whose ends dip into the two vessels—the circuit is established, and the current developed will exhibit itself on the galvanometer. To render the process of bridging more easy, bosses of porous paper saturated with the fluid dip into the vessels, and the substance to be experimented with is usually placed across from boss to boss. It is scarcely possible, by mere writing, to give an exact idea of the ingenuity and adaptability of this contrivance, and we must therefore content ourselves with this brief indication of the method pursued.

Casting our thoughts back upon the muscular current and its law of action: if it be granted that this current is developed in the muscle itself, we can scarcely fail to conclude that it is in a state of circulation during the life of the animal. We know that on connecting the transverse and longitudinal sections a current appears; but such connexion exists naturally in the animal body, and hence the inference is a fair one—that such currents are perpetually present, and that the current which we perceive on the galvanometer is in fact but one of the branches of these pre-existing currents. This premised, we are in a position to understand the important facts now to be described The porous bosses were removed from the vessels of salt and water, and a live frog was so placed that its two legs dipped into the two vessels. We know from the experiment of Nobili that a current exists in the frog, directed from the foot upwards, but in the case before us we have two such currents, one from each foot, which meet at the junction of the limbs, annul each other, and consequently produce no effect upon the needle of the galvanometer. But let us suppose one of these currents to be enfeebled, while the other retains its full strength; the result will be, that the excess of the latter current will produce a deflection. The ischiatic nerve of one of the frog's legs was severed, and the limb thus deprived of all power of motion; the animal was then poisoned by strychnia, and strong convulsions followed; the uninjured limb contracted violently, its muscular current was thereby diminished, and the current of the other limb was immediately exhibited by the galvanometer. A single step now carries us to an experiment which forms the climax of this fine inductive argument. Instead of the frog's feet, Du Bois placed the first finger of his right hand in one vessel, and the corresponding finger of his left hand in the other; but instead of cutting his nerves, as in the case of the frog, he suffered the left arm to remain at rest, and contracting the other forcibly, produced a deflection of the needle. When the left arm was contracted and the right one suffered to remain at rest, the needle was deflected in the opposite direction. The current always proceeded from the hand of the contracted arm to the shoulder; but remembering the fact that it is the excess of the current of the motionless arm which is here observed, we are led to the inference that in the normal state of the arm the direction of the current is from the shoulder to the hand.

The publication of this result created a considerable sensation; it was received by many with doubt and misgiving. Some eminent men undertook to repeat the experiment; their results were negative, and for a time the opinion was predominant that Du Bois was in error, and that M. Humboldt, who took a conspicuous part on the affirmative side of the question, had suffered himself to be misled. The fault, however, rested neither with Du Bois nor with M. Humboldt. Those who attempted to make the experiment had neglected its prime conditions, and their failure was a matter of course. The fact indeed is undeniable, whatever may be the fate of its explanation. We have ourselves repeated the experiment ten or fifteen times in the course of an afternoon, and always with the same constant result.

A most remarkable deportment of the nerves, discovered by Du Bois-Reymond, is now to be noticed. Let a long nerve be imagined, with its transverse section placed against one of the porous bosses already mentioned, and a point of its exterior surface against the other boss; the nervous current will exhibit itself according to the law already stated. Let a portion of the nerve near its free end, and entirely out of the circuit of the nervous current, be placed between the poles of a voltaic battery, and let a current be sent through it. The moment the voltaic current passes, a remarkable change of the nervous current is observed. If the direction of the voltaic current coincide with that of the nervous current, an increased deflection will be exhibited; but if the two currents are opposite in direction, the deflection is diminished. It must be carefully borne in mind that the modification of the nervous current is not due to the irruption of the voltaic current into the circuit of the galvanometer; there is no such irruption. The effect is similar to that produced upon a long bar of soft iron, one end of which is introduced into a helix, through which an electric current passes. This end will be magnetized, but the magnetism is transmitted through the molecules to the other end of the bar. A similar molecular modification must be supposed to occur in the case of the nerve, which increases or diminishes its proper action according to circumstances. To this state of the nerve the term electro-tonic has been applied, a term first introduced by Faraday, to express a state of matter which Du Bois considers to be in some respects similar to the above.

The important idea is here suggested, that the transmission of an impression to the brain is effected by a molecular change, which takes place along the line of transmission. The velocity of this transmission may be a quantity capable of accurate measurement. The important researches of Helmholtz,* indeed, prove that the moment an impression is made by no means coincides with the moment when it becomes evident to consciousness. If two portions of the skin be simultaneously excited, both excitations travel forward along the nerves to the brain; but the impression made upon the point nearest to the brain arrives first. If a harpoon be struck into the tail of a whale 80 feet long, the animal, in all probability, is not conscious of the wound for a second after its infliction; and before the command to strike can be transmitted through the motor nerves to the tail, another second probably elapses. In the human subject Helmholtz finds the velocity of nervous transmission to be about 195 feet a second, and in the frog 86 feet a second. It would be of the highest interest to examine whether these numbers do not express the speed with which the electro-tonic condition is transmitted through the nerves.

In his later memoirs, Du Bois has given us the results of an elaborate investigation of the influence of various states of the skin in the production of currents. By heating the parts unequally, by immersing the skin for unequal periods in the solution which contains the platinum terminals of the galvanometer, by stretching the skin, by removing it altogether by means of blisters, by causing it to perspire unequally, he has opened to us a series of electro-motive agencies, which while they excite our astonishment, impress us forcibly with the extreme caution necessary to the successful prosecution of researches upon this subject. As in the moral world improved means imply increased responsibilities, so here in the physical, the use of instruments of such surpassing delicacy, demands a proportionate watchfulness on the part of those who use them. But whatever the fate of animal electricity, as a distinct portion of science, may be, these investigations will remain as records of masterly experimental skill, and of consummate ingenuity in combating the million difficulties by which the subject is surrounded.

John Tyndall.

Review IX.

1. *Traité pratique des Maladies Vénériennes, contenant un chapitre sur la Syphilisation, &c.* Par S. G. Maissonneuve, M.D., et H. Montanier, M.D.—*Paris*, 1853. pp. 564.

Practical Treatise on Venereal Diseases, with a chapter on Syphilization.

2. *Traité des Maladies Vénériennes.* Par A. Vidal (de Cassis).—*Paris*, 1853. pp. 549.

Treatise on Venereal Diseases.

3. *Rapport à M. le Préfet de Police, sur la question de savoir si M. le Dr. Auzias Turenne peut être autorisé à appliquer ou à expérimenter la Syphilisation. à l'Infirmerie de la Prison St. Lazare?* Par MM. les Docteurs Melier, Philippe Ricord, Denis, Conneau, et Marchal (de Calvi).

* Philosophical Magazine, November, 1853.

Report to the Prefect of Police, on the question whether Dr. Auzias Turenne be permitted to practise or experiment on Syphilization, in the Infirmary of the Prison St. Lazare?

4. *Syphilitic Diseases, their Pathology, Diagnosis, and Treatment, including Experimental Researches on Inoculation, as a differential agent in testing the character of these affections.* By JOHN EGAN, M.D., M.R.I.A.—*London.* 1853. pp. 346.

5. *Observations on Syphilis, and on Inoculation as a means of Diagnosis.* By JOHN CROWCH CHRISTOPHER, M.R.C.S.—*London,* 1853. pp. 74.

6. *Traité pratique des Maladies Vénériennes.* Par le Dr. PHILIPE RICORD.—*Paris,* 1851.

7. *La Sipilizzatione studiata qual mezzo curativo e preservativo delle Malattie Veneree.* De CUSIMINO SPERINO.—*Turin,* 1853. pp. 903.

Syphilization treated as a Curative Means in the Venereal Disease.

8. *Traité des Maladies Vénériennes, contenant le recit d'une tentative de Syphilisation, et de plusieurs expériences d'inoculation pratiquées sur les animaux.* Par le Dr. MELCHIOR ROBERT.—*Paris,* 1853. pp. 502.

Treatise on Venereal Diseases, with an account of an experiment on Syphilization. and of many inoculation experiments on animals. By Dr. MELCHIOR ROBERT.

(*Concluded from vol. xxiv. p.* 398.)

PURSUING our plan of investigating the morbid processes to which the syphilitic poison gives rise, we come to the consideration of its effects upon the lymphatic system.

The opinions of Hunter concerning the use of the absorbent vessels, have given a bias to all subsequent reasonings upon this subject; and the theories based upon his experiments are commonly received, even up to the present time. Hunter, assisted by his brother, and by Drs. Clayton, Fordyce, Michaelson, and others, found that when he confined some warm milk in a portion of small intestine, and tied the artery and vein which supplied it, the lacteals of the part were presently filled with the white milk. Upon puncturing the vein upon the distal side of the ligature, it was soon emptied of its blood by pressure with the finger; but no white fluid could at any period during the continuance of the experiments be seen in the vein. Upon a repetition of the experiment, in which the circulation through the mesenteric vessels was left free, the blood in the vein was carefully examined and compared with that in the neighbouring veins, but it was found not to be light-coloured, nor milky, nor could any difference whatever be detected in it. It was found that even by firm pressure (which was continued until the intestine burst), the milky fluid could not be made to pass into the veins. In another animal, some thin starch, coloured with indigo, was introduced into the small intestine, and the lacteals soon appeared to be filled with a fluid of a fine blue colour. A vein in this part of the mesentery was opened, and the blood which flowed was allowed to separate into coagulum and serum. The next day the serum had not the least bluish cast. An injecting pipe was then fixed in an artery of the mesentery, where the intestine was filled with blue starch, and all communications, both in the mesentery and intestine, were closed, with the exception of the vein corresponding with the artery. Warm milk was then injected by the artery until it returned by the vein. This was continued until all the blood was washed away, and the vein returned a bright white milk. The milk thus circulating through the intestine containing the coloured starch was not in any degree changed in colour. In a third animal, some musk in warm water was confined in a portion of intestine. After waiting a little time, some of the lacteals of the part were opened with a lancet, and some of the watery fluid which they contained was received into a small spoon. This was found to smell strongly of musk. Some blood received in a clean spoon from one of the veins of the same part, had not the least smell of musk.

From these and similar experiments, Hunter arrived at the inference "that the red veins do not absorb in the human body;" and consequently that the lymphatics were "the only absorbents." (On the Venereal, p. 253.) These premises naturally led to the further conclusion, that poisons were necessarily absorbed by the lymphatic vessels; and accordingly we find Hunter asserting "that the venereal matter is taken up by the absorbents of the part in which it is placed, and carried along the absorbent vessels to the common circulation." (*ib.* pp. 256, 257.)

This view, deriving, as it does, such an apparent confirmation from the frequent occurrence of inflamed lymphatic glands in conjunction with venereal ulcers, has been adopted, with more or less modification, by almost all subsequent writers.

The accuracy of the experiments upon which Hunter based his theory has justly been called in question by other physiologists, but the theory itself, strange to say, has hitherto scarcely been questioned. MM. Tiedemann and Gmelin, after mixing various substances, which might easily be detected, with the food of animals, not unfrequently found unequivocal traces of these substances in the venous blood and urine, whilst it was only in a very few instances that traces of them could be discovered in the chyle. In repeating Hunter's experiments, Mayo proved, that half-an-hour after a solution of starch and indigo had been placed in the cavity of the intestine, the lacteals appeared of a clear blue colour, and those present were for a short time satisfied that the indigo had been absorbed. But upon placing a sheet of white paper behind the mesentery, it was found that the blue tinge disappeared,—the vessels were simply empty. On removing the white paper, they reassumed their blue colour. Thus a repetition of the Hunterian experiments rather goes to prove that the function of the lacteals is limited to the absorption of chyle. But perhaps the most conclusive experiment on this subject is that of M. Ségalas. A fold of small intestine was drawn out of a wound in the belly of a dog. All the bloodvessels passing to and from it were tied but one large artery. A vein, punctured upon the mesentery, allowed the blood to escape, which would otherwise have stagnated in the part. The lacteal vessels and nerves were left entire. The fold of intestine was then tied at both extremities, and an aqueous solution of the alcoholic extract of nux vomica was poured into it. During the hour which followed, the poison produced no symptoms. The ligature being then removed from one of the veins, the blood was allowed to return in the natural course of its circulation. In six minutes from this time the poison took effect. The experiments of M. Magendie, illustrating the same point, in which the poison of the upas tienté was introduced into the system of a dog, through a limb which had no connexion with the body, excepting through the blood-vessels, is well-known.

From these facts, it appears certain that Hunter's idea of the lymphatics being the only absorbents, is incorrect, and we are thence naturally led to the consideration of the value of the theory which was based upon that notion.

An extensive observation of cases of syphilis, will, we believe, establish the two following very important points in relation to this subject: first, that in those instances in which the irritation of the lymphatic glands is the greatest, and where, consequently, we have the best evidence that the morbid matter has entered them, there is very seldom indeed any secondary syphilitic affection; and secondly, that the best-marked cases of constitutional infection are as rarely preceded by any very evident signs of inflammation of the absorbent glands. In the first class of cases, we may trace, in the most satisfactory manner, the progress of the syphilitic virus along the absorbent vessels as far as the first lymphatic gland that it meets. In any part of this course, the poison may be arrested, and produce a fresh chancre, thereby affording unequivocal evidence of its presence. But neither experiment nor observation affords any proof that the virus is conveyed unchanged through these glands. All the evidence which we have upon this subject tends to an opposite conclusion. We find even Hunter asserting:

"We never find the lymphatic vessels or glands that are second in order affected. When the disease has been contracted by a sore or cut upon the finger, I have seen the bubo come on a little above the bend of the arm, upon the inside of the biceps muscle; and in such where the bubo has come in that part, none have formed in the arm-pit, which is the most common place for the glands to be affected by absorption." (Op. cit. p. 260.)

In like manner, after artificial inoculation, we may trace the poison, in many cases, along the lymphatic vessels, as far as the first absorbent gland which they enter. Here, however, some change is produced. Beyond this the specific characters of the poison can no longer be detected, either by its natural effects, or by artificial inoculation. After the virus has undergone digestion or concoction in a lymphatic gland, we no longer have evidence that it is capable, either locally or constitutionally, of producing its specific effects. The grounds upon which it has been assumed that the syphilitic poison enters the system through the absorbent vessels, we therefore consider most inconclusive. The usual mode in which the system does become infected by syphilis, may, we believe, be traced in another and a much more satisfactory manner.

In the development of primary syphilitic disease two processes may distinctly be recognised: one, that by which the surrounding tissues become indurated; the other, by which the same parts are ultimately removed. This second result may be accomplished either in the natural process of growth, by ulceration, by sloughing, or by different modifications of these. But beyond the parts which are involved in these processes other actions are going on, of a more subtle nature, and not so easily appreciable by our senses. In the absence of more positive knowledge, we may ascribe these to the molecular changes in the nutrition of the parts surrounding a chancre. That such actions are in active operation beyond the sphere both of the adhesive and ulcerative processes may be readily demonstrated, although we may be unable to define their exact nature. Were this not the case, we should have nothing to do in the case of a primary syphilitic sore but entirely to remove the indurated and ulcerated tissue, and the disease would, as far as the part is concerned, be at an end. Experience proves that such is very far from being the case.

When a syphilitic sore is removed by excision, as may readily be done when it is situated on the extremity of the prepuce, the cut surface will in a few days take on a specific action. This will occur, as we have witnessed, even when the greatest care is taken not to allow any of the matter from the chancre to come in contact with the cut surface. Such an action taking place in a part apparently healthy, at some little distance from the original sore, presupposes some antecedent change in the tissues in which it originates—a change produced by the infecting poison, but not capable of being appreciated so long as the diseased action had its development in the original chancre. As soon, however, as the first centre of the morbid action is removed, a similar action is commenced upon the neighbouring cut surface. The observation of such cases proves the existence of a subtle morbid process affecting the tissues in the neighbourhood of the part first affected, and necessarily producing *some* effect upon their nutrition. It appears, under these circumstances, much more in accordance with that which is known to happen in the case of other poisons, to suppose, when the constitution becomes affected with syphilis, that the disease is communicated directly to the blood circulating through the parts in which the above-mentioned morbid actions are going on, than to refer the symptoms to the passage of the poison primarily through the absorbent system.

When the constitution becomes affected in consequence of the inoculation of the vaccine or the variolous poison, an affection of the lymphatic vessels and glands certainly forms no essential part of the process. Few, indeed, have thought it necessary to invoke the aid of the absorbent system in order to account for the action of these poisons upon the animal economy, and we believe that it is equally unnecessary in the case of the poison of syphilis.

It has appeared necessary thus to enter at length upon this subject, to prepare the way for the consideration of the somewhat discordant views which we have presented to us in recent works, and especially in order that we may clearly distinguish between the entrance of morbid fluids (even if impregnated with syphilitic matter) into the lymphatic vessels, and the absorption of the syphilitic virus into the constitution.

M. Vidal, with Hunter, acknowledges four kinds of absorption of the syphilitic poison, by means of the lymphatic vessels:

1st. Where the venereal matter has been applied to a sound surface, and has produced no local effect upon the part, but has been absorbed immediately upon its application.

2nd. Where the absorption takes place from an inflamed surface. Such is the way in which a bubo is formed in cases of gonorrhœa, some of which M. Vidal maintains to be really syphilitic, although there may be no ulceration of the urethra.

3rd. Where the syphilitic poison enters the absorbent glands from an ulcerated surface; and, 4th, from a wound. All these four kinds of absorption by the lymphatics we acknowledge, but not as being capable of producing general syphilitic infection.

Independently of these means, a bubo is said, by many authors, to originate from sympathy. M. Vidal justly asks, sympathy with what? Whence does this sympathy originate? How is its influence determined to one particular part?

We must confess that this word "sympathy," as applied to the formation of buboes, has always appeared to us most unintelligible. We can readily conceive that parts which have a natural relation to each other may be affected by sympathy. Thus we can easily believe that the breasts may sympathise with the uterus, or the testicles with the urethra. But that one particular class of absorbent vessels should have a peculiar sensibility for the diseases of another part with which they have no particular connexion (independent of the transmission of their contents), it is difficult to imagine. That an uncertain amount of an unknown influence should in this unknown way be conveyed from one part of the body to another, we cannot but regard as one of the mysteries of an occult science. MM. Maisonneuve and Montanier divide syphilitic affections of the absorbent glands into those in which the glands are enlarged (*engorgement*) and those in which they are manifestly inflamed. To the latter they apply exclusively the term bubo.

"The real bubo," it is said, "is only found with unindurated chancre; the indurated chancre, only, is on the other hand, accompanied with an indurated enlargement of the glands: from this it consequently follows that a bubo properly so called, like the chancre which precedes it, is only a local affection, and never a sign of the infection of the system. If the chancre is already healed, a bubo will indicate that it was unindurated; an indurated enlargement of a gland, on the other hand, will without doubt afford evidence that an indurated chancre has existed, or is still present." (pp. 164, 165.)

"The induration of a chancre is the certain proof of a constitutional infection. It is necessarily accompanied by an indolent enlargement of the absorbent glands nearest the seat of the disease, and in a longer or shorter interval, generally within six months, is necessarily followed by constitutional symptoms, when not prevented by medical treatment." (Robert, p. 211.)

"The bubo is of no consequence, but as a local disease; the enlarged ganglions are of the highest importance in relation to constitutional syphilis." (Maisonneuve, p. 165.)

"A simple chancre accompanied by a suppurating bubo never communicates constitutional syphilis." (*Ib.* note, p. 140.)

M. Vidal does not allow that the condition of the absorbent gland is of the value as a diagnostic sign which is implied above. Inflammatory bubo, he justly says, may come on independently of the absorption of any syphilitic matter, and the indolent non-inflammatory bubo may happen in consequence of strumous affections, as a result of a simple unindurated chancre, or as a secondary syphilitic affection. The observations of

Dr. Egan, again, have led him to conclusions somewhat differing from those of M. Ricord.

"Buboes," he says, "usually consequent on gonorrhœal inflammation are uninoculable, although here a mild description of secondary symptoms is occasionally met with." (p. 39.) Dr. Egan believes likewise that, after abrasions of the mucous membrane, and also after superficial non-indurated primary ulcers (whether the secretion from them is capable of being inoculated or not), a mild form of constitutional symptoms may ensue.

The opinion of the French school, represented by MM. Ricord, Maisonneuve, Montanier, Robert, &c., is, as we have seen, opposed to these views. These gentlemen find, that whenever they have had an opportunity of tracing the natural progress of the disease, an indurated chancre is always communicated by an indurated chancre, and they believe that this alone will give rise to constitutional syphilis.

The experiments and observations of Dr. Egan have led him to the conclusion that there are two syphilitic poisons. From the fact that when the matter of a phagedenic ulcer is successfully inoculated, the result is an ulcer presenting the same characters, he infers "that the virus generated by the simple primary ulcer and the phagedenic sore is as dissimilar in quality as it is in its effects." (p. 54.)

The results of the experiments upon which this idea is founded we regard as manifestly inconclusive, because the inoculations were made on the patients themselves, in whom the primary affections had already assumed a phagedenic character. It is likely enough that, if in these patients one venereal sore presented a phagedenic character, a second or third would do so likewise. In order to be of any real value, the experiment must be performed upon persons who have not previously had the venereal disease, and whose constitutions are free from any unusually disturbing influences. As such experiments, for obvious reasons, cannot with propriety be performed, we are thrown back for our facts to clinical observation and to the information which may have been obtained from inoculating patients already infected. From the former we believe the point cannot be established; and from the latter it has been shown that ulcers produced from the inoculation of the discharge of simple venereal sores will sometimes become phagedenic.

> "If pus, taken from a simple non-indurated sore which has lasted a fortnight, and is accompanied by inflammatory bubo, be inoculated, a simple venereal ulcer will result, provided the inoculation be properly performed. This ulcer may become phagedenic, serpiginous, or gangrenous. But it never will produce an indurated sore." (Maisonneuve, p. 139.)

If, then, the virus from a simple sore will produce a phagedenic ulceration, as we have no doubt it will, we cannot regard the effect as depending upon any peculiarity in the poison.

In reviewing the whole of the works which have recently appeared on the subject of syphilis, we find, after all the experiments which have been made, and after all the attention that has been directed to the subject, that authors are not agreed as to what precise forms, either of primary or secondary venereal affections, are capable of affording a secretion which may be absorbed so as to infect a healthy person. They have hitherto failed to associate the external appearances of particular kinds of primary affections with any definite forms of secondary disease resulting from them as a necessary consequence. We have at present no generally recognised and well-defined mark of distinction between those diseases which are syphilitic and those which are not. Upon the subject of the absorption of the syphilitic poison in particular, it must be allowed that the accounts are sufficiently obscure: and we cannot but be struck with the absence of any sufficient physiological explanation of the real or supposed facts adduced upon the point in the works of the many able men lately published. Something more satisfactory, we believe, may be arrived at by attentively considering the earliest stages of the morbid processes which are involved in the absorption of the syphilitic poison. The great author of this mode of investigating morbid actions has prefaced his treatise on the venereal disease with the following remarks, which we offer no apology for introducing here, as we believe they have never received the amount of attention which they deserve, and have never, since Hunter's time, been applied in their full extent to the illustration of our present subject. "No two actions," says Hunter, "can take place in the same constitution, nor in the same part, at one and the same time; no two different fevers can exist in the same constitution, nor two local diseases in the same part, at the same time."

It might appear strange to any one who had not considered the subject in its physiological relations, that these ideas should occupy so prominent a position in Hunter's work on the venereal disease, and that they should be dwelt upon in this rather than in any other of his writings. We believe, nevertheless, they are the principles upon which much that is apparently obscure in relation to this disease may be explained, and that they afford a remarkable instance of that intuitive insight so peculiar to our great physiologist, by which comprehensive general ideas were appreciated in their extent and simplicity, even where their applicability to particular details might not have been traced.

For truth and clearness, the description of a primary syphilitic ulcer has not been excelled since Hunter's time. "A chancre has commonly a thickened base, and although in some sores the inflammation spreads much further, yet the specific inflammation is confined to this base." This *specific* action, in which the arteries throw out coagulable lymph, depends, according to the Hunterian nomenclature, upon *adhesive inflammation.* The action by which pus is formed is named *suppurative inflammation*, and that which removes parts the *ulcerative inflammation.* These three effects of inflammation Hunter regards as distinct actions, and therefore incapable of being produced in the same part at the same time. Now that which is peculiarly characteristic of the syphilitic action in a part, is a specific adhesive inflammation, which has no connexion at all, and indeed, according to the Hunterian doctrine, is incompatible, with the ulcerative inflammation. And as we have before shown that there is no evidence that the poison is taken into the system by absorption (through the lymphatics), we conclude that ulceration may form an accidental, but not necessary, part of that process. This peculiar form of the primary disease is the only one of an inflammatory nature which we can, in a satisfactory manner, trace as peculiarly and necessarily connected with secondary affections. This specific adhesive inflammation is for syphilis what the vaccine vesicle is for the cow-pox—at once a local manifestation that the disease has affected the tissue of the part to which it has been applied, and an evidence of its subsequent and consequent influence upon the constitution.

It is true that we almost always find that a part affected with syphilitic induration is ulcerated upon its surface, and in the more advanced stages of the disease, that the parts which were at first indurated pass into ulceration. In the first case the adhesive and ulcerative inflammations affect different parts (although in close proximity to each other); in the second these distinct actions affect the same parts, but at different times.

An action commenced in a part will continue until the cause determining it ceases, or until it is superseded by some other more powerful action. If, therefore, the ulcerative action is set up by venereal infection, it will continue until the poison has expended its influence, or the part is attacked by mortification, or some other action of sufficient power to supersede it. Hence it follows that when the ulcerative inflammation has once attacked a part, it can never be followed by specific adhesive inflammation, unless some fresh poison be applied; and not even then, unless the action of the poison be sufficiently powerful to overcome the action already established. The same reasoning holds good with regard to suppurative inflammation. A most important distinction hence arises between those cases of venereal infection which are characterized *in their origin* by specific induration, and those which are accompanied by the ulcerative or suppurative inflammation.

The former class will, with tolerable certainty, affect the system, unless prevented by medical treatment, or the presence of some peculiarity or disease; the latter are never, we believe, followed by constitutional syphilis. Ulceration and suppuration, like mortification, destroy the vitality of the parts which they attack, although in a more gradual manner; and, as the syphilitic poison requires a living nidus for its development, it is destroyed in these actions before it becomes, in the process of growth, taken into the system.

These conclusions, based upon the Hunterian doctrines,* we venture to affirm, may be borne out by practical observation.

If a part inoculated with syphilitic virus be affected from the first with ulcerative inflammation, or if, from the first, there be a free secretion of well-formed pus, or if the parts affected mortify in the early stage of the disease, the existence of the syphilitic virus will cease with that of the parts which it has infected. The disease, as far as its specific characters are concerned, will be a local one.

From what has already been said, it may be inferred that when a part inoculated with syphilitic matter suppurates freely from the first, or is attacked with mortification, specific ulcerative inflammation does not take place: and, accordingly, practically we find that in these cases there is seldom any affection of the lymphatic system.

* It is more than probable that Hunter would himself have arrived at similar conclusions, had not his mind been preoccupied by the idea derived from his physiological experiments of the lymphatics being the only absorbents.

When, on the other hand, ulcerative inflammation is early established, the absorbent glands become suddenly inflamed, and they generally suppurate, be the medical treatment what it may.

It is not intended by anything that is here stated to imply that vitiated fluids may not enter the circulation through the absorbent system, even although the morbid processes which give rise to their formation may have had their origin in the venereal disease. Well-marked cases from time to time present themselves, in which the lymphatic vessels, the absorbent glands, and even the thoracic duct, are found distended with puriform or sanguinolent fluid. Such diseased products poured into the circulation must, necessarily, have a deleterious influence upon the constitution, and must give rise, occasionally, among other symptoms, to eruptions upon the skin, which may, more or less, resemble true syphilitic affections. Such eruptions sometimes follow an inflammatory bubo, especially when it does not suppurate. They usually appear before the primary affection to which they are attributed has subsided; they generally last but for a short time, and do not recur. They subside readily of their own accord without any specific treatment. These affections, as we have said, may depend upon the absorption of inflammatory products resulting from venereal infection, but we cannot regard them as arising from the presence of the syphilitic virus itself.*

A somewhat remarkable case of this form of disease lately presented itself among the out-patients at King's College Hospital. A woman, who had previously had only a leucorrhœal discharge, applied for an ulcer upon her chin; this was circular, excavated, red, and glazed upon its surface. It gradually and slowly increased till it attained the size of a shilling. It was surrounded by a good deal of general induration. The glands under the chin were, from the first, enlarged and indurated, and remained so after the sore was healed; those in the neck were not at all affected during the period of the patient's attendance. The secretion from the sore was carefully inoculated on the patient's arm. On the third day a small red point marked the seat of the inoculation; but on the fifth day this had disappeared, and no results of the inoculation could subsequently be discovered.

Before the sore had healed, an eruption appeared over the body of a reddish brown colour, and covered by thin scales. This affection we could not, from its characters alone, have distinguished from the commencement of a syphilitic eruption. It, however, disappeared in a few days, and did not return. No specific treatment was used in this case.

The syphilitic poison presents several peculiarities in its mode of absorption, upon which many of the peculiarities of the diseases to which it gives rise appear to depend.

There are three modes of absorption usually recognised.

1. When poisonous substances are applied to an internal and vascular membranous surface, or are introduced into a wound, or by friction upon the surface of the body are forced through the epidermis, so as to enter

* This class of cases is probably included in Dr. Egan's "mild form of constitutional symptoms." We may have on one side an ulcerated surface producing an inflammatory bubo, and on the other, an indurated sore producing a chronic enlargement of the glands only. The system will, under these circumstances, become infected; but the disease on the side where the suppurating bubo is, will not have contributed to such infection.

into and affect the system, they find their way directly into the blood, through the coats of the bloodvessels.

2. The chyle formed during the digestion of the food is taken up from the mucous surface of the intestines by the lacteals.

3. When the molecular structure of the body is absorbed* in the removal of parts which are not at the same time replaced, as happens in ulceration, the lymphatics are the agents employed.†

From what has been said, it will appear that the syphilitic poison is absorbent in none of these ways, and therefore that the usual laws which apply to poisons absorbed by any of these means do not necessarily apply to syphilis.

From experiments which have now been far too often repeated, it is proved beyond a doubt that the syphilitic poison may remain in contact with an abraded mucous membrane, or be inserted beneath the cuticle, and allowed to remain for two, three, or four days, and no absorption will take place. If, at the expiration of this time, an action is set up which is incompatible with specific adhesive inflammation,—if, for instance, the part is made to slough by the application of caustic,—no effects of the poison will anywhere be perceived. From this it is evident that a certain time must elapse (during which the poison enters into a kind of combination with the part to which it is applied, and produces in it a specific action) before any absorption can take place. This requisite period of incubation it is that secures the system against infection in cases where from the first ulcerative or suppurative inflammation has taken place. A part in the course of being contaminated becomes by these processes dissolved or removed before the act of absorption can be completed. Fresh parts may continue to be attacked, but these, in their turn, are destroyed before they can act as the channels of infection to the constitution. Hence arise often extensive local intractable ulcerations, which are not followed by any secondary symptoms.

It may be asked how it is, if a primary ulcer can produce a bubo which may be proved by inoculation to be syphilitic, that the poison is not absorbed from this fresh source? The answer to this is implied by what has gone before. The inflammation excited in the bubo will be, in all probability, of the suppurative or ulcerative kind, and these actions, as we have seen, are not compatible with the absorption into the system of the syphilitic poison from the parts in which they occur. We may add to this, that experience proves that every time that a fresh inoculation takes place in the same individual, and from the same original source, the effects of the poison will show themselves with less severity. We have here the secret of the absence of constitutional results from the numerous experiments on syphilization alluded to in the first part of this article. The inoculations, however often repeated, produce directly suppuration or ulceration, and consequently are not followed by any general consequences. When a gland is affected with syphilitic ulceration or suppuration, the conditions

* We have noticed one form of bubo which appears to depend in an especial manner upon molecular absorption, independent of ulceration. After an unindurated sore has healed, as the general surrounding thickening disappears, one or more glands will become greatly enlarged. They will not suppurate nor be followed by any constitutional symptoms. This we have observed particularly after ulcers produced by inoculation.

† Mayo's Physiology.

are somewhat similar to those under which a patient is placed by syphilitic inoculation. Many small spots of syphilitic ulceration and one large spot, would, we conceive, be nearly equivalent to each other. In neither case is the poison absorbed. This circumstance it is that has given some of our continental brethren the ideas—1st, That syphilization is a protection against syphilis; and, 2ndly, That each person can only have constitutional syphilis once during his lifetime. It is, we believe, undoubtedly true that during the time that a syphilitic sore is undergoing ulceration or suppuration, any fresh sore which may be acquired will immediately do the same, and we believe it possible that this tendency may be kept up, by repeated inoculation, for a considerable time, during which no fresh absorption of syphilitic poison will take place. The same immunity, we believe, would be afforded to the system by an ulcerating or suppurating bubo, during its continuance, and perhaps for a considerable time after it had healed. But in either case, allow an interval to elapse, during which this tendency to ulceration or to suppuration shall have worn itself out, and the system will again become subject to genuine syphilitic infection, and be again liable to fresh forms of secondary disease. We confess that we are surprised at the confidence with which some of the French surgeons speak in reference to the occurrence of constitutional syphilis once only during the lifetime of each individual.

"For a long time," say MM. Maisonneuve et Montanier, "we have sought in vain for a person who has at two different times had an indurated chancre, and we have not yet found such a case. Lately M. Diday, the senior surgeon of the *Antiquaille de Lyon,* defied any one to produce a well-authenticated case of a person having twice had constitutional syphilis; and we believe that no one has hitherto responded to his challenge. The law is easily understood. It is the same for all diatheses. Constitutional syphilis is a disease under which the patient either recovers, or under the influence of which he remains for the natural period of his life. Once cured, he has acquired an immunity from the disease for the rest of his life. Is it not the same with the small-pox, the cow-pox, &c.? We are, however, open to admit, although no cases have hitherto been recorded in the annals of science, that as a patient might have the small-pox twice, as an exception to the general rule, so might he twice have constitutional syphilis.

"This by no means proves that a person having had an indurated chancre may not contract other sores, or may not be inoculated an indefinite number of times, only that these sores will not become indurated, and consequently will not give rise to constitutional symptoms." (pp. 38, 39.)

Let us test these principles, first by what these authors themselves say in other places, and then by observation. At page 15, in a passage quoted at length in the former part of this article, these gentlemen affirm, in describing the results of the inoculation of pus from a chancre, that, "after the fifth day the subjacent tissues which before had undergone no change, or were only slightly œdematous, now become *infiltrated* and *hardened* by the effusion of plastic lymph, which to the touch gives the sensation and resistance of certain forms of cartilage." Now, as these surgeons profess to inoculate only those who are already the subjects of the disease, and affirm that a medical man is never justified in performing the experiment under any other circumstances whatever, (note, p. 139,) we naturally ask, Whence did these gentlemen gain their knowledge? By their own showing, the assertion made in one part of their work, that a patient could only have one indurated chancre, is contradicted in another, when it is said that inoculation will give rise to a second.

Experience likewise fails to confirm the views in question. Cases every now and then present themselves, in which, after patients have had constitutional syphilis, they contract indurated chancres. We have at present under treatment a gentleman, who returned to England after an absence of two years, having, before his departure, had a syphilitic sore, followed by an eruption, the stains of which were evident upon his return. He contracted fresh disease during his stay in London. Two well-formed circular and indurated chancres presented themselves on the glans penis. These were followed, in a few weeks, by a well-marked crop of syphilitic lepra, of a bright copper colour, and quite distinct in appearance to the dark-brown stains of the first eruption. Similar cases might easily be multiplied, but we conceive this to be unnecessary. In such instances, one of two things must happen: either the sore which first produced the disease must have been unindurated, or else the patient must have had two indurated sores. Both these events are considered impossible, under ordinary circumstances, by the authors whom we have quoted.

We conclude, from all the observations which have been made, that a person may have an infecting syphilitic sore at any period of his life, whether he has had a similar affection before, or not; but that during the time that the system is under the influence of the syphilitic poison, the sores produced by any fresh infection will have a tendency to pass directly into suppuration, or ulceration, or mortification (including phagedæna); that the disposition to any one of these actions may be continued in the system after all other traces of the poison by which the system was first affected have ceased; that as long as this disposition lasts, the system is comparatively secure from any fresh infection, because these actions are incompatible with that by which the poison is absorbed.

It must, however, be borne in mind, in reference to these processes, that any one of them may succeed the specific adhesive inflammation, and then it will not, of course, prevent the infection of the system, which may already have taken place; nor will the character of the disease then afford evidence of its having been thus infectious.

The specific adhesive inflammation may continue a very short time; it may be of a very limited extent, sometimes not exceeding in depth the thickness of a layer of parchment; or it may be masked by surrounding general infiltration. These accidental peculiarities will not prevent the entrance of the poison into the system, but the circumstances attending them will influence the form under which its secondary effects appear.

The existence of non-inflammatory modes of contagion, such as the absorption of the syphilitic virus without any change of structure in the part to which it is applied, or the transmission of constitutional syphilis by means of mucous tubercles, or excrescences not having an inflammatory origin, forms a subject for separate consideration.

Henry Lee.

Review X.

1. *Homœopathy : its Tenets and Tendencies.* By J. Y. Simpson. Third Edition. 8vo.—*Edinburgh ; London*, 1853.
2. *The Sophistry of Empiricism.* 8vo.—*London*, 1853.

In religious or moral inquiries we have definite courts of appeal for the settlement of doubtful points, and for testing the accuracy of views, statements, or opinions. We appeal either "to the law and to the testimony," (and "if they speak not according to these, it is because there is no truth in them,") or to our own conscience, which either "excuseth or accuseth." It is true that different interpretations of meaning may be given by different apparently equally educated men, to the same *dictum;* but, *cæteris paribus*, they agree so much in the main, that sufficiency for rule of conduct and life is attainable from these sources, even by the "wayfaring man, though a fool." Religion and morality are psychically essential in society, as food and raiment are physically; hence they are more or less practically or theoretically viewed by all thinking men. Science, in the ordinary sense of the word, is not essential; and the so-called "departments" of science appeal with interest only to certain definite classes, which classes, even in combination, form quite the minority of mankind. We shall presently see that this is a necessary condition of society, and that science grows slowly, and almost invisibly; that it is as a shut book to the majority, and that a thorough appreciation of any one even elementary subject, may demand a considerable acquaintance with several departments of science. For example: who could understand astronomy, without the knowledge of mathematics? or physiology, without the assistance of chemistry, botany, and anatomy? And each one of these subjects has, in like manner, its dependencies. So that, really, there is nothing like isolation in any department of science. Its various subjects are distinct for a time, like the springs and streamlets of a mighty river, but like them converging to its course, and intermingling their particles in its onward flow to the ocean, which ocean again gives forth its clouds and mists and rains, again, in their turn, to become springs, streamlets, rivers, and oceans. And yet we find men, with a temerity equalled only by their ignorance, asserting without evidence—believing without facts—experimenting without observation—manufacturing sciences, forsooth, out of clever-sounding words and showy tricks; and palming them off—upon whom? My most credulous public—the million; and the million likes it—and pays for it—and flatters itself that it is scientific—and congratulates itself on the "progress of knowledge," and buys a guinea's worth of ready-made science; or goes and hears a lecture on mesmerism, phrenology, globulism; or purchases a book on hydropathy, table-moving, or ghost-seeing, or spiritual rappings. And others like it—"*media*" like it—we mean the discoverers of these sciences. Of course they like it—they have every reason to like it. It is Australia and nuggets, without the journey or digging, to them—with the reputation of being the founders of *new sciences*, for so the million designate these delusions.

In religion and morality they have the best guides—but in science, nothing beyond common sense, which, though they may possess it, they cannot always use it, for they have not learned "the trick of answer." Nothing, however, is so essential to them as a knowledge of the sources of fallacy in scientific observation. Now, in reviewing these works and systems of popular delusion, we shall *first* examine the natural progress of scientific truth, and, *secondly*, give certain popular tests of scientific accuracy, which may form a "law and testimony" to those who have neither the time, nor inclination, nor "knack," to think out *tests* for themselves.

A knowledge of the sources of error is the truth-seeker's best safeguard; for error is everywhere, oftentimes concealing the truth utterly, and oftentimes so simulating its qualities, that it is mistaken for it. Before we decide upon the question of the right or wrong of anything, we must first settle what is the *kind* of inquiry we are about to institute. Is it to determine the error of truthfulness, or the truthfulness of error?—and even this decision may be a source of fallacy, for our judgment may be warped, or our reasoning illogical. We verily believe that there are few errors so erroneous, or few fallacies so fallacious, but that they contain in their history some gleam, if not a remnant, of truth; and as error is the negation of truth, or its absence, though a quality positive in its action, so there is no truth so pure, so brilliant, or so absolute, but that it may be sullied, dimmed, or shaken by error—not ultimately so, for truth is eternal, but for a time; for error, though of short duration, is like "sorry weeds of rapid growth."

We are about to speak of pseudo* science. Now, what is SCIENCE?

The derivation of the term partially answers the question—("*scientia*"†) KNOWLEDGE; but its conventional usage implies something more—"knowledge systematized," "knowledge arranged in departments," &c. The imperfection of our acquaintance with the secrets of nature, and of our capacity for the absolute acquisition of *all*‡ knowledge, forces upon us the necessity of division and subdivision of science, which is *one*, a perfect entity, into parts and parcels.

To investigate the details of these particular divisions, to group them under general principles, to view their co-relation with other general principles in other divisions of this systematized knowledge, so as to trace the phenomena of the whole range of the so-called sciences, physical and metaphysical, to their *ultimate facts*,—is the highest ambition and the most steady aim of the natural philosopher.

Imbued with a perfect love of truth-seeking, he commences this inquiry acknowledging two great principles:

First, that there *are* ultimate facts in nature—points beyond which he cannot go, bounds to his finite reason.

Secondly, that science is perfect in proportion to the *paucity* of its ultimate facts.

Facts which are ultimate now, may have a resolution in some more distant law as yet unknown. The full appreciation of this feeling, therefore, is a difficult attainment, and humiliating to a degree; elaborate pro-

* "Ψευδής, false, lying, deceiving."

† Scientia was used in a subjective sense by the Romans, *not* in the objective, like "*doctrina*," or "*disciplina*."

‡ In connexion with this idea, the reader might consult the views of Bishop Butler in his fifteenth sermon, "Upon the Ignorance of Man," with great advantage.

cesses of logical induction, yielding magnificent results in any particular inquiry, have to be forgotten; and the results themselves, which seemed the very goal of a most worthy ambition, serve only as the starting point for a new view, like the apparent limit of a vanishing perspective.

Philosophers are not, like poets, born philosophers—they have to make themselves so; they are instructed by that great schoolmaster, Observation, and they educate themselves by reflection and judgment. The reason that we can learn better than our ancestors—that is, more in the same time—is not that there is proportionably less to learn, but that everything is more systematized, and hence appears less, because it is not scattered. Our "helps to read," and think, and experiment, are better—and posterity's will be better than ours, if we do our duty. In this world, this school of science, we find nothing ready made, save the implements and subject of study; but we do find better tools and better appliances than our ancestors possessed. 'Tis nothing that you boastingly point to Optics, Chemistry, Physiology, Mechanics, and say, See how these have suddenly enlarged!—how improved—how perfected! All this is true. But why is this? It is not contrary to the law that science is necessarily of slow growth, for experience and the evidence of our senses teach the reverse; the reason of these rapid strides is, that after ages of incubation, the fulness of time has arrived in their special histories. The April shower descends precipitately and unwarningly from a sky a moment ago blue and serenely beautiful, but its cloud has been long forming, from the accumulated myriads of invisible vesicles of vapour. A coat of mail is knitted and jointed in a few hours, of which the numberless links each take as long to form. And so with these apparently rapid growths of science; they are not the evolution of one mind, of one moment, or of one generation, but the developments of patient study, of quiet speculation, of steady and repeated experiment. Convinced of this, every scientific man looks with doubt, great doubt, on ready-made sciences. That, mushroom-like, they should spring up in a night, is, at least, a most suspicious feature in their character, if not an absolute *test* of their fallacy. Wisely he suspends his judgment, if he be not prepared to deny their claim to truth, until, by thought inductive and deductive, with experiment, he have tested their accuracy. Then he boldly denies their premises of argument; or admitting these, their modes of reasoning; or admitting both, their powers or qualities of observation. And how is he met? He is referred to Galileo, and told of his sufferings for truth, or to Harvey and his persecution. He is told that the "blood of the martyrs is the seed of the church," and all such illogical effrontery—effrontery as far as they apply to him; for such assertions are the very *petitio principii* in dispute. Admit them martyrs—Galileos or Harveys—and the question is settled: they are right, and you are wrong; they are persecuted, and you are a tyrant. But mark the difference. How did Galileo arrive at his conclusions, and what use did he make of them? Where is the analogy? . . . How absurd this line of argument!—they require of you to believe in a dogma of their own creation, because some one did not believe in certain dogmas not of their own creation, and were wrong; and they never, of course, show you the analogy between the cases, either in doctrine, discoveries, processes, or results. That is to say, *somebody did not believe something which turned out to be correct, therefore you should believe everything, lest you should fall into the same error!*

If this line of argument lead to anything, it at once tends to stop all inquiry into the truth of assertion or experiment, and demands a tacit belief upon the *ipse dixit* of anybody. Not that we would have the philosopher or scholar troubling his mind with all the trifling trash of the petty inquiries instituted by these pseudo-sciences; he should have higher aims. With a few commonplace tests, they are precipitated from all position, and can hold no dominion in his judgment. Thus far, then, we have given one or two tests or hints at them, by which the "spirits may be tried." But before applying them exactly, let us see how the *real* philosopher proceeds in *his* inquiries. Things appear strong by contrast.

Unprejudiced by preconceptions, his search is after truth—for truth's sake. He is not bent from the even tenour of his way by difficulties, dangers, or disappointments. He loses not the means of success by solely contemplating the goal of his ambition. His life is action, and he is satisfied even with no positive results to an inquiry instituted, provided he be sure the inquiry was logically reasoned. *Negative* results are to him frequently as satisfactory as the most positive sequences, and he would rather give up an hypothesis upon which rested a most beautiful and plausible theory—yes, after cherishing it most dearly—than sell his mind to deception, or give himself up to "believe a lie." He does not jump at conclusions, reasoning from particular instances to general principles. He does not deal in assertions whose accuracy he cannot demonstrate or logically prove. He is never tired of accumulation of evidence, nor in an investigation does he cry out at any moment, until he have conquered the difficulty, "Hold, enough!" Quietly, steadily, patiently, noiselessly he works—by the midnight lamp or in the laboratory, the dissecting room or the studio, impatient only of interruption. He wants not the plaudits of an admiring audience, nor an advertisement in the *Times*, to tell of his discoveries. Your real discoverer is generally too captious of error to court scrutiny till "time and place shall serve," and then too modest to receive praise. Yes, the mental satisfaction of knowledge acquired is too ethereal an exaltation to be approached by the sordid enjoyment which comes from the flattery of others.

But let us now proceed to examine the sources of error in scientific inquiries.

There are twelve very common sources of error, which, if known, may serve as TESTS in the investigation of the accuracy of any subjects, which, either from their intrinsic merit or the weight of authority, are presented to the acceptance of our belief:

I. Errors arising from mistakes about the meanings of terms.
II. Errors arising from the substitution of names for things.
III. Errors arising from the substitution of assertions for facts.
IV. Errors arising from illogical reasoning from correct data.
V. Errors arising from illogical reasoning from incorrect data.
VI. Errors arising from logical reasoning from incorrect data.
VII. Errors arising from partial instead of complete observation.

VIII. Errors arising from mistakes in observation.
IX. Errors arising from incapacity for observation.
X. Errors arising from the innate love of the marvellous.
XI. Errors arising from the system of discipleship.
XII. Errors arising from the system of leadership.

We will now consider these *seriatim*, illustrating what we conceive to be fallacies from various works on systems of popular delusion, whose names, however, do not appear at the head of this article, simply because we would not give them the individual importance that an appearance in a really scientific journal would demand for them.

I. ERRORS ARISING FROM MISTAKES ABOUT THE MEANINGS OF TERMS.

Mistakes arising from a different appreciation of the meanings of terms, is one of the most common sources of error—one, too, into which even the conscientious and scientific may fall without being aware of it. Hence, it is of the highest importance to define clearly all words used in argument or dispute before commencing any set of inquiries. Very often the dispute is ended by such a definition, so that those a moment ago antagonistic and ready for severe strife, find they are of the same opinion on the point in hand. Words are dangerous weapons, they require careful but tight grasping, and when used either in attack or defence should be so equally matched and presented, that, as in a "fair pass of foils," it matters not which we or our antagonist may select, both having the same appreciation of the same weapon.

There are certain words constantly occurring in scientific and pseudo-scientific writings, which have become as it were stereotyped, and which, if they have not by usage been utterly worn from their original impression and sharp outline, still want "biting up." Some words become obsolete for want of usage, but more become obsolete in their exactest signification from being too frequently used, or rather *ab*used; they have palled upon the sense as does monotony on the ear. Take a set of hackneyed words, such as "cause," "effect," "phenomenon," "inquiry," "hypothesis," "theory," "fact," "experiment," "law."

Now, to define these words *here* is not our task—we will reserve this for a future occasion and another place. We merely wish to state, that these words are "suspicious characters,"—we must keep an eye on them—watch them with carefullest scrutiny. Remember that an INQUIRY is not merely asking a question—that EXPERIMENT is not merely *doing* something—that HYPOTHESIS is not merely the Greek for the Latin "supposition," or the English "guessing"—that THEORY is not merely something visionary, or that which is not practical—that PHENOMENA are not merely *wonderful* appearances, or necessarily so—that the word FACTS among such terms is most of all suspicious—very dangerous indeed. We must look very carefully into every sentence which begins thus—"It is a well-known fact"—the more especially if that sentence have a "therefore" in it, or "consequently," or "so that," or any notion of a sequence.

We must remember how we use the terms CAUSE, EFFECT, LAW—that which we call "cause" is at best only an effect of a great cause, and some so-called causes are probably only minor effects of some major cause unknown to us; that effects are resultants of our admitted causes, and that they are as dependent upon one another as rivers and seas. And we must remember, lastly, that to know the meaning of the term LAW in scientific language, requires considerable instruction and much more education.

So much, then, for the importance of attending to the meanings of words, and the necessity for the utmost exactness of definition in scientific argument. The abuse of language arising first from its own imperfections as a vehicle of thought; secondly, from *our* want of exactness and skill; and, thirdly, from our *opponents'* non-appreciation of our positive meaning—is a most common and fruitful source of error. It extends to sentences, phrases, formulæ, and can be guarded against only by rigid discipline and conscientious motive.

II. ERRORS ARISING FROM THE SUBSTITUTION OF NAMES FOR THINGS.

"What's in a name? that which we call a rose by any other name would smell as sweet." Of course it would to Juliet, because she was aware, in the case in hand, that the qualities of "the thing" were not altered by this substitution; nor would it be of the slightest moment if we were, with Juliet, in all the cases to which we refer, equally informed. But this is not so, and it is impossible it could be so at all times. From several works before us upon "Spiritual Rappings," "Clairvoyance," "Mesmerism," &c., we can find numerous examples; let us analyse the first, namely, "Spiritual Rappings." The *thing* in question, that is, admitting it to be something, is a manifestation from the spirit-world to this natural world, and through our material bodies to our immaterial minds. The book says, "The facts are these" (reader, beware of "facts")—"the medium sits or stands in a room near a table, the inquirer asks, 'Are there any spirits present?' whereupon a tap or flutter is heard in affirmatory answer; 'Will the spirit of ——— appear?' is next inquired, and sometimes the request is granted and ——— spirit appears! Any question put by the inquirer through the medium is then answered, either in relation to the past, the present, or the future." Without proceeding further, let us observe, that sound is a physical phenomenon produced by the forcible meeting of two material substances, and thereby causing such a set of undulations in the atmosphere as shall, by striking upon the tympanum of the ear, give that sensation to which we apply the term "hearing." Now, if these sounds be physical, as all sounds must be, they are necessarily not spiritual, for spirit is immaterial, and according to this definition, can produce spiritual or immaterial effects only, unless, as in its conjunction with an organism or body, it have a material agent and suitable apparatus. "Rappers" do not point out any such agency, nor show any organism; nay, they boldly assert, and get the spirits to endorse their statements, that spirit, and spirit only, produces this sound. But we as boldly affirm, and that upon experience and experiment, that if there be "rappings" they must be physical, and if physical they cannot be spiritual. Now, at first, it would appear that this subject scarcely comes under this division, inasmuch as we have shown that it is the substitution of a name for *nothing;* yet, it must be remembered that what we call "nothing," and believe to be nothing but gross deception, these "rappers" vaunt as equal in integrity to the prophecies of old, or the direct revelations of God to man, and then impudently attach it to the physical sciences as a method of physical research.

The worst effect of a nomenclature is, that the moment we have given anything a name, it more or less stops inquiry into its history and nature, especially if the name have a popular signification. In cases like the present, where a positive name is given absolutely to nothing, it gives that nothing an existence in the minds of the uninitiated, and thus far has a creative power, so to speak, that is, makes something out of nothing; but in other cases where a name is given, which more or less defines some qualities of the thing described, the thing having a real existence, this naming too often stops inquiries, as in the other case, and we substitute it for the qualities of things; that is, if the thing be named properly some of the qualities are substituted for all the qualities, or the mind rests satisfied with some while we might know many more. This is remarkably the case in all partially developed subjects, and is at the same time a cause of that partial development. In therapeutics, for example, opium has been called a "narcotic;" many persons whenever they want a narcotic give opium, not as opium, but as a narcotic, and only a few look into the thing further and perceive that opium is something more, and is a compound of many diverse and extraordinary substances of different effects—and so we might say of almost every vegetable drug in the Pharmacopœia. Now, what is the result of all this? why, our therapeutics consist in great measure of a system of names, and how could we expect otherwise?—names are prescribed, that is are substituted, for things, and of course you can get names only as the result.

Thus, this source of error is not confined to the pseudo-sciences, but certainly is most richly developed in them, as might be expected; and in real sciences is found, as before hinted, in those principally which from partial development have fewest absolute facts.

A balloon ascends, a stone falls, and if we ask any semi-scientific man the cause of these apparently diverse phenomena, he considers that he has acquitted himself as a scholar, if not something more, when he answers flippantly, with a supercilious pitying of your ignorance, "gravitation;" we are, however, not satisfied, although we believe in "gravitation," and while we leave this scholar to tell us *what* gravitation is, and how much there is of the substitution of a name for things in *his* use of the term, we will go on to the next head—viz.

III. ERRORS ARISING FROM THE SUBSTITUTION OF ASSERTIONS FOR FACTS.

We know the value of "on dit" in relation to all inquiries, and most of us look suspiciously at any statement backed only by "they say so;" and yet many will readily believe anything which appears in print—"it must be true," as is a common expression, "because I read it in a newspaper," or in a book, forgetting what Burns has sung—

> "Some books are lies frae end to end;"

and if we might pay attention to the *traditional* pseudo sciences we might continue—

> "And some great lies were never penned."

Now we have before us a book of the class referred to, "Table-Moving, by a Physician," a most appropriate name, effect, and cause, neatly combined! for when the table did move it was most likely by the assistance of the physician. But Professor Faraday's experiments and writings in connexion with this subject so thoroughly settled the question, that we need do no more than quote part of them; referring the reader for further information to the 'Athenæum' for Saturday, July 2nd, page 801:

"The proof which I sought for, and the method followed in the inquiry, were precisely of the same nature as those which I should adopt in any other physical investigation. The parties with whom I have worked were very honourable, very clear in their intentions, successful table-movers, very desirous of succeeding in establishing the existence of a peculiar power, thoroughly candid, and very effectual. It is with me a clear point that the table moves when the parties, though they strongly wish it, do not intend, and do not believe that they move it by ordinary mechanical power. They say, the table draws their hands; that it moves first, and they have to follow it,—that sometimes it even moves from under their hands. With some the table will move to the right or left, according as they wish or will it,—with others the direction of the first motion is uncertain;—but all agree that the table moves the hands, and not the hands the table. Though I believe the parties do not intend to move the table, but obtain the result by a *quasi* involuntary action,—still I had no doubt of the influence of expectation upon their minds, and through that upon the success or failure of their efforts. The first point, therefore, was, to remove all objections due to expectation, having relation to the substances which I might desire to use:—so, plates of the most different bodies, electrically speaking—namely, sand-paper, millboard, glue, glass, moist clay, tinfoil, cardboard, gutta percha, vulcanized rubber, wood, &c.—were made into a bundle and placed on a table under the hands of a turner. The table turned. Other bundles of other plates were submitted to different persons at other times,—and the tables turned. Henceforth, therefore, these substances may be used in the construction of apparatus. Neither during their use nor at other times could the slightest trace of electrical or magnetic effects be obtained. At the same trials it was readily ascertained that one person could produce the effect; and that the motion was not necessarily circular, but might be in a straight line. No form of experiment or mode of observation that I could devise gave me the slightest indication of any peculiar natural force. No attractions, or repulsions, or signs of tangential power, appeared,—nor anything which could be referred to other than the mere mechanical pressure exerted inadvertently by the turner. I therefore proceeded to analyze this pressure, or that part of it exerted in a horizontal direction—doing so, in the first instance, unawares to the party. A soft cement, consisting of wax and turpentine, or wax and pomatum, was prepared. Four or five pieces of smooth, slippery cardboard were attached one over the other by little pellets of the cement, and the lower of these to a piece of sand-paper resting on the table; the edges of these sheets overlapped slightly, and on the under surface a pencil line was drawn over the laps so as to indicate position. The upper cardboard was larger than the rest, so as to cover the whole from sight. Then, the table-turner placed the hands upon the upper card,—and we waited for the result. Now, the cement was strong enough to offer considerable resistance to mechanical motion, and also to retain the cards in any new position which they might acquire, and yet weak enough to give way slowly to a continued force. When at last the tables, cards, and hands all moved to the left together, and so a true result was obtained, I took up the pack. On examination, it was easy to see, by the displacement of the parts of the line, that the hand had moved further than the table, and that the latter had lagged behind;—that the hand, in fact, had pushed the upper card to the left, and that the under cards and the table had followed and been dragged by it. In other similar cases when the table had not moved, still the upper card was found to have moved, showing that the hand had carried it in the expected direction."

This simple experiment of Professor Faraday's is a beautiful illustration of the true method of scientific inquiry. Not that for one moment we would suggest that the importance of a scientific phenomenon was sought for in these "table-movings," or that the attention of a scientific philosopher called to the investigation of any subject necessarily confers upon that subject any of the importance and moment of his ordinary pursuits; but this philosopher's mode of investigation is a happy contrast to the ready belief of the partially informed and those of the class to which we have previously referred, who quote Galileo, Harvey, and the Christian martyrs, as proofs of subjects which bear no analogy to their references.

IV. ERRORS ARISING FROM ILLOGICAL REASONING FROM CORRECT DATA.

This is not so common as many other sources of error, and frequently proceeds from carelessness rather than from incapacity; we might almost say invariably so, if the data or premises of the argument have been discovered or observed by the reasoner for himself. For correct observation involves so many exact and careful qualities of mind, and so much common sense, with at the same time no little mental calibre, that any one having once exercised these qualities on any particular point, would almost necessarily have power enough to conduct a process of correct inference towards a judgment or conclusion thereon; for observation, let it be remembered, is not merely looking at a thing. "The tables move," says a learned author in a work before us, "I have many times seen them. I know there is no wish to deceive on the part of the manipulators; moreover, the moment you take your hands away the movement ceases, *therefore* (the italics are our own) it *must* be from some magnetic or similar influence exerted by the manipulators upon the table." Therefore? there is no "therefore" in the case; it is no more a consequence of any expressed premise in this argument than if the conclusion referred to the price of corn in the year 1828, or the probable cycle of the next planet to be discovered. A table moves, you see it, you are convinced that no intentional deception is practised or attempted, the movement ceases when the hands are removed, *these are your expressed premises*, and so far, all is correct. Now, according to experience and ordinary observation, the most legitimate conclusion is, that the hands as physically moved the table as if they had intended to do so; and further, according to experience and ordinary observation on "magnetic and similar influences," *im*position of the magnetizing body is not required to produce the so-called similar effects, but simply its proximity; and further, the so-called similar effects are not similar but different: and in short there is nothing but dissimilarity observed throughout any *exact* comparison of these phenomena, and table-movings would be as likely to remind any one who was really acquainted with magnetic influences of those influences, as they would of the revolution of our earth, or "Belshazzar's feast," or anything else *not* connected with the subject.

We were going up Oxford-street immediately after writing the few foregoing sentences, when we observed that the greater part of a frontage was altering. None of the men were at work at the time; but one man stood with the palm of his hand against that part of the wall which had not been taken down; it seemed to move, and when he took off his hand the motion ceased. We are convinced this man had no intention to deceive any one in the matter; nevertheless, on reapplying his hand the wall fell down, much to his astonishment and our own, but we certainly did not come to the conclusion that this was the result of "magnetic or similar influences;" nay, rather that the premises and foundation of this wall were like the premises and foundation of our friend's argument, too weak for its support.

Now we will tell this gentleman how he might have given us a little more trouble in the argument—simply by taking a little more himself, and by adding one *datum* or premise more to its support—namely, this—"And I further know that none of the experimenters exerted any physical force in the trial." Although we should think this a rash statement, we would nevertheless endeavour to meet it, because we believe the gentleman to be conscientious; but we should remove the consideration to the next head, namely, illogical reasoning from incorrect data.

V. ERRORS ARISING FROM ILLOGICAL REASONING FROM INCORRECT DATA.

Illogisms of this kind are necessarily most fraught with error; they are so apart from truth that we verily believe it is the boldness with which they are uttered and promulgated only which demands for them any credence—wrong in foundation—wrong in process—wrong in result. We will take an example from that admirable work of Professor Simpson, on "Homœopathy, its Tenets, and Tendencies, Theoretical, Theological, and Therapeutical," third edition. It would seem as if Dr. Simpson had taken for his motto in writing this book, "*Out of thine own mouth will I judge thee*," for such a collection of contradictory assertions—such a mass of confused and trifling reasoning—such an absence of all legitimate conclusions from the data reasoned upon—were never perhaps presented to the public, except in the writings in which these select specimens were originally presented to the world. The book consists of twenty-one chapters and an appendix, and will well repay any one's perusal even as a source of amusement. We should especially recommend Dr. Simpson's happy quotations to professors of logic, as a kind of text-book of "examples in illogisms," or exercises for students upon the logical classifications of various errors.* Does nature cure by similars? "*Similia similibus curantur*," answers Hahnemann. This motto at the real basis of homœopathy is crude, quaint, and false, (and not even the best Latin in the world; we should have preferred "*medentur*," though, unfortunately for Hahnemann, that would not have altered his facts or given them a show of truth.)

In the first place, "like diseases" do not "cure like diseases;" secondly, if they did even in some instances, which they do not, *all* like diseases do not cure all like diseases, and this is the tacit *petitio principii*. Now this is important, though generally overlooked, for the error in this respect is a good instance of Hahnemann's style of reasoning. He thought he found out this grand secret from the observation of the effect of quinine in ague and in health, and forthwith from this special instance reasoned to

* Page 151.

the general law, "*similia similibus curantur.*" The observation, even if correct, could be suggestive only of an *hypothesis;* the evolution of the *law* was illogical. Now let us take another vaunted proof of homœopathic curing—"Your hand is burned, and the best cure is to hold it to the fire." Now those who with any pretensions to surgical skill make such a statement are either ignorant or dishonest, or both. First let us look at the case; surgeons make a very important distinction between different burns, founded upon the degree of their intensity; they are called of the first, second, or third degree, and it is to the first two varieties only that this plan of treatment refers, and therefore not to "burns," but to *some* burns. But after all it is not a *plan* of treatment, but merely a temporary resort, and that not according to the law *similia similibus curantur,* in which case the attempt should be to *simulate the burning,* that is, to burn again; but a nurse applies the heat temporarily *to keep away the cold air,* and not to simulate the burning. This, however, is just the style of illogical reasoning from incorrect data which pervades the whole system of homœopathy.

VI. ERRORS ARISING FROM LOGICAL REASONING FROM INCORRECT DATA.

These are errors more common among scholars and scientific men than almost anywhere else. The qualities for the observation of facts or phenomena may be perfect, but unexercised; the things stated unobserved or unexamined; and processes of reasoning only gone through, rather than a thorough sifting, not only of arguments, but the grounds of the argument. There is a very striking exemplification of this in the history of one of the most advancing sciences of the day—chemistry. Till a comparatively few years ago, it was stated that there were four *elements,* and that these four elements were, earth, air, fire, and water. The chemical definition of an element was then as perfect as now, but we have now very different notions both of the number and nature of the elements. Given, these four elements, and all material substances must be made up of them—either by a direct or indirect union—the differences in the substances produced being the result either of the nature of the union, the quantity of the material, or the method of commixture. This is necessarily a logical and legitimate conclusion from these data, but the data are false; for example, EARTH is made up of between fifty and sixty elements, and is thus far itself a compound, both chemically and mechanically. AIR is composed of all substances capable of existing in the gaseous state at the ordinary temperatures, and essentially of one part of oxygen mixed or diluted with four parts of nitrogen. FIRE is no element at all, but the resulting phenomenon of chemical combination attended with heat and light. And WATER, finally, is a compound easily separable into its parts. Now, the reasoning was logical enough beforehand, but the facts, it will be observed, were wrong, and therefore the conclusions were false.

Now, the pseudo-science, phrenology, in many particulars, and specially in its *stand-punkt,* or foundation argument, is equally erroneous. It is conceded that the brain is the instrument, or, if you will, the *seat* of the mind—so far all physiologists allow; but phrenologists state further, that the brain has organs, or special developments, which correspond with certain qualities of mind, intellectual, moral, and animal; and that these are marked by certain prominences and depressions upon the cranium; that therefore these being known, the character of the individual may be described. Now, if these data were correct, the conclusion would be true, for the reasoning would be logical; but, unfortunately for phrenology, in the first place the brain itself has no such organs, and they have, therefore, never been demonstrated: in the second place, there is no *necessary* correspondence between the surface of the brain and the *external* conformation of the skull: and, thirdly and lastly, the *suppressed premises* in this phrenological argument are also incorrect, or subvert the argument itself —for, allowing there to be organs in different situations upon the brain, when any combination of them is called into exercise, *what is it which communicates between the one and the other?* How does *it know* how to select the right organ, and when? *And if it do know all this*, what need has it of these organs at all? for surely there is mind enough in this very selective power for all the effects we ever see produced by mind—the mind must have all the feelings, capacities, perceptions, &c., without the organs, or it would not know one organ from another; and if it have all these things, they are not the cause of variety in mind, therefore they can be no guide to that variety; and they are not the effect of variety in mind, for they are developed in childhood, in absolute infancy, nay, in fœtal life, ere yet the mind can exercise variety; for, say the phrenologists, the cranial developments are the result of a moulding process of the brain and its organs upon the bone. Now, either the brain moulds the shape of the bone, or the bone moulds the shape of the brain. The phrenologists must believe this (though we do not), for they assert that the one corresponds with the other, otherwise, indeed, their developments would be of no use. Now, if the brain moulds the shape of the bone, it does so at a period when the mind is so utterly undeveloped that no cephalic organs can be the result of its exercise. On the other hand, if the bone moulds the brain, then, as the organs are the development of the exercise of the mind, therefore the mind must be in the bone! and the brain is not the seat, organ, or instrument of the mind—but they started with that statement—"*reductio ad absurdum.*"

VII. ERRORS ARISING FROM PARTIAL INSTEAD OF COMPLETE OBSERVATION.

This is too evident a source of error to render it necessary for us to dwell long upon it. Who that has read the admirable tale of "The Chameleon," can ever forget this cause of fallacy? We have witnessed this error in chemistry, in reference to earth, air, fire, and water. Then again in phrenology—no one who will take the trouble carefully to examine the subject practically, will doubt that not only does not the external plate of the skull correspond with the surface of the brain, but the external plate does not correspond with the internal plate, much less with the brain, between which latter two there is a potential cavity.

If observation be not complete, we almost necessarily conclude erroneously on the subject in hand. It is difficult enough for most people to reason correctly with a perfect knowledge of the facts of the case on which a conclusion is sought; how much more so when part of these facts only is known!

VIII. ERRORS ARISING FROM MISTAKES IN OBSERVATION.

These errors are similar (in effect, though different in the process of evolution) to the last style of error referred to. They may be caused by carelessness, or by what will presently be mentioned, "incapacity for observation."

The mistakes in this respect may be in reference to the facts or foundation of the argument—the motive of experiment—the process of experiment—the appreciation of the experimenter's reasoning—or the real nature of his results. Hence it is that most practical philosophers are not satisfied with one inquiry simply. Hence it is that they vary processes—working upon the same data—in order to see if the results they have obtained are "products" or "educts." Hence it is that they do not "rush to the press" before time and patient study have justified them in publishing to the world not what *may be*, but what *is*.

From this source arise misquotations, and perversions of meanings. An amusing instance of this occurred in a provincial paper which reviewed Dr. Simpson's work *against* homœopathy—as it most decidedly and evidently is—as being an admirable treatise in its favour. To our scientific readers, examples of controversies arising simply from misquotations or incomplete references will at once occur; some mistakes being as ludicrous as the one we have just referred to. How many persons are satisfied with partial quotations, forgetting the grand fact, that to quote —*out* of context—is, in nine cases out of ten, to misquote. These mistakes do not necessarily involve incapacity for this mental exercise, as we have before hinted, but rather impatience of result, carelessness, or prejudice in favour of this or that opinion.

The homœopathists at one time, convinced of the ridiculous figure they made before the thinking part of the community, specially in reference to their decillionths of grains of nothing—as "cogent, potent, and powerful"* doses—seized, by a *mistake* in *observation*, upon a thing one Mr. Rutter invented, called a "magnetoscope." (What's in a name?) This celebrated combination of mahogany, brass, string, and sealing-wax, was equal in effect to the divining-rod of old, or the magic triangles and circles of the Egyptians. Thus writes Dr. King, of Brighton, to Mr. Rutter, the wonderful inventor of this extraordinary instrument.

> "I may be thought too fanciful in the view I take of your beautiful, and, as I think, sublime discovery; but no reflecting mind will deny that we stand in need of some new principle, or truth, to enable us to turn to full account those which we have already received. . . . . When I first saw your machine prove the polarity of a decillionth of a grain of silex, and when I first saw it respond to the billionth of a grain of quinine, I was seized with the same kind of awe as when I first studied the resolution of the nebulæ, and as when I first saw globules of blood and the filaments of the nerves through the microscope."

We do not wonder at this awe-struck "King of thought" being so powerfully affected, for such phenomena are, to the weak, overpowering; while we reflect on the beautiful truth that "milk is for babes," we know

* This alliteration—so to speak—of ideas seems to be a kind of homœopathic mental convulsion, producing however no accumulative effect, and puts us in mind of the man who, when wishing to be striking in a remark, began, "I suppose, think, and imagine."

that any appreciable amount of milk is stronger than the decillionth of a grain of silex, or the billionth of a grain of quinine! Alas! alas! for Dr. King, the would-be scientific homœopathists have found it essential to give up "magnetoscopic demonstrations," and to leave him in his awe-struck rhapsody with "silex," "quinine," "male fly's-wings," and his wonderful bump of credulity; and we do not think that any reflecting mind will deny that Dr. King really does stand in need of a great many new truths.

IX. ERRORS ARISING FROM INCAPACITY FOR OBSERVATION.

Who told you so?—Who saw it?—Who performed the experiment?—These are most important questions; they strike at the root of belief on evidence. But they should have a reflex influence. Thus, are *you* qualified to observe the point at issue, leaving this for your own decision? We may remark, that we should not consult the premier of England on a case of heart-disease, or a medical man on the suspension of the *ever* "impending crisis" in political affairs.

But besides special adaptations to particular inquiries from pursuits and habits, there are qualities of mind which when possessed enable some to judge of some subjects better than others. Those who possess accuracy, veracity, no love of the marvellous, and whose education enables them to grapple with the subject in hand, are worthy of the highest credence, but these are not the followers of the popular delusions now so rife; and if some men of eminence have graced the heterogeneous ranks of these disciples of the pseudo-sciences, we feel sure that those among our readers who know such persons well, will be able to indicate without difficulty the point where the love of excitement or the thirst for novelty has been able to pervert the judgments of a mind otherwise capable and true.

There are indeed persons so weak and credulous as to be unworthy of all consideration. These are the people who quote as scientific and veritable *facts*, instances of proof from Mrs. Crowe's 'Night Side of Nature.' Most admirable title!—Most unhappy reference! For never, as far as *facts* and *conclusions* are concerned, were either author or readers so much in the dark!

X. ERRORS ARISING FROM INNATE LOVE OF THE MARVELLOUS.

Those who have not been in the habit of exercising their reasoning faculties upon subjects above the ordinary affairs of every-day life—and very few really have—cannot be expected to go through a process of logical induction upon abstruse matters, or matters out of *their* ordinary modes of thought. Numberless things are constantly occurring about them, of which they can give no satisfactory account to others, and indeed upon which they have not satisfied themselves. Cause and effect are to them almost foreign matters; or they are accustomed to resolve all effects into some generally acknowledged and popular cause, which, in nine cases out of ten, is nothing more than a NAME. The more astonishing the name, the more it is apart from their ordinary conception, the more satisfactory the mental process becomes; and they deceive themselves into a belief that they have been *reasoning*.

Instead of arriving, like the real philosopher, at an "ultimate fact," they arrive at an ultimate step only—something beyond which *they* cannot go, although it is surpassed by hundreds daily. They do not recognise the mental process by which they deceive themselves, and, getting thus confused, shake their heads mysteriously and say with Hamlet, "there are more things in heaven and earth than are dreamt of in your philosophy;" which, if it mean anything, means this: because they are not included in philosophy, they must be believed as facts. This will do for the stage, but not for science. Hence arise lazy belief, the reception of facts, theories, principles, and phenomena on hearsay, and the conviction that any conclusion is better than suspension of judgment. It is one of the highest acquisitions of mind to be able to suspend judgment and acknowledge our doubtful state to others. If more of us did this, there would be much less error in the world. Some people seem to think that there is no position between decision for or against a point—between belief in a principle or statement and disbelief of the same. And others think that if they cannot point out the fallacy in an argument, that argument must necessarily be correct, and its conclusion logical. We would have such people visit some of our courts of law, not to learn the petty quibbling and playing upon words in the *pro* and *con* argument, but to see the state of mind we refer to exemplified—namely, the SUSPENSION OF JUDGMENT until the evidence be perfect. It would at first sight seem that such a decided state of mind upon *all* topics, is a sign of acumen and considerable mental calibre; but this is not really the case. It is the result of absolute laziness, for it is much more easy to decide than to judge or discriminate; the one is merely affirming a conviction, the other is collecting the evidence for that conviction; the one is asserting a principle, the other is tracing its development; the one is talk, the other is work. But men prefer, for the most part, revelation to induction; they are not satisfied, so to speak, with Moses and the prophets, but wish one to rise from the dead. It of course saves a great deal of trouble, and of that style of work to which they are not accustomed.

Now the ready-made sciences—THE PSEUDO-SCIENCES—such as mesmerism, phrenology, homœopathy, electro-biology, and hydropathy, appeal one and all to these qualities of mind. We do not wish to be uncharitable; but look at the followers of these delusions. Some of them, innocent enough, merely, like so many *dilettanti*, amuse themselves most harmlessly; some cure a sore throat in two hours by the decillionth of a grain of nothing; others bless the days they live in when they can hear of the winking virgin of Rimini; others hire their servants by the development of the cranium; others devote themselves to the production of "sweet slumber" by mysterious motions of their arms, hands, fingers, and eyes; while others, most harmless of all, torture themselves with heat and cold, in the vain hope of a coming "crisis." Enjoy your pleasing delusions for a time, illustrious *savans!*

XI. ERRORS ARISING FROM THE SYSTEM OF DISCIPLESHIP.

There are two classes of society—the leaders and the led; they are necessarily disproportioned in numbers, the led proving by far the majority; this, after all, is less from the absence of capacity among the led, than presence of boldness, activity, and industry among the leaders. Many are leaders, not because they possess higher mental qualifications than their fellows, but because the minds in the circles in which they move are sluggish and inactive, and prefer being thought for to thinking, or, rather, *prefer* nothing at all.

There is a moral view of this and the following consideration (XII.) which cannot be too forcibly impressed upon the mind; that is, our accountability for opinions on these subjects, if not to a higher power, which we believe, in common justice to our fellow men. There is a sense in which a man has no right to "enjoy his own opinion"—that is, when he forms it without a proper reason for his conviction, but merely as a whim or fancy, as it must invariably be when he follows a leader simply because he will not seek a path for himself.

After what we have stated, it cannot be supposed that we imagine all men must or may be leaders, or that there is any littleness of mind in being led; we think, rather, that it is a mark of a high and exalted intellect, when, after the application of those tests to this or that argument which are qualified to carry conviction with them, a man boldly affirms his adherence to an opinion, be it moral, religious, political, or scientific. It is not of such we speak; but mark the difference. Is it not notorious that individual opinion becomes merged in sects, schools, and clubs? *Given*, any particular question for a decision; and if you know a man's *clique*, you may generally affirm his decision upon the subject in hand. This is observed less among scientific men than among others, but simply because their questions of investigation are less popular. There are *schools* of science—there are sects among them; there are clubs and coteries—rivalries of the lowest kind. If you will be led by this man or that journal, you may be sure of applause, if not of place. It is of no consequence to them whether or no you think with them, although it may be and is of consequence to science.

As an instance of the errors arising from discipleship, look at the delusion of mesmerism. Who among medical men in this country and in these times would have believed in mesmerism as it is now defined, had they not had a leader, who, at the period of this heresy, was assuredly one of the most popular physicians of the day? It was then, and has been since, on this subject, "follow the leader!" and now mesmeric doctors are as common as if they had any ground for belief in their particular views. Homœopathy and hydropathy have had leaders of another kind—money and interest; while, in most cases, they have had a propulsive ejectment from legitimate medicine, a *vis a tergo* from poverty, and lack both of patience and patients. It matters not what the leader may be, money, fashion, names, men, or systems, let them take the lead, and as long as men do not investigate for themselves with a conscientious regard for truth, and an acknowledgment of the fact that they are responsible for their opinions, the system of discipleship will be a never-failing source of error.

XII. ERRORS ARISING FROM THE SYSTEM OF LEADERSHIP.

A degree of *éclat* attaches to a man who is the originator of this new view, or that particular doctrine. If he have selected his subject well, there is a positive source of profit. There is often a kind of power obtained by this popularity, which in men of little minds and who love to patronize, gratifies even more than the *éclat* of the unthinking audience.

Unfortunately this is so commonly felt and tacitly acknowledged, that men act upon it without questioning its morality, or their own qualifications for the task of leadership. Hence the present fashionable system of taking up *specialities*, particularly in the medical profession; the speciality in nine cases out of ten being taken up, not because of a man's innate qualifications for the thing—not because his pursuits have by force led him to the subject—not because his previous education has been so elaborate and his studies so deep in the numerous, peculiar, and allied sciences of this noble profession—not because his comprehensive genius, grasping the whole range of medicine, sees that here or there he can elaborate a weak point or develop a hidden mystery—but because here or there "there is an opening." Then the journals are *used*—then his conversation swells bigly with laboured thoughts on *his* speciality—then he rushes about among his friends with profound and mysterious hints on the importance of this or that original idea—then he speaks at societies through his peculiar trumpet—then somebody who can't speak either punctuates his large discourse with "notes of admiration," or, as we have noticed more than once, by a "full stop" in the shape of some quaint remark which is very humiliating; till, not heeding this hint, at length he commits the grand mistake of supposing that he is an authority, because he has obtained the envied goal of his laborious ambition—the dictatorship of a leader!

Nevertheless, specialities are good, but those must be the specialities of special men, not the men who just having finished their studies, or lacking practice, seek notoriety, and look out for "openings" or "riders" into notoriety. To be sure, notoriety they may get, but not fame—a peculiar cab or an extraordinary hammer-cloth will give them notoriety, but it must be a peculiar thought and extraordinary talent that will give them fame. The man who is to be *the* leader, in the sense of an authority, cannot help it—the position is as naturally conceded to him as if he were born to the title; but your *soi-disant* authority, if he have impudence enough, may, meteor-like, flit across the circumscribed heaven of his own little sphere, but as soon disappears from the horizon, and the "place thereof knoweth him no more."

But, also, specialities are bad, in this latter sense, because men have to assume to *be* what they *are not*—they have to put on the *show*, instead of working for the *reality*. And, although they may deceive themselves into the belief that they are "working out one idea," they take with them such *media* of vision in its investigation, that their every thought is tinged by their colouring; so true is it that "the eye sees what it looks for." Hence, not only are they a source of error to the unthinking multitude, but a living deception to themselves.

There may be, and doubtless are, many other specific sources of error in scientific observation than those now alluded to; but all the general causes are, we think, here embraced; and if the reader were to carry out their spirit and real application, either in the investigation of original subjects, or in testing any doctrines, systems, or opinions, which may come in his way, there would be very much less error in the world in a few years than at present exists. He would neither use nor admit the usage of words whose meanings were not accurately defined and fixed; he would argue about things, and not names; and would not substitute conventional phrases for absolute realities. He would receive nothing on the testimony of another which he could prove for himself; or in receiving or observing evidence as data of induction, he would reason logically—he would not admit incorrect data, or be satisfied with a logical argument from premises which he had not tested. His observation would be complete and accurate; and thus, in such an exercise, would he gain greater power for further observation. Satisfied with those mental endowments with which his Creator had blessed him, he would seek a high culture and a legitimate use of them, casting away all dealings with the marvellous, and looking suspiciously, rather than otherwise, at anything bordering upon the miraculous. He would not tamely follow a leader because others did, or pin his faith to this or that opinion because of the prestige of names or dates, or the fascination of novelty; nor would he, with a vaulting ambition, seek to be a leader where he had not the power to command respect, and the genius to direct opinion.

Without the bombast and parade of assumed authority, his opinions would be quietly, but nevertheless powerfully, expressed; and, aided by truth, they would work like the "little leaven," influencing society, first in small, and then in large masses, until the sphere of each one's influence would meet, not to clash and rebound, but first to yield, then merge, and then coalesce, in one large sphere, in whose centre would be placed the light of all truth, from which the divergent radii, spreading to the distant circumference, would dispel the darkness of the night of ignorance, and introduce to all mankind the daylight of eternal truth.

Boon Hayes.

Review XI.

Ueber parenchymatöse Entzuendung. By Rud. Virchow. ('Archiv. für patholog. Anatomie und Physiologie u. f. Klin. Medicin.,' vol. iv. pp. 261—321.)

On Parenchymatous Inflammation. By Rudolph Virchow.

Though much has been written about inflammation, it must be confessed that the pathology of this form of deviation from health is still far from being sufficiently elucidated. The more we inquire into the various phenomena of inflammation, influenced as they are by the tissue affected, by the constitution, by the exciting cause, &c., the more satisfied are we that it is in many instances impossible to form a line of demarcation between the healthy process of nutrition and that aberration which is called inflammation, or that which leads to the formation of pseudoplasmata, or of hypertrophy, or various other morbid processes.

The author of the article before us appears to be well aware of this difficulty, and endeavours to study the nature of inflammation by the phenomena observed in the elementary components of the various tissues. In explaining his views, we intend to make use as much as possible of his own words, by which the reader may be best enabled to judge of the value of his researches.

The term *parenchymatous inflammation* is applied by the author to those inflammatory processes in which the characteristic and essential changes are met with in the elementary components of the tissue, without any appreciable exudation taking place, either into the interstices of the tissues, or upon the free surface of membranes. It will be seen, therefore, that Virchow applies the word *parenchymatous* in a different sense to that in which it is generally used. By most pathologists the "parenchymatous inflammation" of an organ is opposed to the inflammation of its lining membranes. Thus, many pathologists speak of a *glossitis mucosa* (inflammation of the lining mucous membrane of the tongue), and *glossitis parenchymatosa* (inflammation of the substance itself); of a *nephritis parenchymatosa*, to distinguish it from the inflammation of the mucous membrane, (nephritis mucosa, generally called *pyelitis*), and from inflammation of the external serous membrane (nephritis serosa, or *perinephritis*). According to Virchow the inflammation of the lining membranes of organs may be as well parenchymatous as that of the substance of the organs (generally called the parenchyma) itself; to be parenchymatous inflammation it requires only that the principal changes shall take place *within* the elements themselves, without exudation *into* the interstices, or *upon* the free surface. We shall return afterwards to the consideration of the propriety of this expression of parenchymatous inflammation. At first we must briefly state his views on the intimate nature of inflammation. We cannot fulfil this better than by quoting from an earlier article "On the Dilatation of the Small Vessels."

"Some have endeavoured," he says, "to attribute the origin of inflammation to the vessels, others to the nerves, others to the tissue; while disputing about the outset of the phenomena, they have neglected the question about the intimate nature of the process. Doubtless all the factors on which the nutrition of a part depends, must have their share in its inflammation, the blood as well as the nerve; the nerve as well as the membrane of the vessels, as the tissue. As soon as real inflammation is established, all elements must participate. This diseased state of all the constituents of a part may be originated by any one of the single factors of nutrition; the blood as well as the nerve, as the vessel, as the tissue, may form the prime mover of the inflammatory disorder, which later influences the relation between all of them, and which is not to be considered as inflammatory unless *all* the factors are participating, as without this there would exist merely *hyperæmia, neuralgia*, &c., but not real *inflammation*." (Arch. f. path. Anat. and Physiol. iii., p. 459).

"If, therefore," continues the author in the present essay, "inflammation is to be considered as a diseased state of all the constituents of a part, it can, of course, not be permitted that affections of merely a single constituent, as hyperæmia, neuralgia, exudation, metamorphosis of the tissue, shall be considered as inflammatory." (vol. iv. p. 279.)

We must abstain here from entering fully into this question; it appears to us, however, difficult to prove the view of the author, that in the above processes considered by him as elementary (hyperæn ia, &c.) only one element or constituent is affected, and that metamorphosis of the tissue can take place without a simultaneous or previous affection of the vascular constituents and the nerves of the same part, or without an altered relation between them.

We need only shortly mention that Virchow does not consider the inflammation of a part as a perfectly new or specific process, but, like every other local pathological process, only as an aberration of the normal nutrition. In this respect he is, therefore, in accordance with our older pathologists. He sees in the varions phenomena of inflammation " the excess of all or of certain single processes of nutrition." (p. 275.) The healthy state of nutrition is understood to be conserved by the equilibrium between two currents of fluid, the one going from the capillaries to the tissue, the other from the tissue to the capillaries; or, to use with him a short though not quite accurate term, in a certain state of diffusion between blood and tissue. The acts of absorption and of exudation, which represent these two currents, are considered as being under the constant influence of the nervous system.

"If we suppose this," he says, " we find in analyzing the phenomena of inflammation—1. That the equilibrium in the exchange between blood and tissue is disordered in such a manner as to produce either increased absorption, or increased exudation, or both together; 2. That the nerves of the diseased part are in the state of excitation or irritation. If the absorption is increased, it must lead to inflammatory atrophy; if the exudation is excessive, inflammatory tumefaction must be the consequence; if, at the same time, exudation and absorption are in excess, we find the elements of the tissue to become atrophied, while in their place exuded matter is deposited, which, at first amorphous, undergoes afterwards some one or another metamorphosis."—(pp. 275, 276).

The matter exuded during inflammation may be deposited either into the interstices between the elements of the tissue, or on the surface of the tissue, or into the elements themselves. According to this threefold possibility, Virchow makes the subdivision into inflammations with an interstitial, with a free, and with a parenchymatous exudation. He does not, however, deny the possibility of the co-existence of two of the forms, or even of all three, in the same morbid process. We are inclined to think that rarely one of the forms does exist alone. We can scarcely imagine an inflammatory effusion to take place upon the free surface without any such participation into the elements of the tissue, or into the interstices, nor does it appear to us possible tbat an anomalous fluid is effused into the elementary constituents without a simultaneous or rather preceding effusion into some kind of interstitial space between the blood-vessel and the elements through which the normal as well as the abnormal nutrition takes place.

As one of the best examples for the explanation of the phenomena of the parenchymatous inflammation, Virchow describes in detail the *inflammation of the muscles*, repeating the words he made use of in 1847, in a lecture before the University of Berlin.

"The inflammation of the muscles," he says, "is very similar to that of the kidney commonly called *Morbus Brightii;* the only difference between them consists in the circumstance of the muscles being provided with a greater quantity of interstitial tissue. In the inflammation of the muscles we find, therefore, the exudation either only in the interstices between the primitive fibrillæ, or simultaneously in the interstices and in the fibrillæ, or only in the fibrillæ. To commence with the last-named form, we see, if it is of acute source, at first *a change of colour and cohesion* of the flesh of the muscle,—phenomena which both must make us think of a change in the molecular composition of the tissue. The flesh assumes in some cases a violet-blue colour, in some a greyish-red and brown, in others it approaches a pale white, yellow, or green; it is brittle, and easily torn into pieces. The microscope shows that the ultimate fasciculi of the fibrillæ take at first a more homogeneous appearance, their transverse striæ become indistinct, they easily break into irregular fragments, and at their ends these fragments are frequently seen to divide into longitudinal fibrillæ. Later the contents of the ultimate fasciculi become still more dingy, lose the yellowish colour, and turn still more grey; within their sheaths gradually a molecular substance is observed of great density, which, by acetic acid, becomes clear, and exhibits all the characteristic reactions of protein; here and there, also, a fat-globule may be observed in it. In the most acute course, the connexion between the primitive fasciculi is soon destroyed, the sheath is ruptured or dissolved, the molecular contents flow together into one cavity; the muscle is said to be in a state of *inflammatory softening*. If the case is less acute, we see within the molecular substance at first, some globules of fat, the number of which is gradually increasing, until the whole fasciculus appears filled with fine granules of fat, connected by a scanty azotised substance. Not rarely the whole series of these changes may be observed in one single fasciculus. At last we meet with cases, and these too may yet be of a comparatively short course, in which the fatty metamorphosis takes place without a previous decay of the primitive fasciculus into molecular matter. In these cases we observe the fat-globules arranged in neat and pretty rows, one placed behind the other like a string of pearls, in the longitudinal axis of the fasciculus, exactly corresponding to the longitudinal fibrillæ of the muscle,—a fact which affords a new proof for the universality of the law of the fatty metamorphosis in the azotised components of the body.

"Besides these there are other forms of a slower course, and with a different termination. Some of these cases exhibit the fatty metamorphosis of the fibrillæ; in others, however, the visible phenomena are not striking. To the naked eye the colour of the muscle appears changed, somewhat of a greenish yellow or whitish yellow; under the microscope the ultimate fasciculi are not any longer yellowish, but perfectly (?) colourless, while within them small groups of glittering molecules of a yellowish colour are seen; the whole has the appearance as if the colouring matter of the muscle, which formerly had been equally diffused over the entire substance, were collected into granules deposited only on some spots. The ultimate fasciculi, at the same time, more easily break into pieces than in their healthy state, their transverse striæ have become indistinct, the longitudinal fibrillæ make their appearance without previous preparation. But in whatever manner the metamorphosis of the ultimate fasciculi may vary in the chronic course of the local process, after some time increased absorption always takes place, the changed contents of the fasciculi are re-absorbed into the circulation, and then sometimes a loose tissue of a rather fibrous nature is observed, with a great number of longish and oval nuclei. Later the spot formerly affected appears shrunk below the surface of the surrounding part; it is occupied by a more or less dense reddish or white areolar tissue (*Bindegewebe*—connecting tissue), which exhibits at the termination of the process a splendid, tendon-like appearance—*the tendinous macula* (*Sehnenfleck*) *of the muscle*.

"As we have stated already, these may be the only changes, or they may be connected with an exudative process into the interstitial cellular tissue of the muscles. These combined forms are well described by *Gendrin*.* We restrict ourselves to mentioning here, that the exuded substances may consist in some cases of an albuminous, in others of a fibrinous, in others of an hæmorrhagic fluid. These exudations may proceed in their metamorphosis, and it is principally putrefaction (*Verwesung*) or *abscess of the muscle* they generally lead to." (pp. 265—268).

* Histoire des Inflammations. 1826.

As to the circumstances and clinical symptoms under which this morbid process takes place, Virchow mentions that the cause generally known is of a traumatic nature, but that very often also the so-called *rheumatic pains* have their origin in an inflammatory state of the muscles, and that most diseases accompanied by rheumatic symptoms lead to changes in the muscular apparatus.

The best specimens of rheumatic inflammation of the muscles the author met with in the muscle of the heart.

"You observe," he says, "the flesh itself grow pale, while there exists at the same time a moderate hyperæmia of the interstitial vessels; the red colour of the flesh changes, by degrees, into a greyish red and yellowish red, lastly into a dirty yellowish white or greenish white, while you see under the microscope, within the ultimate fasciculi, the alteration just described. If the process proceeds further, retaining its acute decourse, the change of colour advances more and more, the tissue becomes so brittle that it may lead, as I have witnessed in one instance, to rupture of the heart. In general, however, the process assumes the chronic form, you see the ultimate fasciculi in some places entirely disappear, the spot becomes sunk in or contracted by a callous or tendinous cicatrix, a change which already *Morgagni* had described as *vitium carnis cordis in tendineam naturam degenerantis.* (Ep. xlv. Art. 23)." (p. 270).

As another morbid condition of the system with which muscular inflammation leading to tendinous cicatrices (*schwielige Muskelentznendung*) is frequently connected, Virchow mentions secondary *syphilis*, which differs from the rheumatic affections, principally by the constancy with which it persists in one single or only a few muscles.

Another instance by which Virchow's meaning of "parenchymatous inflammation" may be explained, is given by his view of the intimate nature of Bright's disease, as being a parenchymatous inflammation of the kidney.

"In another place," he says, "I have already demonstrated that, while in the canaliculi recti and a part of the canaliculi contorti, fibrinous cylinders (i. e., free inflammatory exudation) are found, those changes which give rise to the characteristic anatomical condition of the kidney, must be looked for only in the epithelial cells of the canaliculi contorti. In the first stage these cells enlarge, and their molecular contents increase. In the second stage this increase may reach such a degree that it leads to the complete breaking down of the cells, in consequence of which the canaliculi appear filled with a molecular, albuminaceous substance, a state at Vienna detached from the *Morbus Brightii*, under the name of *lardaceous infiltration*. The cells may also undergo the fatty metamorphosis, they may become filled with minute granules, forming thus the *stearosis*, while the aggregated fat-globules (*Fettaggregatkugeln*) represent the long-known exudation corpuscles (*Entzuendungskugeln*). In the third stage all of these aggregations of oil granules break down, an emulsive fluid is formed, which, during the development of the fourth stage, is either absorbed or excreted with the urine. In some cases the whole process appears entirely confined to these changes in the epithelial cells, no free fibrinous exudation being poured out into the cavity of the canaliculi. Then all the exudation takes place into the elements of the tissue, except the albumen which is carried off with the urine." (p. 265).

We cannot enter in this place into the pathology of Bright's disease, but concerning our present subject, Virchow's own words appear to us sufficiently to show, that the expression of "parenchymatous inflammation," in the author's meaning, is not well chosen for the disease in question. The circumstance that no free fibrinous exudation is poured out into the canaliculi, does not entitle us to conclude that the whole morbid process is confined to the epithelial cells. But the fact of albumen being poured into the canaliculi, and passing off with the urine, proves that *not* "all the exudation takes place into the *elements* of the tissue," but at least also on the free surface, if not, at the same time, into the interstices too.

There are, however, organs in which we can detect scarcely any palpable interstices; in these, therefore, the inflammatory changes can be observed almost only within the elements of the organs. Such is the case with the cornea, the cartilage, the bones, &c. In the *cornea* Virchow created inflammation by the application of intense caustics and various stimulating agencies. The changes manifested themselves at first in the cells of the cornea, which became larger, exhibited some fat-globules, and an increase of their nuclei in size and number, as described already in Bowman's Lectures. At a later period the intercellular substance became dim, opaque, more dense, easily dividing into single fibrillæ, approaching, in its appearance, that of the sclerotica, sometimes exhibiting a granular aspect as if covered with dust, and likewise displaying oil-globules under the microscope. The process may be arrested here, and give rise to the various degrees of opacity (Leucoma &c.), or it may lead to softening of the substance of the cornea (Keratomalacia) and superficial ulceration. But in no case did Virchow find any free exudation between the lamellæ of the cornea, or between the single fibres. The inflammatory process in the true cartilage is described as very similar to that in the cornea. The cells become larger, the number of the nuclei increases, some or all of them may undergo fatty metamorphosis, while the intercellular hyaline substance becomes fibrous, divides into filaments, and assumes the appearance of a more or less soft, gelatinous, cellular tissue. Virchow's observations on this subject are, therefore, almost entirely in accordance with those of Ecker,* Goodsir,† and Redfern.‡

Some, perhaps, maintaining the view that inflammation cannot take place in tissues which possess no bloodvessels within themselves, but only in their circumference, will object that the above-described changes in the cornea and cartilage ought not to be adduced as specimens of inflammatory alterations. But the progress we have made in understanding the process of healthy and unhealthy nutrition, takes away the force of such an objection. The circumstance that no bloodvessels are distributed within these tissues, does not give rise to a real difference in the process of their nutrition. There are no tissues the elementary components of which are in a perfectly *immediate* connexion with the blood; the distance between both is greater in some, less in other tissues. Organs in which we find the former arrangement (i.e. those with few bloodvessels, or none, within their substance) possess a less rapid change of matter and a greater independence of constitution, than those which in all their parts are richly provided with capillaries; but physiological experiments as well as pathological facts prove that the former are likewise in a constant change; we see them partake in the morbid processes of other organs; we see their nutrition deviating from the standard in various modes—why

* Archiv f. physiol. Heilkunde. 1843, Band ii. p. 235 ss.
† Anatomical and Pathological Researches. Edin. 1845.
‡ Monthly Journal of Medical Science, 1849, 50.

then should that aberration be excluded which we call inflammation? And if we observe that in these bloodless tissues certain changes are produced by the application of those stimuli which are known to excite inflammation in the vascular tissues, changes which are, *cæteris paribus*, identical in both tissues, why should we not use the same term for both processes? It is true that changes in the cornea and cartilage, similar to those which we call in some instances inflammatory (if produced by stimuli usually exciting inflammation, &c.) are met with in the so-called alterations of old age. But this is no proof against the inflammatory nature of the former; these alterations of old age are found also in tissues provided with capillaries, and also in these they are like the changes produced by inflammation. We know that the same change may be effected by various means, that it is not the result by itself from which we can always conclude on the nature of the process.

In treating of the inflammatory affections of the bones, Virchow distinguishes from the true *ostitis* (the parenchymatous inflammation) those conditions which are connected with exudation on the free surface of the bone, or into the interstices between the elements of the tissue (*periostitis* and *endostitis*). In the inflammation of the *substantia ossis propria* the first change observed by him frequently consisted likewise in the fatty metamorphosis of the cells of bone. Groups of small oil globules made their appearance in the cavity of the cells, and single ones sometimes also in their tubuli. At the same time, often also without this change, some larger cells were seen, and in rare instances these had a double nucleus.

"On inflamed ribs," he says, "but principally on the lower articular surface of a carious tibia, I saw at some distance from the exterior surface of the cells the commencement of a disjunction of substance. Some almost completely round, only slightly oval, bodies become disconnected from the intercellular tissue, exhibiting the appearance of enlarged cells of cartilage provided with tubules, being separated from the surrounding still homogeneous substance by only a very thin fissure. At the same time other formations were seen, already more disunited, in which also the shape of the cell was much changed; nothing but rather soft, mouldy, granular aggregations could be detected, which exhibited indistinctly the tubules here and there filled with the most minute oil globules—rarely the osseous corpuscle itself was seen. By washing these, pulpy masses could be removed, after which process the superficies of single pieces of bone offered the appearance so well described by Howship, i.e., roundish cavities, on the one side more or less open, on the other more or less provided with a wall of not yet softened osseous tissue, 'as if they were chiselled out by a half-round chisel.' Amongst the more generally known microscopic objects, the margin of a cut through the pulmonary vesicles would give the best idea, though in size rather too large." (p. 303.)

The most interesting phenomenon in this process is. that the osseous tissue does not undergo the alteration in an uniform manner, but in single partitions, each of which represents the province of a single corpuscle of bone, a circumstance highly illustrative of the function which these elements perform in the nutrition of the bones. The principal product of this inflammatory process is softening, and later, rarefaction of the osseous tissue; we observe in this respect much resemblance with those morbid conditions which we in general comprise under the head of *osteomalacia.* While C. Schmidt and C. O. Weber consider the rarefaction of bone in osteomalacia to be the effect of a chemical solution of the calcareous matter by means of a free acid, Virchow is of opinion that parenchymatous inflammation of the bone is the intimate nature of the disease.

Very similar to the alterations of the *cornea* and *cartilage* are those of Virchow's "parenchymatous inflammation of the *areolar tissue.*" They may be seen in the circumference of inflamed tissues, for instance the skin, where both phenomena are displayed, the fatty metamorphosis of the fibres and the multiplication of their nuclei.

In those tissues which are principally composed of cells, the inflammation of the parenchyma, in Virchow's sense of the expression, might be almost called an inflammation of the cells. As an instance of this we have mentioned already Virchow's parenchymatous inflammation of the kidney. Another is given by the author in the lobular inflammation of the liver. He is inclined to think that, on account of the change of colour and the whole physical appearance (to the nude eye) of the parts affected, they are not rarely considered as lobular abscesses, while they contain no pus, but merely the changed elements of the tissue (principally the glandular cells).

After having briefly considered the morbid conditions which Virchow describes in illustration of the *parenchymatous inflammation,* we may judge of the propriety of this expression. We must conclude that our author understands by the word *parenchyma* the constituent elements of a part in contact with the interstices between the elements, and with the free surface beyond the lining membrane. Parenchymatous inflammation would therefore be opposed to interstitial inflammation and inflammation on free surfaces. But he certainly would not admit the expression "inflammation on the free surface." It is evident that he uses his term only in consideration of the exudation which takes place during the process of inflammation, into the elements themselves, and into the interstices between them; or, we would rather say, in consideration of the inflammatory changes in the elements, in the interstices, and on the free surfaces. But we cannot think it correct to call the inflammatory process with alteration of the elements themselves, parenchymatous inflammation. Even if we were to adopt, with the author, the word *parenchyma* as signifying the elementary constituents in contact with the interstices between them, yet we could speak only of "inflammations with parenchymatous alterations," not of "parenchymatous inflammations." Virchow's expression might lead also to the erroneous idea, that the morbid process was going on merely within the principal elements (muscular fibrillæ, cells of bone, &c.), without affection of all the constituents of a part, though our author himself says, in several places, that inflammation cannot exist without participation of as well the blood as the membrane of the vessels, as the nerve, as the tissue.

We are inclined, therefore, to retain the expression "*parenchymatous inflammation*" in the old sense, i.e., signifying the inflammation of the parenchyma of an organ, in opposition to that of its lining membrane, and to consider Virchow's "parenchymatous inflammations" as inflammations with principal affection of the constituent of the tissues. But though we do not adopt the expression chosen by Virchow, yet we cannot but observe, that every one may gain much information by carefully perusing the author's essay on parenchymatous inflammation. Some will blame him, perhaps, for attributing too much importance to the anatomical, and too little to the clinical facts. While the anatomist pays his principal regard to the changes produced by inflammation, the physiological pathologist inquires after the intimate nature of the process which gives origin to these changes. Though it must be confessed, that in the present essay Virchow has entered but little into the question *how* the changes take place, yet this does not prove that he neglects the study of the nature of the process. On the contrary, the only purpose of the whole of his treatise is to throw light on this subject; the way only which he has chosen differs from that generally adopted; instead of entering into *theories* about the influence exercised by the blood on the vessel, or the nerve, or the tissue, he examines what he can *see*, the change effected *by* and coincident *with* the morbid process. Some, perhaps, will say, what Virchow describes is not inflammation itself, but only one of its events—*exudation.* But can we any longer maintain the view that exudation is not an intrinsic part of the process of inflammation, that it is a mere consequence? If we ask how does the normal nutrition take place? we can but answer—by means of constant exosmosis and endosmosis, of constant exudation and absorption. During inflammation the nature of these processes remains the same, only the factors (blood, vessel, tissue, &c.) are altered. Therefore, as exudation forms an inseparable part of normal nutrition, as well as of inflammation and other varieties of abnormal nutrition, the alterations which Virchow describes in the elements of the tissue have been overlooked, or considered as secondary, by the majority of observers, and yet they are of greater importance for the understanding of the intimate nature of the process than the more palpable phenomena (the abundant exudation into the interstices between the elements and into the cavities); indeed, the latter are apt to make us forget the important fact, *that inflammation is only one of the various shades of deviation from the normal process of nutrition.*

In the present, as in any other of Virchow's essays, the reader will meet with an abundance of new ideas, worthy of the most careful consideration; he will find in every page sufficient proof of the author's energetic desire to eradicate all views and theories which are not borne out by indisputable facts; everywhere is seen the ardent contest against what he considers to be the general character of medical writings—a mixture of arbitrary rationalism with the most rude empiricism (*Gemisch von willkürlichem Rationalismus und crassem Empirismus*). The only way approved of by Virchow is that pursued in physics and other branches of natural science, which in medical science is that of unprejudiced observation of the healthy and morbid processes under various influences, and in their various stages and modifications. That he has met with full approbation cannot remain doubtful, if we consider that his authority is generally acknowledged to be of the highest order in most of the branches of scientific pathology, though scarcely ten years have elapsed since he finished his university studies. Still young in years, he is a veteran in observation, and few men have been equally fortunate in discovering new realms for science, and in avoiding the quicksands of imperfect observation.

REVIEW XII.

Elements of Psychological Medicine; an Introduction to the Practical Study of Insanity, adapted for Students and Junior Practitioners. By DANIEL NOBLE, M.D., F.R.C.S., Medical Officer to the Clifton Hall Retreat, &c.—*London*, 1853. Post 8vo, pp. 340.

THIS little volume contains Eight Lectures on Insanity, which were delivered by Dr. Noble during last session, at the Chatham-street School of Medicine, in Manchester, and which he has published at the request of his medical brethren, who constituted a large proportion of his auditory. We happen to know that these lectures were listened to with great interest by several of the most distinguished members of the profession in that city; and that the request for their publication was not an idle compliment, but a genuine expression of their sense of the value of what they had heard, and of their desire that others should benefit by it. To the valuable recommendation of the book which this fact conveys, we are glad to find ourselves able, after a careful examination of its pages, to add our own very favourable testimony. Dr. Noble does not put it forth as a systematic treatise on the subject, but designs it merely as an introduction, arranged upon a plan that shall take the student up, as it were, at the point where the completion of his lectures and hospital attendance leaves him. He has connected the pathology of mental maladies, as much as possible, with the present state of our knowledge of cerebral physiology; adopting in nearly every particular, the views enunciated by Dr. Carpenter, in his reviews of "Noble on the Brain," in our twenty-second volume; and abandoning, with a candour most creditable to him, the greater part of the phrenological doctrines which he formerly upheld, but which his more matured judgment and larger experience (especially in regard to insanity), make him regard as no longer tenable. And he has explained the principles of treatment by constant reference to those more general ones, which are applicable in the ordinary practice of medicine, and with which, therefore, the advanced student, as well as the practitioner, ought to be acquainted.

In his preface Dr. Noble makes some very pertinent remarks regarding the ignorance which prevails through a large part of the profession, in regard to the diagnosis and treatment of insanity. "Well educated persons out of the profession," he truly remarks, "will frequently judge as soundly concerning the condition of a patient, as many who are within it." Our hospitals, colleges, and universities make no provision for training the future physician or surgeon in a practical knowledge of insanity, so that men who possess adequate information upon this branch of their duties, are necessarily self-educated to a great extent. And yet any two practitioners, having no special knowledge of the disorder, are empowered by law to take away the personal freedom of any individual, by certifying to the fact of his insanity; a fact about which those who have no familiarity with the disease are peculiarly liable to be deceived, the most accurate diagnostic powers being often required even by the most experienced.

The first lecture contains a general *introduction* to the study of insanity, and embodies Dr. Noble's view of its essential nature. He defines it as consisting in "chronic disorder of the brain, inducing perversion of ideas prejudicial to, or destructive of, the freedom of the will;" each point of this definition being more fully discussed in subsequent lectures. Thus, after giving in the second lecture such a sketch of the present state of our knowledge of the functions of the brain and nervous system, as may serve to make his readers comprehend their probable connexion with the healthful activity of the mind, he proceeds in the third lecture to discuss the *General Pathology of Insanity;* and under this head he makes good his position as to the essential connexion between this disease and cerebral disorders. His arguments on this point are extremely acute; and we can only regret that he did not fortify his position a little more, by showing how cerebral disorders, that may leave nothing for the morbid anatomist to discover, may be excited either by perverted blood, or by morbid sympathies with other parts of the system. These points, it is true, are not altogether passed over; but they scarcely receive the prominence to which they are entitled. The fourth and fifth lectures embrace an account of the *Varieties and Particular Characteristics of Insanity:* the primary forms of the disease recognised by Dr. Noble being the *notional*, the *intelligential*, and the *emotional*. In the first there is a primary perversion of ideas, giving rise to settled illusions, of which the mind cannot get rid by any effort of its own. In the second there is a primary perversion of the reasoning faculty, quite irrespective of any fixed or characteristic illusion. The term *emotional* insanity is not used by Dr. Noble in precisely the sense in which it is employed by many recent writers; since he refuses to admit into the category of insanity those forms of mental disorder which do not involve some amount of perversion of ideas,—a limitation in which we cannot accord with him,—and applies the term emotional insanity to those cases in which, while the prominent derangement obviously consists in disorder of the emotions, there is at the same time some perversion of ideas.

The sixth lecture treats of the *Diagnosis, Prognosis,* and *Etiology;* the seventh lecture gives a general view of the *Exciting Causes and Physical Treatment;* and in the eighth lecture the principles of the *Moral Management* of the insane are discussed,—the whole being summed up in the following admirable formula:

> "Deal with the physical characteristics which may accompany insanity, as you would deal with them under other circumstances, and act in correspondence with sound principles of medical practice, always remembering that, with high nervous susceptibility, depletion will be but indifferently tolerated, more especially when the ailment refers itself to causes exclusively psychical. For the relief of insanity itself, properly considered, trust almost entirely to hygienic and moral treatment; withdrawing circumstances likely to aggravate the special features of individual cases, and supplying to the mind such objects of attention and excitants to activity [and, we should add, such motives to self-control], as may be best calculated to arouse and sustain a new and more healthful mode of operation."

Holding it, as we do, to be the imperative duty of every practitioner to qualify himself to give a sound diagnosis, and to direct the treatment in any case which may present itself to him, and feeling sure that without some special guidance even those most generally well informed may be led into grievous error, we have great satisfaction in being able warmly to recommend Dr. Noble's little treatise, as extremely well adapted to supply the desideratum which we have long felt and lamented.

PART SECOND.

Bibliographical Record.

ART. I.—*Memoirs of John Abernethy, with a view of his Lectures, Writings, and Character.* By GEORGE MACILWAIN, F.R.C.S. Vols. I. & II.—*London*, 1853.

THESE volumes would give us an opportunity of discussing two interesting points—viz., the character of a great man, and the portrait drawn of him by his biographer—did not the mass of books we have to review warn us that a lengthy dissertation will not be in our power for some time to come. But the volumes themselves will, we hope, be perused by all our readers. They are extremely interesting, and not only give an account of Abernethy, which cannot fail to be read with benefit, but they discuss incidentally many questions of medicine and of medical polity. It may be said, indeed, that sometimes Mr. Macilwain is less biographer than essayist; but this is a fault we can easily pardon. Abernethy's life and character have given him a theme, and in illustrating it he has thrown in something of his own. Indeed, it was difficult to avoid this. In connexion with Abernethy and St. Bartholomew's, the tempting subject of the hospital system could not be passed over. The obligation of our science to Abernethy, naturally led to a consideration of the present position and prospects of the medical art and profession. On both these topics, Mr. Macilwain has a good deal to say, and he has said it well.

The moral character of Abernethy, as depicted in these volumes, strikes us as a just one, as far as we may be permitted to judge without personal knowledge of that remarkable man. Too much is made, we think, of the eccentricity and roughness which are (and, no doubt, in part truly) attributed to him; but the true benevolence and worth of that sterling character are drawn with a faithful pen. Nature had gifted the heart of Abernethy with a rare tenderness and generosity; she had placed on the surface only, the ruggedness which springs from mental disquietude, and from failing health.

We are less satisfied with the intellectual portraiture; and here, we think, in making his work a popular one, Mr. Macilwain has not done justice to his own powers of analysis. Abernethy was no Hunter, and he was no Lawrence; his character, perhaps, was in some respects both inferior and superior to his teacher and his successor. We cannot venture to do so, but Mr. Macilwain might have advantageously contrasted him with these two great men; the study of the various proportions of depth, subtility, and clearness of mental vision in the characters of each, would be very interesting.

Mr. Macilwain is fond of anecdotes, and has inserted a great number; this does not render his work less pleasant reading. We recommend it most strongly, as an interesting, and, at the same time, instructive treatise.

ART. II.—*A Text-book of Physiology.* By VALENTIN. Translated and Edited by WILLIAM BRINTON, M.D. Part II.

THIS part concludes Professor Valentin's great work, and finishes Dr. Brinton's labours. The book will now take its rank in this country as a standard authority on physiology. Its chief characteristics are its abundance of facts, and its conciseness of expression. Scarcely ever is there a word too much, and in this lies its merit. The work consists of 2291 paragraphs, which occupy 669 closely-printed pages. It is illustrated by numerous diagrams and illustrations, which have been very carefully done, and a copious index makes reference easy. In its *getting-up*, therefore, as well as in its proper essence, so to speak, the work is equally commendable.

At the same time, this treatise is not without some drawbacks. The facts are communicated with great clearness, it is true, but with equal dryness. In his power of interesting the reader, as well as in elucidating an obscure subject by detail and illustration, Professor Valentin is by no means equal to Dr. Carpenter; nor, we believe, will his work ever be so generally liked as this latter writer's late masterly treatise. To teachers, and to the higher class of students in physiology, Valentin's work will certainly be very useful, but it is not the kind of book which the commencing student can properly handle.

Dr. Brinton appears to have performed his laborious task with great care and judgment. The notes he has occasionally appended show his own knowledge of the point in question to be great, and that he has thought profoundly on his chosen subject.

ART. III.—*On the Advantages of the Starched Apparatus in the Treatment of Fractures and Diseases of Joints; being the First Part of an Essay to which the Council of University College have awarded the Liston Clinical Medal.* By JOSEPH SAMPSON GAMJEE.—*London*, 1853. pp. 89.

THIS is a practical work, intended to show the beneficial effects of the treatment of fractures by the starched apparatus, as employed of late years by M. Seutin. This apparatus is very simple: splints are made from pasteboard soaked in water, and are then covered both inside and out with a thick coating of starch; the limb is bandaged with a roller covered with wet starch, and the splints are then moulded on the limb, all depressions and tuberosities in which are filled or protected with cotton-wool. An outer bandage, also covered with starch, is then applied, and the limb is kept quiet until the whole is dry, which occurs in about 36 hours. When dry, the bandages are slit up in order to see that the application of the splints has been properly performed, and if any swelling of the limb requires a little loosening of the bandage, or if any shrinking of the bandage requires a little tightening. This being done, a bandage starched on the outside is reapplied, and after this has dried, the patient may leave his bed. The great advantages of this plan are, that uniform pressure is applied, reduction is maintained, and confinement to bed for a long period (as common in fractures of the leg, thigh, and femoral neck, treated on other plans) becomes unnecessary. Mr. Gamjee also states that swelling from extravasation of blood, or from inflammation, need not prevent the application of the apparatus if it be judiciously used; and that if the fracture is a compound one, the only difference of treatment is that the wound may be left uncovered by cutting a piece out of the splint.

In support of these assertions, 17 cases treated in University College Hospital are related, and some judicious remarks are attached to each. Without going into their analysis, or into the details of the manipulation required for each particular fracture, we may observe that the evidence, as far as it goes, is satisfactory.

In succeeding chapters Mr. Gamjee recapitulates the advantages derived from the use of the starched bandage, shows its applicability to some cases of diseased joints, and finally gives a short retrospect of the history of the practice.

The work is well written, and will prove useful, but we must remark that Mr. Gamjee appears to be under a misapprehension when he supposes that the merits and demerits of the starch apparatus (such as they are) are not well known to English surgeons, and have not been commented upon by English writers.

ART. IV.—*Sketch of the Operation and of some of the most striking Results of Quarantine in British Ports since the Beginning of the Present Century.* By GAVIN MILROY, M.D.—*London*, 1853. Small 8vo, pp. 38.

THIS is the re-issue of a paper published in the Association Journal. Like all Dr. Milroy's writings, it is an able paper, but to our minds somewhat fragmentary and inconclusive. The authorities are not quoted with sufficient precision, and the account of the epidemics of Yellow Fever at Gibraltar, &c., is very meagre and, we must say, unsatisfactory. We attach little importance to the voluminous controversy which has taken place on those outbreaks, but if the subject be touched at all, it should be discussed fully. The subject of the *Eclair* fever is only alluded to, and the important question of importation into Boà Vista is passed by. Dr. Milroy may allege that it was not his purpose to do more than consider the subject as it affects British ports, but this limitation of the subject is much to be deprecated. In this way the most cogent example of importation is omitted, and we are called upon to draw a conclusion from the limited experience of a single country.

While we thus express our dissent from the mode in which Dr. Milroy has treated his subject, we fully agree with him that the quarantine system is full of inconsistencies, and requires remodelling. But remodelling is not sweeping away, and quarantine, based on what appears to us an indisputable truth, may be faulty in administration, but must be right in principle. We have always deeply regretted that the Sanitary Reformers, who have really done so much to enlighten the public, and to pave the way for great improvements, should have embarked in a crusade, a hopeless crusade too, against quarantines. Some evil spirit has prompted the word "abolition" instead of "reform," and has, by the unhappy suggestion, cut professional support from under their feet, and will eventually shake the popular faith in their principles and their doctrines. Attempts may be made in periodicals of general literature to influence the public mind, but in the long run the medical profession must decide on a medical question. We are happy to see that Dr. Milroy does not entertain the extreme views adopted by some of his colleagues, and we trust that his knowledge and good sense may eventually lead to a modification of the position assumed on the subject of quarantine by the sanitary authorities of this country.

ART. V.—*Sandgate as a Residence for Invalids.* By GEORGE MOSELEY, M.R.C.S. Eng., &c.—*London and Sandgate*, 1853. 8vo, pp. 136.

IN spite of the comparative ease with which individuals requiring a mild climate may be sent to the south of France, Italy, Spain, or Madeira, every practitioner knows that, in many cases, there are reasons connected with the period of the disease or with the circumstances of the patient, which may induce him to select, if possible, some favoured locality on our own coasts. Hastings, Torquay, the Isle of Wight, &c., are, in many respects, better fitted for invalids than France or Italy. English comforts can be procured, and good English medical attendance is at hand. The vicissitudes of the climate, too, such as they are, are known, and the invalid is never astonished, as at Nice or Montpellier, by the bitter blast of some ice-laden wind, colder far than any he has ever shrunk from in his northern home. To the popular places of resort on the south coast, Mr. Moseley desires to add the name of Sandgate, in Kent. He has written a little book, giving first an account of the town of Sandgate, and of its climate, and then branching off to a general essay on consumption and hygienics. This latter part is evidently intended for the *plebs*, and although true enough as far as it goes, and sensibly written, need not detain us. We turn with much more interest to what Dr. Moseley has to tell us on the climate of Sandgate. He informs us, in the first place, that "sea fog" is comparatively infrequent—no trifling advantage, as those who know Hastings or the Undercliff will acknowledge; then he asserts that, compared with the Undercliff and Hastings, Sandgate has an advantage in the prevailing winds.

"On an average of years," he writes, "there are 24·12 days of north wind in Sandgate, 24·46 in the Undercliff, 62·50 in Hastings; of north-east, 45·80 in Sandgate, 54·61 in Undercliff, 43·50 in Hastings; of east, 37·69 in Sandgate, 60·34 in the Undercliff, 34·50 in Hastings; of south-east, 33·24 in Sandgate, 18·85 in the Undercliff, 15·50 in Hastings; of south, 27·53 in Sandgate, 26·72 in the Undercliff, 17 in Hastings; of south-west, 75·31 in Sandgate, 96·97 in the Undercliff, 137·50 in Hastings; of west, 65·29 in Sandgate, 52·24 in Undercliff, 28· in Hastings; of north-west, 56·35 in Sandgate, 30·95 in the Undercliff, 12· in Hastings." (p. 21.)

In respect of temperature, Mr. Moseley asserts that the annual mean of Sandgate is high (62·14), and that the alternations are gradual. The mean of the seasons is as follows:

Winter.	Spring.	Summer.	Autumn.
41·76	49·50	61·30	56

We subjoin Dr. Martin's table of the temperature of the Undercliff, Isle of Wight; and of Hastings, from Dr. Mackness:

Place.	Winter.	Spring.	Summer.	Autumn.
Undercliff	41·80	49·82	61·31	53·95
Hastings	39·13	47·68	60·61	55·15

The mean daily range of the thermometer at Sandgate is not considerable; according to our author, it is indeed not much more than in Madeira, and in some months is even less. The mean height of the barometer is 29·85; the number of days on which rain or snow falls in the course of the year is 154, the mean annual quantity 30·50 inches. At Hastings the corresponding numbers are 155·43 degs. and 31·94 inches. The humidity of the atmosphere is less than that of the Undercliff.

After this exposition Mr. Moseley has a chapter on the healthiness of the district, which appears to be satisfactory, and thus he considers that he has made the practical affirmation of the proposition that benefit might be expected from the favourable position and climate of Sandgate. We have brought these statements as we find them before our readers, for we possess no data by which to test Mr. Moseley's facts. His book appears to be written truthfully, and without exaggeration. We see no reason to doubt that he has exercised care in the collection of his facts, and if so, we cannot question that Sandgate may take its place by the side of, or at any rate very near to, those favourite places of resort, Hastings and the Undercliff.

ART. VI.—*An Expository Lexicon, of the terms ancient and modern in Medical and General Science.* By R. G. MAYNE, M.D.—*London,* 1853. Part I., pp. 153.

DR. MAYNE has been engaged for many years in the preparation of this lexicon, but the great expense attending the publication of a work of the kind has hitherto delayed its appearance. The present Part is the first of six, and reaches to the beginning of the letter C. We have looked through it, and can safely say that, if Dr. Mayne can carry out the rest of his work in the same way, he will have conferred an advantage on science generally, and especially on medicine.

ART. VII.—*The Microscope, in its special application to Vegetable Anatomy and Physiology.* By DR. HERMANN SCHACHT. Translated by FREDERICK CURREY, Esq., M.A.—*London*, 1853. 8vo, pp. 131.

MR. CURREY appears to have translated his author faithfully, and has given to the English public a book which will be very useful to all who are learning how to apply the microscope in botanical researches. A work in a similar scale and form, showing the mode of investigation in animal, and especially in human, anatomy and physiology, would be more useful to medical men; but yet much information in the manipulatory part of the process may be learnt by the medical student even from this volume on vegetable micrology.

ART. VIII.—*Medical Reform, being the Sketch of a Plan for a National Institute of Medicine.* By AZYGOS.—*London*, 1853. Pamphlet, pp. 77.

THE worst of Azygos is, that he is too good. Give him a clear field, a *tabula rasa*, on which his genius for organization may have fair play, and a full-grown national institute, with "scientific, educational, practical, financial, political, and miscellaneous departments!" would appear, and beneath it would range, in perfect order, all the affairs of hospitals, dispensaries, colleges, and poor-law boards. The objection to the plan is, that it is totally impracticable; it is the dream of a man who has a clear sense of what *should be*, but who ignores, without the least difficulty, everything *that is*. In this pamphlet is a great deal of sense; there are numerous observations which no one will gainsay, and many suggestions which might be adopted with advantage. But its plan commences at the wrong end. Medical reform must come by the re-adjustment and mutual blending of existing institutions, and not by universal destruction and a new creation. We are afraid that, like the constitutions of Sieyes, or of Bentham, the plan of Azygos wont work.

ART IX.—*A Treatise on the Venereal Disease, by* JOHN HUNTER, *with copious additions by* DR. PHILIP RICORD. *Edited, with notes, by* F. J. BUMSTEAD, M.D.—*Philadelphia*, 1853. 8vo, pp. 520.

DR. BUMSTEAD has reprinted Dr. Babington's edition of the treatise on the venereal disease, and has added the notes of Ricord which are contained in the second French translation of Hunter's great work. A few notes are also added by himself, or are copied from Babington. He has thus made an extremely useful work. We have all of us wished to know what Ricord has to say on various Hunterian doctrines, but as no one wishes to read Hunter except in his native, idiomatic, and expressive dress, we can better hear Ricord in English, than Hunter in French. The language of the disciple is justly subservient to that of the master.

We have lately considered Ricord's opinions at length, and need not now recur to them. Those who wish to see them shortly stated and clearly brought out, as derived from, or as modifying, Hunter's doctrines, cannot do better than consult Dr. Bumstead's edition.

ART. X.—*Transactions of the Pathological Society of London.* Vol. IV. *London.* 8vo, 1853. pp. 286.

WE have expressed on several occasions our sense of the value of the successive volumes issued by the Pathological Society. Our own pages, indeed, have shown that the facts recorded in them can be turned to good account. The present volume is, we think, superior even to its predecessors, and contains many instructive cases and commentaries, among which we may especially mention Dr. Peacock's account of the mode of closure of the foramen ovale, and Dr. Hundfield Jones' highly important cases of degeneration of the mucous membrane of the stomach. On this subject, we wait with anxiety for extended and independent observation. Several beautiful illustrations are introduced, and the arrangement of the subject, and the general getting-up of the volume, leave nothing to be desired.

ART. XI.—*Summary of New Publications.*

IN addition to the works reviewed or noticed in previous pages, we have received a number of other treatises, which we propose at present merely to enumerate. We shall have occasion to return to certain of them more at length.

In *Medicine*, there are numerous new books. We may notice first, the commencement of an elaborate work on 'Pathology and Therapeutics,' edited by Virchow,* who is assisted by fourteen other physicians, among whom is Vogel (who undertakes kidney-disease), Lebert (who will treat of disease of the blood and lymphatic system), Hasse (who will write on diseases of the nervous system, &c.) Hebra (who will discuss the cutaneous affections), and others equally eminent. The first half of the first volume has alone appeared; it is entirely contributed by Virchow, and treats of the common forms of disease, of fever, inflammation, congestion, dropsy, &c. The work will be one of great extent, and will, doubtless, be of extreme value and repute.

Dr. Guy has brought out a fourth edition of Hooper's 'Physician's Vade Mecum,' a useful work, too well known to require comment from us.

Mr. Parkin has published a short pamphlet on the 'Remote Cause of Epidemic Diseases,' which is a continuation of a large work on the same subject. Mr. Parkin has investigated the course of cholera and yellow fever with great assiduity, and seeks to connect these diseases with volcanic action. This is a curious hypothesis, which has been worked out fully by Noah Webster, and others, but which is still unsupported by any convincing evidence. Mr. Parkin's former work, and the present pamphlet, are, however, well worthy of perusal.

Mr. Finlay has written a short paper on the 'Remittent Yellow Fever of the West Indies,' which we shall consider with other works on the same subject.

As might be expected, treatises on 'Cholera' are becoming numerous. Dr. Scott Alison, Dr. Hearne, Dr. Gilkrest, and Mr. Barwell, are the authors of the smaller ones before us. Dr. Hearne speaks in very strong

* Handbuch der speciellen Pathologie und Therapie. Erlangen, 1854.

terms of the utility of the administration of acetate of lead (three grains) and opium (1 grain) every fifteen minutes, in the diarrhœal stage. The review of these treatises will be best deferred till the close of the epidemic.

Dr. Lindwurm has published a treatise on the 'Typhus in Ireland,'* in which the opinions of Jenner on the non-identity of typhus and typhoid fevers are assailed. Dr. Lindwurm's conclusions are not, however, supported by his facts, and after a careful perusal of his work, we cannot congratulate him on the accuracy of his deductions, although we willingly admit the faithfulness with which his facts were collected. We shall enter more fully into detail hereafter; at present we content ourselves with directing the attention of those interested in the point to this rejoinder of the "single-fever" school.

A useful treatise on 'Small-pox,'† with much historical information, from the pen of Dr. Eimer, will well repay perusal. The committee of the Epidemiological Society, who contemplate, it is said, an extended treatise on this subject, will find much to interest them in this little work.

'The Blood, in its Diseased Conditions,' ‡ is the title of a work by Dr. Kehrer, from which we expected great things, but which has disappointed us. We shall extract what there is of novelty in it when Lebert's article on the blood, in Virchow's new work, reaches us.

Dr. Bennett's late treatise on 'Tuberculosis,'§ will, of course, be read by every one. The object of the book is twofold; first, to vindicate the author's claim to the credit of having introduced the use of cod-liver oil into this country, and secondly, to state his opinion in detail on the tuberculous and allied conditions, and to give evidence of the use of the oil. As to the first point, there can be no doubt that Dr. Bennett is entitled to the credit he demands, as, most certainly, he first made known the great powers of this now so universally used remedy. With respect to his views on tubercle and tuberculosis, there will, doubtless, be great differences of opinion, and we doubt whether Dr. Bennett can maintain some of his positions. No one can read the work, however, without profit, and if, on a future occasion, we may have to express dissent in some cases, in a much larger number we shall only have to express decided adhesion to Dr. Bennett's views.

Dr. Theophilus Thompson has just issued a reprint of his useful and practical 'Clinical Lectures on Consumption,' which we shall review with Dr. Bennett's Treatise.

Dr. Stokes's long-expected work on the 'Diseases of the Heart,' has just appeared, but too late for review in our present number. It is scarcely necessary to say that it is a most valuable work, although in some points Dr. Stokes speaks less confidently of the diagnosis of heart-disease than we should have expected.

A work on 'Rheumatism and Gout'‖ has been published by Dr. Wiss. It contains nothing novel, and has many omissions. The hot-air bath is very strongly recommended in both affections.

* Der Typhus in Ireland, Beobachtet im Sommer, 1852, von Dr. J. Lindwurm.

† Die Blattern Krankheit, &c., von Dr. C. Eimer. Leipzig, 1853.

‡ Das Blut in seinen krankhaften Verhältuissen, von Dr. F. Kehrer. Giessen, 1853.

§ The Pathology and Treatment of Tuberculosis, by John H. Bennett, M.D., F.R.S.E.

‖ Ueber Rheumatismus und Gicht, von Dr. Wiss. Berlin, 1853.

On 'Dysentery,' two pamphlets have reached us; one from Dr. Macpherson, and one (a translation) from Dr. Canta, of Calcutta. Dr. Macpherson's paper is an answer to a recent Report of the Bengal Medical Board, on Mr. Hare's treatment of dysentery by the long tube, by injections, and by quinine. The general Board came to a very decided opinion in favour of this mode, and Dr. Macpherson endeavours to show that this opinion is ill-founded. The arguments he adduces are certainly very strong, and have shaken our faith in the accuracy of Mr. Hare's observations.

A work on 'Paralytic Insanity and General Paralysis' has been published by Dr. Fabret.* Like most French treatises, it is extremely verbose, and although it gives a good summary of the subject, it contains nothing novel. Still it may be useful to those who are occupied with these affections.

A great work has been published by Wedl on 'Pathological Histology,'† which we shall review at length as soon as we can find room.

In *Surgery* the press has been less prolific. Professor Erichsen's comprehensive 'Science and Art of Surgery' will be reviewed at length in our next issue. Professor Syme's treatise on the 'Diseases of the Rectum' has reached a third edition, and is now a well known work. The third edition of the 'Principles of Surgery,' by Professor Miller, has likewise appeared, and is a work of established reputation. Mr. Druitt has issued a sixth edition of his excellent 'Surgeon's Vade Mecum,' a work which we do not hesitate to call one of the best of its class. In matter, in arrangement, and in style, it is equally commendable.

Mr. Wilde's 'Aural Surgery' requires a separate review. It is a great work, and will become a standard authority.

In *Midwifery* no special works have reached us except an American work,‡ which is somewhat quaintly but justly termed by its author a "skeleton collection of facts." There is nothing novel about either the facts or their arrangement, but the plan is good and the work is an useful one.

In *Materia Medica* the work of Dr. Pereira has been completed by Drs. Taylor and Owen Rees. In the preface the editors observe that as the author is no more, they feel themselves at liberty to state that the work, "in copiousness of details, in extent, variety, and accuracy of information, and in a lucid explanation of difficult and recondite subjects, surpasses all other works on Materia Medica." No one can hesitate to endorse this judgment, and we may add that the last 600 pages, which have been revised by the editors, merit an equal eulogium.

The second edition of Dr. Royle's 'Manual of Materia Medica' has appeared. It is much improved, and bears the marks of careful revision. Mr. Headland has assisted Dr. Royle in its preparation, and the work is most creditable to both its authors.

Mr. Darby has translated Dr. Wittstein's 'Pharmaceutical Chemistry.' It is a very useful work, and should be possessed by every working pharmaceutist.

* Récherches sur la Folie Paralytique, et les diverses Paralysies Générales. Paris, 1853.

† Grundzuge der Pathologischen Histologie, von Dr. Carl Wedl. Wien, 1853.

‡ A Manual of Obstetrics, by Dr. Thomas F. Cock, M.D., Physician to the New York Lying-in Asylum, &c. New York, 1853, pp. 250.

'The Druggist's Handbook,' by Mr. Branston, is one of those books which contain all the little formulæ which a druggist may have to know, from a recipe for cold cream, or for artificial asses' milk, to the mode of preparation of hydrofluoric acid, or phosphoric ether. It is certainly an *omnium gatherum*, but obviously is a useful work for those who want an easy reference for simple and common things. It is a small book. The contents are arranged alphabetically and are easy of reference.

A work on 'Quinine,' by Briquet, is reserved for separate review; so also is a treatise by Werber on 'Therapeutics.'*

In *Physiology*, another part of Ludwig's profound work has appeared. —Dr. Brinton has completed the translation of Valentin's work.—Dr. Axmann† has published some important observations on the 'Microscopic Anatomy and on the Physiology of the Nervous System,' which we shall consider at length.—Lehmann's great work on 'Physiological Chemistry' has reached another edition. A few additions have been made, but the chief alteration consists in the omission or modification of passages which were not formerly in harmony with each other; for, as the volumes of the former edition (the second) were published at successive intervals—about a year elapsing between each volume—the statements in the first volume were sometimes at variance with those of the second and third. One of the additions describes the mode in which Lehmann has succeeded in forming "blood-crystals" in quantity, viz., by passing streams of oxygen and carbonic acid through the blood.

Bischoff has written a very interesting work, on 'Urea as a measure of the Metamorphosis of Tissue.' We have prepared a review of this, and of the second part of Bidder and Schmidt's work, but want of space has compelled us to defer it to our next number.

An excellent treatise on 'Animal Chemistry' has been published by Heintz.‡ The analytical details are given with extreme care.

Dr. Hoffmann has published a translation of the second edition of Liebig's 'Handbook of Organic Analysis.' The work is of course entirely technical, and deals merely with the methods employed in analyzing organic substances.

Among *miscellaneous subjects*, we may mention Erasmus Wilson's edition of 'Hufeland's Art of Prolonging Life,' a work containing much good sense and useful information. Hufeland was a "sanitary philosopher;" and as Mr. Wilson justly remarks, "the 'sanitarians' of our own day have added little to his sensible instructions." Besides this, there are some curious speculations in this work, especially as to the duration of life in the animal world, in the fourth and fifth chapters, for which we shall endeavour to find room, in connexion with a review of Dr. Van Oven's late interesting treatise on the 'Decline of Life in Health and Disease.'—Dr. Hartwig's 'Practical Treatise on Sea-bathing' must be characterized by a reversal of the usual reviewing phrase; it contains little information in much space.

* Specielle Heilmittellehre. Erlangen, 1853.

† Beitrage zur mik. Anat. und Phys. des Ganglien Nervensystems, von Dr. C. Axmann. Berlin, 1853.

‡ Lehrbuch der Zoochemie. Von H. W. Heintz. Berlin, 1853.

PART THIRD.

Original Communications.

Art. I.

The Blood—its Chemistry, Physiology, and Pathology. By Thomas Williams, M.D. Lond., Extra-Licentiate of the Royal College of Physicians; formerly Demonstrator on Structural Anatomy at Guy's Hospital, and now of Swansea.

(*Continued from No. 24, p. 486.*)

Truth not seldom lies concealed in recesses approachable only by a single path. The path must first be seized in order that the destination may be reached. From the vantage-ground only of a clearly conceived *principle* of inquiry can the mind blend into a symmetrical edifice the ununited elements of a novel truth. How often have acute and sagacious minds neared, but missed, a grand discovery! The history of science abounds in apt illustrations. To the artist who works with the materials of observational truth, it is not given as to the sculptor, to embody the first conceptions of the intellect at once, in the faultless and finished Apollo. The errors of one generation, dimming the lustre of truth, are eliminated by the next. In these papers the general proposition has been more than once confidently enounced, that in the zoological series the *fluids* of the living organism grow simpler and simpler in chemical and vital composition in proportion as the lower extreme of the scale is approached, and that, correspondently with the fluids, the fixed solids display a constantly increasing tendency to simplification. To reverse the proposition, the free fluids and the fixed solids constituting the mass of the body acquire, in a corresponding ratio as the animal chain is tracked upwards, a greater and greater complexity of composition.*

As respects the *fluids*, this principle has been substantiated by reference to demonstrative facts. This law of progressive complexity on its application to the floating and fixed *solids* remains to be unfolded. The standard of the fluids is raised by an increase in the proportion of albumen, and by the superaddition, at a certain limit in the scale, of fibrine; the standard of solids is raised by the production of new organized constituents

* In my paper on 'The Fluids,' which was published in the 'Philosophical Transactions' in the year 1851, I ventured to state in general expression the same proposition. At that time, however, I had not through practical research attained to a confident knowledge of the individual facts upon which such a wide-spreading generalization could securely repose. Subsequent investigations enable me to shape a rude prevision into a well-marked principle. I am not at present aware that any approach to the generalized views stated in the text has ever been made in comparative physiology. It is no hasty enthusiasm to predict, that they will confer upon this branch of physiology the character of scientific *constancy* more than any other principle of organization developed by modern research.

within the elementary cells. The relation between the fluids and solids of the living organism is much more intimate, however, and recondite, than that which is implied in this general statement. The blood-proper is the highest form under which the nutrimental fluid occurs in the animal kingdom, but it is not perfect in its composition at its first appearance in the series; it is comparatively simple in its first-born condition: it gradually increases in complexity by a successive increase in the *number* of its ingredients. The *chylaqueous fluid* in the annelids, its superior limit, exhibits a composition much more complex than that which it possesses in the lower radiated animal. In the lowest animal its albumen is least in amount, its floating corpuscles present the lowest features of organization. It may be affirmed as an absolute principle in the chemistry of living beings, that what is not, or never has been, present in the fluids, never can constitute an integral ingredient of the solids.

If *fibrine* forms no part of the fluids of an animal, it cannot exist as a constituent of the solids; it is an absolute organic law that this proximate principle can only be *produced* in the fluids; it is used by the solids only as a building material. Fibrine, properly so called, cannot be manufactured *de novo* out of the elements of albumen by the elementary cells of the fixed solids; these latter cells are capable of no further effort than that of modifying a principle already prepared, into a new and higher organic compound.* Neither albumen nor fibrine exists as such in the interior of any sedentary cells; such situations are occupied only by a principle developed from fibrine or albumen. Below the limit in the zoological scale at which fibrine disappears from the fluids, albumen rapidly falls in relative amount; above this limit both these principles increase in a similar ratio—that is, that animal fluid which contains the largest proportion of albumen contains also the largest amount of fibrine, and conversely, until the latter ceases altogether. A very small proportion of albumen suffices for the production of the simplest order of floating cells; the presence of fibrine is required for the evolution of the highest.

It will be now shown, for the first time in physiology, that the same gradation from simple to complex, from a lower to a higher standard of organization, is traceable in the elementary cells of the fixed, as in those of the floating solids. As the floating solids of the chylaqueous fluid are to the fixed structures of the animals in which this fluid only exists, so are the corpuscles of the true blood to the sedentary solids of those animals in which true blood only exists. Disregarding the floating cells, this new and important physiological law may be thus enounced:—The chylaqueous fluid produces simpler solids than those developed from blood-proper: consequently, the solid structures or organs of those animals in which the chylaqueous fluid constitutes the exclusive medium of nutrition, are more simple than the solids of those animals in which true blood exclusively exists.

Let now the foregoing generalized views be submitted to the test of a

* In a former paper, I stated that in the lowest animal forms, the cells of the *fixed* solids might generate those compounds which, in the example of the higher animals, were produced by the *free* cells of the fluids. More recently, an extended series of observations has assured me that this statement can be accepted only in a qualified sense—that the fixed solids owe their character to the constituents of the fluids in a much more complete and intimate manner than has ever yet been supposed by physiologists.

concrete examination; let the several systems of the fixed solid structures be traced in their serial evolution; let each system be studied in the characters of its *ultimate cell;* let the *nerve*-cell be first investigated, and in connexion with it the apparatus of the special senses; let then the *muscle*-cell be followed throughout the zoological series; let this new path in organic science be opened by the enunciation of the first grand fact, *that the blood-proper system, and therefore fibrine, the striated muscle-system, and the nervous system, with its associated apparatus of special senses, first appear in the animal kingdom at one and the same limit—viz., at the echinodermata!* Let the zoochemist reflect for a moment on the grounds whereon this generalization is based. Can muscular tissue, in its higher striated phase, exist in the solids without fibrine in the fluids of the organism? If muscle-fibrine is not absolutely in every minute particular identical with blood-fibrine (Liebig), the comparative histology of muscle-tissue will hereafter place it beyond doubt that the latter in its higher variety is derived from the former. *In its inferior phases it is developed from albumen.* Can any physiologist for an instant withhold assent to the self-evident proposition that where there is no nervous system there can exist no special senses? But let the facts be stated in a less extreme form. No comparative anatomist has ever yet indubitably demonstrated a nervous system below the echinodermata; why? because it has never been looked for in the right way. Below this limit, this system does not exist under the same characters with those by which it is so readily distinguished above this standard. The nerve-cell, like the muscle-cell, the floating cell, and the fluids, here begin in a marked manner *to simplify as they descend the scale.* Their characters severally are no longer the same. Like the fluids, or rather coincidentally with the fluids, they change. In order to the pursuit of the inquiry, the fact of this altered physical character in the elementary cells must be first known. But why should these extraordinary events occur at this particular limit? Because here, if traced upwards, the true blood-system, and therefore fibrine, begins; if traced downwards, ends in the animate chain. What can be the signification of this remarkable relation, hitherto completely unrecognised in physiology, between the nerve and the blood-system? Wherefore this agreement of origin, this parity of development? Both systems (*in their ordinary characters*) are least developed in the star-fish, both most evolved in man. Abundant evidence drawn from actual dissection justifies the inference that the presence of the nervous element in the organism implies that of the blood-proper, and conversely. From such evidence it is certain that the lower forms of the chylaqueous fluid are deficient in those elements which are essential to the production of nerve-matter. Below the echinodermata, therefore—that is, in the medusæ and zoophytes, in which no trace of blood-system, but only of the chylaqueous fluid, exists—there can obtain no nervous system, as commonly understood in comparative anatomy; the nerve-cell becomes more and more devoid of contents, as above this point in the scale it becomes more and more pregnant.

As the chylaqueous fluid is to the true blood, so is the source of current power below the echinodermata to the *ordinary* nervous system. This "unknown quantity" in organization is yet undiscovered. It must exist, and science will at no distant time define its nature. It will exhibit the same relation to the chylaqueous fluid with that which the higher nervous system displays to the true blood. To affirm, with some physiologists, that in the zoophytes and acalephs, *the* nervous system exists under a diffused form, were to prejudice this novel inquiry by the employment of the vague nomenclature of gratuitous conjecture. It must be as unlike the cerebro-spinal power as the true blood is dissimilar from the chylaqueous fluid. Two fluids so diverse in vital standard and chemical composition cannot evolve identical products. The highly pregnant cells of the grey nerve-matter in the brain of the mammal could not be developed from such incomplex fluids as those of the star-fish.*

The relation of mutual dependence sought here to be established between the nervous and the blood-proper systems, is most instructively exemplified in the fact of the parallelism presented in evolution of the brain or cephalic ganglia and the heart, the *centres* of these two systems respectively. The cephalic ganglia first appear where a central propulsive power for the fluids first appears—viz., in the crustacea in the articulated series. How inferior the standard of the brain in the fish, how degraded in the reptile, how exalted in the mammal! How many groundless speculations in science would not the clear apprehension of this simple principle of organization have saved. What philosophical anatomist will *now* delve into the maze of the lining structures in vain search for indications denoting the presence of special senses in those classes of animals in which the conditions (as respects the fluids) essential to the existence of a centralized nervous system are wanting? It will be afterwards shown that M. Quaterfages, in describing the eyes of the annelida, has fancifully constructed optical mechanisms which could by possibility have existed in presence only of those organic conditions which obtain in the highest animals. If special senses exist in those classes of animals which are situated on the scale below the limits of the *ordinary* nervous and the blood-proper systems, they must necessarily present characters simplified in a corresponding ratio to that distinguishing the elements of the solid parts of

* It is a remarkable circumstance, that a most earnest and extended study into the comparative histology of the elementary cells of the solids and fluids throughout the animal kingdom, should have led me at the same time, but independently, to conclusions on the subject of the "cell-theory" directly opposed to the views so masterly developed by Mr. Huxley in the last number of this Review (October 1st). To me it has constantly appeared, that as the eye of the observer traces upwards the cell of the same *system of solids*, it becomes more and more laden with contents. It is the "endoplast" of Mr. Huxley, in diametric variance to the tenour of his own argument, which *really* increases. The cell-wall, *his* "periplast," as the scale is ascended, becomes evidently more and more subordinate. This element of the cell may indeed project into angles, or alter its contour in other ways; but this is not the symbol of *increasing development*. The purpose answered by the angular elongations of the cell-wall, in the example of the elements of cellular tissue, is merely mechanical—it is connective and subordinate. It is the *grey contents* of the ganglionic nerve-cell which disappear as the scale is traced downwards, not the *cell-wall:* followed upwards, it is *not* the cell-wall (*his* "periplast") which exhibits signs of increasing development—it is the cell-contents, *his* "endoplast." Nerve-matter exists in the acalephs and zoophytes under the simple character of a cell-wall, without any other contents than a homogeneous fluid.

The same observations precisely will apply to the instance muscle-cell. It is the *cell-wall* which first appears in an identifiable form in the animal series. The cell-contents, as the observer mounts upwards in the chain, assume a palpably organized aspect. The *striæ* appear. The *nucleus* acquires a character of increasing importance. These *facts* are utterly irreconcilable with the theory of Mr. Huxley. If true of any form of elementary tissue, his views can only apply to the cells of *areolar* tissue. In this instance, the cell-wall, *his* "periplast," does seem indeed to perform a function, and to assume physical characters superior to those of the cell-contents, *his* "endoplast."

the organism in the same classes; they must see without eyes, and hear without ears! At no distant period, however, more extended and philosophical views will *direct* the researches of science in the resolution of these recondite points in vital dynamics.

The comparative histology of the muscle-cell teaches a lesson of extreme interest and value in these inquiries; the study of this tissue will hereafter afford material aid in unriddling the enigma of the origin, uses, and destination of the floating solids of the nutritional fluids. On a future occasion detailed reference will be made to the researches of Mr. Bowman and M. Lebert upon this branch of histology. Neither of these distinguished observers has seized the clue to the real law by which the development of muscle-cell in the zoological scale is governed. There can be no doubt, that in the instance of this tissue as in that of other elements of the solids, the *principle of embryonic development* is strictly analogous to the zoological—that is, as the fluids grow in complexity of composition with the growth of the embryo, so the elementary cells of the solids rise in the scale of organization. The author has verified this principle in the most satisfactory manner with respect to the floating cells of the embryonic fluids. They exhibit gradations which distinctly correspond with the several steps in the animal series; there is a remarkable agreement between the embryonic and zoological laws; future science will prove this to be more intimate than it is now conceived. The *striation* of the muscle-cell begins at the crustacea; this character is lost below this limit; but the muscle-cell does not disappear; it exists under a simpler construction; it consists of a cell of variable shape, filled with a homogeneous albuminous fluid. The muscle-cell in the acalephs and zoophytes has never yet been clearly traced; it will be subsequently described. In these lowly forms it is the *cell-wall only* that exists. The *irritability* of this cell is *inversely* as its relative position in the scale — its *contractility directly*. The characters of the muscle-cell in its rudimentary or simplest form, prove with great clearness that *irritability* is the primary property of this cell, and it indwells in the *cell-wall*. Fluid is incapable of altered bulk. The contents of this cell, therefore, in its first condition, being simple fluid, cannot minister to the dynamics of the organule; *contractility* is a quality which is superadded to the former at the highest stages of its growth. The *contents* assume in this comparative view an unquestionably higher organic title than the cell-wall; but the latter is not an inert element, it is endowed with active properties; they persist after the departure of the former. The completeness of the former marks the maturity of the cell. The cell-wall is comparatively perfect, even at its first appearance in the zoological series. It is evident, therefore, that irritability is a lower order of power than contractility; the former inheres in the muscle-cell, and is independent of the nervous system; the latter is superadded coincidentally with the occurrence of nerve-tissue in its higher form in the organism, and with the evolution of fibrine in the fluids. It is the product of the reagency of nerve-power upon muscle-cell in the higher conditions of the latter. *Voluntary* muscular action, therefore, presupposes a certain organization in the muscle-cell; under the circumstances of the *simplest* form of the latter, this mode of action is not possible. In tracing the muscle-cell upwards in the animal series, it is not the *cell-wall* which acquires greater and greater complexity, it is the nucleus and the protoplasm;* not the sarcolemma, but the nucleus. The growth and increment of living matter does not occur in the cell-wall; this is stationary; these vital movements occur endogenously not exogenously, centripetally not centrifugally. To the cell-wall, therefore, belongs unquestionably a nutritive, formative, productive value, as well as a dynamical. Comparative histology *proves* it to exist *anteriorly* to the contents; the latter must, therefore, owe their existence to the agency of the former.†

Thus, only a prophetic outline has been unfolded—views of organization, which are destined to grow in after stages into the importance of the *first principles of physiological science.* They impart to this department of knowledge the character of *constancy.* They invest it with all the fascinations of a *true science.* They claim an exalted rank. They prove it to be capable of wide-extending generalization. Given the fluids, it is required to judge of the zoological rank of the solids. Science smiles with joy at the captivating newness of the problem. The *unity of organization* is no longer a phrase; it is a substantive reality; it is a demonstrated principle. It is not a vision of an enraptured brain, but a veritable law. *One idea* pervades all animate nature. It is legible in the solids as in the fluids. Both obey the same mysterious impulse of evolution. Both march in parity of growth. Both acknowledge the same governance. The clear apprehension of generalizations is at once the triumph and the summit of science!

This apparent digression is not chargeable with irrelevancy. It elucidates the chief question now being considered. If the fixed solids in those classes of invertebrata in which the chylaqueous system only exists, differ so signally from the *fixed* solids which accompany the blood-proper system, it is certain that the *free,* floating solids of the fluids in these classes respectively, must be separated by no less marked differences.

Echinodermata.—In the zoophytes and medusæ no *cavity* exists *external* to the digestive apparatus. Everything is solid. The stomach is multiplied into diverticula. These diverticula are equivalent and homologous to the splanchnic chamber; they contain a corpusculated fluid. It is the only and exclusive ministering agent of solid nutrition. This is unmixed "*Phlebenterism*"‡—that is, the stomach opens directly into those canals

* I would suggest the use of the word *mesoplasm* to distinguish that compound which occupies in all cells the space between the nucleus and cell-capsule. The word *protoplasm* introduced by Schleiden involves *a theory.* The term *mesoplasm* simply affirms locality.

† The argument developed in the text is not the offshoot of a reckless speculative fancy. It is the justified growth of very extended and laborious research. I undertake to substantiate the propositions stated in the text only in general terms, by subsequent reference to facts, details, and illustrations drawn from actual nature. I am very deeply imbued with the belief that these views, when elaborated into the ripeness of full and finished demonstration, will constitute hereafter grounds in physiological science, whereon to rest wider and grander induction than has ever yet been attempted.

‡ At a future stage of these studies, while discussing the fluid systems of the mollusca, I hope to set at rest the controversy between M. Quaterfages and Messrs. Alder and Hancock, on the subject of '*Phlebenterism.*' At present, I will only venture to state, that neither of these distinguished controversialists has clearly apprehended the deep meaning which lies concealed beneath the exterior of this term. The English naturalists are undoubtedly further removed from the truth than the French *savant.* I claim the privilege only, in this place, to declare, that at least for twelve months before the memoirs of M. Quaterfages had come to my notice, my views with respect to the structure and uses of the cæcal diverticula of the alimentary system in the entozoa, annelida, and mollusca had been matured and published. (See Report on the British Annelida, Trans. of Brit. Association, 1851, and Paper in Phil. Trans., in 1852.)

which represent the blood-system—gastro-vascular. The word *Phleben-terism* is strictly applicable to the digestive and vascular system of no other class of animals than the zoophytes and acalephæ. Here the digestive diverticula are charged with the true nutrimental fluid—they are "Veins." In the echinodermata these parts are no longer the homologons of vessels. They are filled with chyme, not nutritive fluid in its matured form. The latter is lodged in a *separate* and *closed* chamber. In this class, for the first time in the animal series, the immediate products of digestion are divided off from the true nutritious fluids. The peritoneal cavity, destined in the vertebrated animals to prevent friction between the abdominal viscera and the parietes, in the lower invertebrata becomes a reservoir of nutritional fluid. It is the normal anatomical place of the chylaqueous fluid. In no single example above the echinodermata does it *directly* communicate either internally with the digestive organs, or externally with the surrounding elements. Its fluid contents are derived from that of the digestive cæca. It is an independent system. In this class it is the grand agent of nutrition. Though holding in solution only a small proportion of albumen, and scantily corpusculated, its nutritive capacity admits of conclusive demonstration. The inferior echinoderms, the Asteriadæ and Echinidæ, are remarkable for the passive inertness of their habits. Distinguished for the sluggishness of their muscular power, they move by indirect mechanical provisions. Delle Chiaije, Tiedemann, Sharpey, and Dr. Grant describe a *nervous system* in this class. The author has instituted numerous dissections in search of this system. He is persuaded that, like the blood-proper system in the Asteriadæ, Ophiuridæ, and Ophiocomidæ, Tiedemann's description exaggerates its true and real proportions. The nervous, blood-proper, and muscular systems in these inferior genera of echinoderms, present *equally* the same character of *incipiency.* This fact is one of extreme interest. It will be presently shown to be destined to reflect bright light on the question which respects the character and origin of the floating solids. If the echinoderms were endowed with a high degree of muscular activity, it might be predicated with certainty that they were also gifted with a correspondingly developed vascular and nervous systems, and the opposite. One system could not, *by its very laws,* exist without superinducing the others. If the scalpel of the zootomist demonstrates the presence of a clearly defined *nervous* system, the physiologist, enlightened by a right apprehension of the principles herein advocated, may infer with confidence that a true-blood system must also exist. This is *science* in its most exalted state. Such method of reasoning is uplifted immeasurably above empiricism. It clothes chaos in the attractive apparel of certainty.

In the echinoderms, possessing the blood-proper and nervous systems only in their most indistinct and rudimentary conditions, the *organs of special sensation* are not physiologically possible. An apparatus of special sense supposes a complex evolution of some part of the periphery of the nervous system, if not also of the vascular. Can the circumference of a system antecede the development of the centre? Such supposition is opposed to every canon, everything that is *certain* in organic science. The ocelliform spots observed by Professor E. Forbes at the extremities of the rays of certain species of Stelleridæ, and supposed by this naturalist to be connected with the extremities of nerve-filaments, and described as protected by a peculiar arrangement of minute spines around each, are really nothing but pigment points. They are not constant in different individuals of the same species. They do not approach in minute structure to that of true ocelli. There lies beneath them, or in connexion with them, no concentration of nerve-matter. They are remote from the real centres of the nervous and blood systems. They possess not one character distinctive of an optical instrument. The microscope proves what *à priori* principle demands—that they are not eyes.

The echinoderms may be emphatically distinguished as the "region of cilia." *In them muscularity is transmuted into ciliarity.* As the muscle-cell surrenders its contractility it acquires an extra proportion of irritability. What it loses in the one property it gains in the other. This fact offers a significant clue to the discovery of a new law in biological science —that *ciliarity* is really only that property in an epithelial cell, which in the muscle-cell is denominated *irritability.**

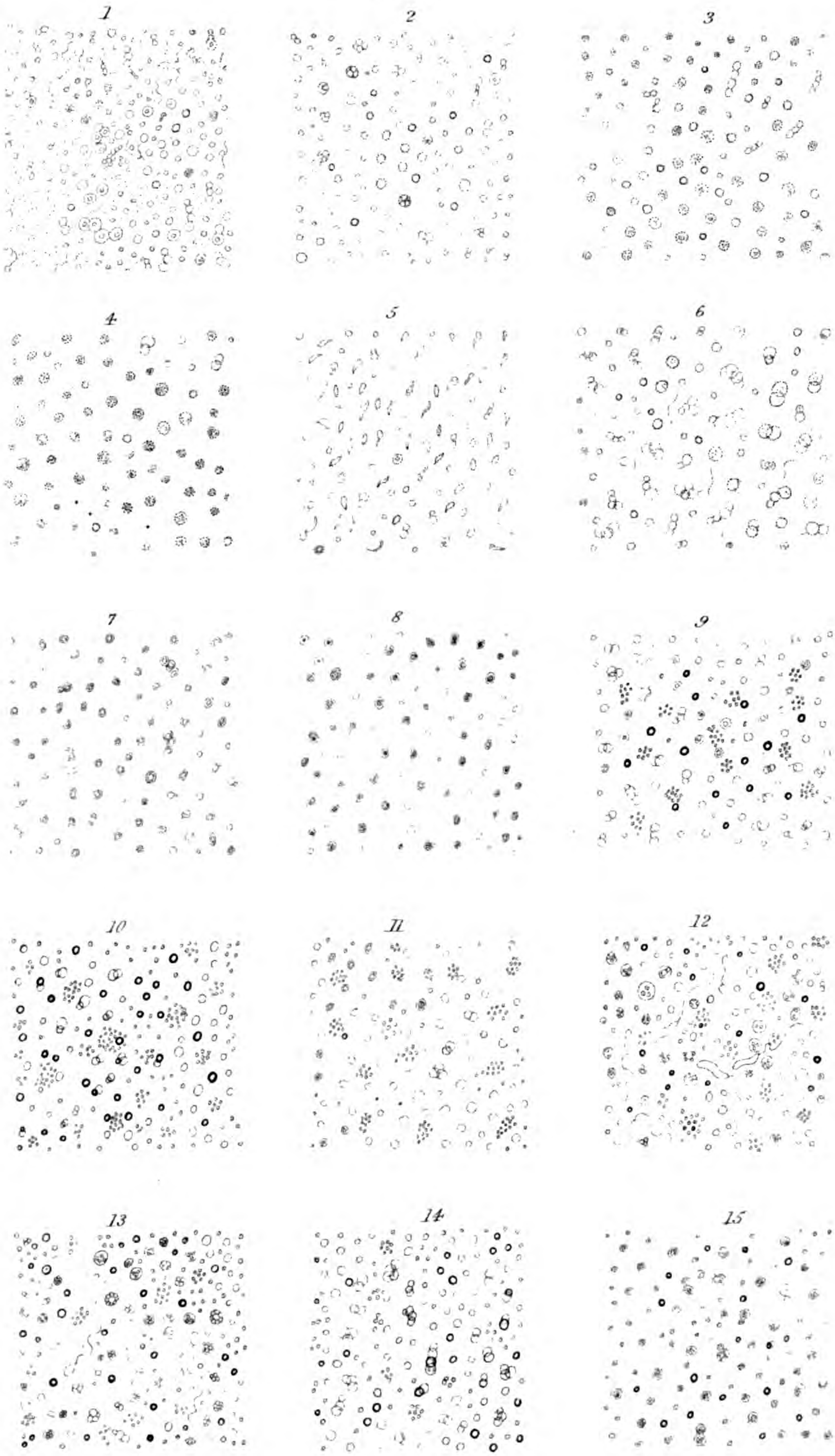
In these torpid, motionless animals, ciliary replaces muscular power. The arms of the Ophiuridæ display an exquisite degree of apparent sensibility—that is, they writhe and contract for some time upon the contact of the minutest object. And yet, in the soft structures of these sensitive parts, it is impossible to detect any other element than the smooth, *irritable* cylindrical cells, so abundant in the soft structures of the actiniform zoophytes. These cells offer no approach to *striæ. Striation* in muscular fibre expresses by its degrees the measure of *voluntary* power. The doctrine which contends for the interchangeableness of muscular and ciliary power, receives support from another and most extraordinary order of evidence. In insects and crustacea a true ciliated epithelium does not occur. *These are the very classes in which the muscle-cell first acquires the striated character!* The suppression of the *ciliary* is marked by an augmented development of the *muscular* system. Is not this signal coincidence a demonstrative proof of the convertibility, of the correlativeness of these two great molecular forces? The word "voluntary" implies a *centre* of volition. In the radiata such centre has no existence. If this, the highest order of power, has been denied to the radiated and annulose animals, the *striated* muscle, its executive instrument, *cannot* have been provided.

Thus through phenomena the mind mounts to the apprehension of principles—it rises from matter to "force."

Among the Asteriadæ and Echinidæ the author has instituted numerous and rigorously exact dissections, with a view to define the character of the

* The numerous and varied researches which, during the last ten years, I have prosecuted into the organization of the invertebrated animals, have amassed in my mind abundant evidence tending to this conclusion. The muscle-cell of the actinia is highly irritable. This property inheres in the *cell-wall*. The cavity of the cell is filled only with a non-granular fluid plasma, in which it is impossible that any such property can reside. In these animals, of whose ultimate structural elements irritability is so marked a characteristic, the *ciliated cell is capable of altering the form of its outline*—that is, the *membrane* of the cell-wall, as well as the cilia, which are substantively its continuations, *contracts* and *dilates*, like the involucrum of the muscle-cell. This extraordinary fact cannot be observed in a single detached ciliated cell; but in *an adherent group* it admits of convincing demonstration. This plain principle divests ciliarity of its mystery; it is no *new* power; it is muscularity seated on an unexpected element. It is the normal property of the lowest form of muscle-cell manifested by a differently configurated organule. Kölliker's discovery will hereafter extend its range from the deep regions of the skin to the superficies—from the dermis to the epidermis.

Plate 1.



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nervous system. He is *certain* that grey nerve-matter, the ganglionic, has no place in the organization of these genera of echinoderms. The *low* character of the muscle-cell, the incomplete condition of the blood-proper system, impress his mind deeply with the belief that in such simply constructed organisms, ganglionic centres, grey nerve-matter, can have *no purpose* to *answer*. The blood-system here does not *circulate* its contents. The parietes of the vessels are not endowed with a power of *contracting* upon the contained fluid. This fact admits of ready and unquestionable proof in the conspicuous bloodvessel of the *Sipunculus*. The first business of the nervous system is to confer upon the true-blood system the power to circulate its fluid contents. Fluid cannot move of itself; its motion always is due to an applied force. The absence of this circulating power involves necessarily the absence of a *centralized* nervous system. Thus theory reaches the eminence of a new truth, the practical demonstration of which may remain to distinguish a future epoch in anatomical science.

Innervation is executed in these inferior beings through the instrumentality of an apparatus, the characters of which are not yet known. Analogy drawn from the demonstrated descensive simplification which occurs in other systems of organs, convinces the author that a corresponding simplification *must* take place in that of the nerves.*

Could the brain of a mammal be sustained materially or dynamically by the fluids of an echinoderm? This is an extreme interrogatory; but it imparts cogency to the argument. It gives substance and validity to the principle which demands that the fluids and the solids should stand in a *direct* ratio to each other.†

The simple nerve-cell of the radiata and zoophytes will hereafter be proved to bear the same relation to that of the higher animals with that which the muscle-cell of the former does to that of the latter.

The fluids of these classes respectively will exhibit a similar relation.

In organisms of which the solids are so simple‡ the fluids must exhibit

* On a future occasion, while discussing the *unity* of the laws by which the fixed and floating solids are governed, I will verify the statements contained in the text, by the testimony and authority of illustrations drawn with the utmost care from nature.

† I may be pardoned here for digressing so tangentially into a subject which, however, grows most naturally out of the chemico-vital question argued in the text. I prophecy, with a feeling profoundly earnest, that when pathological fluids and pathological solids shall be studied in their reciprocal relations, in accordance with the logic of the general arguments which I have endeavoured to pursue in these papers, with reference to the physiological fluids and solids, a distinctive era will be established in pathological inquiries. *Why* is it that a muscle-cell has never been developed from the constituents of a pathological fluid—never demonstrated among the elements of a pathological solid? I am persuaded that the *nerve*-cell of inferior pathological solids, when clearly and comparatively defined, will be found to bear the stamp of organic degradation, to exhibit the same relation to the fluids out of which it was evolved, as the physiological nerve-cell in the same organism does to the physiological fluids. But in the human body, how is the pathological element of the fluids, *if separable*, to be separated? Not by immediate demonstration. A clear apprehension of the abnormity of the fluids can only be inferentially reached through a rigorous study of the visible, measurable, demonstrable constituents of the pathological *solids*. Not the *cell*-elements only, but the organized pathological systems, the vessels, and the nerves. These are hints diffidently projected as incentives to acuter minds and more experienced pathologists. No one at present can say what *sort* of pathological solid will accrue from a *given* description of pathological fluid. Nor until this refined summit is attained can pathology rightly claim the honoured distinction of *a science*. *Pathology* is now really nothing but a vast wilderness of *un*united facts. The quickening *principle of order* and classification is wanting. The time, however, is fast approaching when a far-seeing and wide-extending genius will pronounce over this sandy waste the solemn fiat, "*let there be light!*" and there shall be light.

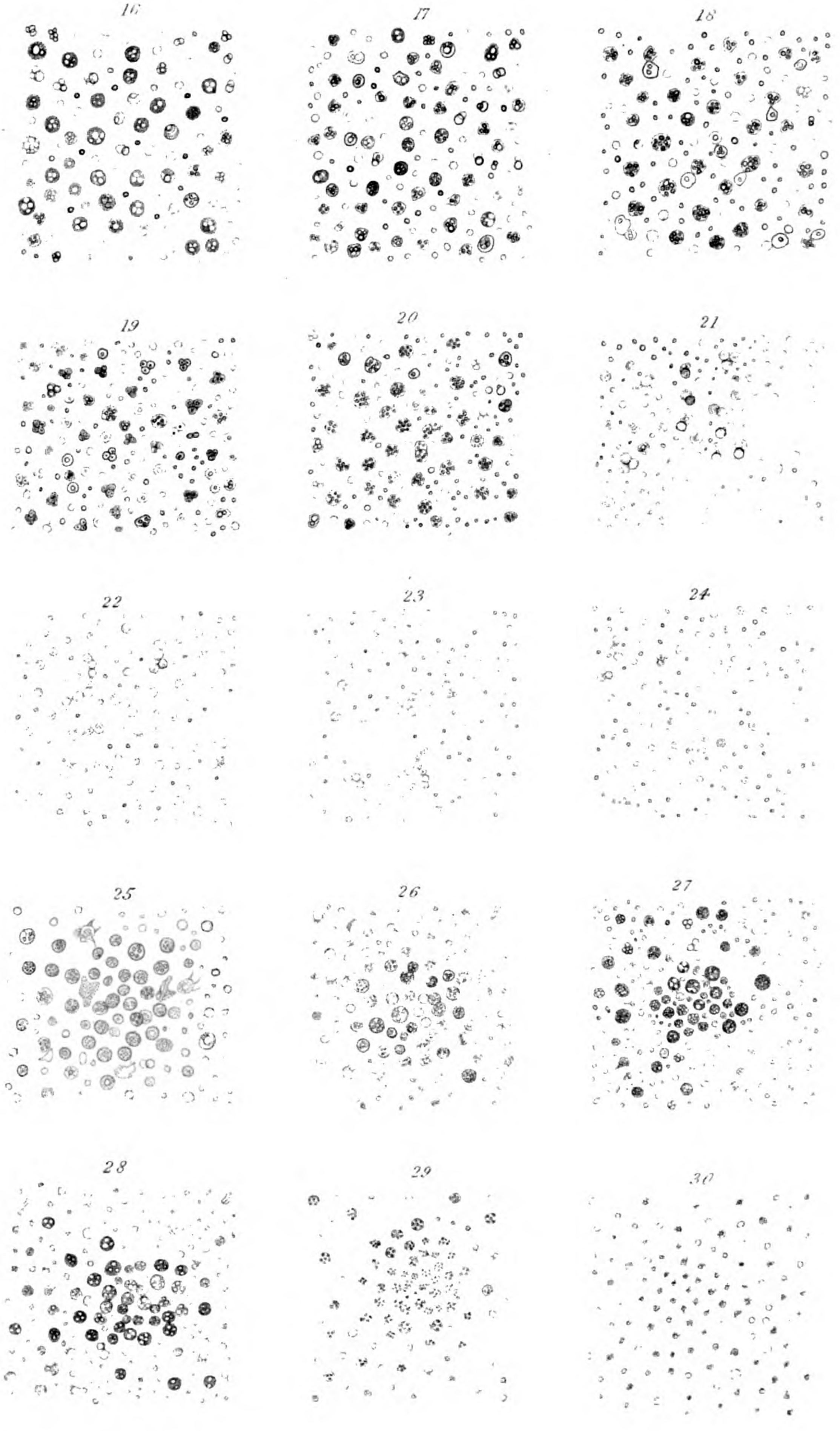
‡ The words "simple" and ' complex," "simplicity" and "complexity," which occur so

a *correlative* simplicity. Thus a *new* and powerful argument is developed by the study of the solids, which lends its force to sustain the conclusions derived from the immediate examination of the fluids. The physiological capacities of the fluids cannot be satisfactorily proved, so long as the fluids only are subjected to analysis. The results of this analysis must be compared with those obtained by a similar investigation of the solids. A comparative *chemical* analysis of these two grand constituents of living beings rewards the labourer far less satisfactorily than a *physiological.* To prove that both contain albumen, or that both contain certain salts in common, &c., is labour fruitless of good. It accomplishes more for science to demonstrate that, in *all animals,* the liquid *un*organized and the solid organized moieties of the living body, are *invariably* and *necessarily* linked into unity by an intimate agreement in general properties. The simple fluid produces a simple solid, the complex a complex. This method of investigation will afford novel aid in unravelling the tangled knot of the rise, function, and decline of the floating cells of the fluids. These latter must stand in the same relation to the fluids as the organized fixed solids. Both are derived from the same source. If *fibrine* does not exist, nor ever has existed, in the fluids, it is quite certain that in neither the locomotive nor sedentary solids *can* it be present, unless it be manufactured *de novo* out of some other element by the latter. Such an idea is as yet wholly gratuitous and untenable. It is supported by no single ascertained fact. The fluids are the scene wherein are *prepared, produced,* the organic principles engaged in the fabrication of the solids. True fibrine and albumen exist nowhere but in the nutritive *fluids.* By the muscle-cell the former is modified only into musculine; by the areolar and cartilage cells the latter is *modified* only into gelatine, &c. Zoochemistry is indeed at present little acquainted with those liquid principles which are contained in the interior of the cells of the sedentary solids. Organic chemists have long supposed that a certain class of solids can be derived from a certain correlative class of the constituents of the fluids. By the late Dr. Prout this argument was prosecuted with great ability into the domain of pathology. Upon its basis his genius raised a new theory of therapeutics. Comparative histology lends to this ancient doctrine an unexpected sanction. It is *quite certain* that those elements in the organized solids of the higher animals which do not exist in the organized solids of the lower, must be derivatives of those elements in the fluids of the former which are not present in those of the latter.

The preceding discussion is not barren of results. It pioneers an easy road to a new country. The science of life and living things must be studied as a whole, not as a part. Nothing in the vital organism can be understood if isolated from the systems within systems of which it is an integer.

repeatedly in these papers, should be here clearly defined. The word "simple," in the acceptation in which it is employed in the text, is designed to denote an organic substance the elements of which are believed to be less numerous than those of the "complex." The terms "inferior" and "superior," "perfect" and "imperfect," "degraded" and "exalted," are objectionable in their application to natural things. Everything in nature is *perfect in its place.* Nothing is more exalted than another. The "complex" could not fulfil the ends of the "simple." In the *place* of the "simple," the "complex" would be valueless in the mundane system. The muscle-cell or nerve-cell of the zoophyte is more "simple" than the corresponding structures of the mammal, because the former contains a lower order and a fewer number of elements than the latter.

Plate II.



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Having now proved that in the echinodermata the whole apparatus of the fixed solids is remarkable for its "simplicity," let the study be resumed which concerns the *free solids of the fluids.*

In this class of animals the nutritive fluids are first gathered into separate and independent systems. The digestive organs are no longer in open communication with the receptacles of the nutritional media. The latter are enclosed in closed independent chambers. Thus is established, for the first time in the animal kingdom, the distinctness of the chylaqueous system. It is a criterion of advanced development. The zoophytic character is here lost. This fact, estimated apart from all its organic relationships, bears the impress of no significance. But view it in its relative bearings, what does it signify? It marks a new and extraordinary epoch in the *creative* history of organic nature. Coetaneously with it come also into existence, under the same circumstances of rudimentary incipiency, the nervous, the muscle, and the blood-proper systems. This is not a fortuitous concurrence of epicycles; one necessitates the other.

It has already been proved that, although this is the exact limit, in the chain of animal life, at which *four* new systems of organs arise, it is *not* the inferior boundary of that of the floating cells of the fluids. The *phlebenteric* fluids of the zoophytes and acalephæ *corpusculate.* The tendency to corpusculation is co-extensive with the animal fluids themselves. The floating cells must, therefore, envelope some recondite meaning. What is it in the instance of the fluids of the echinodermata? The corpuscles of the chylaqueous fluid in the Asteriadæ and Echinidæ are very distinctive in microscopic characters (pl. 2, figs. 21 to 26). They look like spherules of hard and very minute granules of coagulated albumen; they are scantily distributed only throughout the mass of the fluid; they are remarkable for the *absence* of the oleous principles; they possess neither a detectible nucleus nor involucrum; the constituent granules of each corpuscle feebly adhere together; they are readily diffused into individual molecules. This circumstance argues the absence of *fibrine.* It is the cohesive substance, the cement, which, in the case of the blood-corpuscles of the crustacea, unites the component granules. The proportion of albumen contained in the chylaqueous fluid of the Asteriadæ and Echinidæ is remarkably small. The acids render it only opalescent. No clot is formed by heat. It betrays indeed all the *apparent* characters of inorganic sea-water. For this element it was actually mistaken by Tiedemann, Sharpey, and Müller, and the multitudinous copyists after them have re-enacted the error. The presence of albumen, though in minute quantity relatively to the bulk of the fluid, admits of two modes of demonstration. If the fluid, which in these genera is readily collected to any amount, be first strained through fine linen, in order to separate the corpuscles, the complete freedom of the fluid from the latter being tested by the microscope, heat and nitric acid will throw down a conspicuous cloud of albumen. It requires repeated observations to familiarize the eye with the exact characters of the corpuscles. They may be mistaken for the ciliated cells, which are detached into the fluid from the parietes of the containing cavity. They are *undoubtedly* peculiar to, and formed in, the chylaqueous fluid. They are at once the evidence and the product of the *vital* properties of this fluid. They are constant in the same individuals of the same species, different in different.

A close examination of the behaviour of these living solids, conducts to a new proof of the organic nature of the fluid in which they are formed. When placed in pure sea-water they soon fall to pieces. The fluid insinuates itself between the component granules and separates them. If that contained in the peritoneal cavity were really, as supposed by Tiedemann, *un*living sea-water, it is certain that they would behave in the same way in it as in that drawn directly from the sea. The physiological presence of these corpuscles in the fluid of the peritoneal cavity, is, however, placed beyond all dispute by subjecting the little Ophiocomidæ to examination as transparent objects. The animal, being perfectly fresh and perfectly uninjured, will inject and distend fully the integumentary membranous processes with the fluid of the peritoneal cavity. As it whirls in the little transparent cæcal process, its *corpuscles* may be recognised and defined with perfect clearness.

In Comatula Rosacea, abundant in Langlan Bay, on the coast of Swansea, a second (anal) orifice is added to the alimentary system. This fact does not involve a change in the characters of the chylaqueous fluid. Its corpuscles (pl. 2, fig. 21), correspond with those of the Ophiocomidæ (figs. 22, 23, 24), and Asteriadæ (figs. 25, 26, 27). It occupies the same cavity. As in the case of the latter genera, it is *internally ciliated.* The corpuscles are small in size and spherical in figure. They are destitute of nuclei. The oil-element is microscopically and chemically wanting—ether extracts none. A few transparent filmy cells are intermixed with the proper granulous corpuscles. This description of pellucid empty cell is found in every variety of chylaqueous fluid. Its origin is mechanical and accidental.

One type of cell prevails in the chylaqueous fluid of all the Ophiocomidæ. It is a minute, delicate, granulose, spherical cell (figs. 22, 23, 24). In the larger Asteriadæ these bodies present a slightly augmented development. They are larger than those of the Ophiocomidæ, relatively to the size of the animal. The component granules are larger and denser. Sometimes a slight shining molecule appears in the centre of the cell. It has the apparent character of an oil drop. It cannot be so, for ether dissolves out no trace of oil from a mass of these corpuscles. Those taken from the chylaqueous fluids of Solaster Papposa (fig. 26), Cribella Oculata (fig. 27), conform in every particular to the description just given. Though in the Echinidæ a second opening occurs in the digestive organs, the fluid contained in the great chamber of the shell agrees accurately with the chylaqueous fluid of the Asteriadæ. It is not more highly organized. Its corpuscles are not more numerous. In composition they are not superior to those of the latter orders. They possess no nucleus, nor do they contain oil (fig. 28). The light, bright molecules in the interior are optically produced. In Spatangus Purpureus (fig. 29) they are less crowded with granules.

In passing from the Asteriadæ and Echinidæ to the Sipunculidan genera, a very striking change in the number, form, and structure of the corpuscles of the chylaqueous fluid is remarked. The cell is no longer orbicular; *it is a flattened oval.* The Sipuncle is neither a spheroidal nor stellate animal; it is vermiform and cylindrical. Can the *figure* of the corpuscle of the fluid bear *any* relation to that of the body of the animal? Innumerable facts disprove this conjecture.

In the cell of the chylaqueous fluid of the Sipuncle (figs. 32, 34), the hard points of coagulated albumen, the granules, *disappear*. They are replaced by one, two, or more molecules of an oily character in each cell; these molecules display a faint reddish tint. *This fact should be signalized as the first essay of nature to develop pigment in the interior of floating cells of the fluids.* The chylaqueous fluid of the Sipuncle, viewed in mass, exhibits a marked pinkish hue; it presents the same precise colour as the fluid contained in the bloodvessel. In all the Sipunculidan genera it is very thickly charged with albumen. A dense clot is precipitated by heat and acid. The contents of the corpuscles *fibrillate* on bursting, and a stringiness is exhibited by the precipitate carried mechanically down by the subsiding corpuscles. But the presence of fibrine in the nutritive fluids is rendered inferentially *certain* by the augmented development manifested by the muscle and nerve cells. The cephalic end of the Sipuncle is capable of vigorous voluntary muscular action. It protrudes and withdraws its fringed tentaculated head with great activity. This, then, completes the descriptive account of the corpuscles of the chylaqueous fluid of the echinoderms. Let the inquiry be now instituted as to the *process of corpusculation;* what is the meaning of this tendency? where and whence does it originate? whereunto does it point, in this class of animals? At the echinoderms, as already stated, begins the *closed fluid series.** The latter represents a mixture of chyme and a nutritive fluid, the former of blood and water. The latter is *in part* an *aquiferous* system, the former in part a digestive.†

That which the author has recently distinguished as the chylaqueous order of fluids, has for half a century been known to natural observers under the name of the *aquiferous*. The aquiferous system of naturalists supposes an immediate passage of the external water into the cavities of the animal body. The author admits that, into the chambers containing what he has ventured to designate as the chylaqueous system, the external element can only find admission through the digestive and cutaneous parietes—that is, through *imperforate membranes by endosmosis*—for the chamber containing the true chylaqueous fluid is a closed space: it has no *direct* communications with the outward medium. The external element must, then, pass through a *living* partition in order to gain the cavity of the peritoneum. The organic principles can only be derived from the digestive cæca. In these receptacles chymification is accomplished. The author has lately proved, by very clear observations both in the Asteriadæ and Sipunculidæ, that the nutritive fluids, *before* they leave the digestive system, that is, while they are yet *within* the alimentary diverticula, *manifest a very distinct tendency to corpusculation.* This fact is really a crucial demonstration. It is conclusive of one point in the history of floating cells—that they are capable of arising *spontaneously* in the fluids—that is, without the intervening agency of any pre-existing solid, fixed or free. In

* See the author's paper in the Philosophical Transactions for 1851.

† In recently re-examining the corpuscles of the fluids of the acalephs and zoophytes, it appeared to me certain that they were distinguishable from those of the chylaqueous fluid of the echinoderms in two prominent features—1st, that they were absolutely and relatively larger; 2nd, that they contained oily molecules. Such characters are distinctive of the corpuscles of the *open* fluid series, and which in future I propose to denominate the *phlebenteric corpuscles.* Corresponding phlebenteric corpuscles will be afterwards described in the entozoa, annelida, and mollusca.

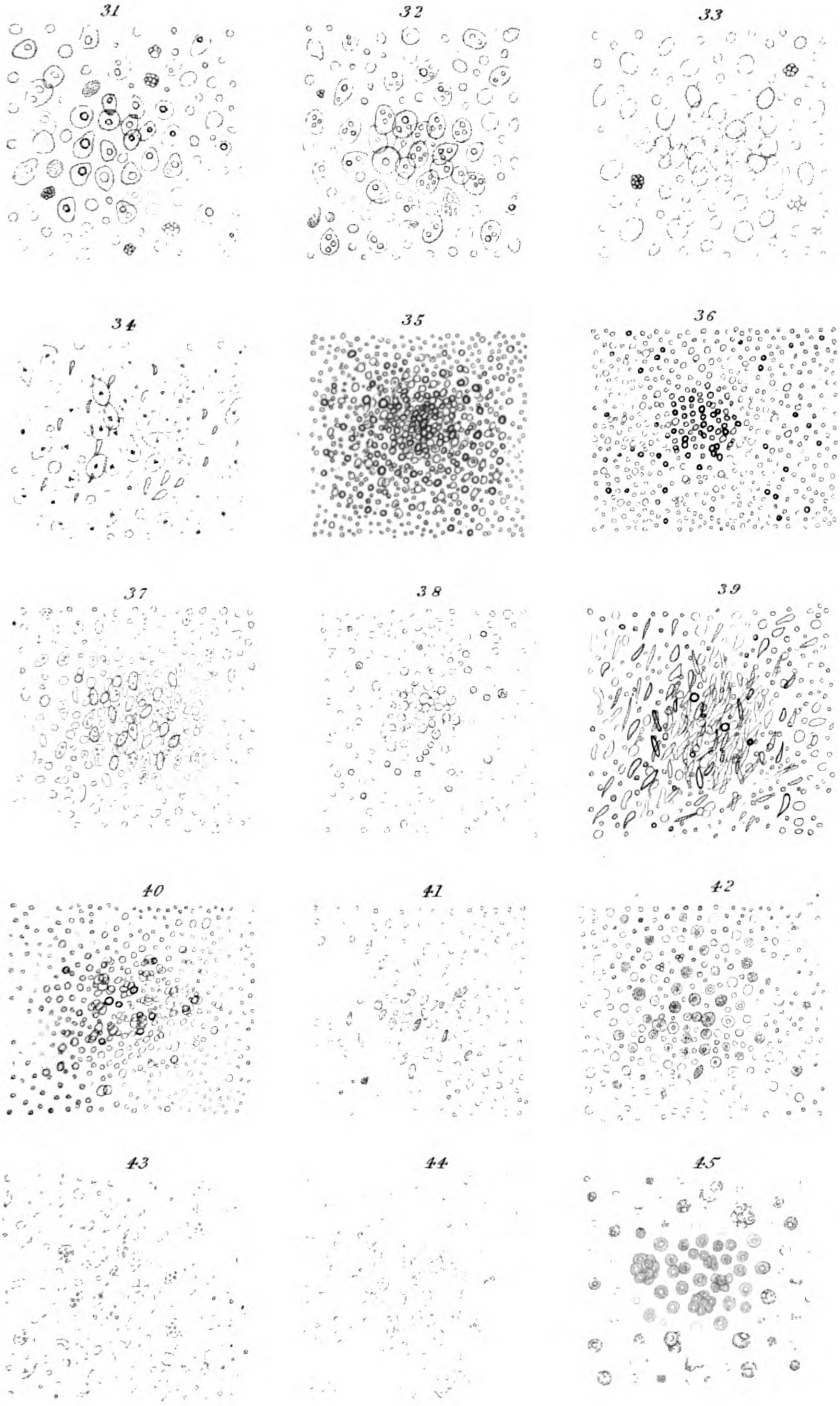
this case the chyme has not traversed a partition of *living cell-structure*, as in the instance of the contents of the thoracic duct of mammals. It has undergone admixture only with the biliary secretion poured into the digestive cæca by the glandular parietes of the latter. It is the re-agency of this fixed cell-product upon the albuminous fluid resulting from the digestive process, which really and immediately confers upon the latter fluid the *disposition to corpusculate*.

The cells which thus arise in chyme are *identifiable* beyond question, with those which float in the fluid of the peritoneal cavity. In the Sipunculidæ the former are round (fig. 30), the latter flattened and oval (figs. 31 to 34). The former represent the latter grown only to a certain point. In *this* situation they can attain no greater dimensions. They can acquire the matured and perfect size and figure only in the splanchnic cavity. The chylous corpuscles which appear in the nutritive fluid while the latter is yet *within* the digestive system do not pass bodily through the parietes into the peritoneal cavity—they *first fluidify*. The fluids only traverse this partition. This act of passing from one cavity to another through an interposed membrane, is undoubtedly, in this case, one of simple physical exosmosis, aided by compression. The digestive diverticula of the star-fish are lined by only a *single* stratum of liver-cells (consisting of a *closed* hyaline involucrum filled with adipose molecules, which differ in colour in different species), *irritable* areolar filaments, and externally, that is, facing the peritoneal cavity, a layer of ciliated epithelium. There is here no trace whatever of blood-proper vessels—no nerve-threads. The act of passage, then, is really one of physical exosmosis. It is the *tendency to corpusculate*, not the corpuscles themselves, which travels with the fluid from the digestive cæca into the peritoneal cavity. Arrived at the splanchnic cavity, the fluid corpusculates *de novo*. But here the tendency in this direction is intensified; cells more rapidly and numerously arise; they attain to a further limit of growth.

In conclusion, the author is anxious to state one fact, with reference to the corpuscles of the chylaqueous fluid of the echinoderms, more especially of the Sipunculidan orders, which he has *proved* by numerous and most scrupulously exact observations. It is the *cell-wall* of the corpuscle which *first* appears. It arises, unquestionably, in the homogenous fluid—not through the procreative agency of an anterior parental corpuscle, but in the amorphous fluid itself, by a spontaneous act of cellulation. The cell-wall, now a closed vesicle, grows larger and larger by the inbibition of fluid from without. In process of development a minute, highly refractive pinkish molecule* arises at some point (not *necessarily* its mathematical centre) in the interior of the cell. It is the special depository of the pigment. In structure, refractive power, and colour, it is totally unlike the cell-wall. It seems impossible that it could at any time grow into the latter, and thus perpetuate the cell. It never exceeds the dimensions of a mere speck, a molecule, compared with the size of the containing involucrum. If it be only an adipose molecule, and not a physiological nucleus, then these cells *have no nucleus*. Is not the inference uncanonical? not necessarily, for the office of the cell-wall is to *secrete*

* Let the reader carefully examine the *gradations* amongst these cells represented in figs. 31, 32, 33, 34 (plate 3), which are drawn from the real objects.

Plate III



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from the surrounding fluid the contents of the cell. This act of cell-agency results in the production of a plasma more highly vitalized, further modified, than that external to the cell-wall. In the former (*intra*-cellular plasma) a *nucleus* is evolved, in the latter (the *inter*-cellular liquor) a cell-wall only is produced. The former is a higher creative act than the latter. The materials used in the first instance stand above those employed in the last, in the scale of organic principles.

(*To be continued.*)

ART. II.

On Collapse of the Lung and its results, considered in relation to the Diagnosis and Treatment of certain Diseases of the Chest. By W. T. GAIRDNER, M.D., one of the Ordinary Physicians in the Royal Infirmary of Edinburgh.

IN the 'British and Foreign Medico-Chirurgical Review' for April and July, 1853,* as well as in some preceding papers, I have endeavoured to establish, from the point of view of morbid anatomy, certain conclusions, which, if correct, must be admitted to have an important bearing on the diagnosis and treatment of thoracic affections. It is the object of the present article to embody some of those practical ideas, which appear most directly to spring from the pathological doctrines alluded to: and to point out such of the opinions and practices current in the present day as appear either to be founded essentially in error, or to be liable to receive an erroneous bias from imperfect pathological views. To fulfil the first part of this programme would be to write a treatise on thoracic diseases; to do justice to the last would require a complete critical review of the opinions of the present day, as expressed in the works of the most accredited authors. I shall, however, attempt to confine this paper within bounds more suitable to the space assigned to it; and, by seizing the most familiar and clear aspects of the subject, to bring before the reader a few points of great practical importance, free from all unnecessary discussion. The previous elaboration of the preliminary matter will, I trust, prove a sufficient excuse for any appearance of undue dogmatism, or neglect of contemporary statements, in the present article.

The most important conclusions enunciated in the communications referred to above, as the result of recent inquiries into the pathology of the thoracic viscera, may be shortly stated in a few sentences. It is, I think, sufficiently clearly proved, that among the different forms of recent condensation of the pulmonary tissue, by far the most frequent are those depending on simple collapse of the air-cells, sometimes attended by more or less serous effusion, or vascular congestion. These forms of condensation, when not dependent on external compression of the lung, are, as I have endeavoured to prove, intimately related to bronchial obstruction, which may be looked upon as their chief exciting cause; the amount of obstruction in the bronchi necessary to produce collapse of the air-vesicles being

* April, p. 454, and July, p. 209. Articles on Collapse of the Lung, Bronchitis, Emphysema, and Dilatation of the Heart.

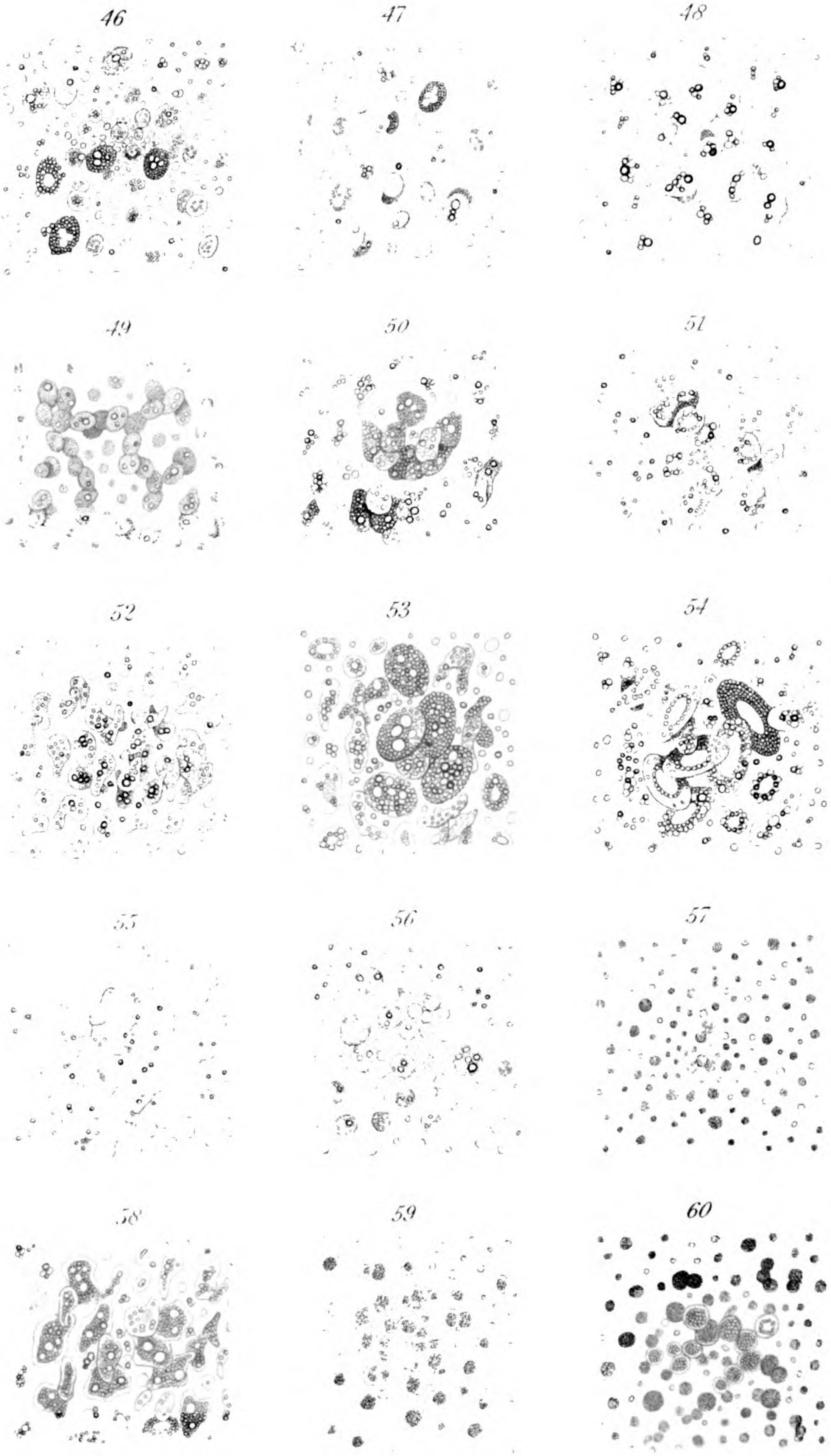
in inverse proportion to the strength and vigour of the patient. The dependence of atrophy of the lung, of emphysema, and of dilatation of the heart, upon these forms of condensation, (the first as a direct consequence, the other two by means of the inspiratory expansion of the chest bearing unduly upon the sound and dilatable parts of its viscera,) has also been fully discussed in the papers referred to. It remains to show wherein these doctrines may be expected to modify to any considerable extent the diagnosis and treatment of chest affections.

The diseases which have been considered, since the application of the modern physical diagnosis to the study of the diseases of the chest, as the chief sources of condensation of the lung, or of dull percussion-sound in the thorax, are pneumonia, pleurisy, or hydrothorax, and tubercular phthisis. Every work on physical diagnosis, every course of clinical and systematic instruction, has usually treated in detail of the modifications of percussion-sound due to each of these causes, and of the collateral auscultatory and other phenomena distinguishing pneumonic condensation from pleuritic effusion, and both of these from tubercular disease. On the other hand, the existence of other causes of condensation, and of dull thoracic percussion, than those above-mentioned, has usually been either overlooked, or has occupied so small a space in the field of elementary instruction, as to disappear almost entirely from the view of the student. In particular, the connexion of bronchitis, and of other diseases producing bronchial obstruction, with these phenomena, can scarcely be said to be acknowledged, otherwise than by a few very cursory, and, as it were, unwilling admissions, in the great majority of standard works. This statement might be easily borne out by extracts from the numerous and excellent treatises on chest disease, which are in the hands of practitioners both in this country and the continent; but the reader will readily pardon the omission of references which are familiar to him, if he finds that in his own experience he has usually regarded dull percussion of the chest as being, in the vast majority of cases, a valid distinction between pure bronchitis and most of the other acute and chronic diseases of the thoracic viscera with which it may be confounded or complicated.

From a consideration of the conclusions mentioned above, it will appear that a correct view of thoracic pathology demands a much wider and more general recognition of the existence of pulmonary condensation from bronchial obstruction, than it has yet received. It must, indeed, be admitted that in one department of practice more just ideas have long prevailed among the best-informed pathologists; and that in infantile diseases the origin of pulmonary condensation in simple collapse of the vesicles in a large proportion of cases, has been taught by numerous observers, to whose elaborate researches and correct appreciation of facts I have endeavoured to do justice in a previous paper. Confining my observations at present chiefly to adult pathology, I shall endeavour to show how far the diagnosis and treatment of pneumonia, pleurisy, phthisis pulmonalis, and bronchitis, seem to be liable to serious error from the imperfect knowledge which has prevailed of the forms of condensation dependent on bronchial obstruction.

1. *Pneumonia.*—Not one of all the acute inflammatory diseases has been the subject of so much and so careful observation as pneumonia. It

Plate IV.



has been in the present century, as pleuritis was among the later Greeks and Arabians, the *princeps morborum acutorum*, the battle-ground of opposing principles, both in pathology and practice. There is scarcely a great physician, from the time of Sydenham to the present day, whose opinions in respect to it have not been repeatedly canvassed in the monographs and systematic treatises of the last few years. By the medical writers of the present century it has received an unexampled amount of discussion. Its symptoms, its physical signs, its causes, its degree of fatality under different circumstances, its varieties of type according to age and epidemic constitution, its relation to all other diseases, its treatment by bloodletting, by tartar-emetic, by stimulants, by ptisans and *extractum graminis;* its behaviour under Brownism, Broussaisism, Rasorism, and every other variety of *ism;* under homœopathy and every other variety of *pathy;* finally, its issue when left systematically to nature with regulated diet and regimen (for none of the *expectant* physicians have ever trusted nature in these latter particulars);—all these subjects have been discussed under every conceivable aspect, and with the aid of every resource that modern learning and ingenuity can supply for the solution of such questions. In particular, it is to be observed that pneumonia, together with typhoid fever, phthisis, pericarditis, rheumatism, and a few other diseases, has occupied for many years past the most prominent position in all our statistical inquiries, and owes this position no doubt to the conviction that it is a pathological condition more easily recognised, more uniform in its character, and therefore less apt to lead to fallacy than most others. It is certain that the public, and even a considerable portion of the medical profession, are often induced to repose with undue security on numerical inquiries, under the impression that the diseases treated of under one name are made up of identical or nearly similar pathological units. This impression, originally propagated in perfect good faith, by men whose object was to avoid fallacy, and who sacrificed every other consideration to the cause of truth, has recently been turned successfully to account by many persons of another description: men self-devoted to the service of quackery, having no other interest in medical science than a personal one, and ready, accordingly, to confound truth and error by the careful selection, the studious suppression, or the indiscriminate adoption of data.

There can be little doubt that the study of morbid anatomy in connexion with physical diagnosis, has very greatly contributed to fix in the public mind the idea of pneumonia as a separate and peculiar pathological unity. Among the ancients, the *peripneumonia* was, as epilepsy, neuralgia, and typhus fever now are, nothing but the name for a series of symptoms or external phenomena, the presence or absence, the severity or mildness of which in individual cases guided the mind of the practitioner in diagnosis, and his hand in action. No doubt speculations existed as to the anatomical seat of this affection. The expectoration, the pain or uneasiness of the chest, the functional disturbance which it created, were all such as to point to the part affected: and the name of the disease, accordingly, bears the impress of a rough anatomical appreciation of its seat. But the great uniformity of the descriptions of this affection, taken in connexion with the considerable diversities of opinion as to its anatomical nature,* show that it was by the study of the evident symptoms alone, and not by supposed anatomical differences, that the ancient authorities concurred in giving this disease a position in the nosology, and particularly in distinguishing it from pleurisy.

Nor did this view of the subject cease with the introduction of morbid anatomy as a means of pathological inquiry. The discovery of *hepatization of the lung* dates from an early period in the history of that science, but notwithstanding multiplied investigations in the seventeenth and eighteenth centuries, Morgagni found it impossible to give an exact anatomical description of the peripneumonia as distinguished from pleurisy; and his successors up to the end of the last century were in no better predicament, as we find that the endless inconsistencies of previous nosological writers on this point, and the unprofitable nature of their classifications, drove Cullen into the admission that no sufficient distinction between pneumonia and pleurisy could be found, at least of a kind available at the bedside. The two diseases, therefore, which were described by the ancients, were placed by Cullen under the single genus, Pneumonia: and were only separated into distinct species in deference to prior authorities (*opinionibus et consuetudine medicorum aliquid concedere volens*).† In this respect Cullen only followed the example of Hoffmann; and though Pinel and his followers, tracing the first lines of an anatomical nosology, placed the two diseases widely apart, most of the great clinical observers of the eighteenth century‡ justified Cullen's views on the subject, by showing that no close relation could be discovered between the symptoms and the anatomical distinctions. I shall hereafter point out the source of much of the confusion that prevailed in the eighteenth century, by showing that the modern idea, both of pneumonia and of pleurisy, is essentially different from the ancient, and leads necessarily to confusion when compared with it. In the meantime, it is sufficient to have shown, as an unquestionable fact, that up to the present century the study of morbid anatomy did not render in any degree more precise the nosological distinctions of pneumonia derived from the Greeks.

The art of percussion, however, as introduced by Avenbrugger and Corvisart, began to find followers about the beginning of the present century. From that epoch the anatomical characters of chest affections assumed a new importance as the elements of diagnostic distinctions. The tendency of the age, moreover, was towards the localization of disease to the uttermost; and the increased attention given to morbid anatomy tended to keep up the nosological definitions which had identified peripneumonia with hepatization or condensation of the lung. The labours of Laennec confirmed these distinctions, but showed that the phenomena revealed by mediate auscultation were capable of affording new diagnostic assistance, and of recording the progress of pulmonic condensation with even greater exactitude than percussion. From the moment that crepi-

* See the interesting chapter in Cœlius Aurelianus, Acut. Morb. II. 28; entitled, 'Quis locus in peripneumonicis patitur.'

† Synopsis Nosolog. Method. sub voce Pneumonia.

‡ Especially De Haen and Stoll, who habitually, and almost constantly, use the compound word pleuro-peripneumonia, introduced by Vincent Baron in the preceding century, and in later times much employed by Andral.

tating râle, dull percussion, bronchial respiration and voice, became the recognised exponents of pneumonic inflammation, the symptoms assumed a secondary position in reference to its clinical history; the anatomical change in the lung was *the disease,* the symptoms were the accidental epiphenomena.

This great revolution in the nosological idea of pneumonia could not fail to produce a marked effect upon the records of medical practice in that disease. The ancient peripneumonia was a disease always acute, always febrile, characterized by great functional disturbance, and great risk of suffocation. So fatal, indeed, was this disease, that even the pain which sometimes accompanied it was altogether subordinate to the danger.* The modern pneumonia is an affection which, as almost all the best observers have assured us, may vary indefinitely in its symptomatic characters, and not less as to its prognosis; nay, which may in some not unfrequent instances be altogether latent, as far as symptoms are concerned. The idea of a latent pneumonia would have appeared to Aretæus a strange contradiction, because in the symptoms was involved the very existence of the disease. To Grisolle a latent pneumonia appears scarcely admissible, because, although the symptoms may be entirely wanting or masked, condensation of the lungs can rarely occur so as to be unappreciable by physical signs.† The pneumonia of Grisolle might be no pneumonia, nor even any disease of the lungs, to Aretæus. I shall afterwards show, in relation to bronchitis, that the pneumonia of Aretæus might be no pneumonia to Grisolle.

It is remarkable that these facts, plain enough even on a superficial consideration of the subject of pneumonia, should have been so often overlooked by those who have compared the ancient with the modern practice in this disease. Laennec himself did not fall into this error. He was fully aware that cases of pneumonia, according to his own diagnosis, and according to that of his predecessors, were not comparable quantities; and he accordingly rarely attempts to compare them, either as to treatment or as to anything else. When, in stating the results of his own practice, he foresees that such a comparison is inevitable, he expressly warns the reader of the difference in the character of the cases, and particularly remarks, "*first,* that auscultation allows us to recognise pneumonia *much earlier* than we can do it by the observation of symptoms; and, *secondly,* that according to all appearances, many cases of simple pleurisy, or of pleuro-pneumonia with predominance of pleurisy, are necessarily comprised under the name of peripneumonia," in the accounts of the practice of his immediate predecessors.‡ This passage is of very great importance in reference to the history of pneumonia. Not only is it most creditable to the candour of Laennec, by whom it is put forward as an explanation of his own apparent success in treatment, but it is, in reality, one of the best statements in any author of the *nature* of the difference, in certain cases, between pneumonia before physical diagnosis, and pneumonia since that period. It may, therefore, fitly form the starting-point of a few more detailed remarks upon this subject.

* Celsus, l. 4, c. 7, "Plus periculi quam doloris habet." See also Aretæus, l. 2, c. 1, De causis acutorum morborum.

† Traité de la Pneumonie. Paris, 1841, p. 434.

‡ Auscultation Médiate. 2nd edit., vol. i. p. 504.

A modern physician, in all respects well-informed and skilful, will form his diagnosis, and determine his treatment, in a case of pneumonia, chiefly from some combination of the following phenomena—viz., fever, local pain, dyspepsia, characteristic expectoration, cough, crepitating râle, altered percussion-sound, bronchial respiration, bronchophony, with perhaps the aid of some other less constant alterations of function, or of the physical signs, in peculiar and doubtful cases. He knows, however, that several of these phenomena may be absent in any particular case, and that one or two of them may be expected to be absent in a large proportion of cases. He will, therefore, not readily be induced to resign any of his means of exploration, and least of all the general or rational symptoms, which he will always regard as giving the chief indications for treatment, even where the diagnosis cannot be securely rested upon them. Such would, I think, be the verdict of most sound and experienced physicians, as to the competing value of rational symptoms and physical signs. It was evidently by these principles that Laennec was uniformly guided, as every page of his great work proves. No one has asserted these principles with more clearness and strength of conviction than Dr. Stokes. "In the cases we are every day called to treat," says he, "the value of physical signs must be tested by the history and symptoms, and these in their turn must be corrected by the physical signs. Whoever neglects either source of information will fall into the most fatal errors."* Similar statements are to be found in all the most trustworthy authorities. At the same time, it is quite evident that the invention of physical diagnosis has a tendency to diminish the *apparent* value of symptoms, not by circumscribing the field of their application in diagnosis, but by extending the field of diagnosis taken as a whole. It is also unquestionably true, that an undue estimate of the relative importance of physical signs has caused, in some minds, a neglect of the diagnostic value of symptoms; which is the more to be regretted, as it is so far removed from the spirit of Laennec himself, and the best of his successors.

It is natural to look to France for the first fruits of the new system; and I have already indicated that Laennec himself was the first to point out that "pneumonia" had, to some extent, changed its nosological character and its relations to medical practice, under the accessions to the means of diagnosis. I am the more anxious to direct attention to this point, because M. Louis has extracted from the words of Laennec above noticed, a meaning which it appears to me they cannot justly be made to bear; and because the commentary of Louis on the text of Laennec places in an instructive point of view the characters of pneumonia, as the disease was regarded by these two celebrated and excellent physicians. In his "Recherches sur les Effets de la Saignée," Louis refers to the passage of Laennec above quoted, and remarks upon it as proving, "that in a certain number of cases Laennec trusted to auscultation *exclusively* to indicate to him the existence of pneumonia; that crepitation, independently of every other local symptom, appeared to him to be sufficient for a secure diagnosis of this affection: so that he must have admitted cases of pneumonia among individuals who offered only crepitation, without rusty and semi-transparent sputa; without any alteration of the respiratory murmur; without any

* Diseases of the Chest, part i. p. 40.

degree of dulness of the chest on percussion, at any point."* Upon the above estimate of Laennec's method of diagnosis, M. Louis attributes to him a want of sufficient caution in admitting cases of pneumonia, and maintains that he must have frequently mistaken the crepitating râle of bronchitis of the smaller tubes, for that of pneumonia. Without entering at present into the question whether this mistake was probable, or whether it would, as M. Louis supposes, account for the comparatively high apparent success of Laennec's practice, I think it may be safely said that nothing in the passage quoted by Louis, nor in any other portion of Laennec's work, goes to bear out the idea that he habitually assumed the existence of clinically important pneumonia, from an observation of auscultatory pneumonia *only*.† His assertion is simply, that auscultation allows of a *more early* recognition of pneumonia; in other words, that a crepitating râle in the lung, added to symptoms as yet not precisely characteristic, will enable an observer to predicate the disease with certainty. This is the only explanation of Laennec's meaning which his words require; it is likewise the only meaning consistent with the rest of his published opinions, which always describe the symptoms, and particularly the fever, as going along with simple uncomplicated pneumonia *from the beginning*.‡

The above observations seem sufficient to show that M. Louis has inadvertently misrepresented the principles of diagnosis advocated by Laennec. Nevertheless, it seems that in relation to the important question of curability, there was an important difference between the "pneumonia" of Laennec and that of M. Louis. Under a similar treatment (by tartar-emetic in large doses), the latter lost three patients out of twenty, who were in good health at the time of the attack, while Laennec treated sixty-two patients with only six deaths, all of which fatal cases were in very unfavourable, and some in desperate circumstances at the time of admission. It is this discrepancy which M. Louis seeks to explain. I think it may probably (so far as it was not accidental) be explained as follows:—The cases of Louis were, in two points of view, *select* cases. They were, or appear to have been, a favourable selection for treatment, in respect that the individuals were all in good health at the time of seizure, and that all the cases springing out of pulmonary catarrh were carefully excluded.§ On the other hand, they were all, *without exception*, cases of a serious character, considered with respect to the pneumonia itself; inasmuch as M. Louis, not content with a few conditions in fixing the character of the disease, seems to have demanded, as absolutely indispensable points in his diagnosis, fever, pain, rusty expectoration, crepitant râle, bronchial respiration, and more or less dull percussion—in other words, the

* Louis, Recherches sur la Saignée, p. 65.

† This remark applies only to the second edition; in the first, Laennec seems, as might be expected, less alive to the risk of error in practice from a purely physical diagnosis. See Auscult. Mediate, 1st edit., vol. i. p. 170, par. 205.

‡ "La pneumonie, *dès son début*, est accompagnée d'une fièvre aigue; il est *très-rare qu'elle manque ou même qu'elle soit peu intense*, et cela n'arrive guère que dans les pneumonies partielles très-peu étendues."—*Auscult. Med.*, vol. i. p. 436, in the 2nd edition, which compare with the passage quoted above.

§ In his first series, composed, after this exclusion, of 78 cases of pure simple pneumonia, the cases excluded were 45 in number. As regards this exclusion, Louis says, "J'ai cru devoir écarter ces faits de mon analyse, pour que tout fût comparable. Aucun autre fait n'en a été écarté; de manière que j'ai réellement fait une énumération complète, ou l'analyse de tous les faits de même espèce que j'ai recueillés."—*Recherches sur la Saignée*, p. 9 (note).

proof of a pneumonia not merely begun, but continued to a dangerous extent.* It seems highly probable that Laennec, on the other hand, not being, like M. Louis, engaged in a strict comparative numerical "analysis of facts of the same species," and not in fact proposing to himself any rigorous analysis at all, gave all the cases which had satisfied *his own mind* of the existence of pneumonia, even at the earliest stage. Among these may doubtless be reckoned some cases of very slight pneumonia, and some, as M. Louis suggests, of "catarrhal pneumonia;" on the other hand, Laennec undoubtedly included some which were not in good health at the time of seizure, as his list of detailed fatal cases shows. Both Laennec and Louis have probably excluded the "peripneumonie des agonisans," and the "pneumonia" occurring as a mere complication of other severe acute diseases, with such symptoms as to be of secondary importance. On the whole, the estimate of the disease by these two great physicians does not appear to have differed in any material point; nor is there, I think, any ground for supposing that either of them allowed his judgment to be swayed considerably by the study of physical signs, to the exclusion of symptoms. M. Louis's exactness of method and conscientiousness of character place his facts beyond all shadow of suspicion in this respect, when viewed by the light of his own words. Laennec, speaking confessedly with less severe exactness, may sometimes have too lightly admitted some pseudo-pneumonic cases, on grounds to which I shall presently refer. But his object was simply to show that, in using the treatment by tartar-emetic for a series of years, he had lost very few cases *absolutely* (not comparatively, as the elements for a comparison did not then exist), in a large field for observation; and his method of showing this was eminently conscientious; he gives the whole of his cases, according to his own unbiassed judgment, and likewise the whole of his deaths, even when these were the result of complications, or of pneumonia itself a complication of other diseases, or of pneumonia treated *in extremis*. Further, he expressly warns the reader that his results are not fairly to be compared with those of his predecessors, and that, if so compared, they give the *stethoscopic* pneumonia an apparent advantage, which is not an advantage in fact.

I have next to inquire, whether the neglect of rational symptoms and exclusive attention to physical signs which M. Louis attributes to Laennec, have, to any considerable extent, interfered with the conceptions of other physicians of eminence, or biassed the judgment of the medical public, with respect to pneumonia. That M. Louis was aware of the danger of a fallacy from this cause, may be presumed from his having allowed the suspicion of error to arise in his mind with respect to Laennec's cases; for, it is in no spirit of detraction, but of genuine scientific caution, that he states his convictions on this subject. Further, M. Louis was one of the earliest observers who described accurately in the adult, and in the course of fever, the peculiar affections of the lung which simulate pneumonia in that disease, without producing any of its characteristic symptoms; and, although he evidently considered these affections a peculiar kind of pneumonia, the sequel of slight pulmonary catarrh in enfeebled subjects, there can be little doubt that he learned from these cases the danger of admitting crepitation, and even dull percussion, to a level with rational symptoms in pointing

* Recherches, &c.; compare pp. 15 and 34.

diagnosis or indicating treatment.* From an examination of his own cases, it appears that his treatment was never modified by the observation of these phenomena; and in describing them he gives the following important warning:—"It is very important to know these results of auscultation, so as to avoid the grave errors into which I have seen a physician fall, who had little practice in auscultation, but was in other respects very skilful. Whenever he met with the râle of which I speak, in the course of a typhoid affection, even of slight intensity, he supposed, notwithstanding the trivial character of the dyspnœa, that he had to do with an intense pulmonary catarrh, and prescribed bleedings, *without success*."† These observations of M. Louis show very clearly the source of his criticisms on Laennec's supposed error, and demonstrate that they were by no means founded on an hypothesis invented to explain a difficulty, but on a fallacy actually observed in practice.

M. Andral was the first author of repute who, after the publication of Laennec's great work, laid so much stress on the physical signs as to maintain the existence of pneumonia, *demanding active treatment*, in the entire absence of its characteristic symptoms. We have seen that Laennec himself did not do so; and this is made still more plain by a consideration of his article on symptomatic and latent pneumonia, and by some of his criticisms on the cases and inferences of Andral, in the second edition of the 'Auscultation Médiate.'‡ These criticisms show a disposition on the part of Laennec to vindicate to the utmost the application of the stethoscope to the diagnosis of pneumonia; but at the same time demonstrate his perfect freedom from that love of paradoxical and startling assertion, which has unfortunately, in the writings of many advocates of physical diagnosis, formed the apology of the sceptic, and the bane of the art. He maintains strongly, perhaps too strongly, that pneumonia of clinical importance is always recognisable by physical signs, but guards with great care against the conclusion that pneumonia recognisable by physical signs is always of clinical importance. When speaking of one form of what he believed to be latent pneumonia, or pneumonia without symptoms (that which occurs in the dying), he speaks of the diagnosis as having only been made "pour exercer les élèves."§ I may here notice that M. Chomel's doctrine is more *conservative* (so to speak) than that of Laennec, or indeed that of any other modern French author, in regard to the necessity of symptoms, as supplementary to physical signs in the diagnosis of pneumonia. He says that the crepitating râle is sometimes wanting where evident pneumonia exists, and sometimes present over a certain extent of the chest in cases where the absence of symptoms compelled

* Louis, Fièvre Typhoide, Paris, 1841. Compare the chapter, 'Des Poumons,' in the first volume, with that of "De la Respiration," in the second; refer also to the cases quoted in the latter. The first edition of this work of Louis was published in 1829.

† Op. cit., 1st edit., vol. ii. p. 283-4. The above remarks refer more particularly to the dry sonorous râle, which was usually present out of proportion to the symptoms. But the coexistence of crepitation with this râle is mentioned immediately afterwards as occurring in a good many cases (*un assez grand nombre*), without symptoms: and in p. 286, the same thing is stated with respect to six of the cases that recovered, in one of which, a young girl, who was very much enfeebled, the crepitation was extended, and lasted for a month, along with dull percussion at the corresponding point. But in this case, as in the others, the expectoration had no characteristic appearance."

‡ Op. cit., sect. ii. ch. 6, art. 2 and 5, vol. i. pp. 425, 478, *et seq.*

§ Op. cit., vol. i. p. 430.

him to renounce the idea of pneumonia. "I will add," says he, "that viscid and sanguinolent sputa, without crepitating râle, are, according to my experience, a much more certain sign of inflammation of the lungs than crepitation unaccompanied by this kind of sputa;"—a statement with which I can most heartily concur, as well as with a practical caution which he gives elsewhere, regarding examination of the back of the chest in the pneumonia of debility, "where the least movement may bring on mortal faintness," and where "an honest physician will rather remain in an uncertainty which has no bad result, than compromise the last breath of life by a curiosity which, to say the least, is not well-timed."*

We have seen that before stethoscopic examination became general, the name of pneumonia was applied almost exclusively to a class of cases having well-marked symptoms, and fatal chiefly in proportion to the intensity of the external phenomena. We have likewise seen that the use of physical diagnosis, and particularly the importance attached to the crepitating râle by Laennec himself, brought into view for the first time a new series of cases, in which the symptoms were no longer diagnostic phenomena, and in the discovery of which the stethoscope alone was to be trusted. This form of pneumonia was called by Laennec latent and symptomatic.† "It is mostly," he says, "when pneumonia is complicated with another disease that it may easily escape the attention of the physician." Frequently such affections were "rather agonies than diseases;" *peripneumonie des agonisans*, as Laennec termed them. Others were the consequence of catarrh; a consequence rare indeed in proportion to the frequency of this latter disorder, but not unfrequent in the variety long known as *suffocative catarrh.* Phthisis, eruptive and continued fevers, intermittent fevers, were likewise looked upon by Laennec as frequent sources of pneumonia, which was truly secondary and symptomatic, being masked in most cases by the primary disease. Such affections were occasionally, in young and robust subjects, accompanied by a notable increase of fever; but in old and exhausted persons, in whom they commonly occurred, these "latent" pulmonary inflammations were, on the contrary, coincident with sudden prostration, loss of consciousness, typhoid symptoms, and the commencement of the agony. In the intermittent fevers, owing to the slight mortality of these affections, their pathological anatomy was little known to Laennec.‡

In discussing the subject of collapse of the lung in the papers formerly referred to, I have adduced a large amount of evidence, showing, in detail, that the name of pneumonia, under which all these affections have been confounded up to a comparatively recent period, is singularly inapplicable, even in morbid anatomy, to a large number of them. The condensation of the pulmonary texture, and obstruction or temporary obliteration of the air-cells, which is common to them all as a physical phenomenon, has now been satisfactorily proved to be,§ in most of these cases, a mere me-

* Dictionnaire de Médecine. Art. Pneumonie, vol. xvii. pp. 231, 232. Paris, 1827.

† Laennec, op. cit., vol. i. p. 478, *et seq.*

‡ Op. cit., vol. i. p. 481, *et seq.*

§ See the paper referred to in the beginning of this article, in this Review for April; also my separate monograph, 'On the pathological Anatomy of Bronchitis, &c.'; Edin. 1850; and an excellent sketch of one large department of the subject in Dr. West's 'Lectures on the Diseases of Infancy and Childhood,' lect. xiii. and xiv.; from which sources the entire history of this complicated subject, so far as known to me, may be gathered.

chanical change, sometimes connected with a certain degree of catarrh or bronchitis, but even more commonly the result of deficient evacuation of the normal or habitual bronchial secretion in enfeebled individuals. This altered view of the pathology of these affections clearly explains the latency of the so-called pneumonia; its connexion with typhoid, adynamic, and catarrhal conditions, and its familiar presence on the death-bed; while at the same time it shows what a fearful amount of havoc must have followed the application of antiphlogistic methods to a disease emphatically, in most cases, one of debility, when physical diagnosis too exclusively pursued, and erroneous pathology unsuspectingly followed, combined to mislead the physician. It is not an agreeable task to dwell on the serious errors of honest and able practitioners; but having taken a certain degree of trouble to point out rocks and shoals which have often caused me much anxiety, I think I should scarcely be justified in leaving them without a few beacon-lights for the benefit of future navigators. I shall, therefore, give a few examples of some misconceptions, which appear to me to have been most destructive in their tendency, and which, from the great and deserved reputation of the writings in which they occur, have been most widely influential in diffusing errors of theory and practice. It is satisfactory to reflect that they must have been often neutralized by the practical tact and sagacity of individual practitioners, and by the cautious and considerate spirit which commonly prevails in the practice of medicine at the present day.

I have already indicated M. Andral as having fallen under the criticism of Laennec, on account of some of his statements on the subject of pneumonia. Indeed, it is not improbable that Laennec himself had been led into similar errors, but had spontaneously rectified them, with that instinctive self-control which is conspicuous throughout his writings, and which constitutes one of the chief elements of his greatness. A comparison of the first and second editions of his work, as already indicated, would show that a certain number of statements, scarcely containing positive errors of observation, but liable to incorrect interpretation, were severely repressed in the latter. The observations, however, of M. Andral, show that while on the one side he perhaps fell into the error attributed to him by Laennec, of detracting from the value of physical signs as a means of diagnosis in certain forms of pleuro-pneumonia, he was far more chargeable with the opposite error of relying upon them, in cases where they appeared to present distinct indications, to an extent which Laennec would not have done. This will appear from the perusal of the chapter on "pleuro-pneumonia, with absence of one or several of its characteristic symptoms."* The forty-third observation is one of so called catarrhal pneumonia. From the absence of characteristic expectoration, the perfect integrity of the pleura, and the state of the bronchi as found in dissection, there is the strongest reason to refer this case to the category of simple bronchitis, with pulmonary lobular and diffused collapse. It was, however, distinctly an inflammatory affection, and was, therefore, treated correctly in principle. It is to be observed that the blood drawn was buffed, and the patient died asphyxiated, thereby attesting the real violence of the disease. In the forty-fifth case, which was not fatal, there was catarrhal

* Clinique Médicale, 3rd edit., vol. iii. p. 366, *et seq.*

expectoration, cough of three weeks' standing, slight dyspnœa and fever (no pain mentioned), the ordinary symptoms of the mildest bronchitis. The feeble habit of this patient, and the slight character of the symptoms, appeared to the physician to contra-indicate active treatment, while *a crepitating râle in the left side, with suppression of the respiratory murmur, and slightly impaired percussion,* seemed, on the contrary, to demand it. Observe the compromise of opposing principles! *Fifteen leeches* were applied to the *left side;* and next day it was covered by a *large blister,* which was caused to *suppurate.* A little broth was the only nourishment allowed."* There can be little doubt that this was a case of the simplest and slightest form of bronchitic collapse in an enfeebled patient, and requiring expectorants, or, at most, an emetic or two, for its cure. But M. Andral claims great credit for the stethoscope, which revealed in this instance a pneumonia that would otherwise have been certainly overlooked (*infailliblement méconnue*). "Such a mistake would have no serious consequence," he says, "if it did not frequently cause the employment of *proper therapeutic means* to be neglected." It will scarcely be contended by any one that the additional refinements in physical diagnosis, or the greater diffusion of its principles since the time of this case, have precluded the possibility or even probability of a similar employment of therapeutic means in the present day. But in the sixty-first observation, we see the principle of an exclusively physical diagnosis carried to its height. In a case of chronic diarrhœa, with debility and emaciation, treated by astringents and tonics with good effect for a month, the patient was carried off by a renewed febrile attack, followed, in a few hours, by a cadaveric expression of countenance. The complete absence of local symptoms left the cause of the relapse obscure. There was no cough or dyspnœa; but slightly impaired percussion and crepitant râle on the right side again subjected the patient to "proper therapeutic means"—viz., twenty leeches on the right side of the thorax, two blisters on the thighs, and, two days after, when the patient, having become progressively worse, was moribund, a blister to the right side. From the short account of the dissection, it seems impossible to say whether there was any inflammatory element in this case at all; but in the opinion of M. Andral, there is "no doubt that the pneumonia was the cause of the relapse and of the death."† It is scarcely possible to share in M. Andral's security of feeling in this point, at least to such an extent as to approve of the treatment.

The great and brilliant reputation of M. Piorry, as a follower in the footsteps of Laennec, invites an examination of his opinions on this subject, more especially as the profession owes to him a most acute and searching investigation into the nature of the *hypostatic* condensations of the lung, which include, for the most part, the cases of Laennec's "peripneumonie des agonisans." In his first researches into this affection, especially as it occurred in the Salpêtrière, M. Piorry was struck with its extreme frequency, both as a cause of death, and as a curable affection, accompanying almost all the diseases of old men, and disappearing during the convalescence. These researches satisfied him that this condensation or "pneumonia" was a peculiar disease, not developed (as had been supposed by some) after death, but distinctly to be traced, as indeed

* Loc. cit., p. 405.

† Loc. cit., p. 457.

Laennec had shown, by auscultation and percussion practised on the living patient. Since the introduction of his novel nomenclature of disease, M. Piorry has resigned the name of pneumonia as applied to this condition, and now regards it as a congestion (engouement) of the depending portions of the lung, which has a great tendency, in some cases, to pass into inflammation, but is not necessarily, or *per se*, inflammatory.* His remarks on the affection are, in many respects, striking and true; the latent character of the symptoms is admitted, as in Laennec's observations; the influence of catarrh on its production is also distinctly stated. The stethoscope is said to be an unfaithful guide as respects râles, but the absence or at least diminution of the respiration is said to be a valuable sign; and percussion, by indicating diminished clearness of tone, completes the diagnosis. But in the treatment, the phantom of congestion is scarcely less exacting a fiend than that of inflammation, and we hear, with horror, that although there is no indication for local bleeding, there being *no local irritation*, "the blood which congests the organ, and by its own proper weight accumulates in its lower portions, *must be removed*;" and accordingly "*general bleeding may be more or less frequently repeated, according to the effects which it produces.* . . . Moreover, when (by examination of the pulse, heart, &c.) we are assured that the circulation is languishing, this fact is *no reason for omitting to remove blood;* for the more considerable the column of liquid to be moved, the more motive power is required."† The organs of circulation are therefore to be stimulated by tonics and wine, while at the same time blood is detracted for the relief of this supposed congestion. The results of this truly heroic doctrine cannot be better exhibited than by an extract from the memoir of MM. Hourmann and Dechambre, upon the pneumonia of old men, which they observed, in common with M. Piorry, at the Salpêtrière.

"Venesection is indicated at the commencement of latent pneumonia, when a passive congestion has been the starting-point. The evacuation of blood in these cases has a treble effect: to remove a part of its material from this congestion, to combat the consecutive inflammation, and to diminish directly the asphyxial condition. *Unfortunately*, these indications, clear and rational as they are, cannot always be carried out;" the state of debility being the obstacle, on which ground the authors recommend the plan of stimulation and bleeding, mentioned above as that of M. Piorry, together with large sinapisms freely applied. The result must be given in the words of the authors:—"We have seen patients in whom the pulse was such as to require bleeding (le pouls invitait à la saignée) cease to expectorate *immediately after it had been practised, and die in less than twelve or fifteen hours.*"‡ A more graphic picture of a destructive practice was probably never given to the world; for the reader will observe that the retention of the expectoration is the direct *consequence* of debility, and the equally direct *pathological cause* of the "latent pneumonia," or collapse of the lung, which is here in question.§

* See the chapter on *Pneumonémie hypostatique;* Piorry, Pathologie Idtrique, vol. iii. p. 401.

† Loc. cit., pp. 422, 423.

‡ Archives Générales de Médecine, 2e série, tom. xii. p. 190.

§ To what extent this erroneous practice may have been followed out at the Salpêtrière, it is only possible to guess from the following data. MM. Hourmann and Dechambre repeatedly

The statistical returns of the Salpêtrière furnish, as has been shown in an able paper by M. Valleix, abundant proof of the frequency of this so-called "pneumonia" in the aged; and a comparison of the changes in these returns, over a series of years, taken in connexion with the pathological views known to be prevalent during the same periods, shows remarkably the revolutions of medical opinion in respect to pneumonia. Thus in 1819, when Laennec's work was first published, the adynamic and ataxic fevers of Pinel still held their ground, to some extent, in the nosology; gastro-enteritis had only begun to take its place among the fatal diseases of the aged, and pulmonary catarrh caused a considerable number of deaths; pneumonia, on the contrary, presented few deaths, averaging in this and the two succeeding years seventy-two per annum. With the rise of Broussais on the one hand, and the collateral increase in the knowledge of physical diagnosis on the other (from 1821 to 1829), pneumonia, encephalitis, and gastro-enteritis monopolized the field of nosology, and the ancient reign of idiopathic fevers was, to a great extent, overthrown. A still later period superseded gastro-enteritis by typhoid fever, and entirely supplanted the adynamic fevers of Pinel by pneumonia, which, owing to the assiduous study of crepitating râles and dull percussion, became emphatically *the* acute disease of the aged, and took to a considerable extent also the place of pulmonary catarrh. In point of fact, we find that pneumonia presented, from 1829 to 1839, more than three times the annual average of deaths in 1819-21; while the fevers so studiously classified and described by Pinel, in this very hospital of the Salpêtrière, as the overwhelmingly frequent disease of the aged, disappeared altogether from the returns.* We have little doubt that if the doctrine of collapse of the lung shall succeed in occupying the attention of our Parisian brethren, we shall see pulmonary catarrh restored to something more like its ancient position as a fatal disease among the aged, while pneumonia will be destined to another revolution; though whether the idiopathic fevers, so signally routed by the combined forces of Laennec and Broussais, will be permitted to collect their scattered forces under any new name, it would, perhaps, be useless to conjecture.

As to the diseases of children, I have already remarked that the differences of type in the "pneumonia" of early life have been the subject of much more careful and successful investigation than in the case of the similar affections of adults. Nothing can be more precise than the anatomical descriptions of the atelectasis, lobular pneumonia, carnification, état fœtal, &c., as it is described under different names by different authors.

state that pneumonia was by far the most frequent fatal disease of the inmates of that hospital, and that its frequency among those affected primarily with other diseases, and recovering, was also very great; but that the immense majority (370 to 53) among those who recovered, were either latent affections, or at least presented no symptom of acute pneumonia, but only of "congestion." The manner in which these discoveries were made is also remarkable. The authors remark, "It is clear that physical signs only can prevent error, and we shall therefore announce a proposition, which will certainly not be contradicted by any physician who has practised for some time at the Salpêtrière. This is, that to prevent all danger of mistake, it is necessary to examine by percussion and auscultation the chest *of each patient at every visit*, whatever the affection be which has obliged him to enter the infirmary. There is scarcely a day in winter when there will not be found pulmonary congestions, and even true hepatizations" (condensations), "which could not have been otherwise suspected."—Loc. cit., p. 176.

* L'Union Médicale, vol. ii., Nos. 101-4-7; and Brit. and For. Med.-Chir. Review, April, 1849, p. 547.

The symptomatology of these affections was also studied with great care, even before their nature was rightly understood; and there has been much less misapprehension, in a practical point of view, of the latent and asthenic forms of pulmonary disease. M. Trousseau, one of the latest expositors of French pathology on this subject, has remarked that the true acute pneumonia, which scarcely occurs at the earliest ages, is beyond the second year so manageable a disease that he treated during six months twenty cases without a single death; whereas, the far more frequent catarrhal pneumonia was in his hands so fatal, that among thirty cases in hospital, not one survived.* I cannot help suspecting that M. Trousseau has here made a somewhat arbitrary division of his cases; and the pathological hypothesis by which he illustrates the two diseases, comparing the one to phlegmon, and the other to erysipelas, is by no means calculated to communicate clear ideas.†

In concluding this first and most complicated department of my subject, I shall endeavour to collect into a few words some general principles as to the pneumonic and *pseudo-pneumonic* condensations of the lung. It will easily be understood that these affections have been frequently confounded, although in well-marked cases, and in the dead subject, they are sufficiently well marked in their distinctions. In the *first* place, the connexion of the pseudo-pneumonia, or collapse of the lung, with catarrh, is such as, in some instances, to communicate to it acute symptoms resembling those of pneumonia proper; *secondly*, where it is not accompanied by special acute symptoms, it often occurs as a secondary phenomenon, in the course of acute diseases; *thirdly*, even where neither of these are present, the physical signs are such as to deceive the incautious observer, and to suggest the idea of a *latent pneumonia;* an idea which will cling to the mind of the practitioner, and will be suggestive of incorrect and deadly practice, in proportion as he has been accustomed to rely upon the refinements of physical diagnosis to the exclusion of symptomatic phenomena. In fact, not only have these affections been confounded during life by the most able and competent observers, but anatomy itself has been misled, and the scientific nomenclature of this science has been overrun with an immense number of useless and obscure terms, corresponding to the ideas of those who were vainly endeavouring to reconcile the simplicity of nature with the perplexing and complex principles floating in the contemporary literature of medicine, and involved in the general doctrine of some of its ablest teachers.

As regards the *pathological nature* of the pseudo-pneumonic condensations, it may be said that they all depend upon *collapse of the pulmonary air-cells,* in connexion with some degree of obstruction of the bronchi. This obstruction may be owing to an increase of the secretion from the mucous membrane; or, as is frequently the case in old and enfeebled persons, to an impaired excretion of the ordinary mucus, which accumulates

* Med. Gaz., vol. xlviii. p. 1089.

† It is remarkable enough, that in modern times pneumonia should have been regarded by many as *peculiarly* a disease of old age and infancy. Aretæus speaks of it as "for the most part fatal to youth and persons in the flower of life."—*De Morb. Acut Causis,* lib. ii. 1. The examination of the disease in detail, as understood by the ancients and moderns, explains this discrepancy.

chiefly in the branches near the root and central parts of the lung. In the latter case the resulting disease is totally devoid of an inflammatory or febrile character, and is found to be developed with an intensity proportionate to the debility of the patient. When, on the contrary, the bronchial secretion is increased and altered in character, a certain degree of febrile excitement may attend the disease, the amount of which is usually proportionate to the inflammatory character of the affection, and the amount of active treatment required. In some of the acute forms of pulmonary collapse, acute œdema, or even true pneumonic infiltration of the pulmonary tissue, may supervene, giving rise to a complicated type of disease, which is of course attended by many of the symptoms of pneumonia. Such cases are, according to my experience, very common in connexion with purulent infection after operations, and in the secondary inflammation after erysipelas or eruptive fevers.

The *symptoms* of the pseudo-pneumonic condensations, as distinguished from those of true pneumonia, are often sufficiently instructive, and always of the greatest importance in guiding treatment. In many instances, it is true, the information derived from them is chiefly negative; nor is collapse of the lung, like pneumonia, to be regarded as a separate nosological condition, having a series of tolerably constant physiological phenomena. Its external manifestations vary, both in character and in amount, with the conditions that give rise to it, and are, as we have seen, often absent; the pulmonary collapse being, in these cases, only the secondary sign of a general constitutional condition. In other cases it has the symptoms of acute bronchitis carried to the highest degree of intensity; and in these instances it is to be observed that the *dyspnœa* is a far more striking symptom than in the true pneumonia, especially when considered in relation to the amount of condensation discoverable by percussion and auscultation. The reason of this disproportionate dyspnœa I shall state in the sequel; the fact is well known to all experienced physicians, as an important one in the diagnosis and prognosis of pneumonia and bronchitis.

Two other facts in connexion with the symptoms deserve notice. *Pain* is rarely, to any considerable extent, a symptom of the pseudo-pneumonic condensations—if we except the dull oppressive pain in coughing, which is so well-known as the character of bronchitis. On the other hand, I believe that if we separate from the pneumonic cases those which have been erroneously so described, very few instances will remain of *acute* pneumonia unattended by tolerably acute and distinctly localized pain at the commencement. This fact, no doubt, depends on the frequency with which the pleura is involved in the disease of the lung. It is, however, to be remembered, that acute pleurisy, with all its concomitants, sometimes occurs in bronchitis entirely free from pneumonic complication. Of this I have seen numerous instances, so that the rule as to pain must not be taken too absolutely.

The most important symptomatic peculiarity of pneumonia, as contradistinguished from the pseudo-pneumonic condensations, is undoubtedly the *expectoration*. That of pneumonia, in its ordinary acute form, is so well known that I do not feel myself called on here to describe it. That of the pseudo-pneumonic condensations is essentially *catarrhal*, if indeed it be present at all. Often, as before mentioned, expectoration is absent; and I think I have even observed that expectoration which has existed habitually for some time before the attack may be suppressed, or materially diminished in quantity, on the supervention of the symptoms. It is certain that expectoration is frequently suppressed in the course of this affection; and MM. Hourmann and Dechambre have observed as a "singular fact" in "pneumonia" of the aged, that the cough and dyspnœa with which their patients had been habitually affected, occasionally disappeared during the prevalence of the signs indicating this disease.* The disappearance of cough and dyspnœa, as well as expectoration, only occurs, according to my observation, in the extremely asthenic forms of the affection, or in its latest stages.

The *presence of blood*, in one form or other, *in the expectoration of pneumonia*, has long and justly been regarded as a very characteristic symptom. I believe it to be so, for by far the greater number of cases of pneumonia in which the characteristic expectoration has not been observed, undoubtedly fall under the suspicion of belonging to the category of pseudo-pneumonia. But although the most characteristic types of pneumonic expectoration, tenacious, semi-transparent, and rusty, are little liable to mislead, the mere presence of blood in the sputum of catarrh does not necessarily indicate that hepatization of the lung is present. I shall return to this subject in the sequel.

The physical signs of the pneumonic and pseudo-pneumonic condensations bear in many respects so close a resemblance as to afford no sure and constant ground for a distinction. Crepitating râle is less frequently, or rather less constantly, present in the pseudo-pneumonic affections than in the commencement of true pneumonia; it is, moreover, in the former, frequently merged in the coarser and more audible catarrhal râles. Dulness on percussion is common to both affections in like measure; except that in the sthenic forms of pulmonary collapse it is always slight in proportion to the amount of the disease. In this case emphysema occupies the entire surface of the lung, at least at its anterior and lateral aspects, and leaves only the centre and root occupied by condensation. When condensation is discovered at the root of the lung, therefore, the symptoms being those of a febrile disease of respiration, the presence or absence of signs of emphysema is a valuable criterion, particularly if the disease has attacked a person formerly in good health. The value of this diagnostic peculiarity of the pseudo-pneumonic condensations is the greater, because, as I have shown elsewhere,† and as I am well assured by the most repeated and careful investigation, pneumonia, apart from other affections of the lung, has no tendency whatever to determine emphysema. The phenomena of the voice and of the respiration are not characteristic, as applied to the diagnosis of the pseudo-pneumonic condensations; I shall return to them, however, in treating of pleurisy.

On considering the whole of this subject, it will be seen that the diagnosis of the affections of which I have been treating is sometimes easy, on other occasions of no ordinary difficulty; and that it depends less on refinements of physical diagnosis than on an accurate knowledge of the

* Archives Générales, 2e série, vol. xii. p. 36.

† Pathological Anatomy of Bronchitis, p. 55.

whole phenomena of these diseases, and particularly of the varieties under differences of age, constitution, and other modifying circumstances. The indications of prognosis and treatment are to be drawn chiefly from the symptoms, the mode of access, and the progress of the affection; in general it may be said that the latent forms are never properly made the subject of local treatment, and that the general means employed should be always addressed to the symptomatic phenomena rather than to the physical condition of the lung as revealed by auscultation and percussion. It is also important to observe that the course of these affections in relation to treatment is singularly contrasted: in true pneumonia, the extension of the physical signs is the index of extension of the disease, and increased demand for active treatment; in the pseudo-pneumonic condensations, extended dull percussion is the index of deficient inspiratory power and general debility of the system; requiring tonics, stimulants, nourishment, or otherwise baffling art altogether. But on this subject also I have more to say hereafter.

(*To be continued.*)

Art. III.

Scarlatinal Dropsy. By John W. Tripe, M.D.

Few subjects in the whole range of medical literature have undergone a more complete and radical revision than that of renal dropsy; nor can we select any other disease to show more forcibly that the art of medicine, as well as science generally, has of late years made great and constant progress, and has been marked by great corresponding change. But whilst every advance in the sciences, and especially in the exact, forms a substantial basis on which to erect the future building, it is often the reverse in our comparatively uncertain art, in which crude theories have often been propounded for, and accepted as, well-established facts. But this is now less likely to happen, as we invoke, in most investigations, the aid of the microscope, chemistry, physiology, pathology, and of a more rigorous system of statistical inquiry. The great and certain assistance afforded by these is shown in the history of renal dropsy. Thus, until Dr. Bright brought the aid of chemistry to the examination of the urine, the connexion existing between certain forms of dropsy with albuminous urine and renal disease was not known; and until later observers examined the kidneys with the microscope, the continued presence of albumen in the urine was supposed to arise from one disease only, which was named from the discoverer, Bright's disease. And even now, the number, forms, peculiarities, and symptoms of renal diseases are, and will probably remain, unsettled for years. To prevent misconception, it may be as well to state, that this paper was written chiefly with the view of bringing before the profession the results obtained by a very extended statistical inquiry regarding the influence exercised by various circumstances on the mortality of scarlatinal dropsy. It will also embody, within a short compass, an account of the pathology, varieties, symptoms, complications, and treatment of the disease.

The present article will be devoted to a consideration of scarlatinal dropsy in its most extended sense, including all forms of albuminous effusion, whether it take place into one or more of the serous cavities alone, or involve the cellular tissue also: I, therefore, employ the term "dropsy" to signify the effusion of a non-inflammatory albuminous or fibrino-albuminous fluid into the areolar tissue, the serous cavities, or into both.

When the effusion primarily involves the areolar tissue, it usually first affects the eyelids and face; sometimes the back of the hands, or dorsum of the feet and ankles; sometimes the scrotum and penis; but wherever it may commence, it ordinarily extends, after a longer or shorter period, to the whole surface of the body, and often to one or more of the serous cavities. When the serous cavities are primarily attacked, that of the abdomen is most frequently selected; that of the meninges of the brain next; then of the pleuræ; and lastly of the pericardium. Frerichs* states, that in Bright's disease anasarca is the most frequent form of dropsy; next ascites; and then hydrothorax of both sides of the chest; that of the left being usually largest. He also states that the œdema of the lungs is a common complication, and is frequently accompanied by hydrothorax. Effusion into the pericardium was observed in 14 out of the 169 cases observed by Bright and Malmsten. Out of 680 fatal cases of scarlatinal dropsy which occurred in London during the year 1848, 592 were returned as anasarca; 38 as hydrothorax; and 50 as hydrocephalus. The proportion of males and females was as follows, during the last half of the year:—

TABLE I.†

	Males.		Females.		Total.
Anasarca	290	...	188	...	478
Hydrocephalus	26	...	24	...	50
Hydrothorax	18	...	20	...	38

Of the cases classed together as hydrothorax, two were of hydropericardium only.

The dropsical fluid varies very considerably, being much richer in albumen in that of the pleural cavities than of any other. The specific gravity varies from 1·005 to 1·014; the saline ingredients from 5·2 to 10·8; and

* Die Bright'sche Nierenkrankheit und deren Behandlung; and see British and Foreign Medico-Chirurgical Review, vol. ix.

† The materials for this table, and for nearly all the others, were obtained by a personal examination of the returns of "Causes of death" supplied to the registrar-general by the district-registrars, and of other unpublished documents at Somerset House. The deaths within the bills of mortality for London during the year 1848 were 57,628 from all causes, and 4756 from scarlatina. Each return comprises for this and every subsequent year a statement of the age, sex, occupation and residence, of each person, and also an enumeration of the disease, its secondary affection, and the duration of each. The age, sex, and residence must be correct; and in the case of scarlatina the duration also, as the rash is too remarkable a symptom to pass unnoticed. The duration and period of onset of some of the complications are open to a much greater chance of error, as they might escape observation for some days, but this objection cannot apply to anasarca, as it is too patent to pass unobserved. I have examined upwards of 80,000 of these returns, and have compiled all the tables, some of which, as those including the five years 1848—52, are formed from the returns of 284,306 deaths from all causes. Of these 284,306 deaths, 1600 were caused by scarlatinal dropsy (see Table 8), or in the proportion of 5·6 deaths from it to 1000 from all causes. I have much pleasure in publicly rendering my thanks to the registrar-general, and to Dr. Farr, for the readiness with which they afforded me every facility for making these investigations. I have subjoined the form (No. 1) of the medical

the albumen from 3·60 to 38·0 in 1000 parts respectively. The following table shows this more satisfactorily.

TABLE II.—*In* 1000 *parts.*

Pleura.			Peritoneum.			Cranial Cavity.			Subcutaneous Cellular Tissue.		
Albumen.	Salts.	Author.	Albumen.	Salts.	Author.	Albumen.	Salts.	Author.	Albumen.	Salts.	Author.
28·50	7·55	Schmidt	23·8	10·8	Marchand	10·03	9·69	Schmidt	Very small	10·1	Heller
26·12	7·64	Ditto	11·32	9·77	Schmidt	7·98	8·46	Ditto	Ditto.	8·84	Ditto
25·64	8·30	Frerichs	3·9	9·03	Ditto				5·4	15·62	Ditto
24·9	7·80	Ditto	8·12	8·08	Heller				7·0	9·1	Simon
			38·0	7·06	Percy				3·60	7·70	Schmidt

It is more than probable that the great variation in the composition of the fluid depends in part on a greater impediment existing to the circulation in one case than in another; or to the presence of inflammation; as either of these will cause an increase in the albumen. The fluid, also, often contains urea; and Frerichs states usually in greater quantity than the blood; he also remarks, that the urea is converted, under certain circumstances, into carbonate of ammonia. But the various experimenters are by no means agreed either as to the quantity of urea when present, or to the frequency with which it occurs. Thus, Heller found it absent in

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certificate, and that (No. 2) of the return from each district-registrar to the registrar-general. The names are fictitious.

No. 1.

Certificate of Cause of Death.

TO THE REGISTRAR OF THE SUB-DISTRICT IN WHICH THE UNDERMENTIONED DEATH TOOK PLACE.

I hereby certify that I attended John Jones, aged 8 years last birthday; that I last saw him on January 11th, 1847; that he died on January 12th, 1847, at 7, King-street, Marylebone; and that the cause of his death was

	Cause of Death.	*Duration of Diseases.*
(*a*) First	Scarlatina .	42 days.
(*b*) Second	Anasarca .	28 days.
	Erysipelas .	2 days.

Signed . . . EDWARD LAWRENCE,

Prof. Title . M.D.

Address . . 15, Soho-square.

No. 2.

When Died.	Name and Surname.	Sex.	Age.	Rank or Profession.	Cause of Death.	Signature, Description, and Residence of Informant.	When Registered.	Signature of Registrar.
Jan. 12	John Jones.	Male	8 years.	Father a carpenter.	Scarlatina, 42 days Anasarca, 28 days Erysipelas, 2 days (Certified.)	Martha Hill, Nurse, 14, King-street, Marylebone.	Jan. 15	W. Howard

three cases, and present in one. As regards quantity, Marchand found 4·2; Frerichs, 1·62, 1·05, and 1·45; Simon, 1·2; and Rees, 0·415, and 0·15—respectively, in 1000 parts of the effused fluid.

The *causes* of the disease may be divided into the *predisposing* and *exciting*.

As it would occupy a very considerable space to discuss the influence of the scarlatinal poison, as a *predisposing cause*, we must pass it over comparatively unnoticed. The poison is considered by some authors as the essential cause of the dropsy, and by others as exercising but little direct influence on its production. The former consider that it acts on the kidney in the same way as on the skin; whilst the latter deny any such action. It is, however, quite certain that dropsy follows an attack of scarlet-fever more frequently than of any other febrile disease.

Certain atmospheric peculiarities; the epidemic or stationary constitution of disease generally, by which certain organs are predisposed to be attacked rather than others, may be enumerated amongst the most powerful predisposing causes. An examination of the history of scarlatina shows, during some epidemics, that a large proportion of the patients suffered from subsequent dropsy, whilst in others but few were attacked: that in one epidemic the dropsy was exceedingly severe, in others very slight; and these facts are clearly shown in Tables 14 and 15, by which it is proved that the per-centage of fatal cases of dropsy varied, in the years 1848-52 from 7·0 to 20·2, in each 100 deaths from scarlatina. The former per-centage is that for the first quarter of 1848, the latter for the last quarter of 1848.

Bad living, imperfect ventilation, damp and low situations, not only act as predisposing causes, but also materially increase the fatality of the disease, if the patient continue to be exposed to their influence.

Having thus briefly enumerated the predisposing causes, we next propose to consider, in a few words, the influence of the following *exciting causes*,—viz., cold, warmth, and stimuli, and of certain other modifying circumstances, on the production of, and the mortality from, scarlatinal dropsy. These modifying circumstances are (*a*), *variations in the atmosphere*, including changes in the temperature, humidity and electricity of the air; (*b*) *sex*, and (*c*) *age*.

Cold.—When cold is applied, after an attack of scarlatina, to the surface of the body, it acts very prejudicially, and is one of the most frequent excitants of the disease—or, at any rate, is more often blamed than any other. That cold and wet will have this effect is satisfactorily shown by albuminous dropsy frequently following exposure to their action without any previous febrile disease.* But that they act as supposed by Dr. Johnson, by checking "the process of desquamation and of elimination from the skin," is more than doubtful; for Schneemann and Mauthner assert dropsy to be most uncommon in those cases in which cuticular desquamation has been absent. It seems more probable that the dropsy is caused by the cold and wet impeding or suspending the proper functions of the skin, and by the kidneys (already congested by the scarlatina) being then called on to eliminate the materials ordinarily excreted by the skin.

* I have had several cases of this kind under treatment during the present year (1858.)

That the skin and kidneys are vicarious in their actions, is proved by the experience of all ages. Thus, certain medicines, as nitre, will act on the skin if the surface be kept warm, but on the kidneys if it be kept cool. We should, therefore, expect that the greater the check to the cutaneous functions, the greater will be the extra duty thrown on the kidneys. It is also probable that the kidneys have greater difficulty in performing this vicarious action, than their own peculiar office; and that the impediment to the circulation in the glands is in proportion to the extent of such demand, and to the difference between the matters so excreted and the ordinary constituents of the urine.

Excess of Warmth.—But whilst the keeping a patient in bed, or at any rate in a room of an equable and moderately warm temperature, is the best prophylactic against an attack of post-scarlatinal dropsy, yet too great solicitude may, by an excess of temperature, entail defeat on our plans, and induce that which the treatment was intended to prevent. I have seen several instances in which confinement to a close, hot, ill-ventilated apartment was apparently one of the exciting causes of an attack.

Want of Ablution.—An absence of due ablution after an attack of scarlet fever often acts very prejudicially, and especially amongst the poor, as the parents usually fear some imaginary ill from washing children who are or who have been suffering from an exanthematous disease.

Stimuli.—Many are strongly opposed to the administration of stimulants during the eruptive stage of scarlatina, from a belief that they predispose to an attack of subsequent dropsy; but although I administer stimuli very freely in all the low forms of the disease—even in many instances whilst the rash is still out—yet in but few cases where due attention had been given to ablution with soap and water, proper ventilation, and freedom from cold, have I met with dropsy subsequently. Indeed, in no case of the malignant forms, in which stimuli were administered very early, have I had such a result. But that stimuli given during convalescence, or at any other period, when not required by the state of the system, may induce an attack of dropsy, I freely admit. It has been stated that alcoholic stimuli increase febrile action, and diminish all the secretions except those of the kidney, and that they, therefore, in scarlatina, divert the poison to the kidneys, and excite disease there. But it is by no means certain that wine or other stimuli invariably diminish the cutaneous functions in scarlatina; indeed, on the other hand, I have seen cases in which the skin has resumed its functions under their use. I allude particularly to those cases in which the skin is without unusual heat, and the pulse weak and small. Indigestible food may also be enumerated amongst the exciting causes.

Variations in the Atmosphere.—In addition to the agents just enumerated, variations in the atmosphere exert very considerable influence, if not on the supervention, yet most certainly on the mortality, of scarlatinal dropsy, as we shall see by an examination of the following tables, which show the mortality in the different seasons of the year. The first table shows, in each quarter of the year, the mortality (*a*) from scarlatina; and (*b*) from the consecutive dropsy; and (*c*) the per-centage of the mortality from dropsy, compared during corresponding periods with that from scarlatina:

TABLE III.

	1st Quarter, ending March 31.			2nd Quarter, ending June 30.		
	Deaths.			Deaths.		
	Scarlatina.	Dropsy.	Per-centage of dropsy to Scarlatina.	Scarlatina.	Dropsy.	Per-centage of Dropsy to Scarlatina.
1848	615	43	7·0	816	83	10·2
1849	776	118	15·5	497	67	13·5
1850	199	32	16·1	234	34	15·3
1851	206	28	13·1	169	20	11·8
1852	366	47	12·9	563	62	11·0
	2162	268	12·0	2279	266	12·3

	3rd Quarter, ending Sept. 30.			4th Quarter, ending Dec. 31.		
	Deaths.			Deaths.		
	Scarlatina.	Dropsy.	Per-centage of Dropsy to Scarlatina.	Scarlatina.	Dropsy.	Per-centage of dropsy to Scarlatina.
1848	1560	171	10·7	1765	317	17·9
1849	386	58	15·0	446	90	20·2
1850	316	25	7·9	429	61	14·2
1851		(?)*	(?)	603	99	16·4
1852	668	68	10·9	952	152	15·9
	2930	322	10·9	4195	719	17·0

An examination of the table shows, in the March quarter, a variation in the ratio of the mortality from scarlatina (which was then at its minimum) and that from dropsy—the absolute mortality of the dropsy being 268, and the comparative 13 per cent.;† and the absolute mortality from scarlatina, 2162. It also shows that in the June quarter the mortality from scarlatina increased, whilst that from the dropsy diminished, both absolutely and relatively, being 266 instead of 268; and having a comparative mortality of 12·3 instead of 13·0 per cent. Also, that in the quarter ending September, although the deaths both from the scarlet fever and dropsy were much increased, yet the comparative mortality was less—the deaths from dropsy being 347,‡ and the comparative mortality only 10·9, per cent.; and that in the quarter ending December, the deaths from scarlatina were nearly double, and those from the dropsy nearly treble those of the first quarter – the comparative mortality from scarlatina being 36·3 to 28·7, and of the dropsy 17·0 to 13·0. And this difference in the mortality is rendered more obvious by ascertaining the per-centage of deaths, in each quarter, from dropsy. In the first quarter it was 16·8; in the second, 16·6; in the third, 21·7; and in the fourth, 44·9 per cent.,

* Some of the data were mislaid at Somerset House for this quarter.

† When the term "comparative mortality" is used in connexion with dropsy in any part of this paper, it is to be understood as meaning the rate or per-centage of deaths from dropsy to that from scarlatina. By 13 per cent., as above, is meant 13 deaths from dropsy out of 100 from scarlatina.

‡ One quarter was calculated—viz., that for 1851—as the data were mislaid at Somerset House.

respectively; the totals being 268 in the first quarter, 266 in the second, 347 in the third, and 719 in the fourth.

We may therefore say, that in proportion to scarlatina the dropsy is less fatal during the quarter ending September 30, and most fatal in that ending December 31; and that, absolutely and comparatively (when compared with itself), it is least fatal in the quarters ending March 31 and June 30, respectively, and most fatal in that ending December 31.

By pursuing this method still further, and examining the mortality in a similar manner for each month, we shall find that, both absolutely and comparatively, the deaths in August are much smaller than for any other month in the year. I regret much the comparatively limited extent of this table, as, not thinking it of much moment, I did not extract the monthly mortality for more than the two years 1848 and 1852; still, as these bear out the above statements, they will probably be deemed sufficient.

The first column of the table for 1848* proves that scarlatina prevailed chiefly during the latter half of the year, the mortality being under the average during the first six months of each year; the second column points out that the deaths from scarlatina dropsy were below the average in the first eight months, and above it in the last four. The fifth column, which shows the number of deaths from dropsy in comparison with those of scarlatina (the average proportion of the one to the other being about 1 to 7·6), is the most valuable for comparison, as it necessitates the inference that some causes must be in operation, either to increase the intensity of the scarlatinal virus; to direct it to the kidney; or to render the attack of dropsy more fatal at one period than another. In this column we see that in proportion to the mortality from scarlatina, the dropsy was below the mean in the months of January, February, April, June, and especially in August; and above it in March, May, July, and in the last four months of the year. And an examination of the corresponding columns for the year 1852† leads to somewhat similar results. The mortality from scarlatina and the dropsy was below the average in the first eight months, with the exception of June—when, as well as in the last four months, it was in excess; but when compared with scarlatina, the mortality from dropsy was in excess in January, June, and the last four months of the year. We here see, that although these two and the other years, as shown in the previous table, present one striking agreement—*viz., that the dropsy is most fatal, both absolutely and relatively, in the last four months of the year*, yet that they differ in this respect in several of the others. The question then arises, Will the meteorological elements of the table explain these peculiarities? A comparison of the first three months of one year with those of the other shows that they differed *in toto* as regards the comparative mortality of the dropsy (fifth column)—that of January, 1848, being 3·8 to 10·2 in January, 1852; of February, 1848, 2·4 to 7·7 in February, 1852; of March, 1848, 9·8 to 8·0 in March, 1852. The temperature for January, 1848, was much below the average, and the electricity unusually active; whilst the temperature for January, 1852, was high, and the manifestations of electricity unusually small. As regards the temperature alone, an examination of the 24 months shows it to have been above the average of the respective years

[*Turn to p.* 232.

* See Table IV. on the opposite page. † See Table V. on the opposite page.

Table IV.—*Mortality 4 weeks in each month. Year* 1848.

Deaths.						Meteorology.			
1848.	Scarlatina.	Dropsy.	Per-cent. of Scarlatina.	Per-cent. of Dropsy.	Per-cent. of Dropsy to Scarlatina.	Temperature of Air.	Rain in Inches.	Mean Humidity of Air.	Electricity.
January.	180	10	4·1	1·7	3·8	36·5°	1·2	·837	44
Feb.	192	7	4·4	1·2	2·4	43·2	2·6	·864	38
March.	186	26	4·2	4·5	9·8	42·5	3·1	·839	43
April.	205	20	4·7	3·5	6·9	49·9	3·4	·794	31
May.	227	31	5·2	5·4	9·5	59·9	0·4	·664	31
June.	348	29	7·9	5·0	5·9	58·7	3·5	·764	31
July.	378	47	8·6	8·1	8·7	61·5	2·1	·762	30
August.	480	28	10·9	4·9	4·0	58·5	4·6	·797	33
Sept.	602	85	13·8	14·7	9·9	55·8	2·4	·795	18
Oct.	653	122	14·9	21·1	13·1	52·1	3·5	·853	10
Nov.	499	90	11·4	15·6	12·6	43·2	1·2	·848	2
Dec.	434	83	9·9	14·3	13·4	42·8	2·5	·873	9
Total.	4386	578	100·0	100·0	100·0		30·5		320
Average.	365·6	48·1	8·3	8·3	8·3	50·2	2·5		26·6

Table V.—*Mortality 4 weeks in each month. Year* 1852.

Deaths.						Meteorology.			
1852.	Scarlatina.	Dropsy.	Per-cent. of Scarlatina.	Per-cent. of Dropsy.	Per-cent. of Dropsy to Scarlatina.	Temperature of Air.	Rain in Inches.	Mean Humidity of Air.	Electricity.
January.	119	18	5·1	5·9	10·2	42·0°	3·6	·847	3
Feb.	113	14	4·8	4·5	7·7	40·8	0·9	·879	21
March.	83	10	3·5	3·2	8·0	41·3	0·2	·810	35
April.	154	16	6·6	5·2	7·0	45·9	0·5	·757	8
May.	164	16	7·0	5·2	6·6	51·5	1·9	·773	24
June.	207	29	8·8	9·4	9·5	56·1	4·6	·767	28
July.	189	19	8·1	6·2	6·9	66·6	2·3	·732	34
August.	185	5	8·0	1·6	1·9	62·1	4·5	·728	34
Sept.	261	37	11·1	12·0	9·6	56·8	3·9	·772	10
Oct.	339	63	14·4	20·5	12·2	47·9	3·8	·803	0
Nov.	301	50	12·8	16·2	11·3	48·9	6·0	·828	0
Dec.	231	31	9·8	10·1	9·1	47·6	2·2	·794	0
Total.	2356	308	100·0	100·0	100·0		34·4		197
Average.	196·3	25·7	8·3	8·3	8·3	50·6	2·9		16·4

in 11 months, and below it in 13. During the 11 months in which the temperature was in excess, the comparative mortality was plus six times, and minus five times; whilst in the thirteen months in which it was minus, the comparative mortality was in excess six, and minus seven times. The months in which it was in excess had an aggregate comparative mortality of 85·6 and the months in which it was minus, of 114·4; showing that those months which were below the average temperature presented an excess of deaths from dropsy compared with those from scarlatina, in the proportion—after allowing for the different number of months—of 7·8 to 8·8. The months in which the temperature was minus the mean of 50·6° embraced, with one exception, all those in which the comparative mortality was in excess, as the following shows:

TABLE VI.

	Temperature Plus.			Temperature Minus.	
	Mortality.	Degrees.		Mortality.	Degrees.
	6·9	66·6		3·8	36·5
	1·9	62·1		7·7	40·8
	8·7	61·5		8·0	41·3
	9·5	59·9		10·2	42·0
	5·9	58·7		9·8	42·5
	4·0	58·5		13·4	42·8
	9·6	56·8		12·6	43·2
	9·5	56·1		2·4	43·2
	9·9	55.8		7·0	45·9
	13·1	52·1		9·1	47·6
	6·6	51·5		12·2	47·9
				11·3	48·9
Total .	85·6			6·9	49·9
Average	7·8		Total .	114·4	
			Average	8·8	

The above tables also indicate, that a temperature between 42·0° and 52·1° is the most adverse to patients suffering from scarlatinal dropsy, and consequently, that we should exercise the greatest care in our treatment when the mean temperature is within this range. To the above there was but one exception, in February, 1848, when, although the temperature was as low as 43·2°, yet the comparative mortality was only 2·4. But a comparison between the comparative mortality for the first quarter of 1848 and that of the corresponding quarters for the years 1849, '50, '51, and '52, shows it to have been much under the ordinary average: in 1848 it was 7·0; in 1849, 15·5; in 1850, 16·1; in 1851, 13·6; in 1852, 12·9. But although there is evidently a connexion between the temperature and the comparative mortality, yet an inspection of the table shows that there must be other causes in operation, as there is no accurate corresponding relation between the two; and the same has been observed of scarlatina. There is, however, a considerable difference in the range of temperature most fatal, which corresponds with the excess of mortality from scarlatina, and that which agrees with the excess from dropsy. As regards scarlatina my conclusions,* from an examination of the results of nine years, were as follow:

1. That a temperature below 44·6° is adverse to the progress of scarlatina; whilst a temperature above that point increases the mortality.
2. That the greatest absolute and comparative mortality for the year happened when the thermometer ranged between 49·6° and 56·9°; and the greatest decrease in the comparative mortality when the thermometer was below 40·0.
3. That the increase in the mortality did not occur in the same ratio with the increase in the temperature; nor did a diminution of the temperature take place in the same proportion as the decrease in the number of deaths; the closest correspondence happening in the months of December, January, and February, when the mean temperature was 40°0 or below.

These results may be presented at a glance thus:

49·6° to 56·9° temperature corresponding with greatest mortality from scarlatina.
42·2° to 52·1° „ „ „ from dropsy.

and it is as well to recapitulate that the last named temperature, 42·2° to 52·1°, corresponds not only with the largest comparative, but also with the largest absolute mortality from scarlatinal dropsy.

Electricity.—On comparing the dropsy with the manifestations of electricity, we obtain some remarkable results, for during the 10 months in which they were but seldom detected, the mortality was in excess in 9 months; and in the 14 months in which they were frequent, the mortality was minus in 10, and plus in 4 months. And on comparing the mortality of those months in which the electricity was active, with those in which it was passive, we shall at once see the remarkable difference:

	Comparative Mortality.	
Electricity active in 14 months . . .	. 98·6 =	7·1 each month.
„ passive in ten months . .	. 101·4 =	10·1 „

We may therefore conclude, that an active state of the electricity of the air exerts a favourable influence on scarlatinal dropsy. This agrees with my conclusion as to the influence of electricity on scarlatina—viz., "that an active condition of the electricity of the atmosphere is much more favourable to life, as regards scarlet fever, than a passive condition."

Rain.—The influence exercised on the body by moisture depends chiefly, as shown by Dr. Casper, on the co-existing temperature. I shall therefore divide the 24 months into the cold and wet, and the warm and wet—the cold and dry, and the warm and dry.

* Medical Times; various numbers for 1849.

TABLE VII.—*Comparative Mortality. Years* 1848 & 1852.

	Wet. Cold. − 50·5°		Wet. Warm. + 50·5°
	2·4	....	5·9
	9·8	....	4·0
	6·9	....	13·1
	10·2	....	9·5
	12·2	....	1·9
	11·3	....	9·6
Total	52·8	....	44·0
Average	8·8	....	7·3

	Dry. Cold. − 50·5°		Dry. Warm. + 50·5°
	3·8	....	9·5
	12·6	....	8·7
	13·4	....	9·9
	7·7	....	6·6
	8·0	....	6·9
	7·0	....	—
	9·1	....	—
Total	61·6	....	41·6
Average	8·8	....	8·3

The temperature of 50·5° has been selected as being the average of the two years investigated. It will be seen that 12 months presented an excess of rain, being *plus* 2·5 inches per month for the year 1848; and *plus* 2·9 inches per month for the year 1852; and twelve months *minus* the same sums respectively. Of the twelve wet months, six were below the average temperature, and six above it; the cold and wet months were more fatal as regards the dropsy than the warm and wet, in the proportion (of the comparative mortality) of 8·8 per cent. to 7·3 per cent. Of the twelve dry months, seven had a temperature below 50·5°, and presented an average comparative mortality of 8·8; whilst the five warm months had an average comparative mortality of 8·3. The aggregate comparative mortality for the twelve dry months was 113·2, and for the twelve wet months, 96·8. We here see that the average mortality was less during the wet than the dry months; and least of all during the wet and warm months. This result agrees with that obtained when treating of the influence of humidity on scarlatina—viz., that, with the exception of March and December, the mortality was less in those months in which the degree of humidity was above the mean.*

Sex.—Having thus briefly considered the influence of appreciable atmospheric variations on the mortality of scarlatinal dropsy, we now proceed to another very interesting series of inquiries—viz., to ascertain the relative numbers in which the two sexes suffer; the mortality from the disease at different ages; and also briefly to consider the proportion of the dropsy to scarlatina. An examination of a large number of cases brings out the very important fact, that 60·3 per cent. of the fatal cases were of males, and 39·7 per cent. only were of females; 946 fatal cases out of 1575 being males, and only 629 females. Now that this great difference depends on the scarlatinal dropsy, ordinarily so called (I mean dropsy attended with serous infiltration of the cellular tissue), is shown by an analysis of 478 cases which were registered within the bills of mortality for London, during the last half of the year 1848. Of these 478 cases, 290 were males and 188 were females; being in the proportion of 60·6 males to 39·4 females. The number of males dying from scarlatina within the bills of mortality for London, during the year 1848, was 2473,

* Medical Times, 1849.

and of females, 2294; or in the proportion of 51·8 per cent. of males, and 48·2 per cent. of females. The deaths from all causes in London, during the years 1838—44, under the age of fifteen years, were 85,028 males, and 76,600 females—making a total of 161,628; and a per-centage of 52·6 males to 47·4 females.* I will put these very remarkable results in a tabular form.

TABLE VIII.

YEARS.	——	MORTALITY.			PER-CENTAGE.	
		Males	Females	Total.	Males	Females
1848-52†	Scarlatinal dropsy: all kinds.	946	629	1575	60·3	39·7
1848	Scarlatinal anasarca.	290	180	478	60·6	39·4
1848	Scarlatina	2473	2294	4767	51·8	48·2
1834-44	Deaths under 15 years, from all causes . .	85,028	76,600	161,628	52·6	47·4

We have here these most remarkable facts, that whilst the deaths under fifteen years, from all causes, present an average of 52·6 per cent. of males; and from scarlatina, of only 51·8; those from scarlatinal dropsy amount to 60·3 per cent. It would be very interesting to solve this, but I fear our present state of knowledge is inadequate to the purpose—and especially as we find, by an analysis of 76 cases,‡ *that the proportion of males to females in those attacked is nearly the same as of those who die;* for, of the 76 attacked, 45 were males and 31 females; being 59·2 per cent. males, and 40·8 per cent. females. And to prove that this remarkable difference was not confined to any one year, or portion of the year, I subjoin the following table:

TABLE IX.—*Mortality. Scarlatinal Dropsy.*

1st Quarter:	Males.	Females.	3rd Quarter:	Males.	Females.
1848	18	25	1848	100	71
1849	73	45	1849	31	27
1850	19	13	1850	18	7
1851	16	12	1851	(?)	(?)
1852	38	9	1852	38	30
	164 = 61·2	104 = 38·8		187 = 58·1	135 = 41·9
2nd Quarter:			**4th Quarter:**		
1848	52	31	1848	197	120
1849	37	30	1849	53	37
1850	16	18	1850	42	19
1851	11	9	1851	66	33
1852	37	25	1852	84	68
	153 = 57·5	113 = 42·5		442 = 61·5	277 = 38·5

* I have limited the age to 15 in this last inquiry, as nearly all the deaths from both scarlatina and scarlatinal dropsy occur under this age; wherefore a comparison with the per-centages obtained from a more extended range of age would lead to erroneous conclusions.

† The third quarter of 1851 is not included, for reason assigned p. 229. ‡ Private cases.

TABLE IX. (*Continued.*)

—	Males.	Females.	Total.	Per-centage.		
				Males.	Females.	Total.
1st Quarter . .	164	104	268	61·2	38·8	100·0
2nd do. . . .	153	113	266	57·5	42·5	100·0
3rd do. . . .	187	135	322	58·1	41·9	100·0
4th do. . . .	442	277	619	61·5	38·5	100·0
	946	629	1575	238·3	161·7	400·0

The following presents the deaths in connexion with the attacks:

TABLE X.—*Scarlatinal Anasarca.*

	No. of Attacks.	No. of Deaths.
Males	45 = 59·2	290 = 60·6
Females	31 = 40·8	180 = 39·4
	76	470

Tables 9 and 10 show that the per-centage of deaths of males to females varies in the different quarters; that the largest proportion of females suffer in the second quarter, and the smallest in the fourth quarter—that of the second presenting the ratio of 42·5, and that of the fourth only 38·5 deaths out of 100 of both sexes; whilst the sum of males was 57·5 deaths in the second, and 61·5 deaths in the fourth quarter, out of 100 of both sexes. And an examination of the corresponding quarters for each year proves this variation to be more than accidental, as it occurred in nearly all. *We may, then, deduce from this examination, that males suffer from scarlatinal dropsy in the proportion of* 60·3 *per cent., and females of only* 39·7 *per cent.; that the per-centages vary between* 61·5 *and* 57·5 *per cent. for males, and* 42·5 *and* 38·3 *per cent. for females; and that these variations depend on some unknown but definite causes, which act with different degrees of intensity in each of the four quarters of the year.*

Age.—The age at which persons die from scarlatinal dropsy is early, as might have been expected from the parent disease being a malady of childhood. From an examination of the subjoined table, it will be seen that the fourth year (called *three* in the registrar-general's report) is the age at which it is most fatal.

TABLE XI.—*Mortality. Metropolis.*

Age.	1848-51. Scarlatinal Dropsy. Total.	1848-51. Scarlatinal Dropsy. Per-centage.	1848. Scarlatinal Anasarca. Total.	1848. Scarlatinal Anasarca. Per-centage.	1845-48. Scarlatina. Total.	1845-48. Scarlatina. Per-centage.	1838-44. Deaths—all causes. Under 15 Years. Total Deaths.	1838-44. Deaths—all causes. Under 15 Years. Per-centage.
$\frac{0}{1}$	22	1·8	6	1·3	537	6·6	68,271	42·2
1	90	7·3	32	6·7	1170	14·5	32,940	20·3
2	144	11·6	66	13·8	1455	18·0	18,363	11·4
3	220	17·8	85	17·8	1373	17·0	12,042	7·5
4	180	14·6 = 53·1	71	14·9 = 54·5	1045	12·9	7,969	5·0
5	176	14·2	59	12·3		69·0		86·4
6	122	9·9	47	9·8				
7	99	8·0	44	9·2				
8	59	4·7	27	5·7				
9	41	3·3 = 40·1	17	3·6 = 40·6	1906	23.6	16,145	9·9
10	24	2·0	11	2·3				
11	19	1·5	4	0·8				
12	7	0·5	2	0·4				
13	8	0·6	1	0·2				
14	6	0·4 = 5·0	0	0·0 = 3·7	..	3·9	5,888	3·7
15 to 20	13	0·9	3	0·6	..	1·3		
20 to 30	5	0·4	3	0·6	..	1·0		
30 to 40	2	0·1	0	.. ..	..	0·8		
40 to 50	5	0·4	0	.. ..	..	0·4		
	1242	100·0	478	100·0	..	100·0	..	100·0

From the first and second columns we learn that 53·1 per cent. of the deaths from scarlatinal dropsy occur under the age of five years, and 40·1 per cent. between the ages of five and ten; making a total of 93·2 per cent. under ten years, and 5·0 per cent. between ten and fifteen, or a total of 98·2 per cent. under the age of fifteen, leaving only 1·8 per cent. above that age. We also see that 220 deaths out of 1242 happened in the fourth year, being 17·8 per cent. On comparing these with the next—scarlatinal anasarca, we find the proportion of deaths at an early age somewhat greater, being 54·5 per cent. under the age of five; 40·6 per cent. between five and ten, or 95·1 per cent. instead of 93·2 per cent. under ten; and 3·7 per cent. between the ages of ten and fifteen; making a total of 98·8 of all cases under this age. The fourth year is also the age at which the greatest mortality occurs, for 85 fatal cases happened at this age out of 478, or 17·8 per cent. The ratio of deaths from all forms of scarlatinal dropsy is the same; but the proportion of deaths, under one year, from anasarca, is smaller, being 1·3 to 1·8 per cent. from dropsy. It is also smaller in the second year, being 9·1 per cent. from dropsy, and 8·0 per cent. from anasarca. Anasarca, however, is most fatal in the third and fifth years, in the proportion of 28·7 per cent. to 26·2 per cent. In the sixth year, a marked difference is also perceptible, the dropsy being fatal in 14·2 per cent. to 12·3 per cent. from anasarca. These variations may perhaps in part depend on the greater extent of the tables of dropsy, which extend over a period of four years, whilst that of anasarca was formed from the results of half a year only. On comparing the deaths from scarlatinal dropsy with those from the parent disease, we find that only 1·8 per cent. happen from the former, in children under one year, to 6·1 per cent. from the latter; that 20·7 per cent. supervene from the dropsy in children under three years, and 39·1 per cent. from scarlatina; 53·1 per cent. from dropsy, and 69·0 per cent. from scarlet fever, in children under five years; and 40·1 per cent. from dropsy between the ages of five and ten, whilst only 23·6 per cent. are fatal from scarlatina during the same period. On comparing the mortality from scarlatinal dropsy with that from all causes under fifteen years, we find the deaths under one year to be only 1·8 per cent. of the former to 42·2 of the latter; under three years, 20·7 per cent. from dropsy to 73·9 from all causes; between three and five years, 32·2 per cent. to 11·5 per cent.; between five and ten years, 40·1 per cent. from dropsy to 9·9 from all causes; and between ten and fifteen years, 5·0 per cent. to 3·7 per cent.

Not having any data ready to ascertain whether or not the proportionate mortality from the dropsy is the same in the country as in London, I have arranged the following table of the mortality from scarlatina for all England, in opposition to that for London :

Table XII.—*Deaths from Scarlatina at Different Ages.*

——	Ages.											Total.
	0	1	2	3	4	5-10	10-15	15-20	20-30	30-40	40-50	
England ..	1291	3102	3705	3386	2677	5400	1056	268	228	130	56	21,304
Metropolis	537	1170	1455	1373	1045	1906	310	100	79	65	36	8,076
England ..	6·1	14·6	17·4	15·9	12·6	25·4	4·9	1·2	1·1	0·6	0·2	100·0
Metropolis	6·6	14·5	18·0	17·0	12·9	23·6	3·9	1·3	1·0	0·8	0·4	100·0

It will be observed that the variations are not very great, except at the age of five to ten, when it amounts to 1·8 per cent.; and we may suppose that the variations in the dropsy are not proportionably greater.

The last point which we have to consider respecting age is the influence which it exercises in the susceptibility to or mortality from the dropsy. It has long been believed, and, as the table will show, justly, that a proportionably less number of young children suffer from dropsy than of elder ones. The following table shows the number of deaths which occurred from dropsy in 100 cases registered as caused by scarlatina, at the different ages of life up to fifty years.

TABLE XIII.—*Deaths in the Metropolis.* 1848.

Age.	Scarlatina.*	Scarlatinal Dropsy.	From Dropsy out of each 100 Scarlatina.
0–1	302	10	3·0
1	645	46	7·3
2	848	69	8·1
3	812	125	15·4
4	652	92	14·1
5 to 10	1209	238	19·7
10 to 15	159	26	16·4
15 to 20	45	7	15·5
20 to 30	40	—	—
30 to 40	26	1	3·9
40 to 50	10	—	—

From this we see, that in the year 1848—which has been shown (Table III.) to have had a mortality from the dropsy considerably below the mean of the five years 1848 to 52—19·7 cases of every 100 registered as caused by scarlatina, in the quinquennial period five to ten years, were produced by dropsy; and that the smallest proportion occurred in children under one year, being only 3 per cent. The table also shows that the average mortality from the dropsy of all fatal cases of scarlatina under three years was 6·1; of the fourth year, 15·4; and of the fifth, 14·1 per 100, respectively; *or in the ratio of* 11·4 *in each* 100 *of scarlatina fatal under five years; of* 19·7 *in each* 100 *between five and ten; of* 16·4 *between ten and fifteen; and of* 15·5 *between fifteen and twenty.* We perceive from this that if a child suffering from scarlatina recover from the primary attack, he is more likely to die from dropsy between the age of five and ten than if he be under one year, in the proportion of 19·7 to 3·0; in the fourth year compared with the first in the ratio of 15·4 to 3·0; in the fifth, compared with the same year, of 14·1 to 3·0, or to the second, of 14·1 to 7·3; &c. The question arises, Does this immense difference depend on a less susceptibility to dropsy at an early age, or on death occurring during the eruptive period in greater proportion in young than in elder children? My own belief, from personal observation, is, that young children are less susceptible to the disease than older, but I have at present no data to solve the question.

From the preceding tables and considerations we arrive at the following conclusions: (a) *That the mortality under fifteen years of age, from all causes, is greatest during the first year of existence, being* 42·0 *per cent., and smallest between ten and fifteen years, when it is only* 3·7 *per cent.;* (b) *that the mortality from scarlatina is greatest during the third year of life, when it reaches as high as* 17·0 *per cent.;* (c) *that the deaths from scarlatinal dropsy are greatest during the fourth year of life, when they amount to* 17·8 *per cent.;* (d) *that although the dropsy produces the greatest number of deaths during the fourth year, yet compared with the mortality from all causes under fifteen years, it is by far the most fatal in the quinquennial*

* **Deaths from scarlatinal dropsy are included in the registrar-general's report under the head of scarlatina.**

period of five to ten years; (e) *that, from an average of four years, the period most fatal from the dropsy, when compared with that from scarlatina, is the quinquennial period of five to ten years, in the proportion of* 40·1 *per cent. of the former to* 23·6 *per cent. of the latter; and* (f) *that an accurate examination of the year* 1848 *gives the same result, in each* 100, *as* 19·7 *of all deaths by scarlatina during this period of life, were caused by dropsy.*

Proportion of Deaths from Scarlatinal Dropsy to Deaths from Scarlatina.—We have already shown that the proportion of deaths from dropsy to those from scarlatina differs in each month; and that although these variations may in some measure be accounted for by changes in the atmosphere, yet that some other cause or causes must be in operation for their production. It has been shown—with the exception of January and February, 1848, when the electricity of the air exhibited more numerous manifestations of its presence than during any other months of the two years 1848 and 1852 (save in March, '48)—that the month of August presented the smallest comparative mortality; and that this was the more remarkable from the months of July, September, and October presenting a large comparative mortality. It has also been shown that the comparative mortality was larger during the last four months of the year than in any other. That these variations in the comparative mortality do not depend altogether on the same causes as those which influence the mortality from scarlatina is apparent from an investigation of the five years 1848—52. On examining this period, we find that in 1848 and 1852, when scarlet fever was unusually fatal, the proportion from dropsy was below the mean of the five years; and that in 1852 it was smallest. This will be seen by a glance at the following table:

Table XIV.—*Comparative Mortality from Dropsy.*

1848	12·9
1849	15·3
1850	12·9
1851*	13·8
1852	12·1

The proportions for the quarters of the years 1848—52 were—

Table XV.—*Dropsy. Comparative Mortality.*

	1st Quarter.	2nd Quarter.	3rd Quarter.	4th Quarter.
1848	7·0	10·2	10·7	17·7
1849	15·5	13·5	15·0	20·2
1850	16·1	15·3	7·9	14·2
1851	13·6	11·8	10·9	16·4
1852	12·9	11·0	10·2	15·9
Average.	13·0	12·3	10·9	17·0
Or,	24·6	23·3	19·8	32·3

* One quarter calculated.

We here see that the comparative mortality was least during the first and second quarters of those years—1848 to 1852—in which scarlatina was epidemic; was in the same year below the average in the third quarter, and also in the last quarter of 1852; but above it by 0·7 in that of 1848. We also perceive that the comparative mortality was only 19·8 per cent. in the third quarter, whilst it was 24·6 and 23·3 in the first two quarters, when scarlatina was less prevalent than during the third. A comparison of the absolute mortality from the dropsy with that from scarlet fever also indicates the same. In the first quarter there were 268 fatal cases of dropsy out of 2162 from scarlet fever; in the second, 266 out of 2279; in the third, 322 to 2930; and in the fourth, 719 in 4195. Or in the proportions of—*dropsy*, 17·0 per cent. in the first quarter; 17·0 per cent. in the second; 20·4 per cent. in the third; and 45·6 per cent. in the fourth: and of *scarlatina*—18·7 per cent. in the first quarter; 19·7 per cent. in the second; 25·3 per cent. in the third; and 36·3 per cent. in the fourth. We here see that the relative proportions between the two do not correspond during the year, but agree most closely in the first, and differ most widely in the last quarter; and that 1600 fatal cases of dropsy occurred in the five years 1848—52 in 11,566 fatal cases of scarlatina, being in the ratio of 13·8 cases of dropsy in each 100 of scarlatina. And Tables 11 and 13 show that the ratio varies very considerably with age, being highest, 19·7, to each 100 for each year of the quinquennial period of five to ten years; and lowest, 3·0 to each 100 cases fatal during the first year; the proportion increasing, but not in any given ratio, from the first year to one of the years between the ages of five and ten; most probably to the tenth year.

We next pass on to consider *The Causes of Death* in scarlatinal dropsy, which are many, and may be divided into two classes; in the first we may place those which induce death by what may be termed (1) pathofusical* terminations, and in the other by (2) accidental. In the first we may class death (*a*) by pressure of the fluid on some vital organ, as the brain, lungs, or heart; (*b*) by deterioration of the blood and system generally; and (*c*) by uræmia and its consequences; in the latter we place all those cases in which death is induced by inflammatory disease of the viscera or their coverings. The data which have hitherto afforded such trustworthy information, and have been of so great value, become in this part of our subject comparatively valueless, as so few cases are registered with more than the primary and secondary diseases: that is to say, with more than "scarlatina" and the subsequent "dropsy." Still, as there are some returns which contained a full detail of the complications, and as many were verified by post-mortem examinations, I have compiled the following results, but do not offer them as a standard by which we can ascertain the ratio in which the various causes induce a fatal termination. I may again mention that of 680 deaths registered as resulting from scarlatinal dropsy, 38 were stated to have been caused by cerebral effusion, and 50 by thoracic effusion *per se*, that is to say, uncomplicated with general dropsy. I do not think we should allow so great a proportion to cases of

* From πάθος, *disease*, φυσικὸς, *agreeable to nature*. I propose this term to signify those causes of death which result from disease in its ordinary course, as when death occurs from hepatization of the lungs in a case of pneumonia.

cerebral effusion, for but few were verified by post-mortem examination; but should rather place very many of them to the account of uræmia, as coma and convulsions are symptoms common to each. And it is more than probable that many of the deaths registered as caused by thoracic effusion, were really induced by inflammatory disease of the pleuræ or pericardium. It will, therefore, be impossible to present a table showing the ratio in which all the various causes of death prove fatal. The following, however, will afford some information on the subject, as it gives the cause of death in 128 cases.

Brain and Spinal Cord.—Affections of these organs were the cause of death in 14 cases out of 128; meningitis being fatal in 6 cases; passive effusion into the cerebral cavity in 7 cases; and hemiplegia (cause unknown) in 1 case.

The Larynx was diseased in 3 cases, presenting 2 fatal from croup, and 1 from laryngitis.

Lungs and Pleuræ.—Diseases of these organs were fatal in no less than 40 cases of the 128, or in the ratio of 31·2 per cent. Of these, 3 were caused by bronchitis; 14 by pneumonia, 4 by pleuro-pneumonia, 7 by pleuritis, and 2 by pleuro-pneumonia with peritonitis, making an aggregate of 30 cases by inflammatory disease of the lungs and pleuræ. There were also 1 case of phthisis, 1 of œdema of the lungs, and 7 of non-inflammatory serous effusion into the thoracic cavity.

Heart and Pericardium.—Of the 128 cases, 12 were fatal from diseases of the heart and pericardium, 4 deaths being caused by pericarditis, 3 by endocarditis, and 5 by effusion into the pericardium without dropsy of other cavities.

Liver.—No disease stated; jaundice was a symptom in one fatal case.

The Stomach, Bowels, and Peritoneum.—The following is the analysis of 13 cases fatal from affection of these parts, out of the 128. Of these 13, 2 were cases of ulceration of the colon, with diarrhœa during life; 2 of ulceration of intestines (of which it was not stated), also with diarrhœa during life; 1 case fatal from diarrhœa, but registered without a post-mortem, and most probably fatal from ulceration; and 4 cases were fatal from peritonitis, making an aggregate of 9 cases from inflammatory disease. The other 4 cases were, 1 of disease of the mesenteric glands, and 3 of ascites without effusion into any other serous cavity.

Gangrene, Sloughing, Abscesses, and Erysipelas were fatal in 9 cases out of the 128, in the following proportions: 3 cases of abscess, 2 being of the neck, and 1 of the joints: 1 case of gangrene of the feet; 1 case of mortification (part not mentioned); 2 cases of suppuration of the glands of the neck; and 2 cases of erysipelas.

Death by Uræmia.—By this I mean, death by that class of symptoms so well known by the name of poisoning by urea. The prominent symptoms of uræmic poisoning were manifested in 37 cases out of the 128. Of these 37 cases, 27 were of convulsions, or of convulsions and coma together; 5 of coma; 1 of tetanic convulsions; 1 of epileptic convulsions; 2 of complete suppression of urine (convulsions and coma were enumerated as symptoms in these two cases); and 1 of uræmia.

Having now treated of, at what may perhaps be considered by many an unnecessary length, the influence of season, temperature, humidity, electricity, sex, and age, on the production and mortality of scarlatinal dropsy, and also the causes of death, we will next consider the alterations produced by the disease in the blood and urine, and also the symptoms and phenomena of uræmic poisoning. We shall then proceed to the last division of our subject—viz., a consideration of the other symptoms and peculiarities of scarlatinal anasarca; its diagnosis, prognosis, and treatment.

Alterations in the Blood.—When the scarlet-fever poison is taken into the system, it is generally believed that a peculiar action is set up in the blood, and certain changes effected, by which the phenomena of scarlatina are manifested. In some cases the poison speedily induces secondary diseases of various organs, in others not until some time has elapsed: under the former circumstances, the changes in the blood have been very imperfectly ascertained; in the latter they have been more accurately described. In some patients the alterations in the blood appear to cease when the febrile stage has passed away, but in others they continue; when it assumes a peculiar leuco-phlegmatic appearance, which is quite diagnostic of the approaching dropsy. Sometimes the dropsy sets in before the leuco-phlegmasia has made any progress, at other times not until it is well marked. The period at which it occurs will be considered in the next article.

The alterations in the physical characters of the blood drawn from the body are well marked, for we find the clot either rather small and buffed, or large, loose, and dark-coloured, swimming in a large quantity of greenish or opalescent serum. The appearance of the latter very much resembles that of blood drawn from a typhus patient. The opalescent or milky appearance of the serum, when present, may depend on the presence (*a*) of an albuminous substance in a state of minute division, as shown by Simon and Scherer; (*b*), of fat, also in a minutely divided state, as shown by Trail, Nasse, and others; (*c*) of colourless corpuscles suspended in the serum. Frerichs states one of the two former to be the cause of the turbidity in cases of renal dropsy, for he has met with both, and believes the ultimate cause to be the same in both cases—viz., a diminution of the alkalinity of the blood, by which, in the former, the albuminate of soda is decomposed, and the albumen set free; and in the latter by fat in combination with a less amount than usual of alkali.

The alterations in the serum are not very marked at first, but as the disease progresses, and in proportion to the quantity of albumen voided by the kidneys, its specific gravity diminishes, and the quantity of its saline constituents is reduced. These changes were well known to Christison, who stated the specific gravity to be gradually reduced from 1·030 to 1·022, and subsequently much lower. The quantity of solid matters in the serum he also states to be often reduced from 100 to 60 in 1000 parts: these statements apply, of course, to advanced cases of the disease. Frerichs analyzed the serum of a woman who had been ill with scarlatinous anasarca for eight days only, and found the specific gravity to be 1·019; the saline, fatty, and extractive matters about normal; but only 51·7 of albumen, instead of about 75·0 parts, which Simon considers the normal average. In the early part of the disease, Christison states that the quantity of hæmatosine is normal, being about 133 parts in 1000; and that the fibrine is either normal or more or less increased, sometimes considerably so. When the disease has lasted some time, the blood-corpuscles are much diminished in quantity, sometimes even to one third. Heller made a series of experiments on the blood of persons suffering from renal anasarca, and arrived at the conclusion that the albumen was the constituent chiefly diminished. Urea is usually present to a greater or smaller extent, but is sometimes altogether absent. Christison detected it on the ninth day of the disease. Heller detected it in two cases; in one case he found 1·85, and in the other 1·74 in 1000 parts of blood. Simon gives the analyses of thirteen cases, and states that a considerable quantity of urea was found in most. Garrod also detected uric acid in the serum in three cases of Bright's disease. MM. Becquerel and Rodier* have lately investigated very extensively the morbid changes in the blood in many diseases, and have drawn the following conclusions as regards "acute Bright's disease;" that the quantity of albumen is slightly and speedily reduced, and that the globules and fibrine remain unaltered. The changes in the blood may be summed up, as consisting, in the acute stage, of (*a*) a diminution in the quantity of albumen, (*b*) a slight increase in that of the fibrine, and (*c*) in the presence of urea. In the more advanced stage, by (*a*) a diminution in the quantity of blood-corpuscles, and (*b*) of the albumen, (*c*), a normal or increased quantity of fibrin, and (*d*) the presence of urea and uric acid.†

Alterations in the Urine.—As before stated, the urine usually contains, at some period or other of the eruptive or desquamative stage, a greater or less quantity of albumen for a shorter or longer duration. In some cases I have detected it in the urine in the morning and not in the evening, or *vice versâ; sometimes only in one specimen during the whole period of the disease.* Besides albumen, we also meet with a variable quantity of renal epithelium in the desquamative stage; sometimes fibrinous casts of the renal tubules, containing either epithelial cells or blood-corpuscles; and sometimes free blood-corpuscles. In some cases none of these abnormal constituents appear until the dropsy shows itself, but more generally they occur as above stated, and continue until the supervention of the dropsy. As a rule, when these abnormal constituents, or either of them, persist in the urine for more than ten days after the disappearance of the rash, and especially if they increase in quantity, we may expect an attack of dropsy.

In the first stage of dropsy the urine is at first high-coloured, bloody, or brownish red, scanty, of high specific gravity, often turbid from the presence of a large quantity of uric acid, of lithate of ammonia, soda, or potash, or all mixed, which it deposits on standing. On examining the precipitate we find, in addition to the lateritious deposit, if present, a variable quantity of abnormal cells—to wit, blood-globules, mucous corpuscles, renal and vesical epithelium more or less disintegrated, and large globular, nucleated cells, some of which resemble in size and constitution the parent cells of cancer. Another variety of large cells was often met with by me in 1848, but is rarely found now. They were of a yellow colour, with indistinct nuclei, and were usually met with in apposition

* Gazette Médicale de Paris.

† See Simon, vol. i. p. 322; and vol. ii. p. 515; also British and Foreign Medico-Chirurgical Review, vol. ix. p. 301; and other authorities quoted.

with each other, sometimes forming a cluster, or a single row of four, five, or six; and more rarely, two or three rows joined together. When thus united they often separated from each other, and, as far as could be seen, without the rupture of any intervening membrane. They were of a more or less hexagonal shape, of less thickness than length, and of greater breadth than the fibrinous casts of Simon. In addition to these cells, the sediment usually contained a greater or less quantity of the fibrinous casts of the renal tubules (casts of Simon), which have lately been named by Johnson epithelial casts, granular casts, &c., according to their appearance and constituents. These casts are from $\frac{1}{700}$ to $\frac{1}{500}$ of an inch in breadth, and from $\frac{1}{50}$ to $\frac{1}{30}$ of an inch in length, and are composed of granular fibrine, either by itself or mixed with epithelial cells or blood-corpuscles. When containing blood-corpuscles, they are probably the result of rupture of the vessels of the Malpighian tuft, and consequent effusion of blood into the secreting tubules of the kidney; when containing epithelial cells, of the effusion of fibrine into the same tubes. Frerichs and Johnson are at total variance as to the mode in which the epithelial cells become an element of these casts. Johnson believes that the cells are thrown off from the parietes into the free canal of the tubules; whereas Frerichs states his belief that the cells are only accidentally mixed with the fibrine, being entangled with it. This will be considered more fully hereafter. These casts also sometimes contain crystals of uric acid or oxalate of lime, and sometimes are unmixed with cells or crystals, when they are very pale and almost transparent. The ordinary salts are sometimes decidedly deficient, at others about the normal standard. Frerichs states the former to be the most common; Rayer, the latter. Frerichs also says that the diminution in the quantity of the salts will depend on the greater or lesser number of the tubes blocked up. The urea is usually diminished in quantity; Frerichs states it to vary from 7·9 to 14·2 per 1000; Simon, 7·7. We may roughly estimate the quantity of urea present by adding nitric acid to urine on a slip of glass, and then observing, under the microscope, the quantity of nitrate of urea formed. We also sometimes meet with common salt in combination with urea, in the form of octohedral crystals, which closely resemble oxalate of lime. They may, however, be readily distinguished from the oxalate by their larger size and rounder angles, and by their dissolving in distilled water. When dissolved, they often recrystallize in cubes instead of octohedra; and if treated by nitric acid, dissolve with slight effervescence, producing crystals of nitrate of urea.

The specific gravity varies very much. I have met with it, at the commencement of the disease, as low as 1·010, and as high as 1·048; in the latter case the urine became solid on the addition of nitric acid; but the average of my cases, whilst the urine was scanty, was from 1·020 to 1·025, and this may be considered as the mean of other observers. The albumen varies as greatly as the specific gravity, ranging from 7·86 to 33·61 in 1000 parts of urine. The last-mentioned amount was that given by Simon, and the average may be taken at about 16·5 per 1000. The weight of the albumen thus passed in the twenty-four hours ranges from 75 to 387 grains; the average may be taken at 170. The weight of the fibrinous cylinders varies very much, and is ascertained with difficulty from their intermixture with other deposits. Frerichs found it 6·0 in 1000 parts.

As the disease advances, the urine alters considerably; it is passed less frequently and in greater quantities, is much lighter in colour, gradually assuming the peculiar greenish tint so indicative of the presence of albumen; when gently shaken, it often looks like thin syrup, and froths very much when more forcibly shaken. The blood-corpuscles gradually diminish, and finally disappear; the mucous corpuscles and other nucleated cells, and the fibrinous casts, also diminish in number; the latter, if the disease assume a chronic character, frequently cease to contain either epithelial cells or blood-corpuscles. The renal epithelial cells are also voided less perfect, being more disintegrated, and as the patient recovers gradually disappear from the urine as well as all the other abnormal constituents. But if, during the progress of convalescence, any cause—as improper food, cold, the use of stimulants, &c.—should induce a recurrence of the acute form, the urine will assume its corresponding characters. We may assume as a rule, that the larger the number of blood-corpuscles, fibrinous casts, and albumen, and the less the amount of the urine, the more intense is the renal disease, the longer will be its probable duration, the less perfect the recovery, and consequently the greater the danger.

Uræmia.—One of the most formidable complications of scarlatinal dropsy is uræmia. When well marked, the symptoms are convulsions or coma, or both, occasionally delirium, with the suppression of, or a great diminution in, the secretion of urine; and consequent retention of the urinary constituents. But although coma or convulsions constitutes one of the predominant symptoms, yet in making a *post-mortem* examination, we rarely detect any morbid alterations. Sometimes when the symptoms have come on gradually, we meet with indications of slight inflammatory disease; yet, in the generality of cases, the quantity of fluid in the ventricles and arachnoid cavity, is but little if at all above that of health. In my own cases, I did not detect any morbid products, and Frerichs makes a similar statement, having never met with more than a slightly increased amount of fluid, the largest quantity not exceeding one ounce.

There are but few cases more difficult to detect, and yet more important to diagnose correctly, than these; as the uræmic symptoms may occur suddenly, and present all the ordinary features of apoplexy. Thus we may be told, on our arrival at the patient's bedside, that whilst in the pursuit of his ordinary avocations he was suddenly seized with coma and convulsions. This happened to myself in one most marked case; but on inquiry, I ascertained that many of the ordinary signs of the first stage had been present for some days. These may be enumerated as follows:—sleepiness or heaviness, headache or confusion of thought, dizziness, impairment of vision, dilatation and sluggishness of the iris, lassitude, dislike of motion, and other symptoms of *malaise*. In addition, the patient often complains of diarrhœa or vomiting (the latter more frequently than the former), and sometimes of both; but neither of them is so common in scarlatina as in other varieties of renal dropsy. Frerichs says although others have detected urea in the matters ejected from the stomach, yet that he has not, and on the contrary, has frequently ascertained the presence of carbonate of ammonia. I have not had any opportunity of verifying this observation. If a specimen of urine can be obtained, our diagnosis would no longer be obscure, as we should find it small in quantity, deep coloured, albuminous, containing lateritious deposits, and one or more of the other abnormities just described. The pulse may be either slow and full, 60 to 65, or small, quick, and soft; and the skin is usually dry and harsh, and sometimes desquamating. This latter is a most important sign in the comatose stage, as we cannot obtain any history from the patient, and rarely a specimen of his urine. Another important sign, under these circumstances, is a urinous or ammoniacal odour of the breath. The presence of ammonia may be detected by holding a piece of moistened litmus to the mouth, when it will indicate the presence of an alkali, and by a rod dipped in hydrochloric acid, which will give out white fumes when exposed to the breath. Frerichs says that he has repeatedly shown its existence in the breath of men affected with uræmia, and of animals into whose veins urea had been injected.

If we are called to the patient before the invasion of coma and convulsions, and can effect a permanent increase in the secretion of urine, we can ordinarily avert any further mischief; but if we cannot, especially in the advanced stage, increase the urinary secretion, or cause the ejection of the poison, whatever it be, in any other way, the attack will speedily prove fatal. These observations apply with greater force to the comatose stage.

Many theories have been proposed by as many authors; one believing the symptoms to be produced by arachnitis; another, by cerebral or spinal effusion; others, from the presence of urea in the blood, or of some other compound which acts as a poisonous agent on the brain and spinal cord. The theory of poisoning by urea has been most carefully investigated, and has been accepted by some, and rejected by others, amongst the latter of whom we may enumerate Dr. Rees, Jones, and Frerichs. The former rejects it from the want of correspondence between the amount of urea in the blood and the intensity of the symptoms; and also from the total absence of uræmic symptoms in cases where a large quantity of urea was detected in the blood. In addition to these, the experiments of Vauquelin, Segalas, Bichat, and others, of injecting urea into the blood of dogs, without any other effect than an increased flow of urine, may be adduced in opposition to the urea theory. Indeed, so marked was the diuretic action, that these experiments have led to the successful use of urea as a diuretic. Dr. Rees considers the symptoms of uræmia to be caused by a diminished density of the blood; but experience and observation show his theory to be less tenable even than the former. The last and most plausible theory is that lately propounded by Frerichs,—viz., that the urea is changed in the blood into carbonate of ammonia by the agency of a ferment; that if this conversion be effected suddenly, the symptoms will resemble those of apoplexy; but if gradually, they will assume the form of typhus, with subsequent coma and convulsions. To prove his theory, he performed the following experiments. He removed the kidneys of some animals, and then injected urea into their veins: at first no peculiar symptoms were observed; but after the lapse of some hours, they became restless, and vomited, when ammonia was detected in the expired air; and after a short time, convulsions, alternating with stupor and stertor, or stupor and coma, supervened. After death, ammonia was detected in the blood and in the contents of the stomach. He then injected a solution of carbonate of ammonia into the veins of other animals, when convulsions and stupor quickly came on, but ceased after a few hours, the ammonia having passed off by vomiting, and in the expired air, and probably also by the skin. I will not attempt to discuss the merits or demerits of either theory, as this paper is intended to be as much matter of fact as possible.

The diagnosis of uræmia is often very difficult; but the state of the skin, urine, breath, and the previous history, will, with ordinary care, suffice to distinguish it from any other disease. The treatment of uræmia will be considered in the next and concluding article.

Art. IV.

The Influence of Liquor Potassæ on the Urine in Rheumatic Fever. By E. A. Parkes, M.D., Professor of Clinical Medicine in University College, London, and Physician to University College Hospital.

The present paper is a continuation of one published in this journal in January, 1853, in which the action of Liquor Potassæ (Pharm. Lond.) on the urine in health was discussed. It was then shown that this medicine, when taken into the circulation, without being neutralized in the stomach, speedily passed out by the urine, and chiefly, though not entirely, in combination with sulphuric acid, the formation of which it for the time increased. It appeared a fair inference from the facts then stated, that the Liquor Potassæ, in the healthy system, acted as a destructive agent on the sulphur-containing constituents—i. e., the albuminous compounds of the blood or tissue, and led, among other less-easily recognised changes, to the oxidation and elimination of their sulphur. It was shown also that the quantity of water poured out from the kidneys was increased, as well as the organic substances, included under the vague term "extractives." It was, of course, apparent that the study of the action of the liquor potassæ in the healthy system was by no means completed, and that many points remained unexplored. It seemed, however, that a firm basis for future investigations had been laid down, and it was determined to pursue the inquiry on the urine of disease in the same way, and to pay special attention to the inorganic constituents of this secretion.

In the diseased body liquor potassæ is placed under different chemical conditions from those presented to it by a healthy system, and, therefore, no *à priori* decision as to its action in disease can be possible. Though the truth of this remark will be made sufficiently evident in the course of this inquiry, it will at the same time be found, that its action in health may be taken as the general rule of its action in disease. I must observe that I can pretend to do no more than to give, what I hope will be found accurate and trustworthy experiments towards the elucidation of a subject so vast that the labours of one man must be totally unable to exhaust it.*

* I have not alluded to the observations of Dr. Golding Bird or others who have contributed so much to our knowledge of the action of the diuretic salts of potash. My present object forbids this; but I hope my temporary silence will not be construed into indifference to these useful inquiries.

On the present occasion I propose to give the results of experiments in four well-marked cases of rheumatic fever, treated in University College Hospital. In future papers the effect of liquor potassæ in inflammations, and in some of the acute specific diseases, will be given.

The four instances of rheumatic fever were typical cases, with great pain and with the thermometer rising to 103° or 104° Fahr. Three were in men (aged 22, 23, and 26), and one was in a woman (aged 20). Three were treated with liquor potassæ alone, one with this medicine and with colchicum. In two cases there was old heart-disease, with recent pericarditis in one; in the other two there were recent basic systolic murmurs, apparently of endocardial origin, which, in one case, disappeared after recovery. The day of the disease was reckoned in two of these cases from the first symptom, which was well marked, and which was succeeded on the second and third days by articular pain; in another case there had been a long indefinite period of slight ill-health before the articular pain set in suddenly; the disease is dated from the first articular pain; in the other case (a relapse), the articular pain and high febrile symptoms set in suddenly and simultaneously; for some days previously, however, the pulse had been rising. The disease is calculated from the day of sudden increase.

A fifth case of acute rheumatism is, for certain points, occasionally referred to.

In all these cases, in addition to the examination of the urine, and of the cardiac and articular symptoms, an accurate thermometrical investigation was undertaken. After the termination of the case, all these particulars were tabulated. The tables reach to such an extent that it has been found impossible to publish them, and I have therefore given only the conclusions. It is with considerable reluctance that I venture to publish only the average results, but it may be safely assumed, that every endeavour has been made to present them as accurately as possible.*

The relation of the urinary excretions to the weight and height of the individual has been left out of account (although some data exist for calculating these), as not necessarily connected with the matter in hand, and for the same reasons the thermometrical observations are omitted.

In three cases no treatment was adopted for the first twenty-four hours in order to see the exact condition of the urine; afterwards liquor potassæ was given in ʒss doses in simple water, and was pushed to the extent of ʒiij. to ʒvj. in twenty-four hours.

1. *The water of the urine.* (Normal average in 24 hours, 40 fluid ounces.) —Average before treatment in four observations on the 2nd, 3rd, 4th, and

* The liquor potassæ was the preparation ordered in the London Pharmacopœia (6·7 per cent. of pure potash). Unless the contrary is stated, the diet was the hospital low diet—viz., in twenty-four hours, bread ¼ lb., milk O¼, barley water Oj. No difficulty was found in collecting the whole quantity of urine passed in twenty-four hours, except sometimes in the female. The solids were determined by evaporation; the sulphuric and phosphoric acids by baryta; the chlorine by incineration at a very low temperature, dissolving the chloride of sodium in water, and precipitating and weighing as chloride of silver. The uric acid was thrown down by hydrochloric acid, and in some, though not in all cases, was dissolved in potash and precipitated by acetic acid. At the time these observations were made, Liebig's method of determining the urea and chlorine had not been made known.

5th days, diet low, water ad libitum, = 24 fluid oz. in 24 hours; the highest amount being 31½ oz.

Average during treatment (ʒiv. to ʒvj. of liquor potassæ being taken in each 24 hours; no other treatment; in 16 observations on the 5—10 days, = 34 ounces.

Average in 2 cases on the three succeeding days (11th to 13th inclusive). 30 ounces. In a third case, on the 8th, 9th, and 10th day, = 22 ounces. In a fourth case the urine was lost.

Conclusion.—By reference to the individual cases, as well as from the averages, it appears that the urine was slightly increased in quantity, and that this increase (10 ounces in 24 hours) was attributable to the medicine, and not to improvement in the disease.

2. *The urinary solids.* (Becquerel's average for men = 571 grains* — in 24 hours.)

Case 1.

No determination before treatment. From the 5th to the 8th days (inclusive), ℥iss. of liquor potassæ being taken in this time, 2786·28 grains were passed, or 696·57 in each 24 hours. On the 12th and 13th days, the disease being well, no medicine, diet a little better, 404·57 and 391·05 grains were passed.

Case 2.

No determination before treatment. From the 5th to the 10th days, inclusive, (6 days), ℥ij. ʒvij. of liquor potassæ being taken, 5777·22 grains were passed, or 962·87 in each 24 hours. In the 6 following days no medicine; disease improving, but by no means well; diet same; 3436·34 grains were passed, or 572·72 in each 24 hours. On the 19th day, ʒiv. of liquor potassæ being taken, 917·91 grains were passed.

Case 3.

Before treatment, on the 2nd and 3rd days 713·711 grains were passed daily. On the 4th and 6th days, the urine of the 5th being lost (liquor potassæ ʒiij., vin. sem. colch. ʒj. in each 24 hours; diet same; articular symptoms much better), 1071·67 and 993·7 grains were passed. On the 8th day no medicine; disease well; 697·81 grains were passed.

Case 4.—(*Female.*)

Before treatment, on the 5th day, 469·5 grains were passed. On the 6th, 7th, and 11th days, ℥j. ʒiss. of liquor potassæ being taken, 1676·04 grains were passed, or 558·68 grains in each 24 hours. On the 23rd day, convalescence; good diet; no medicine; 364·32 grains were passed. In this case the urine of the intermediate days was partly lost with the motions.

Case 5.

During convalescence, ʒiss. of liquor potassæ being given, the solids augmented by 56 grains in 24 hours.

Conclusion.—In cases 3 and 4 the solids were above the average before treatment;† they augmented decidedly under the use of the potash. In case 1 no conclusion is possible, as the solids were not determined before treatment, and afterwards; the cessation of symptoms and the disuse of the remedy took place at the same time. In case 2 the solids were

* This is usually considered to be much under the real amount, which may be taken as 650 to 750 grains for adult men on good diet.

† In reasoning on these points, it must be remembered that, in acute diseases, the influence of food, which exerts so great an effect on the urinary solids, is completely excluded; the whole amount is derived from tissue-metamorphosis.

greatly increased; and that this was owing in part to the potash, is evident from the fact that when the medicine was left off, an average fall of 400 grains in the amount of the solids, in 24 hours, took place; the articular symptoms and the fever were, however, still severe, so that the diminution must be attributed to the withdrawal of the potash, and not to improvement in the disease.

It may therefore be said, that of the two possible factors, the disease (to use this well-understood term for the sum of the abnormal processes going on in the body) and the remedy, the increase in the urinary solids was due to both.*

3. *The Urea.*—No experiments were made, but it was noted in cases 3 and 4, that nitrate of urea crystallized at once when nitric acid was added to the unconcentrated urine; as there was at this time an increase of organic solids, it is probable that the urea was in excess.

4. *The Creatine, creatinine, &c.*—No experiments were made.

5. *The Constituents containing Sulphur.* (Ronald). In case 1, before treatment, on the 4th day, besides 52½ grains of sulphuric acid, 5½ grains of unoxidized sulphur were passing off in 24 hours. Whether this was increased by liquor potassæ, was not determined in any of these cases; but as the sulphur containing ingredients of the urine were found to be increased, in an analogous case, by the use of the bicarbonate of potash, it may be inferred that the liquor potassæ had the same effect.†

* I may mention here, that in a chronic non-rheumatic case in which the exact weight of the body was known, and in which the ingesta were unaltered, a much greater loss of weight was found to occur when liquor potassæ had been administered than could be accounted for by the increased secretion of urinary solids which was found to take place. In this case the intestines and the skin were unaffected, and it appeared a fair inference that the loss of weight was owing in part to heightened pulmonary functions; that is to say, that the action of the potash was not confined to increasing the urinary excretion, but that the pulmonary exhalation was augmented. No direct experiments were made, and this inference was drawn simply from the great loss of weight, which was evidently due to the potash, and which was certainly not accounted for by the urine, perspiration, or intestinal excreta.

† In the case referred to, a man aged 34 was admitted with affection of the knee and ankle joints, which it was impossible to distinguish from subacute rheumatic arthritis by any symptoms connected with the joints themselves. But there was no pyrexia (temperature normal); the urinary solids passed in 24 hours were in small amount; the sulphuric acid was not increased, and the chlorine was abundant; the uric acid was deficient in the urine, and was abundant in the blood, as in gout (Garrod). The following table shows the action of bicarbonate of potash on the urine.

Calculated for 24 hours.

	Quantity in ozs.	Solids.	Uric Acid.	Soluble Salts.	Sulphuric Acid.	Unoxidized Sulphur.	Remarks.
In 11 hours, no medicine was taken.	23¼	363·171	0·790	54·36	21·460	1·796	
In the succeeding 13 hours ℥iv. of bicarbonate of potash were taken.	74	847·12	....	273·4	27·5	7·002	The urine was highly alkaline, and contained a great quantity of carbonate or bicarbonate.

The bicarbonate of potash, therefore, had the following action: it passed off very rapidly, and in part as carbonate or bicarbonate, with the urine, and therefore increased the amount of

6. *The Uric Acid.*—(Normal average, 8 grains in 24 hours.)

Case 1.

Before treatment, on the 4th day	8·851 grains.
On the 5th day, (ʒvss. of liq. pot.)	9·51 ,,
On the 6th day, (ʒv. of liq. pot.)	9·975 ,,
On the 7th day, (ʒv of liq. pot.), articular symptoms almost gone	2·013 ,,
On the 8th day, (ʒv. of liq. pot.)	·78 ,,

As in other cases, liquor potassæ was not found to diminish the uric acid; and as the diminution in the above case coincided with decline of the symptoms, it is to be inferred that the effect of the potash on the uric acid was not marked.

Case 2.

No determination before treatment. From the 5th to the 10th day (℥ij. ʒvij. of liq. pot.), 12·694 grains were secreted in each 24 hours. From the 12th to 16th day (no medicine, disease improving), there were passed in each 24 hours 8·154 grains. The potash therefore caused no decrease; it may have caused an increase; but from the facts of Case 1, it is fair to conclude that the uric acid was in excess from the morbid processes, and not from the remedy.

Case 3.

Before treatment, on the 3rd day	4·439 grains.
On the 6th day, (liq. pot. ʒiij., vin. sem. colch. ʒj., being taken on the 4th, 5th, and 6th; symptoms better)	7·632 ,,
On the 9th day (no medicine on the 8th and 9th)	4·830 ,,

There appeared here to be a decided rise under the use of liquor potassæ and colchicum; but considering the known action of colchicum (Chelius, Maclagan), the augmentation may fairly be ascribed to it.

Case 4.—(*Female.*)

Before treatment (5th day)	6·772 grains.
On the 6th day (ʒv. of liq. pot.)	9·200 ,,
On the 7th day (ʒv. of liq. pot.)	9·200 ,,
On the 11th day (no medicine on 10th; ʒiss. of liq. pot. on 11th)	8.59 ,,
On the 23rd day (convalescence; no medicine)	1·692 ,,
On the 45th day (good health; good diet)	1·320 ,,

During the use of the potash there was a slight increase, but whether this was owing to the remedy or to the disease, is doubtful.

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the soluble salts; it augmented also the water by 50 ounces (the calculation was made for 24 hours), the organic solids by 264 grains, the sulphuric acid by 7 grains, and the sulphur by more than 5 grains.

The action of the bicarbonate of potash appears so far to differ from that of the liquor potassæ, that some considerable portion passes off unchanged in the urine; whenever this is the case, the urine is rendered alkaline This passage occurs with such rapidity, that, if it is wished to keep up the increased alkalinity of the blood, the salt must be given very frequently (every hour or so); if given only every four hours, in less than two it is excreted, so that for two hours the blood is unacted upon. The facts now given show how erroneous is the statement Durand Fardel has lately published respecting the action of the alkaline (bicarbonate of soda) Vichy waters. Because some of the salt passes out unchanged, he concludes that all does so, and that it is simply a poison, rapidly excreted with the urine. The same reasoning might be applied to the bicarbonate of potash; because some part of the salt is excreted unchanged in the urine, we might argue that all is so, and that the bicarbonate produces no other effect on the system. Chemical analysis shows, however, that besides the excretion of the salt, the organic solids, the sulphur, and the sulphuric acid, are all increased.

Conclusion.—The uric acid is in most cases in excess in rheumatic urine, but is not always so. Liquor potassæ may cause some increase, but the influence cannot be great, and certainly this medicine will not prevent the fall in the amount of uric acid which occurs when the disease is passing off. Liquor potassæ and colchicum together cause an increase, but this is perhaps due to the latter medicine.

7. *The Hippuric, Oxalic, and other Acids.*—No experiments were made.

8. *The Soluble Salts.*—The total amount of the soluble salts, determined by incineration and dissolving, was ascertained only on three occasions. In case 1, on the 4th day before treatment, 191·614 grains, which were made up almost entirely of sulphates and phosphates, were passed in 24 hours. On the 7th day (ʒv. of liq. pot.) 123·782 grains were passed, composed entirely of sulphates and phosphates. In Case 2, on the 8th day (ʒvj. of liq. pot.) 151·812 grains were passed, composed almost entirely of sulphates and phosphates.

The changes in the soluble salts will be best seen by considering the changes in the chlorine, sulphuric, and phosphoric acids.

(*a*) *Chlorine.* (Healthy average, 90 grains in 24 hours.) The diminution of the chlorides in pneumonia, and sometimes in pleurisy, has attracted much attention (Redtenbacher, Beale); it appears, however, to be equally common in rheumatic fever.

In Case 1, on the 4th day (no treatment), there was an extremely scanty precipitate given by the direct addition of nitrate of silver and nitric acid, to the urine; on the following day there was none whatever; on the succeeding day, traces; on the next day, none; and it was not, in fact, till the 9th day, that there was any quantity; it returned then from day to day; on the 12th day (the diet being only altered by the addition of a light pudding), 22·264 grains of chlorine were passed in 24 hours; and on the following day there was evidently a still greater quantity. That of the three possible factors, the withdrawal of food, the presence of the disease, and of the remedy, the absence of the chlorine was owing to the second, is evident from the fact that the chlorine began to return before the diet was changed; and that in this, and in the other cases, and also in health and in all cases of disease yet examined, the liquor potassæ has never been known to have the effect of diminishing the quantity of chlorine.

In Case 2, on the 7th day, only 4·665 grains of chlorine were excreted in 24 hours; on the following day there was still less; on the 18th day, when the quantity was next determined, nearly 60 grains were passed, although the diet had been only very slightly altered. So also in Case 4, on the 14th day (when the point was first examined, and when the articular symptoms were still severe), there was no chlorine, and it remained absent till about the 19th day, when the disease was passing off. In Case 3 no observation was made.

These facts show that in acute rheumatism there is in some cases (perhaps in all) a diminution in the amount of chlorine excreted during the height of the disease, greater than can be accounted for by the absence of food. It is also evident that liquor potassæ (in the doses above given) does not cause the reappearance of the chlorine.*

* It is of course an obvious suggestion, that the chlorides may in part pass off with the copious perspirations, which in all these cases were very great. Or they may be retained in the system, or even possibly be decomposed.

(*b.*) *Sulphuric acid* (healthy average, 30 grains in 24 hours (Vogel), 17 grains (Becquerel), 24 grains (Author.)

Case 1.

Before treatment (4th day)	52·668	grains.
5th and 6th days, in each 24 hours (ʒv. of liq. pot. on each) .	55·363	,,
7th, 8th, and 9th days (ʒv. of liq. pot. on each), symptoms much better	43·644	,,
10th and 11th days (no medicine; convalescence) . .	32·237	,,
12th and 13th days (no medicine; better diet) . .	24·147	,,

Case 2.

No observation before treatment. There were excreted in each 24 hours, of sulphuric acid, from the 5th to the 10th day (144 hours, ℥iij. ʒv. of liq. pot. being taken) . . .	56.449	grains.
From the 11th to the 18th, in each 24 hours (no medicine; symptoms better; not well)	39·903	,,
On the 19th day (ʒiv. of liq. pot., symptoms of rheumatism well)	44·555	,,
On the 29th and 30th days (no medicine; symptoms well; good diet)	34·164	,,
From the 31st to the 34th (℥j. ʒvj. of liq. pot. being given for experiment; the rheumatic symptoms being well) in each 24 hours	52·035	,,

Case 3.

On the 2nd and 3rd day, in each 24 hours (no medicine) .	32·267	grains.
On the 4th and 6th days (liq. pot. ʒiij. vin. sem. colch. ʒj.) .	40·281	,,
From the 8th to the 14th day, in each 24 hours (no medicine; convalescence)	23·67	,,

Case 4.—(*Female.*)

On 5th day (no medicine)	26·144	grains.
On 6th and 7th days (ʒv. of liq. pot. on each) . . .	37·625	,,
On 11th day (ʒiss. of liq. pot.; symptoms better) . . .	25·008	,,
On 23rd day (convalescence; no medicine)	6·412	,,
On 45th day (good diet)	5·40	,,

Conclusion.—In rheumatic fever the sulphuric acid is greatly augmented; it falls to the normal amount or below it, when the severe symptoms have ceased.* Liquor potassæ increases the amount, and delays, but does not entirely prevent, the fall which occurs during commencing convalescence. The same effect is produced by potash and colchicum.

(*c.*) *Phosphoric Acid.*—(Healthy average, 55 grains. Gruner.) The phosphoric acid was not determined before treatment, and only on three occasions afterwards. In Case 1, on the 7th day (liq. pot. ʒv.) 24·613 grains were passed; on the 12th day (no medicine, better diet), 22·264 grains. On the 13th day (no medicine), 8·73 grains.

Conclusion.—The facts are not sufficient to warrant any definite opinion, but from the low figure of the phosphoric acid on the 13th day, the 24 grains passed on the 7th day (when no food was taken) are, perhaps, an excess of what would have been furnished by the tissue metamorphosis alone, of the healthy system. The excess might be due to the disease or to the remedy.

9. *Insoluble Salts.*—No observations were made.

10. *Foreign Matters.*—Albumen was present in small amount in two

* From Case 5 it was ascertained that the sulphuric acid was in as great a quantity in the urina sanguinis as in the urina cibi; the amount of food was, however, small.

cases; in one before and during treatment; in one during treatment. In both cases it disappeared before the liquor potassæ was discontinued, and was manifestly unaffected by it.

11. *The Acidity, Deposit, &c.*—The urine was highly acid before and during treatment, but the exact amount was not ascertained; it was never alkaline. The colour became paler (independent of dilution) under the influence of the liquor potassæ. The deposit (amorphous, deep-coloured lithates) was not affected by the medicine except as far as the colour was concerned.

The following summary expresses in general terms the action of liquor potassæ on the urine of 24 hours:

Urinary Constituents.	Condition In Rheumatic Fever.	Effect produced by Liquor Potassæ in large doses.
Water	Greatly diminished	Slightly increased; (over rheumatic average.)
Solids	Increased	Still more increased.
Urea	Probably increased	Probably increased.
Uric Acid	Increased	If affected at all, slightly increased.
Creatine, &c.	Undetermined.	
Sulphur containing comp.	In considerable quantity	Probably increased.
Chlorine	Diminished	Unaffected.
Sulphuric Acid	Greatly increased	Still more increased.
Phosphoric Acid	In some quantity	Uncertain.
Bases	Undetermined.	
Insoluble Salts	Undetermined.	

The urine in rheumatic fever appears from these cases to have peculiarities which distinguish it from the urine of other fevers. It resembles the typical febrile urine in its deficiency of water, in its depth of colour, in its great per-centage of solids, and in the rapid deposit of dark urates. But apart from possible differences connected with the urea, and extractives, it differs from the febrile urine of pneumonia and the specific fevers in the greater amount of the absolute excretion of solids (i.e., in 24 hours) and in the enormous excess of sulphur, and its derivatives. The excretion of sulphuric acid is far greater than in any other febrile disease which I have examined,—viz., than in small-pox, typhus and typhoid fever, scarlatina, erysipelas, pyæmia (with purulent arthritis), pleurisy, and pneumonia. In these cases the sulphuric acid has been also in excess of that which would have been formed during healthy tissue metamorphosis, except in some cases of pneumonia in which the sulphuric acid (in common with other urinary ingredients) has been retained in the system during hepatization, and been poured out afterwards during resolution.

The excess of sulphuric acid in the rheumatic urine is not due to an excess of febrile action in this disease, over the other fevers just enumerated. In cases of typhoid fever and scarlatina, the temperature has been higher than in rheumatic fever; and yet the amount of sulphuric acid passed in 24 hours has not reached to half the quantity.

The sulphuric acid is not, then, in any close proportion to the temperature. As the temperature is usually considered to be a correct indication of the rapidity of tissue-metamorphosis in febrile diseases, it follows that rheumatic fever is an exception to the rule, and that the sulphuric acid is in excess of what would have been predicated from the amount of fever.

It appears, therefore, a fair inference that in rheumatic fever there is a source of sulphuric acid, independent of the augmented disintegration of tissues, as measured by the heightened temperature, and it may perhaps be conjectured that chemical analysis will hereafter demonstrate the existence in the blood of some compound richer in sulphur than fibrine and albumen, which during the height of the disease is rapidly disintegrating, and forming, probably among other products, sulphuric acid.

The effect of liquor potassæ, of the bicarbonate, and perhaps of the other alkalies, is at once to aid this disintegration, and to increase the elimination of sulphuric acid, by augmenting the alkalinity of the blood. If this hypothesis be correct, the administration of alkalies in rheumatic fever would acquire a basis more rational than that usually assigned—viz., that they merely neutralize acid already formed.

With respect to the efficacy of liquor potassæ in rheumatic fever, although the cases are so few in number, yet as it is unlikely that additional cases can be treated so rigidly without other medicines, and as in fact other remedies (mercurial purgatives, colchicum, opium, hot air bath, &c.) ought to be employed, I may mention the general conclusions which may be deduced from these four cases.

No symptom was immediately affected, except the pulse; this was generally, but not always lowered, and sank, although the temperature continued high, to 80, 70, and even lower. The febrile heat (as measured in the mouth), the articular pains, and the perspirations, were not affected, except in so far that the duration of the disease was shortened. The effect on cardiac complication was uncertain; in one case pericarditis came on, but there was a strong suspicion that it had actually begun before the medicine was commenced; in two cases basic systolic murmurs appeared, in one case before, in the other (a man), decidedly after the potash was commenced. This murmur, however, disappeared during convalescence.

The duration of the disease was in three cases short, although the severity of the early symptoms led to the belief that it would be obstinate and long continued. The first case lasted scarcely a week; the second, 18 days; and the third (relapse) about 7 days. In the 4th case (old and recent heart-complication, pericarditis) the duration was greater, and the patient was not convalescent till the 23rd day. The average of the whole was 13·75 days from the first symptom; but as an average of 3·25 days occurred before treatment, the actual period from commencement of treatment to perfect freedom from joint-pain and fever, was 10½ days. This result (if it occurred in all cases) would certainly be favourable, but it is well known that other observers have obtained equally fortunate results from very different treatment; so that the superiority of the treatment by liquor potassæ, *per se*, cannot be held to be sufficiently proved.

A great disadvantage in liquor potassæ is its nauseous taste, and frequently also, after a time, if it be given in large doses (and this is necessary), the stomach does not tolerate it well.

PART FOURTH.

Chronicle of Medical Science.*

ANNALS OF PHYSIOLOGY.†

By Hermann Weber, M.D.,
Physician to the German Hospital.

I.—Food and Digestion.

1. *The Ligature of the Pancreatic Duct.* By Professor Herbert, (Zeitschrift für rat. Med., von Henle und Pfeüffer. Vol. iii. Heft 3, p. 389.)
2. *The Amount of Nitrogenous and Non-Nitrogenous Elements in various (English) Dietaries.* By Dr. Beneke, (Archiv für phys. Heilkunde von Vierordt. 1853. Heft 3, p. 409.)

1. Herbert made his experiments (tying the pancreatic duct) principally for the purpose of proving the incorrectness of the observation of Bernard, corroborated

* This portion of the Review is occupied principally with the abstracts of Journal-articles and of short treatises. In each Quarterly Report on Medicine, Surgery, Midwifery, and Medical Jurisprudence, the journals received during three months will be analysed; and most, if not all, the papers in the Foreign periodicals will receive some notice. The British journals are so universally in the hands of our readers, that it is scarcely necessary to do more than glean from them here and there a paper of peculiar interest. The Report on Midwifery will not be commenced till April. Physiology, Micrology, Chemistry, and Materia Medica are discussed in half-yearly and yearly reports.

The space which can be allotted to these Reports is so small, compared with the amount of matter to be analysed, that no pretensions can be made to anything like a complete record of each subject. A full and judicious selection is all that is aimed at. During the months of September, October, and November, 1853, in addition to the British journals, the following periodicals were received:—

GERMAN.

1. Archiv für path. Anat. von R. Virchow. Band v. Heft 4, and Band vi. Heft 1.
2. Archiv des Vereins für gemeinschaftliche Arbeiten; Band i. Heft 1 and 2.
3. Archiv für phys. Heilkunde von Vierordt. 1853. Heft 3.
4. Archiv für phys. und path. Chemie, von F. Heller. N. F. Band ii. Heft 6.
5. Archiv für Anat. und Phys. von J. Müller. 1853. Heft 3 and 4.
6. Zeitschrft für rat. Med. von Henle und Pfeüffer. Band iii. Heft 3.
7. Zeitschrift (Henke's) für die Staatstarneikunde, von Behrend. 1853. Heft 3.
8. Zeitschrift der K. K. Gesells. der Aertze zu Wien, von Helra. (August and September.)
9. Prag Vierteljährschrift für Praktische Heilkunde. 1853. Band iii. and iv.
10. Verhandl. der Phys. Med. Gesell. der Aertze zu Wurzburg. Band iv. Heft 1.
11. Schmidt's Jahrbucher der Ges. Med. 1853. No. 9, 10, and 11.

FRENCH.

12. Archives Générales de Médecine. 1853. September, October, and November.
13. L'Union Médicale. September, October, and November.

ITALIAN.

14. Bulletino delle Scienze Mediche. Bologna. June, 1853.

AMERICAN.

15. The American Journal of the Medical Sciences. October. 1853.
16. The Philadelphia Medical Examiner. September, October. 1853.

† The Annals of Physiology were commenced in July, 1853, by Mr. Gray, but indisposition has unfortunately compelled that gentleman to discontinue their preparation.

by the commission of the Academy of Paris (Magendie, Dumas, Milne-Edwards*) concerning the function of the pancreatic juice. Bernard ascribed to it, as is well known, the property of emulsifying the fatty matter, and considered as its chief action, the power of effecting such a change in the oleaginous constituents of the chyme as to make them fit for absorption. Herbert tied the pancreatic duct in two rabbits, after they had been starved for several days; on the day after the operation, the rabbits were fed with milk and bread a few hours before they were killed. At the post-mortem, the thoracic duct was found to contain a considerable quantity of a bluish-white fluid; the lymphatic vessels of the mesentery were completely filled, and of a milky colour. The same result was obtained by feeding a rabbit after the operation with roasted bacon, potatoes, and water. The further observation of Bernard and the French commission—that the lymphatic vessels of the mesentery do not exhibit the milk-white colour above the entrance of the pancreatic duct into the duodenum, that they commence to do so only two centimeters below this (0·787 inches)—is likewise not corroborated by Herbert; the latter having always found the milky colour of the vessels half an inch above the opening of the duct. The conclusions of Lenz and others are therefore confirmed.

2. Dr. Beneke has written a very important paper on the amount of food allowed in various institutions in London, and has based some interesting calculations on these data. He first gives an account of each institution, to show its object and the class of individuals residing in it. This we pass over, as containing only familiar matter, and proceed to the tables, which are so valuable that we shall give them entire.

1. Educational and Invalid Establishment.

Amount of Food consumed by each Person weekly, in ounces.

Place.	Age.	Sex.	Meat.	Bread.	Potatoes.	Fresh Greens.	Sugar.	Flour.	Fat.	Milk.	Tea.	Coffee.	Cocoa.	Beer.	Cheese.	Rice.	Occupation, &c.
1. Royal Military Asylum, Chelsea	5–14	M	44	105	26	...	4	6	1	105	...	...	3½	...	...	5	8½ hours sleep, 6 hours school.
2. Royal Hospital, Chelsea	Mean 68	M	60	112	112	...	8¾	7	4	30	1¼	...	5¼	140	8	...	Ad libitum.
3. Royal Hospital, Greenwich	Mean 70	M	58	112	70	7	8	14	12	90	1½	...	6	280	...	...	Ad libitum.
4. Royal Navigation School, Greenwich	11–18	M	44	105	42	6½	8¾	6	5½	40	1¼	...	3½	...	...	...	9 hours sleep, 6 hours school.
5. The Victory, Portsmouth	14–60	M	74	112	56	...	12	7	...	...	1½	...	7	...	...	...	7 hours sleep.
6. Christ's Hospital, London	7–15	M	20	108	40	...	...	12	3½	70	...	...	...	70	12	...	10 hours sleep, 7 hours school.
7. Refuge, Dalston	11–32	F	48	112	84	...	8	16	8	70	1	2	...	...	7	...	8½ hours sleep.
8. London Orphan Asylum	7–15	M&F	33	105	36	...	1½	16	4½	70	...	...	...	...	7	...	9-10 hours sleep 6½ school.
9. Hackney Workhouse																	
1.	15–60	M	15	96	26	...	3½	24	7½	...	...	...	2	...	2	...	Various.
2.	15–60	M	10	100	24	...	...	34	4	...	...	...	...	...	3	...	"
1.	15–60	F	15	82	36	...	3½	24	7½	...	...	...	2	...	2	...	"
2.	15–60	F	10	86	24	...	...	34	4	...	...	...	...	...	3	...	"
1. 2.	7–15	M&F	As the women.			...	...	...	...	...	...	...	...	...	...	...	"

* Comptes Rendus hebdom. des Séances de l'Académie des Sciences. Tom. xxvii., p. 249, à p. 288.

2. Prisons.

Place.	Age.	Sex.	Meat.	Bread.	Potatoes.	Greens.	Sugar.	Flour.	Fat.	Milk.	Tea.	Coffee.	Cocoa.	Beer.	Cheese.	Rice.	Occupation, &c.
10. Pentonville Prison	16–60	M	28	140	102	...	10	10½	...	14	...	...	5¼	...	...	...	8 hours sleep.
11. Bridewell Prison																	
Over 2 months .	16–60	M	24	140	32	...	8	10½	...	14	...	...	7	...	...	14	10 hours sleep.
Under 2 months	16–60	M	12	140	16	...	...	19½	...	...	...	...	...	...	...	7	"
Over 2 months .	16–60	F	18	140	...	...	8	12	...	14	...	...	7	...	...	9	"
Under 2 months	16–60	F	12	112	...	...	...	20	...	...	...	...	...	...	...	6	"
Over 2 months .	7–16	M & F	12	112	16	...	...	23	...	...	...	...	...	...	...	6	"
Under 2 months	7–16	M & F	6	112	...	...	...	23	...	...	...	...	...	...	...	3	"
12. Stirling Castle, Portsmouth .	14–60	M	38	161	112	...	...	10½	...	...	...	...	5¼	...	...	...	8½ hours sleep.
13. Norwich Castle. .	14–60	M	...	...	...	...	...	...	...	...	...	...	...	...	...	...	8 hours sleep.
Third Class . .	...	...	6	168	70	2	...	21	...	...	...	...	...	...	...	2	2–6 weeks hard labour.
Fourth Class . .	...	...	12	168	40	3	...	21	...	...	...	...	...	...	...	3	6–12 weeks hard labour.
Fifth Class . .	...	...	10	126	121	3	2¼	16½	...	...	...	...	2½	...	...	3	Over 3 months hard labour.
14. Millbank Prison .	14–70	M	31	154	118	...	7	14	...	14	...	...	1½	...	...	2	9 hours sleep.
	14–70	F	26	140	62	...	7	14	...	14	...	...	1½	...	...	2	

3. Hospitals.

Place.	Age.	Sex.	Meat.	Bread.	Potatoes.	Greens.	Sugar.	Flour.	Fat.	Milk.	Tea.	Coffee.	Cocoa.	Beer.	Cheese.	Rice.	Occupation, &c.
15. Hanwell Lunatic Asylum . . .	15–70	M	21	96	60	10	3½	16	3	20	...	...	3½	140	14	3	Various.
	15–70	F	21	80	60	10	3½	16	6½	20	1¼	...	3½	70	...	3	"
16. Middlesex Hospital	...	M & F	28	84	56	...	...	10½	...	70	...	...	...	...	...	...	Convalescents.
17. Bartholomew's Hospital . .	...	M & F	28	84	56	...	...	7	5¼	70	...	...	...	140	...	...	"
18. Hospital for Consumption . .	...	M & F	28	84	37	...	...	1	...	96	...	...	7	...	...	...	"
19. St. George's Hospital	...	M & F	42	84	56	...	...	10½	7	70	...	...	...	140	...	...	"
20. Westminster Hospital	...	M & F	56	98	84	...	...	21	...	70	...	...	...	...	...	...	"
21. German Hospital, Dalston . . .	...	M & F	28	84	56	...	...	10½	7	140	...	...	...	...	...	...	"
22. Woolwich (Military) Hospital .	...	M & F	38	84	112	...	3½	16	...	70	...	...	...	...	...	5¼	"
23. Royal Sea-Bathing Infirmary .	10–40	M & F	52	120	52	...	...	10	3	80	...	...	2	280	...	9	"
	4–10	M & F	38	96	52	...	...	10	2	80	...	...	2	140	...	9	"
24. Metropolitan Establishment, Margate . .	5–10	M & F	20	70	40	...	...	...	4	70	...	...	...	25	...	12	"
	10–16	M & F	30	98	80	...	7	24	6	35	2	...	...	50	...	...	"
25. Chateau Bellevue, Margate . . .	16–40	M & F	41	98	80	...	3½	10	6	70	1½	...	...	140	...	3	"

Dr. Beneke then proceeds to calculate the various amounts of water in these articles of food. He uses for this purpose the data given by Mülder, Liebig, and Frerichs, for meat; those of Liebig, Horsford, and Krocker, for bread; those of Mülder, Horsford, and Krocker for potatoes, &c. In several cases (in bread, for example) he has also ascertained by direct experiment, that the quantity of water mentioned by these authorities was the same in the English food. He then reckons the relative quantity of nitrogenous and non-nitrogenous food in the water-free substances. Fat he reckons, and chiefly from Liebig's data, as equal to 2·4 parts of starch. The bread is reckoned as containing in every pound $\frac{7}{8}$ of a pound of flour (Liebig). The following table shows at once the data on which he bases the calculations:

Food.	Nitrogenous Proportion.	Non-nitrogenous Proportion. (Reckoned as Starch.)	Water.
Meat	1	1·7	70 per cent.
Wheat-flour	1	4·6	14 " "
Potatoes	1	8·6	75 " "
Sugar	0	1	— " "
Fat	0	2·4	— " "
Milk	1	3	87 " "
Cheese	1	2·59	32 " "
Rice	1	13·7	15 " "

The small quantities of tea, cocoa, and beer are disregarded. Calculating then from these data, Dr. Beneke finds the following weekly consumption of nitrogenous and non-nitrogenous food in these various institutions; the amounts are expressed in ounces avoirdupois:

Place.	Nitrogenous.	Non-nitrogenous. (Reckoned as Starch.)	Proportion of Nitrogenous to Non-nitrogenous.
1 Royal Military Asylum	22·80 ozs.	96·80 ozs.	As 1 is to 4·2
2. Royal Hospital at Chelsea	26·42 "	127·91 "	" 1 " 4·8
3. Royal Hospital, Greenwich	26·58 "	143·28 "	" 1 " 5·4
4. Royal Navigation School, Greenwich	20·80 "	105·69 "	" 1 " 5·08
5. The Victory, Portsmouth	24·06 "	104·78 "	" 1 " 1·43
6. Christ's Hospital, London	22·53 "	101·66 "	" 1 " 1·45
7. Refuge, Dalston	26·83 "	146·04 "	" 1 " 1·54
8. London Orphan Asylum	23·20 "	106·15 "	" 1 " 1·46
9. Hackney Workhouse, I. a	18·14 "	103·37 "	" 1 " 5·7
" " b	19·48 "	97·45 "	" 1 " 5·00
" " II. a	16·45 "	95·58 "	" 1 " 5·8
" " b	17·81 "	89·78 "	" 1 " 5·04
10. Pentonville Prison	24·56 "	123·91 "	" 1 " 5·04
11. Bridewell Prison	23·06 "	116·61 "	" 1 " 5·06
" "	21·87 "	102·14 "	" 1 " 4·67
" "	21·53 "	105·30 "	" 1 " 4·9
" "	18·04 "	82·51 "	" 1 " 4·57
" "	18·89 "	88·22 "	" 1 " 4·65
" "	17·65 "	81·13 "	" 1 " 4·60
12. Sterling Castle, Portsmouth	28·03 "	128·41 "	" 1 " 4·50
13. Norwich Castle	25·95 "	125·80 "	" 1 " 5·09

Places.	Nitrogenous.	Non-nitrogenous. (Reckoned as Starch.)	Proportion of Nitrogenous to Non-nitrogenous.
13. Norwich Castle	25·90 „	121·10 „	„ 1 „ 4·9
„ „	22·73 „	115·89 „	„ 1 „ 4·8
14. Millbank Prison	27·66 „	136·72 „	„ 1 „ 4·8
„ „	23·97 „	115·64 „	„ 1 „ 5·6
15. Hanwell Lunatic Asylum . . .	21·32 „	103·63 „	„ 1 „ 4·8
„ „ „ . . .	17·51 „	97·59 „	„ 1 „ 5·6
16. Middlesex Hospital	18·51 „	78·40 „	„ 1 „ 4·23
17. Bartholomew's Hospital . . .	17·98 „	88·52 „	„ 1 „ 4·9
18. Hospital for Consumption . . .	17·40 „	69·97 „	„ 1 „ 4·02
19. St. George's Hospital	20·07 „	97·83 „	„ 1 „ 4·8
20. Westminster Hospital	25·55 „	104·74 „	„ 1 „ 4·10
21. German Hospital	20·78 „	102·02 „	„ 1 „ 4·9
22. Woolwich Hospital	22·20 „	104·41 „	„ 1 „ 4·2
23. Royal Sea-Bathing Infirmary, Margate	26·23 „	116·80 „	„ 1 „ 4·5
„ „ „	21·77 „	98·57 „	„ 1 „ 4·52
„ „ „	14·59 „	77·21 „	„ 1 „ 5·3
24. Metropolitan Establishment, Margate	21·94 „	119·12 „	„ 1 „ 5·4
25. Château Bellevue, Margate . .	22·30 „	113·60 „	„ 1 „ 5·05

The proportions of nitrogenous and non-nitrogenous food are nearly the same. In the Hospital for Consumption there is a relative excess of nitrogenous substances, and the whole amount of food is small. Dr. Beneke believes that this diet is the most properly calculated, and is most to be recommended. In Hackney Workhouse the non-nitrogenous food is in relative excess, but even here, the amount of nitrogenous food is above the allowance of the Hospital for Consumption, and is probably too great.

The mean proportion may be cited as 1 to 5. The proportion given by Frerichs, as deduced from calculation, is 1 to 7. Frerichs reckoned that an adult man should consume in 24 hours, 2·17 ounces av. of nitrogenous, and 15·54 ounces of non-nitrogenous food; this would give weekly 15·19 ounces and 108·78 ounces, respectively. It will be seen that this theoretical calculation has ranked the quantity of the nitrogenous food too low, and the non-nitrogenous too high, if the practice of these institutions be taken to be the right rule. Is, however, the amount of nitrogenous food too great in these cases? It would appear also, from these tables, that the highest amount of nitrogenous food is not always commensurate with the quantity of meat allowed; the bread furnishes a large proportion of azotized aliment.

II. Respiration and Circulation.

1. *Mechanism of the Respiration and Circulation in a state of health and disease,* By F. C. Donders. (Zeitsch. für rat. Med. von Henle und Pfeüffer. 1853. Heft, iii. p. 287.)
2 *On the Irritability of the Ciliary Epithelium.* By Rudolph Virchow. (Archiv für Path. Anat. von Virchow. Vol. vi. p. 133.)
3. *Experimental Researches applied to Physiology and Pathology.* By Dr. Brown Sequard. (Philadelphian Medical Examiner, August, 1853.)

4. *On the Topography of the Temperature of the Body.* By Dr. Fük. (Müller's Archiv. 1853. Heft 3.)

1. The most important points elucidated by Dr. Donders' experiments are as follows:—

The force of the *elasticity of the lungs*, as seen by their contraction (erroneously called "collapse") soon after the thorax is laid open, has been measured by means of a manometer placed in the trachea. As the standard in healthy men, after a normal expiration, Donders adopts the number of 80 millimeters of water. (1 ^{mm}=0·03937 inch). This number is, of course, much increased by a deep inspiration, amounting even to 243 ^{mm} of water, or to 18 ^{mm} of mercury. During life to this force of the elasticity that of the *tonus* of the contractile fibres under the influence of the nervous system must be added. Donders calculates the latter to be equal to 20 ^{mm}, which would raise the power of resistance of the human lung to 100 ^{mm} of water (or $7\frac{1}{2}$ ^{mm} of mercury.) By the usual inspiration this power is increased to about 9 ^{mm} of mercury, and by the deepest inspiration to 30 ^{mm} mercury. This force must be overcome during inspiration; but it promotes, on the other hand, the act of expiration. Donders does not, however, agree with Kiwisch, who considered the lungs to be the only active organs during expiration. He attributes a considerable influence to the power of gravity on the walls of the thorax, to the elasticity of the latter, to the contraction of the abdominal muscles, and the increased tension of the gas in the intestinal tube pressing on the diaphragm. It is evident that in morbid states of the lungs (alteration of the elasticity, &c.), the result of these experiments must be different, and that the latter may serve as means for the diagnosis.

Concerning the influence exercised by the mechanism of respiration on *the circulation*, Donders calls particular attention to the fact, that even during the act of expiration, the heart and the great vessels in the cavity of the thorax are under a less considerable pressure than the vessels of the other parts of the body; that, therefore, the venous blood is constantly sucked into the cavity of the thorax. The difference of pressure amounts, after a normal expiration to 95 ^{mm} (mercury), after a normal inspiration to 9 ^{mm}, after a very deep inspiration to 30 ^{mm}. These numbers indicate the force by which the venous blood is sucked in after a usual expiration and inspiration, and after a very deep inspiration. The same force must be abstracted from that by which the arterial blood is propelled from the thorax through the contraction of the heart. If, however, during forced expiration, the expulsion of the air from the lungs is prevented, the pressure of the air in the lungs on the heart and thoracic vessels may become increased to more than that of one atmosphere, showing itself by redness of the face, swelling of the veins, &c. &c. Donders alludes to the influence exercised by this diminished pressure within the cavity of the chest on the circulation in the veins, especially those of the abdominal system. Concerning the circulation of the blood through *the lungs*, Donders again remarks, "that the blood in the principal pulmonary arteries and veins is always under a lower pressure than that within the capillaries of the lungs, however the mechanism of respiration may be modified during health or disease." Even during forced inspiration, when the principal thoracic vessels are under a higher pressure than that of one atmosphere, the pressure on the capillary system of the lungs is still greater; the blood is, therefore, always propelled towards the heart. Through this arrangement the circulation through the lungs may still remain normal, while that through the other part of the body is already much deranged by abdominal respiratory pressure.

2. While hitherto no agencies were known, either mechanical, or physical, or chemical, by which the motion of the ciliary epithelium could be excited; while Purkinge and Valentin,* in their manifold experiments, had even found that all the agencies usually exciting motion in the contractile tissues seemed to destroy the motion of the cilia, Virchow has lately detected that by a solution of

* *Purkinge et Valentin,* De phenomeno gen. et fund. motus vibratorii continui. Vratisl. 1835.

potassa ("kalilauge," the strength of the solution not mentioned), the motion of the ciliary epithelium of the human trachea is re-excited after it has completely ceased. The same effect is produced by a solution of soda, but not by that of ammonia, as the latter produces, at once, the decomposition of the cells. Virchow adds the remark, that by this observation we are entitled to consider the contractile substance of the cilia to be similar to that of Lehmann's *syntonin,* in the contractile tissue of the muscles. Though Virchow acknowledges this phenomenon to be produced by chemical action, yet he thinks that the contractile substance of the cilia exhibits also a motion peculiar to itself, and independent of the chemical "*corrosion.*" We are doubtful, however, whether the term "*irritability*" can be used until it is more fully proved that the phenomenon is not a merely chemical one.

3. Dr. Brown Sequard considers the *carbonic acid* contained in the blood to be *the cause of the beatings of the heart.* His theory is based principally on the following observations: 1. If warm-blooded animals are prevented from breathing, the beatings of the heart become more frequent for one or two minutes. 2. The hemadynamometer shows an increased energy of circulation during asphyxia. 3. All the causes which increase the formation of carbonic acid gas in the body increase the frequency of the beatings of the heart. The *rhythmical alternation* of contraction and relaxation is explained by the circumstance of the excitant cause not acting constantly with the same power. The small blood-vessels and capillaries being compressed during the muscular contraction, there is a diminution of excitation during that time; in consequence of the elasticity of the fibres, dilatation is produced as soon as the exciting cause is diminished. The fact of the heart being the only striated muscle with rhythmical movements, is explained by its possessing more blood in its capillaries (more stimulus), and having less resistance to overcome.

4. Professor L. Fük is occupied with observations and experiments on the temperature in various organs and on various localities of the body. The twelve experiments hitherto published are performed on living dogs. The temperature is taken in the carotid (right) vena jugularis, right and left ventricle of the heart, cavity of the thorax, rectum, vagina, urethra, and brain. We shall not now enter into details, as the author himself considers his experiments only as preliminary; but we must draw attention to the interesting and almost uniform result, that no difference was found in the temperature of the right and left ventricle of the heart, as generally supposed to be the case; further, that the highest degree of warmth was always met with in the vagina and rectum (101·75° to 105·79° F., i.e., 1·125° higher than in any other organ of the body); also in the rectum of men Fük found the temperature almost always higher than under the tongue (the mouth having been kept closed), the difference amounting in some cases to more than 3° F.

III.—Lymphatic System and Ductless Glands.

1. *On the Absorption of the Chyle from the Intestinal Tube.* By Prof. E. Bruecke.
2. *On the Origin and Course of the Lacteals in the Walls of the Intestinal Tube.* By the same author.
3. *On the Lacteals and Locomotion of the Chyle.* By the same author.
   (Extracts from the Transactions of the Royal Imperial Academy of Medicine of Vienna. 1853.)

Bruecke states, as the result of his observations and experiments (on man, swine, and other mammalia), that the cylindrical epithelial cells of the mucous membrane of the intestines, through which the chyle passes on its way to the lacteal vessels, do not, as it is generally supposed, consist of a closed cavity surrounded by a complete membrane, but that this cavity is isolated from that of the intestinal tube merely by a thin layer of a mucilaginous substance. Beneke asserts also that they possess a small opening on their opposite side, through which the molecules of fat pass into the interior of the villi. Within the villi he could not detect any vessels for chyle, but merely channels (without a membrane) formed by the interstices between the elements of the tissue (blood-vessels, muscular fibres, cellular tissue). As the mechanical means for the movement of the chyle, Bruecke considers, first, the muscular contractions of the intestinal tube by which the chyle is pressed into the villi, which are in a fit state for imbibition by being distended through the pressure of the blood in their blood-vessels; secondly, when the villi are filled, their muscular fibres contract, and the fluid they contain is pressed into the channels lying between the mucous and submucous membrane. At the same time, however, a part of the fluid contained within the epithelial cells is squeezed out again into the cavity of the intestinal tube. From the lacteals within the walls of the intestines the chyle is propelled by means of the muscular contractions of the tube into the vessels of the mesentery, from whence it is pumped up, so to say, by means of the respiratory actions, into the thoracic duct. The commencement of complete lacteal vessels was recognised (in the gut of a child) in the deeper part of the mucous membrane, exhibiting a diameter of one centimillimeter (0·0003937 inch); these were seen uniting to larger vessels, which, in the submucous cellular tissue, were furnished already with many valves, with an epithelial and also with a distinct muscular layer. In the smaller ramifications (less than two centimillimeters in diameter) no valves could be detected, nor an epithelium, and also no tunica propria could be distinguished from the surrounding cellular tissue forming the adventitia. In the mucous and submucous tissue the course of the lacteals appeared independent of that of the blood-vessels, but on their passage through the muscular layer every couple of blood-vessels (an artery and a vein) was accompanied on each side by a lacteal. The chyle did not appear to enter into the vessels merely from the villi, but also from between them, and principally from between the crypts of Lieberkühn, where it may be seen placed in the same manner as within the villi, i.e., lying freely in the interstices of the stroma. In the weasel the arrangement is similar to that in man; the lacteals, however, do not appear to be provided with valves before their entrance into the muscular layer. In rabbits a difference is observed—namely, in the whole of the wall of the intestinal tube the chyle does not appear to be conveyed in separate channels, but the adventitia of the blood-vessels, together with the continuation of that from the lacteals in the mesentery, form a sheath round the blood-vessels within the wall of the tube; in the interstices between this sheath and the blood-vessels the chyle is carried towards the origin of its own independent vessels in the mesentery. In the rabbit, therefore, during the passage through the whole of the intestinal wall, the blood is separated from the chyle merely by the thin membranes of its vessels, while in man such is the case only in the mucous membrane. Within the lymphatic glands of the mesentery the chyle is once more deprived of its own independent channels; the vasa *inferentia* lose themselves in the porous glandular tissues, from which they join anew to form the vasa *efferentia*. Without further entering here into the minute structure of the lymphatic glands as described by Bruecke, we remark only that he still maintains the opinion expressed in the 'Denkschriften der Wiener Academie' (1850, II. 23), that the lymphatic glands are the seat of the development of the lymph-globules, the germs of which enter into the circulation during the passage of the lymph and chylus through the glands. Amongst the lymphatic glands Beneke enumerates also the glandulæ agminatæ et solitariæ in the various parts of the intestinal tube, the glandular simplices majores (Boehm) in the colon, the tonsils, and some of the glands (the follicular) on the root of the tongue. About the spleen, the thymus, the suprarenal capsules, and the glandular thyroideæ, he refrains from giving an opinion for the present.

IV.—Secretion and Excretion.

1. *Some Experiments on the Secretion of the Urine.* By T. Kierulf, (Zeitschrift für ration. Medicin., von Henle und Pfeüffer.) 1853. H. 3, p. 279.

1. Dr. Kierulf has endeavoured to investigate the influence of considerable dilution of the blood on the quantity and quality of the urine. For this purpose he made the section of the left ureter in dogs (through a wound in the abdominal wall), and collected the urine by means of a glass-tube. On the first dog, the operation was performed between $8^h\ 55^m$ and $9^h\ 40^m$ A.M., with only a trifling loss of blood. At $9^h\ 45^m$, a small venesection (amount not stated); at $11^h\ 40^m$ injection of 495 (16 ounces) grammes of distilled water (temperature 104·00) through the jugular vein. Five minutes after this, the urine was mixed with a considerable quantity of blood; the quantity of urine much increased. At $11^h\ 55^m$, second venesection. At $4^h\ 9^m$ P.M., the urine contained still blood, but in a smaller proportion. At $4^h\ 50^m$, third venesection. At 5^h the urine excreted through the unwounded ureter was likewise bloody. On the following day the urine was normal. Similar experiments on another dog were followed by almost the same phenomena. In order to learn whether the admixture of blood to the urine was caused by the increased pressure of the blood, or by a change in its composition, Kierulf subjected the same dog to another series of experiments; 346 grammes of defibrinated blood (99·5° F.) just taken from another dog, were injected through the vena saphena at $9^h\ 25^m$ A.M. The urine excreted after this operation remained perfectly clear. At $9^h\ 50^m$, injection of 495 grammes of distilled water. (99·5° F.) Soon after this, increased quantity of urine, but no change of colour, and scarcely any albumen. At $11^h\ 30^m$, venesection to the amount of some ounces; at $11^h\ 35^m$, injection of 490 grammes of water. Soon after this, the colour of the urine deeper, decided reaction of albumen. At $11^h\ 50^m$ another small venesection, after which the reaction of the urine slightly alkaline, colour rather bloody, quantity of albumen increased. A new injection of 240 grammes at 12^h, was followed immediately by a considerable increase of the excretion of urine, with more admixture of blood and albumen. Concerning the composition of the blood detracted by the venesection, Kierulf remarks that, while the proportion of the solid substances appeared diminished, in consequence of the injection, that of the fixed mineral constituents (the ashes of the blood) was increased. Kierulf is inclined to consider the latter circumstance to be the cause of the blood-globules exhibiting an indented appearance under the microscope. The inferences which the author draws from his experiments are as follows:—1. Considerable dilution of the blood produces at first the passage of albumen through the kidneys. He is not inclined to attribute this phenomenon to an augmented pressure of the blood. 2. The normal solid constituents of the urine become decreased, while the proportion of salts appears greater than in the normal state. 3. The quantity of urine excreted within a certain space of time does not correspond to the proportion of water in the blood. Kierulf alludes to the light which may be thrown by similar experiments on the pathology of Bright's disease, but feels himself the necessity of further observations.

V.—Nervous System.

1. *On Certain Functions of the Spinal Chord.* By T. Lockhart Clark, Esq. (Proceedings of the Royal Society.) 1853. P. 297.
2. *On the Insensible Spot of the Retina in the Human Eye.* By Dr. Fick and Du Bois-Reymond. (Müller's Archiv. Heft 3, p. 396.)

The principal results of Mr. Clark's investigations, made on the ox, calf, cat, rat, mouse, and frog, are drawn as follows:—1. That the posterior roots of the spinal nerves consist of three kinds; two of them entering the posterior gray substance at right angles, the third kind, with different degrees of obliquity, tending upwards, a small proportion only of the latter taking a longitudinal course, and becoming lost in the posterior white columns. 2. That in no instance were any fibres of the anterior roots seen to ascend with the anterior white columns, before they had entered the gray substance. 3. That besides the transverse bundles forming the anterior roots, a continuous system of exceedingly fine transverse fibres issue from the anterior gray substance, and become lost as they proceed towards the surface of the cord. 4. That from the preceding facts, it may be inferred that nearly all, if not the whole of the fibres composing the roots of the spinal nerves, proceed at once to the gray substance of the cord; and that, if any of them ascend directly to the brain, it must be *those only* of the *posterior* roots which run longitudinally in the posterior white columns. 5. That the communication between the sensorium and the spinal nerves is not established by the posterior white columns, but by the antero-lateral columns, especially the lateral. 6. That many of the fibres belonging respectively to the anterior and posterior roots in different regions of the cord, terminate there by forming with each other a series of loops of various sizes and lengths; and that it is not improbable that some of them may reach even as far as the brain. It is not perfectly denied by the author that a portion of the roots may be connected with the vesicles of the cords, but he considers the evidence of any such connexion as very unsatisfactory. 7. The fine longitudinal fibres described by Stilling have not been found by the author. He is inclined to believe that the gray substance of the cord does not transmit impressions to and from the brain. 8. That there is great correspondence in the fibrous arrangement between the gray substance of the cord and the chiasma of the optic nerves. The author further remarks that the circumstance of the nerve-roots diverging upwards in the cord and intricately intermingling with each other, may explain why impressions made at one particular spot are communicated to distant parts of the cord, so as to excite simultaneous and sympathetic actions in classes of muscles which otherwise would appear unconnected.

2. Dr. Fick and P. du Bois-Reymond differ from Volkmann* in the conception of the influence exercised by the insensible spot of the retina on the perception of the image by the mind. They adopt the view that the impressions of light thrown on the insensible spot are not conveyed to the sensorium, while the impressions thrown on the other parts of the retina are perceived by the sensorium. It is well known that Volkmann and other psychologists (Waitz), are of opinion that the soul forms the idea of the dimension of objects by composing the impressions from the various parts of the retina mosaic-like, and that the piece of the object from which the rays are thrown on the insensible spot (which piece may be called "the unseen space"), falls out from the conception. The image, therefore, would be constructed smaller than the real object. Volkmann draws from this the inference that a line, the image of which is passing through the unseen spot, must be perceived as much shorter as is just proportionate to that piece of the image which is thrown on the unseen space. In opposition to this theory, the authors maintain the view that the soul possesses in itself a notion of space and dimension, and that it fills up the unseen piece of the object by a kind of *deception* according to certain laws. As one of the principal laws, may be regarded that the quality of the perceptions which the soul fancies to derive from the unseen space depends on the quality of those coming from the immediate neighbourhood. We propose to quote some experiments which the authors adduce in corroboration of their theory. If a black stripe be placed on a white surface in such a manner as to make its image pass through the blind spot and exceed it as well above as below, the stripe is seen *unshortened*, exactly as if its image had been thrown on another part of the retina sensible throughout the whole of its dimension (provided the stripe be not too narrow). If a part of the black stripe is cut out from its central portion and the image of the empty (white) middle space is thrown exactly on the blind spot of

* Wagner's Handwoerterbuch. Art. 'Sehen.'

the retina, while the upper and lower piece remain as in the former experiment (i. e., throwing their image just below and above the blind spot),—the black stripe is seen or fancied *unshortened and entire*, as if the central piece were not cut out. If the image of the stripe is thrown on the retina in such a manner that one *end* of it falls on the blind spot, the *stripe* is perceived by the soul *shorter* than it is in reality, and this shortening is proportionate to that piece of the image which falls on the blind spot, the latter being filled up by the soul with the white colour of the surrounding ground. The authors do not proffer a new opinion about the cause of the insensibility of the blind spot, but on account of the extension and form of the unseen space they are not inclined to adopt the view of its being caused by the arteria centralis retinæ.

VI.—Locomotive Organs.

Experimental Researches applied to Physiology and Pathology. By Dr. Brown-Séquard, of Paris. ('Medic. Examiner,' &c. Philadelphia, 1853. August.)

Dr. Brown-Séquard has occupied himself with the study of the nature of the movements of the *contractile tissues* (the muscles of the trunks and limbs, the muscular layer of the digestive canal, the iris, the uterus, the dartos, &c. &c.), which appear to be neither the result of an external excitation nor of an excitation produced by the nervous system. Brown-Séquard applies to these contractions the term spontaneous *contractions*, and considers as one of their causes, if not their only cause, the *carbonic acid* of the blood. This theory is based on the following circumstances:—1. After the section of one of the *facial nerves*, and after the peripheric part has lost its vital property, Brown-Séquard and Dr. Martin-Magron have observed in rabbits *contraction* of the *paralysed* side (so that the mouth deviated to the paralysed side). Whenever the respiration was in some manner prevented or disturbed, they observed the paralytic muscles to tremble, and sometimes even to undergo rhythmical contractions and relaxations. Though in dogs, cats and guinea-pigs, no deviation took place, yet convulsive (sometimes rhythmical) contractions were excited in the paralysed side, whenever the animals were prevented from breathing freely.—2. The spontaneous rhythmical or irregular contractions *in muscles of animal life, after death*.—3. The *deviation of limbs* produced by the contraction of the *paralysed muscles*.—4. The rhythmical movements in the *eye* of the *ink-fish* (Loligo Sepia, L.) after it has been separated from the body.—5. The spontaneous contractions of the *uterus* at a time when the spinal cord has entirely lost, not only its reflex power, but also the power of acting on muscles when directly excited by galvanism, warmth, or mechanical stimuli.—6. The spontaneous rhythmical movements in the *crop* and *œsophagus* of *pigeons* and other birds.—7. The spontaneous movements in the *limbs* of persons who have died of *cholera*.—8. The spontaneous contractions of the *bowels*, the *bladder*, the *iris*, and other *contractile parts* of the body at the time of death, and soon after death.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

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I.—THE ACUTE SPECIFIC DISEASES.

1. *The Indian Plague and the Black Death.* By Dr. HIRSCH. (Virchow's Archiv für path. Anat., Band v. Heft 4. § 508.)
2. *Abdominal Typhus and Cholera Typhoid.* By Professor VIRCHOW. (Verhandl. der Gesell., in Würzburg. Band iv. Heft 1. § 77.)
3. *The Urine in Typhus and Typhoid Fever.* By Dr. G. W. EDWARDS. (Edin. Monthly Journal. Sept.)
4. *The Irish Fever of* 1847. By Dr. PUREFOY. (Dublin Quarterly Journal. Nov.)
5. *The Blood in Cholera.* By Dr. ROBERTSON. (Edin. Monthly Journal. Sept.)

1. UNDER the title of 'The Indian Pest and the Black Death,' Dr. HIRSCH has published an historico-pathological sketch of the disease termed in India the Pali Plague, and of the affections noted in times gone by which are more or less closely allied to it. In his account of the 'Pali Plague,' the author refers especially to the following documents: Papers by Messrs. Maclean, Irvine, Glen, and Panton, in the 'Calcutta Quarterly Journal of Medical and Physical Science,' for 1837; papers by Whyte, M'Adam, Gilder, and Forbes, in the 'Bombay Transactions' for 1838 and 1839; Ranken's 'Report of the Pali Plague,' and Webb's 'Pathologica Indica.' The Pali Plague (so named from its commencement, in 1836, at Taiwali, near Pali in Marwar) was noted first in 1815, in Kutch and Guzerrat; it prevailed epidemically till 1821 in the adjoining countries, and then ceased. It recommenced in 1836 in Marwar, and extended itself for three or four years, when it ceased to be heard of. In 1850 a new outbreak occurred in Gurhwal and Kamoon ('London Medical Gazette,' vol. xi. p. 349), but no further intelligence of this has reached Europe. The symptoms of this disease closely resembled those of the Levant or Bubo plague: shivering, heat, pains in head, back, and limbs, great loss of strength, quickened pulse, hot and dry skin, intolerance of light, redness of the conjunctiva and of the face generally; then followed vomiting of bilious or coffeeground-like matters; the abdomen became hard, distended, and constipated, except towards the end of the case, when there was sometimes bloody diarrhœa; there was intense thirst; the urine was scanty and high-coloured; sopor and delirium were usual symptoms. On the second or third day appeared the distinctive marks of the disease—viz., either buboes, or a peculiar lung affection, or both. The lung affection consisted in severe sternal pain, dyspnœa, cough, with expectoration of either almost pure blood or of mucus deeply tinged with blood. The buboes appeared chiefly in the left groin, seldom in the axillæ. In fatal cases all the symptoms increased in severity, and death occurred on the third day. If the patient lived beyond the fourth day recovery was common; in this case the buboes either rapidly suppurated or became hard, and then gradually retrogressed. The occasional separation of the glandular and lung affection led to two chief varieties being distinguished, of which the lung affection was by far the most fatal. The total mortality was as high as 75·80 per cent. of those attacked.

The author believes (although there are no records of accurate physical investigations of the lung, or of post-mortem examinations) that the lung disease consists not in an exudative pneumonia, but in hyperæmia of the bronchial mucous membrane and hypostasis of the lung parenchyma. In this disease the author sees "a modified bubo plague." He denies its spread by contagion, and refers to Allan

Webb to show that it is endemic in some districts (Gurhwal), and occasionally becomes epidemic, and therefore that it was not derived from the Levant, as some of the Indian observers suggested. He then proceeds to trace its connexion with former pestilences. He refers to the latest accounts of the Egyptian epidemics (Robertson 'Edin. Med. and Surg. Jour.,' 1844; Pruner, 'Krankh. des Orients;' Delory, 'Rapport sur la Peste,' in the 'Rapport à l'Acad. Royale de Méd.,' by M. Prus), to show that in this disease there is no account of any lung affection similar to that of the Pali Plague. He then goes back to the earlier Egyptian epidemics (as recorded by Clot Bey, Bulard, Aubert, Iken, &c.), and to the reports of the Russian physicians on the plague at Odessa in 1827, and then to still earlier accounts (Orræus, Russell, &c.), and can find in none of them any description of pulmonary disease which accords with that given by the Indian observers. Only in one or two German writers of the beginning of the last century, and in one passage in Diemerbroeck ('De Peste,' libri iv. Arenaii, 1646) he finds brief references to cough and bloody sputa as a very infrequent occurrence.

When, however, the author passes further back to the account of the "Black Death" of the fourteenth century, he finds the description of this disease accord with that of the Indian Pali Plague, and he agrees with Hæser, that the "Black Death" was a modified bubo plague, with a peculiar, and hitherto unknown, pulmonary complication. He therefore quotes with approbation the conclusion of Allan Webb, that the "Pali Plague exactly resembles the Black Death."

Finally, the author demands whether, in the thirteenth century, the Black Death did not spread from India, just as in these days we see the Asiatic cholera taking its rise in the same districts, and passing apparently somewhat in the same way over the face of the earth.

2. Professor Virchow relates, at great length, a case of typhoid fever in which some of the symptoms during life, and some of the post-mortem appearances, had a choleraic character. The patient (a woman aged thirty-three) was attacked suddenly with shivering and headache, followed by persistent profuse serous diarrhœa and repeated vomiting. On admission on the fourth day the temperature of the skin was 99·5° Fah.; the pulse 95, small; there was no tenderness of the abdomen; the diarrhœa and vomiting continued. On the four following days the same symptoms; the temperature varied between 99° and 100°; on the following day it rose to 101°. On the tenth day there was some collapse; the diarrhœa and vomiting were less; on the 11th "little rosy red spots" appeared on the abdomen and breast. On the following day there was rather unexpected and sudden death. The condition of the urine is only once noted, two days before death; it was then non-albuminous. On post-mortem examination, in addition to other things, there was unequivocal typhoid exudation into many, and ulceration of one, of the glandulæ agminatæ, and infiltration of the mesenteric glands.

Virchow remarks that this was undoubtedly a case of typhoid fever, but he observes that in the section a series of appearances presented themselves which are found in cholera; there was a cyanotic condition of many peripheral parts; a rosy hyperæmia of the small intestines (which Pirogoff and Virchow consider so characteristic of cholera), and an extensive desquamation of the epithelium, of the intestines, stomach, and gall-bladder; there was also the parenchymatous change in the kidney which Virchow has formerly described as frequent in cholera-typhoid.

During life also the case was noted for the slightness of the febrile and nervous symptoms, and for the extraordinary severity of the serous catarrh of the gastro-intestinal mucous membrane. Altogether, Virchow believes that the resemblances to cholera were sufficiently striking, yet in the place where this case occurred (Würzburg) no cholera has ever yet been seen, and he contents himself, therefore, with directing the attention of pathologists to unusual cases of this kind, with a view to determine whether a combination of processes, so different as the febrile and choleraic conditions appear to be, may not be possible.

[We cannot quite understand the importance which the author attaches to this case. The choleraic symptoms scarcely appear sufficiently marked; the vomiting and purging, *per se,* and the so-called choleraic after-death appearances are scarcely sufficient, we think, to warrant the inference drawn from them.]

3. Dr. G. W. Edwards has published an interesting paper on the urine in typhus and typhoid fevers. In the former disease the urine "is generally pale, is of rather low specific gravity, and, in a majority of cases, contains albumen at an early period of the disease." In typhoid fever the urine is high-coloured, is of high specific gravity, and does not contain albumen, except occasionally towards the end of the case, but gives, with a little acid, a dense non-albuminous precipitate soluble in excess of acid, and consisting of urates.

[It would have been desirable to have known the *quantity* of urine in these cases of typhus and typhoid; it is, we believe, often copious in typhus, and scanty during the first fourteen to eighteen days in typhoid; but after that date the quantity increases, the colour becomes paler, and the acidity diminishes. The precipitate of urates caused by a drop of acetic or hydrochloric acid is common in many diseases.]

4. The Irish Fever of 1847 is stated, by Dr. Purefoy, to have been of remittent character, and to be a "new, or peculiar and specific form." The disease is said to have suddenly commenced with chills, headache, pains in the limbs, nausea, and hot skin. After a few days marked remission of all the symptoms followed, so that the patient left his bed, when a fresh accession of febrile symptoms, or, "as it was generally termed, a relapse," occurred. These exacerbations occurred regularly every 5th, 7th, 11th, or 15th day in some cases, but in others 5, 15, or 21 days elapsed between the attacks. When the disease had lasted 3 or 4 weeks, the exacerbations were severe, and were attended with vomiting and profuse diarrhœa. Maculæ were frequent in the beginning of the epidemic. Among a registry of 260 fever patients, there were 256 of this fever, and only 6 cases of true typhus.

The writer states that, since 1847, this form of remittent fever has continued to prevail in Ireland. "The marked features in this novel form of fever may thus be summed up: a previous state of slight general indisposition of days' or weeks' continuance, followed by chilliness or rigors, and the usual symptoms of mild fever; deceptive symptoms of crisis occurring at an uncertain period of the disease, succeeded by apparent convalescence,—this amendment being of short duration,—when the disease re-appears in a more aggravated form; these *crises and relapses* (improperly so called) being the *true and peculiar characteristics* of the disease now described, which may be justly styled remittent in its nature, symptoms, and progress."

In the treatment blood-letting is recommended if the patient be not already much reduced; purgatives are to be avoided, since the intestinal mucous membrane is apt to suffer. Unduly prolonged diaphoretics are also hurtful; alkalies, prussic acid, and vegetable bitters, are useful, and quinine, though it does not prevent the paroxysms, delays their return. Change of air, as soon as the patient can bear it, is an important remedial measure.

5. Dr. William Robertson gives us the results of the chemical examination of the blood in thirty-five cases of cholera, occurring in 1848-9. The process adopted was that of Christison, as used by Andral. The results are given under four heads—

	Early Stage, 7 cases.	Incipient Collapse, 6 cases.	Complete Collapse, 14 cases.	Reaction, 8 cases.
Fibrine	2·7	3·2	3·2	3·7
Serous solids, Organic	82·2	93·4	102·4	78·2
Serous solids, Inorganic	7·8	6·9	6·9	6·6
Globules	103·4	129·9	129·9	122·6
Total solids	196·1	233·4	242·4	211·1
Water	803·9	766·6	757·6	788·9
Specific gravity of blood	1053·1	1059·5	1066·3	1055·8
" serum	1029·4	1033·7	1036·9	1030·8

In other cases, not recorded, the blood was examined for urea, which, in two cases, amounted to 1·6 and ·73 per 1000.

Dr. Robertson observes that the table warrants the conclusions—that anæmic persons are more predisposed to cholera; that the changes which affect the blood are loss of water, and some loss of salts; that in the period of reaction the serum is rapidly diluted, and that the salts are therefore still more (relatively) diluted. He does not, however, know how to explain the rapid diminution of the albumen which occurs in reaction, with the increase in the fibrine, and the almost stationary condition of the red corpuscles.

II.—The Non-Specific General Diseases.

1. *On Tuberculosis in Egypt.* By Professor Griesinger. (Vierordt's Archiv für Phys. Heilkunde, 1853. Heft 3. pp. 519.)

1. In 363 dissections at Cairo, by Professor Griesinger, there was tubercle in 62 (17 per cent.), but as in 12 it was very trifling and obsolete, it should be said that there was recent tubercle in 50 (13·8 per cent.). (In Stuttgardt and Prague the proportions are, according to Cless and Dittrich, whose observations are referred to for comparative data, 36—37 per cent. in both places.) It was less common in old persons; its greatest frequency was between the ages of 15 and 20; but in general terms it may be said to have been nearly the same between 7 and 40 years. Among the 363 dissections in the hospital were 333 Fellahs and 10 Negroes; the proportion of tubercle was only 11·11 per cent. among the former, and no less than 50 per cent. among the latter. Dr. Griesinger remarks that the disposition of Negroes to tubercle, so common in cold climates, begins already in Egypt. With respect to the implication of particular organs—the lungs were unaffected in one case in which there was tuberculous meningitis; in all other cases they suffered. In 33 cases the disease was confined to the lungs and its appurtenances (pleura and bronchial glands); in 10 cases the lung disease was about equally advanced with disease of other organs; in 6 cases the disease was very trifling in the lungs, but was advanced elsewhere.

The amount of disease in the lung appeared less than in phthisical cases in Europe; the lower lobes alone were attacked in four cases, the extreme apices of the lungs appeared to be spared often, and the tubercle was found about the height of the second or third rib.

In 4 of these 50 tuberculous cases there was pericarditis (not apparently with tuberculous deposit at that point). The peritoneum was tuberculous in 14 cases (28 per cent., whereas in Cless' cases it was affected only in 13 per cent., and in Dittrich's in 7 per cent.). The small intestines were affected 23 times=46 per cent. (in Cless' cases 54 per cent.); the large intestines were affected in 6 cases =12 per cent. (in Cless' cases 24 per cent.). The intestines were thus altogether affected in 50 per cent., while Cless' numbers are 78 per cent. and Dittrich's 72. The mesenteric glands were affected in 22 cases=44 per cent. (in Cless' cases 25 per cent.); the liver was tuberculous in 9 cases=18 per cent., (in Cless' cases only 1 per cent.); the spleen was affected in 23 cases (46 per cent.), and between the ages of 7 and 30 this organ was affected in no less than 87 per cent.; in Europe the frequency of spleen-tubercle is much below this; the kidneys were affected 12 times (in 4 cases very greatly); this number is also much higher than in Europe; thus in Egypt in 24 per cent., in Cless' cases 4 per cent., in Louis' 2 per cent. In 3 cases there was tuberculous meningitis, in 2 cases tubercle in the brain. The following is the order in which the organs were attacked: lungs, bronchial glands, spleen, small intestines, peritoneum, pleura, kidneys, mesenteric glands, liver, large intestines, pia mater, brain.

Dr. Griesinger then remarks that tuberculosis generally, and phthisis pulmonalis in particular, are far less common in Egypt than in Mid-Europe; the causes of this are, perhaps, the mild climate, the mode of occupation, which is never hardly sedentary, and the infrequency of bronchitis and inflammatory affections of the lungs. The investigations show also the relative infrequency of tuberculosis in children; while, on the other hand, the extremely frequent implication of the mesenteric glands, peritoneum, liver, spleen, and kidneys, makes the tuberculosis of adults in Egypt approach, as far as organs are concerned, the tuberculosis of children in Mid-Europe.

The important question whether Egypt (Cairo) is a good residence for tuberculous Europeans, is answered by Dr. Griesinger in the affirmative, and cases are referred to in which the disease was decidedly arrested. Nevertheless, the disease should be in an early stage, and without bowel implication, as dysentery is very apt to ally itself to it. The patients should arrive in October in Alexandria; should go to Cairo in November, and there remain, or go to Upper Egypt or Nubia. In March, or at the beginning of April, they should leave Egypt, and go to Syria.

III.—The Diseases of the Thoracic Organs.

1. *The Examination of the Size and Situation of the Organs of the Chest.* By Dr. Conradi. (Archiv des Vereius. Band i. Heft 1.)
2. *On Laryngeal Ulceration.* By Dr. Rheiner. (Virchow's Archiv für Path. Anat. Band. v. Heft 6. pp. 534.)
3. *On Interlobulur Pneumonia.* By Dr. Weber. (Virchow's Archiv für Path. Anat. Band. vi. Heft 1.)
4. *On Diaphragmatic Pleurisy.* By M. Gueneau de Mussy. (Archives Générales.)
5. *On Thoracenteris.* By Dr. Schnepf. (Archives Générales. Oct.)

1. The following extract contains an abridged statement of Dr. Conradi's paper on the method of examination practised in the "Medical Clinik," at Giessen, under the direction of Professor J. Vogel.

The position usually preferred, if the person can be out of bed, is the upright, with the arms close to the sides; if in bed, the patient lies flat on the back, (the pillows, &c., having been removed,) for the examination of the front, and then sits up for that of the back. The time of meals, the quantity of food or fluid taken, are noted.

Examination of the thorax. At first general inspection of the formation of the chest, with particular attention paid to the so-called "angulus Ludovici," (prominence produced by the union between the manubrium and body of the sternum.) Then exploration of the *diameters* of the chest. Three *transverse* and two *antero-posterior* (*gerade*) diameters are adopted. The instrument of which use is made for measuring them consists of a beam divided into centimeters,* of two arms sliding thereon, and a centrepiece. For measuring the transverse diameters, the centrepiece is applied to the median line of the thorax, (from the incisura semilunaris sterni to the processus xiphoideus,) while both arms of the instrument are applied in equal height to the sides of the body. The antero-posterior diameters are found by merely applying both arms of the instrument. The *first transverse diameter* goes from one fossa axillaris to the other. The centrepiece is applied to the angulus Ludovici, the arms of the instrument as nearly as possible to the ribs, which is assisted by varying the degree of abduction of the arms from the thorax, (from 50 to 60°). The *second transverse diameter* lies in the height of the ninth rib. The centrepiece is placed on the basis of the processus xiphoideus, the arms on each side over the ninth rib. The *third transverse diameter* is determined by applying both arms of the instrument to the eleventh rib, and the centrepiece to the median line of the abdomen (line from the point of the processus xiphoideus to the symphysis pubis.) The *first antero-posterior diameter* extends from the angulus Ludovici to the processus spinosus of the third and fourth dorsal vertebræ; the *second* from the basis of the processus xiphoideus to the processus spinosus lying nearest

* 1 Centimeter = 0·3937 inch of English measure.

to the line drawn from the lower angulus of the one scapula to that of the other. The width of the shoulders is determined by applying the centrepiece to the angulus Ludovici, and the arms to the deltoid muscles, the degree of development of which is always described. To aid the exploration and description of the size and situation of the internal organs, the following four lines are adopted:—1. The *linea mediana* (see above). 2. The *linea mammalis:* from the union between the anterior and middle third of the clavicula downwards through the nipple. 3. The *linea axillaris:* from the middle of the axilla to the anterior end of the eleventh rib. 4. The *linea cristo-dorsalis:* from the middle of the spina scapulæ downwards parallel to the internal margin of the scapula and the external prominent ridge of the muscul. iliocost. (sacro lumbal.)

As to the exploration for the size of the single organs, the following remarks are made. 1. For *the right lung*. Percuss from above downwards in the just mentioned lines, and mark exactly where you meet in every line, at first the less sonorous and then the perfectly dull sound. Conradi found the average distance between the point where the sound became less sonorous and that where it became perfectly dull, to be about 3 centimeters (=$1\frac{1}{5}$ inches) in healthy male individuals between 20 and 30 years. In the linea mammalis, he marked in general the beginning at the 5th intercostal space; in the linea axillaris, at the 7th rib or 7th intercostal space; in the linea cristo-dorsalis, at the 9th intercostal space. During deep inspirations, the lower margin of the lung descends about $1\frac{1}{2}$ to 2 centimeters below the line where it is marked during normal inspiration; it is raised on the other hand about 1 to 2 centimeters over its level by forced expiration. The examination must therefore be always made during quiet respiration. The percussion on the posterior wall ought to be more powerful than on the anterior; it must be slight for the exploration of the lower border of the lung, strong for that of the superior border of the liver.

2. For the *examination of the left lung* percuss in the linea mammalis, axillaris, and cristo-dorsalis downwards until the dulness from the heart and spleen is reached. The line which unites the point of the former with that in the linea axillaris, pointing out the beginning of the dulness from the spleen, is considered to indicate the situation of the lower border of the left lung. Conradi in general met with the beginning of the heart's dulness at about the 4th intercostal space, from whence he could, by slight percussion, trace the internal margin of the lung covering the heart directed in a transverse line outwards and downwards to the left until to the 6th rib. In the linea axillaris he mostly found over the 8th rib or intercostal space, the inferior margin of the lung meeting the superior of the spleen. In the linea cristo-dorsalis, he constantly met with the dulness from the spleen on the 10th intercostal space.

3. For *the internal margin of the lungs and the heart,* percuss in the median line from above downwards, to ascertain the beginning of the heart's dulness; starting from thence, explore by light percussion the internal margin of the right lung, which in most cases may be traced from the 4th left intercostal space downwards on the left side of the sternum as far as the union between the sternum and xiphoid process, where it passes over to the right side, and is farther explored as described above. In the examination for the heart, a distinction is made between the space of complete dulness (*Herzleerheit*), produced by the part of the heart situated immediately below the wall of the thorax, and that of incomplete dulness (*Herzdaempfung*) by superposition of the borders of the lungs over the heart. The former is detected by light, the latter by powerful percussion. It is recommended to percuss in various directions, from the circumference of the thorax towards the heart; on the right side, principally in a line parallel to the superior margin of the liver, and in another direction from the point of the 3rd intercostal space, situated in the linea mammalis, inwards and downwards; on the left side, in the same line, and in another from the 5th intercostal space towards the sternum. Conradi found, on the right side, on percussing in the inferior line, the dulness from the heart reaching beyond the margin of the sternum; in the superior he met with

it just at the margin, or between it and the median line; in the upper line on the left, over the 4th intercostal space; in the lower over the 5th, slightly outwards of the impulse. The highest point of the dulness was in general met with in the height of the 3rd intercostal space on the left margin of the sternum. By uniting these 5 points through lines, a conical figure is produced, showing the circumference of the dulness from the heart. Within this figure we may trace another triangular one, indicating the space where sound of percussion is completely toneless; the lateral borders of this figure are formed by the internal margins of the lungs, the basis by a line extending from the point where, on the right side, the dulness from the heart and liver meet with the extreme point of the heart's dulness on the left. The superior angle (the point of diverging of the internal border of both lungs) is found in general at the 4th left intercostal space; the right lower angle in the middle of the processus xiphoideus; the left in the 5th intercostal space, slightly inwards of the linea mammalis. The basis, therefore, of both figures is given by the same line.

2. Dr. Rheiner has written a long and able paper on ulcerations of the larynx. He briefly notices, first, the alterations which occur after death in the diseased laryngeal membrane; the redness disappears, especially at the points where the elastic elements of the mucous membrane are abundant; swelling, also, disappears, if it be occasioned by fluid exudation; simple softening of the mucous membrane is a post-mortem appearance, dependent either on decomposition or maceration. He then considers ulcerations; 1st, *as regards their anatomical origin.* Disease may commence externally in the structures of the neck, from abscess or tumour, and penetrate inwards; more frequently it commences in *perichondritis laryngea,* and passes inwards; finally, the ulceration may begin on the internal surface of the mucous membrane and pass outwards. 2nd. *As regards their seat.* In phthisis the posterior commissure of the vocal chords, and the bases of the arytenoid cartilages, especially suffer; in syphilis, and in follicular pharyngeal disease, the parts above the chordæ vocales are most concerned; perichondritis laryngea implicates especially the cricoid, according to most authors, but according to Dr. Rheiner, the arytenoid. 3rd. *As regards their etiology.* Ulcerations are now and then of traumatic origin; they occur in syphilis, sometimes in typhoid fever, very frequently in lung disease, and sometimes in pharyngeal disease. Into the connexion between lung and laryngeal diseases the author enters rather fully, and discusses the possible origin of laryngeal ulceration from disease of the nerves, from participation in the specific lung-disease (pneumo-tuberculosis), from "infection" (as some have supposed), from irritation from the sputa, &c., and decides that at present a plausible explanation of the connexion between laryngeal and lung disease cannot be given.

The author then proceeds to consider laryngeal ulceration from an anatomical point of view, and makes the following varieties:

(a.)—*The simple catarrhal ulceration,* which is preceded by catarrhal inflammation. This includes many specific ulcerations, as those of syphilis and of phthisis. The author describes the *macroscopic* characters of the ulcer in the ordinary way; then passing to the *microscopic,* he observes that the first step of the process is the dislodgment of the epithelium (as occurs, indeed, in even simple inflammation without ulceration); the condition of the "homogeneous layer" immediately beneath the epithelium cannot be well made out; the tissues of the mucous membrane become crowded with little cellular elements (as occurs also in old catarrh without ulceration) to such an extent as to call to mind the iliac swellings in typhoid fever; these cells are heaped together in masses, and arise from "proliferation" of the pre-existing normal elements; when in great amount they may press on the vessels and impede the circulation of the part; they may then form pale prominences, visible to the naked eye, which may be called tubercle; the tissues enclosed between these cells, and often the cells, are then set free, and are thrown off and form part of the sputa; in this way an ulcer is formed and extends itself. In a healthy larynx no papillæ are found on the mucous membrane, but papillary or villous growths are found on the borders of the ulcers, and are pierced with vessels; sometimes they become so large as to be true vegetations. At a greater distance from the ulcer, the mucous membrane is tumefied from serous effusion; and the corpuscles of the uniting tissue are larger. The perichondrium does not long escape alteration; traces of increased cell-formation appear here also, but on account of the toughness of the membrane, it does not readily ulcerate. The alteration in the cartilages from neighbouring ulceration is difficult to determine, as these structures are so prone to alteration from age: still it is known that an habitual catarrh of the larynx without ulceration hastens the ossification (calcification) of the cartilages; ulceration appears to have the same effect, that part, of course, being chiefly or wholly affected which is nearest to the ulcer; for ossification to take place, however, the ulcer must be of some standing. The author then discusses the question of ulceration of the cartilages, and sums up thus:—It may occur in two ways—*first*, in perichondritis; then if the cartilages have been previously ossified, as in old persons, necrosis or caries may ensue, according to the amount of deprivation of blood-supply; *secondly*, from ulceration of the mucous membrane passing outwards; and here, if the ulcer be chronic, and not in a very young person, ossification occurs; especially at the adjacent borders of the cricoid and thyroid, if the ulcer happens to be situated here. The necrosis or caries may come on as in perichondritis, or if not, the cartilage, from loss of nutrition, becomes yellow or brownish, and softened to a jelly-like consistence. If, however, the cartilage continues to be sufficiently connected with the vascular parts to preserve its nutrition, then ensue the changes described by Redfern and Virchow. The cells exhibit a greater number of fat globules, and are in fact filled by these; the nuclei are divided, and give rise to a number of shining corpuscles in the cells or in the intercellular tissue, when the cells are broken down.

The condition of the bloodvessels in ulceration is determined with difficulty; they are compressed and obliterated by the formation of cells, or disappear before the ulceration; the nerves, on the other hand, are destroyed with difficulty, and are sometimes seen on the ulcerated surface as white fibrillæ, which on microscopical examination are found to be unaltered nerve-fibres. The follicles suffer in catarrhal inflammation no special alteration. A variety of the catarrhal ulcer is the superficial aphthous erosion of the larynx and trachea.

(b.)—*The Follicular Ulcer.*—In catarrhal inflammation the glands suffer only in common with the intermediate membrane; in this disease they suffer very differently; in their interior an important cell-growth occurs and distends them to the utmost; the delicate uniting tissue between them becomes thinned; the follicular walls come in contact, then burst, and the union of several follicles forms a cavity which is surrounded by areolar tissue, and forms a prominence above the level of the mucous membrane; at its apex is a yellowish white point, from which fluid can be pressed; at last the walls at the prominent part burst, the contents are discharged, and an ulcer is formed.

(c.)—*The Diptheritic Ulceration.*—The author relates in illustration one case of croupo-diptheritic inflammation of the pharynx, larynx, and trachea, in which in the pharynx and the highest part of the larynx there was also loss of substance of the mucous membrane.

(d.)—*The Syphilitic Ulcer.*—Many syphilitic ulcers are of catarrhal origin, but in these cases there is also peculiar disposition to the formation of vegetations.

3. Dr. Weber describes under the name "interlobular pneumonia," the morbid anatomy of the disease in cattle known by the name of the "lung pestilence," and states that he has observed a similar affection in the lungs of men. In the lungs of cattle the interlobular uniting tissue is much more developed than in men; like the pleura, it is fed by branches of the bronchial artery. On account of this latter circumstance, the pleura is always attacked at the same time as the interlobular uniting tissue. This is the case also in interlobular pneumonia in men. On accurate examination of a lung thus affected, besides the pleuritic effusion, the surface of the lung

showed a dark colour, diversified, however, by numerous white-yellow streaks, three or four lines broad, which surrounded the lobules. The lung was heavy; when cut there was considerable resistance; the section showed hundreds of little dark red islands formed of congested lobules, separated by white-yellow streaks of altered interlobular tissue, as on the surface. On further examination, the mucous membrane of the greater and smaller bronchial tubes showed no trace of croupous exudation; it was normal in the larger, and in the smaller bronchi had only a slight catarrhal injection. The vessels of the air-cells were greatly engorged, but the cells themselves were filled with fluid (œdema pulmonum), not with firm exudation, except in some very few instances. In some cases there were greater or less sized extravasations of blood. The exudation into the interlobular substance was not microscopically distinguishable from other inflammatory exudations; it was frequently more or less organized into new uniting tissue; especially at the outer portion of the streaks: the inner portion was less advanced, and this at a future period often softened.

The coats of the larger branches of the bronchial arteries were thickened and surrounded by organized exudation; often the calibre was blocked-up by fibrinous coagula. The smaller branches, even to the capillaries, were lying in the exudation in the interlobular tissue, and were often much dilated and blocked-up with coagula. This blocking-up of the vessels is considered by the author to be secondary.

An after-stage of this condition was given by softening of the unorganized mass which lay in the centre of the more developed exudation; purulent collections were found, in which sometimes large masses of lung substance floated loose, which in some cases, though not in all, were gangrenous.

In some cases these pieces of detached lung became surrounded by a capsule, so that it might have seemed to be a tuberculous cavern, only the examination of the encapsuled piece of lung showed the lobular structure.

In the commencement of this disease, the interlobular uniting tissue was hyperæmic, somewhat swollen, and yellowish in colour; at the same time the pleura underwent a similar change; then an amorphous thin exudation occurred in both places. At this time the capillaries of the air-cells often showed little hæmorrhages, which gave the section a dark pointed appearance. Then occurred acute œdema of the air-cells, and no air was able to penetrate into them.

In human lungs a similar process, though to a much less extent, has been traced by the author, and in one case he found a separated piece of lung enclosed in a fibrinous capsule, as is so frequent in cattle. In the pleuritis of children the interlobular tissues often also suffers. The author refers to a future paper he intends to publish on the interlobular pneumonia of men.

4. M. Guneau de Mussy has written a long memoir on diaphragmatic pleurisy. He observes that the ancients considered delirium to be an urgent symptom of inflammation of the diaphragm, whence arose one of the terms applied to the structure (φρενες, diaphragm). This opinion was generally accepted until its incorrectness was vigorously proclaimed by J. P. Frank; but even at the present time, "periphrenitis" is often used to denote inflammation of the diaphragm, and of the pleura covering it.

Laennec does not seem to have considered the diagnosis of this affection possible, except when the ordinary signs of pleurisy succeeded, such as dulness on percussion and abolition of the vesicular murmur, and sometimes ægophony.

Diaphragmatic pleurisy presents several varieties; sometimes the inflammation is concentrated between the base of the lung and the diaphragm; sometimes, having commenced here, it spreads to the costo-pulmonary pleura. In a third variety it begins in the costo-pulmonary pleura, and thence spreads to the diaphragmatic pleura.

The chief object which the author has in view is the diagnosis. This is made by attending to the following symptoms which accompany the febrile condition:

(a.)—The pain is severe and radiates from one or other hypochondria, along the costal edge to the epigastrium; it is increased by pressing from the abdomen upwards, and there is also a spot about two fingers' breadth from the middle line at the height of the 10th rib, which is peculiarly tender on pressure; close to the spine in the last intercostal space is another tender point.

(b.)—Pressure between the two insertions of the sterno-mastoid gives pain from the pressure on the phrenic.

(c.)—There are frequently spontaneous supra-clavicular and scapular pains. There are sometimes twitches of the face.

(d.)—The attitude is peculiar; the patient is usually in a sitting position.

(e.)—The dyspnœa is urgent; the respiration chiefly, but not entirely costal.

(f.)—The cough is dry, or with a little mucus; there are no physical signs except diminution of vesicular murmur at the base, and perhaps sub-crepitation from some pulmonary congestion.

Some of these symptoms occur in other cases; as, for instance, there is sometimes pain in the neck and over the course of the phrenic in pericarditis, but if these symptoms are present, and positive signs of other diseases are absent, the diagnosis of diaphragmatic pleurisy may be made.

The author (contrary to Laennec) speaks doubtingly of the prognosis. On the subject of treatment, nothing new is communicated.

5. Dr. Schœpf relates 3 cases of acute, and 1 of chronic pleurisy, treated by thoracentesis, and discusses at length the advantages and supposed dangers of the operation. While he believes it to be useful in cases of simple pleuritic effusion, he holds it to be hurtful when asphyxia is imminent and the forces are prostrate, and therefore he dissents from the views of those who employ it merely *in extremis*. He also declares that the entrance of a *small* quantity of air into the pleural cavity is *useful*, as aiding the outflow of the fluid. In the first case the operation was performed on or about the 13th day of an acute and rapidly increasing pleurisy, and 29 ounces of fluid were removed. The symptoms were at once relieved, and the recovery was complete in 18 days afterwards. In the second case the operation was performed on the 12th day of an acute pleurisy, and 35 ounces of fluid were removed; the symptoms at once improved, and the patient left the hospital 23 days afterwards. In the third case, chronic, and in a tuberculous subject, 32 ounces of fluid were removed with temporary benefit, but the effusion returned; after, from the influence of medicines, a partial absorption occurred. In the 4th case, one of acute pleurisy, thoracentesis was performed on the 21st day, and 43 ounces were removed. The benefit was very marked, and 9 days afterwards the patient left the hospital.

In all these cases the after-treatment seems to have been very simple; after the operation a troublesome cough came on in some of the cases, which is attributed to the penetration of air into the pulmonary vesicles which had been previously compressed by the effusion.

IV.—The Diseases of the Digestive Organs.

1. *The Examination for the Size and Position of the Liver and Spleen.* By Dr. Conradi. (Archiv des Vereins. Bd. i. Heft 1.)
2. *Black Fur on the Tongue.* By Dr. Eulenberg. (Vierordt's Archiv für Phys-Heilkunde. 1853. Heft 3. S. 490.)
3. *The Pathology of the Pancreas.* By Dr. Eisenmann. (Prag. Viertel-jahsch. 1853. Bd. iv. S. 73.)

1. In the paper referred to in a previous section, Conradi refers to the *exploration of the circumference of the liver*. The upper border is considered to lie where the sound elicited by percussing from above downwards in the lines formerly mentioned, commences to be less sonorous. To ascertain the diameters of the liver, the breadth is measured in the linea axillaris, mammalis and mediana. In describing the breadth of the left lobe to the left of the linea mediana, the distance from that line must be always noted. It must also be stated in each case how far to the left

of the linea mediana the dulness may be traced. As the result of the examination of 50 healthy male individuals, between 18 and 25 years of age, Conradi states—1. That the border of the liver is met with in general at the 7th rib, or intercostal space in the linea axillaris, at the 5th rib or intercostal space in the linea mammalis.—2. That the situation of the lower border differs widely within the limits of health. In the linea axillaris the lowest was found 46 m in the linea mamm. 76 m below the arcus costalis. The dulness from the border of the left lobe coincides almost with a line from the umbilicus to the left angle of the dulness from the heart. — 3. The diameters do not increase in proportion to the height of the individuals.—4. The lower lobe of the right lung covers a rather larger portion of the liver in taller than in shorter individuals.

With respect to the *spleen*, Conradi tried particularly to explore the situation of the anterior end of its breadth in the linea axillaris and cristo-dorsalis. The situation of the anterior end is described by noting the distance between the linea axillaris and another line, the *linea costo articularis* (adopted by Hamernjk, passing from the point of the 11th (floating) rib to the articulatio sterno-clavicularis.) To explore the longitudinal diameter, the use of the above-described instrument is advised, in order to avoid the variations produced by the difference in the convexity of the ribs when measured with tape, &c. After having explained the difficulties connected with an accurate exploration of the size of the spleen, Conradi states as his experience—1. That the anterior end mostly reaches to the linea costo articularis, but that considerable enlargement may exist without its passing this line. In the linea axillaris the superior border is generally met with in the 8th intercostal space or 9th rib, the lower border in the 10th intercostal space or 11th rib. 3. In the linea cristo-dorsalis the superior end is found in most cases at the 9th rib.—4. The longitudinal diameter, when taken by the instrument, measures about 13½ centimeters (called the *straight* diameter), when taken by a piece of tape over the convexity of the ribs 19½ centimeters (called *convex* diameter).

2. Dr. Eulenberg records a case of a boy aged 2 years, whose tongue from apex to base was thickly coated with a perfectly black layer. At first attributed to the use of some article of diet, it was soon seen to arise from a black formation on the tongue itself. When removed, the tongue commenced to become again coated in the centre, and then towards the tip, and then backwards to the very root. All the papillæ were very large, and the papillæ vallatæ in particular remained black even when the rest of the tongue was clean. This condition of tongue continued for three months. At first the child had also a little diarrhœa; this ceased, and the general health was unimpaired. Under the microscope there were numerous very much thickened brown-coloured epithelial cells, and between and especially on their borders were numerous pigment-corpuscles, which were sometimes arranged mosaic-like, and were enclosed by no cell or membrane. After the case had been observed for some time, it was treated and completely cured with chlorine water. Dr. Eulenberg remarks that this is a case of formation of pigment without any appreciable impairment of the general health, and reminds us of the cases in which black pigment appears in the sputa of simple catarrh.

3. Dr. Eisenmann assembles in an instructive paper all the recorded cases of fatty discharges connected with pancreatic disease, in order to see whether the opinions of Bernard and the statements of Moyse are correct. He enumerates the three cases of Bright, a case of Lloyd's, one of Elliotson's, one of Gould's (Anat. Museum of the Boston Society, 1847, p. 147), one of Susanna's (Giornale Veneto di Sc. Med. t. ii.), and then communicates one of his own. He then discusses the question rather briefly, and concludes that the presence of fat in the stools renders a functional disorder of the pancreas probable, but not certain, and that, on the other hand, the absence of fat from the stools does not negative the diagnosis of pancreatic diseases if other symptoms indicate it. We have discussed this point at such length* lately, that it is not necessary to consider this paper at length.

* See the July number (1853) of this journal for a review on Pancreatic disease and fatty discharges.

V.—The Diseases of the Urinary Organs.

Change in the Urine, produced by the Inhalation of Arseniuretted Hydrogen. By Professor Vogel. (Archiv des Vereins. 1853. Heft 2. S. 208.)

Professor Vogel relates a case in which the inhalation of arseniuretted hydrogen gas by a gentleman who was experimenting with hydrogen, produced, in addition to other symptoms, a peculiar dark colour of the urine, which was found to depend on a large quantity of dissolved blood-colouring matter. In order to determine the cause of this, Vogel made many experiments on a dog, and found that the same effect was always produced by inhalation of the gas. The urine, of course, contained albumen at the same time, but no blood-corpuscles were visible. Vogel supposes that the gas acted destructively on the blood-corpuscles, and remarks that, in several diseases, as, for example, in typhus, there appears to be also a similar destruction and elimination of blood-pigment with the urine.

VI.—The Diseases of the Cutaneous System.

On Epiphytes of the Skin. By Dr. Gudden. (Vierordt's Archiv fur Phys.-Heilkunde. 1853. Heft 3. S. 496.)

Dr. Gudden communicates two papers on the diseases of the skin, produced by parasitic plants, and a third is promised. In neither paper is there any novelty, although the subject is well treated. As we reviewed Robins's complete treatise on parasitic vegetables in our last number, we shall pass these papers over. We may observe, however, that in order to see the plant in pityriasis versicolor, the author recommends a blister to be applied; the raised cuticle is taken, laid on a piece of glass, and the white layers infiltrated with serum are peeled off its under surface. Then nothing remains but a thin transparent layer, which permits the plant to be seen well; the plant grows most near the hairs and also freely on the borders of the sweat glands, but not in their interiors; it is contained in, and covered by, the most superficial layers of the cuticle, and forms two layers, the deeper being composed chiefly of thallus, the superficial of spores. The spores grow at the end of the thallus, and form clusters, which are so thickly set together that their mode of attachment to the thallus can only seldom be perceived; it is, however, by means of a little pedicle. In an appendix to his paper, Dr. Gudden communicates a case of partial alopecia of the hair on the pubis, which was found to depend on a fungus forming *round*, but not *in* the hair.

QUARTERLY REPORT ON SURGERY.

By John Marshall, F.R.C.S.

Assistant-Surgeon to University College Hospital.

I.—Injuries to the Chest and Abdomen.

1. *Impalement upon a Pichfork-handle entering per Vaginam.* By Dr. Sargent. (American Journal of Med. Sc. 1853, Oct., p. 355.)
2. *Extensive Laceration and Contusion.* By Dr. G. S. Bryant. (American Journal of Med. Sc. 1853, Oct., p. 399.)

1. The following case is recorded by Dr. Sargent. A lady, aged 37, slipped down upon a pitchfork-handle, which passed into the vagina 22 *inches*, and was supposed by Dr. Sargent to have gone upwards " between the uterus and the rectum, in front of the left kidney, behind the spleen, and between the diaphragm and false ribs, peeling

up the pleura till it reached the scaleni muscles." The first left rib was fractured, and emphysema was found above the left clavicle. The treatment consisted of rest, bandaging, and morphia. No pleurisy or pneumonia was detected! The recovery was rapid!

2. A case is related by Dr. G. S. Bryant. It occurred in the person of a negress, aged 25, who jumped down on to some fodder, in which was a tobacco-stick four-and-a-half feet long, and one inch square. The stick passed up the vagina, and on the right side of the os uteri, entered the abdomen, and finally lodged against the twelfth and eleventh ribs. There was considerable hæmorrhage from the vulva, and, subsequently, severe constitutional disturbance, with symptoms of peritonitis. On the fifth night after the injury, enormous hæmorrhage occurred from the bowels; the patient slowly rallied, being convalescent at the end of a fortnight, and perfectly well in a few weeks after.

II.—Aneurisms.

1. *Aneurismal Tumours upon the Ear, treated by the Ligature of both Carotids.* By Dr. Mussey. (American Journal of Med. Sc. Oct., p. 333.)

The American taste for journalism displays itself clearly in regard even to the medical profession; for their list of periodicals devoted to medicine and the collateral sciences is at least three times as long as ours. Nor can we hesitate to say that the national character for energy and activity is ably supported by our transatlantic brethren in surgery.

Dr. Mussey, of Cincinnati, records an interesting case of aneurismal tumours, situated on the external ear, treated apparently with success by ligature of both common carotids. The patient, a male, was 19 years of age. The tumours, of which three were on the left ear, and one below it, were as large as a grape or nutmeg, were soft, and compressible almost to obliteration; they pulsated strongly, and appeared to communicate like aneurismal varices. The integuments of the ear were hypertrophied. A cutaneous nævus had existed from birth in front of the left ear; but the tumours had first appeared as small elevations, only eight years since. One of the swellings had once burst, and caused alarming hæmorrhage, which recurred on removal of bandages and compresses. A farinaceous diet having been strictly enforced, the left carotid was first tied; the pulsation in the tumours ceased, and never returned; the tumours slowly reduced in size; but as it was doubtful whether a cure would result, the right carotid was tied four weeks after. The reduction in the size of the tumours was now more rapid. In about 3 weeks collodion was applied to the swellings with benefit, and 7 weeks after the second operation, scarcely a vestige of them was left. The patient was dismissed, and three months afterwards the cure appeared to be permanent. Contrary to Dr. Mussey's opinion, we should not feel quite confident of this.

III.—Tumours.

1. *Case of Fibrous Polypus of the Antrum, occurring in the Practice of Professor Syme.* By Dr. Dobie. (Monthly Journal, Oct., p. 307.)
2. *Exostosis of the Thigh Bone.* (Ibid., Nov., p. 420.)
3. *Cancerous Ulcer in the Temporal Region.* (Ibid., Oct., p. 306.)

A formidable example of fibrous polypus of the antrum, which occurred to Mr. Syme, and was treated by evulsion, is interesting, from the rarity of the disease, and the promptitude and success of the operation. The patient, a male, was 20 years of age; the disease first manifested itself by attacks of epistaxis, then by swellings of the right cheek, and partial obstruction of the right and left nostrils. Frequent and alarming bleedings accompanied the growth of the tumour. This, which was as large as an orange, was placed between the cheek and the jaw; it was lobulated, softer than an ordinary fibrous tumour, and seemed to grow from the antrum, but there was no bone over it. Provision having been made to plug the right nostril if required, access to the growth was obtained by making an incision from the angle of the mouth along the cheek, and the tumour, with some difficulty and delay, was torn out of its bed, within the antrum. One part of the mass reached backwards "between the pterygoid plates;" in detaching this, the internal maxillary artery was torn across, but speedily secured. The cure was rapid.

2. A curious instance is recorded by Dr. Dobie of the accidental fracture of the neck of an exostosis growing from the inner side of the thigh bone, a little above the knee-joint. The patient, a female, aged 23, had noticed a small lump for many years, in the situation of the tumour; its growth was slow, but it was about the size of an orange, when she fell from a ladder placed between a boat and a steamer, and struck herself in the situation of the growth. On examination, a large swelling was found a little above the knee, in which a hard movable body was felt. This was exposed by an incision, and easily removed, as it was quite detached. The case did well. The tumour was three inches long, and eight inches in its greatest circumference. The pedicle was bony, and measured two inches over at the fractured surface. The surface of the tumour was lobulated and cartilaginous; it was covered with a perfect synovial investment or bursa, a structure often before noticed by Mr. Syme in similar cases. A woodcut is given of this exostosis.

3. A case of cancerous ulcer situated on the temple is worthy of note, as well as another of the face, mentioned in the same report, from the mode of operation employed—namely, dissection of the diseased mass, instead of cauterization by chloride of zinc, or potassa fusa. Of the advantage of this plan we can venture to speak from actual experience. A case of cancer of the conjunctiva (Case 7), treated by frequent removal of the local disease without success, and afterwards requiring extirpation of the eyeball, is remarkable for the seat of the cancerous growth.

IV.—Amputations and Resections.

1. *Malignant Ulcer of Thigh—Amputation at Lesser Trochanter, occurring in the Practice of Professor Syme.* By Dr. Dobie. (Monthly Journal, Nov., p. 413.)
2. *Case of Cartilaginous Exostosis of the Condyle, Ramus, and Angle of the Lower Jaw, in which Resection was performed.* By Dr. Brainard. (American Journal of Medical Science, Oct., p. 397.)

1. A very satisfactory amputation of the thigh, immediately below the small trochanter, has been performed by Mr. Syme, for a malignant ulcer 8 inches long and 6 broad, situated on the inner surface of the upper part of the thigh, reaching to within 1½ inch of Poupart's ligament. The surface of the ulcer was elevated, and the discharge "muco-purulent," profuse, and acrid. It had commenced seven years before in one of a few small pimples on the skin. The patient's groins, lower part of abdomen, and right thigh, were deformed with extensive and prominent cicatrices, the result of burns received 21 years before. One of these crossed from thigh to thigh below the level of the symphisis pubis. The patient was 41 years old, and of a tolerably healthy appearance, but suffered much and severe pain. The glands in the groins were not (apparently) affected. Amputation being decided on in preference to excision, owing to the size of the ulcer, its proximity to the femoral artery, and the small chances of a good result, an anterior incision was made parallel to Poupart's ligament, from below the pubic symphisis to a point a little below the anterior superior spine of the ilium. A large outer and posterior flap was then made, the knife carried round the bone, and this latter sawn just below the lesser trochanter. Both the superficial and deep femoral

arteries were divided. With the exception of a slight tendency of the posterior flap to retract, reported on the 7th day, nothing occurred to interrupt the cure, which was complete in about 9 weeks. (Case xv.)

2. In the 'American Journal' will be found an account of an operation by Dr. D. Brainard, on a cartilaginous exostosis of the condyle, ramus, and angle of the lower jaw, in which resection, with removal of the zygomatic arch and the parotid gland, was performed. The operation would seem to have been performed with sufficient boldness, but the entire report is drawn up in so slovenly a style, that it defies the operation of resection on itself. The wound healed favourably, but left a sinus for many months; and the jaw fell so much to one side, that the patient became emaciated from the impediment to taking food.

V. Affections of the Genital Organs.

1. *Obliteration of the Seminal Passages. Sterility consequent on Double Epididymitis.* By M. Gosselin. (Archives Générales de Médecine, Sept. 1853.)
2. *Case of Stricture of the Urethra, which had existed from early Infancy, cured by Operation.* By Dr. Kelburne King. (Monthly Journal, Sept. p. 201.)
3. *Two Cases of Stricture of the Urethra, treated by External Incision.* Reported by Dr. Dobie from the Practice of Mr. Syme. (Monthly Journal, November. Case xvii., p. 416; and Case xxii., p. 421.)
4. *Recto-vaginal Fistula cured by Operation.* By R. D. Mussey, M.D. (American Journal of the Medical Sciences, Oct., p. 333.)

1. In a paper published in the 'Archives Générales de Médecine' (vol. xiv. and xv., 4th series, 1847), M. Gosselin drew attention to certain facts which seemed to show that temporary or permanent obliteration of the vas deferens, at the tail or lower part of the epididymis, might possibly happen as a consequence of any kind of inflammation of those parts.

Since 1847, M. Gosselin has confirmed his views by other dissections upon the human body, and has further made some experiments on animals, as well as certain clinical observations upon patients suffering under the consequences of consecutive epididymitis.

In the cases of two dogs which were killed, one 10 months, and the other 4 months after, a portion of the vas deferens measuring 1 centimeter was cut out, he established the interesting fact, that such an operation is not necessarily followed (at any rate, within ten months) by any wasting of the testicle itself, although the vas deferens was quite interrupted. On the contrary, the gland presented its normal characters; nor was its secreting power altogether destroyed, for the flexuous canal of the epididymis was found distended by a fluid which contained a multitude of spermatozoa. It is not stated, however, whether these spermatozoa were active, but we are probably correct in inferring that they were. The remaining experiments on animals were fruitless.

The principal point in M. Gosselin's recent inquiries, and to which we now pass, consisted in the microscopic examination, at different intervals, of the semen of individuals, labouring under the effects of epididymitis, and having a more or less hardness opposite the lower part or tail of the epididymis.

Having justified the prosecution of such a subject out of regard to the interests of science, of individuals, and of families, the author preserves a discreet silence as to the mode in which the prosecution of his researches was rendered possible. Let us rather turn to his results.

Setting aside all cases of epididymitis occurring on one side only, and also examples of tuberculous or other organic disease of both testes, he refers to the results of the examination of 20 cases of double (bilateral) affection, consequent on gonorrhœa. In 15 of these cases, in which the epididymitis had lasted only a few weeks or months, the semen was examined from one to four times, at intervals of several weeks. They all agreed in three important particulars: 1stly, there was an induration opposite to the tail of each epididymis; 2ndly, the genital functions were seemingly restored in all respects, the semen appearing quite normal; but, 3rdly, no spermatozoa could, in the earlier periods of examination, be detected in that fluid. Pus-corpuscles (from the vesiculæ or the urethra), blood-corpuscles, molecular granules, and crystals of ammoniaco-magnesian phosphate, were occasionally found in it. Two cases only were quite followed out, and in these the return of spermatozoa in the semen occurred after some months, and coincidently with the complete disappearance of the induration of the epididymis on one side of the body. In one of these cases the right epididymis became affected in September, 1852; the left, two months later. On the 12th December the semen did not present any peculiarity to the naked eye, but contained no spermatozoa. On the 3rd January, 1853, the same conditions existed. The 16th May,—i.e., eight months after the first orchitis, and six after the second,—a few spermatozoa were found, and the induration on the right side was gone. On the 18th July, there were relatively more animalcules, and the left induration was only slightly evident. In the other case, the right orchitis took place in August, 1852; the left, in January, 1853. The semen examined on the 1st March, and again on the 5th April, was bloody, and contained no spermatozoa. These bodies were still absent on the 25th April and the 13th May; but on the 8th June they were again found, the left induration being effaced, whilst the right was still of the same size as before, and painful.

In the remaining 5 of the first-named list of 20 cases, the double epididymitis had taken place several years back. In one man, aged 45, the disease had occurred 20 years before, but the left induration no longer existed, and spermatozoa were found in the semen. This patient was at the time under mercurial treatment for secondary syphilis. In another man, the disease had lasted 5 years; the general health was good; but no spermatozoa could be detected. In the 3 other cases, the duration of the disease was 10, 6, and 4 years respectively. There was hardness on both sides; the testes were otherwise unchanged, and free from pain, even after the act of emission; the signs and acts of virility were apparently perfect, and the semen had its ordinary appearances; the individuals were all married, but had no children; moreover, the semen was destitute of animalcules. One of them had had children by a former wife, before the attack of double epididymitis. In another, the semen had, some years before his attack, been examined, and found to contain spermatozoa.

From these details, which we have preferred to give at length, since no addition to the author's facts is likely to be made on this side of the Channel, M. Gosselin concludes, that in consequence of bilateral or double epididymitis, the ejaculated male fluid may cease for months, or even for years, or probably for the rest of life, to contain any spermatozoa, and may therefore be incapable of fecundating the ovum. Further, he believes that the absence of these spermatic animalcules is due to the obliteration of the seminal passages by "the effusion and organization of lymph around and within the flexuosities of the canal of the epididymis." To the objection offered by M. Rayer, that this effect might be owing to general debility, he replies, that the observations were made at periods when the patients had recovered their strength and apparent virility. That it is due to the effect of inflammation in the testicles themselves, he thinks unlikely, until it is shown that when the induration is gone, the spermatozoa are still absent. There appears, on the other hand, to be a direct relation between the mechanical obstruction and the absence of spermatozoa. Only one of the patients had syphilis, and in his case, as well as in other individuals labouring under that disease in its secondary stages, spermatozoa were found; hence M. Depaul's suggestion, that constitutional syphilis might have arrested the formation of the spermatic animalcules is also groundless.

In a physiological sense, M. Gosselin believes that his results show that Hunter's opinion as to the small relative proportion of the testicular fluid to that secreted by the vesiculæ seminales, is correct; and even that the quantity of the former is less than had been imagined. Hence the occurrence of copious ejaculations, even when the absence of spermatozoa indicated that no fluid came from the testes; and hence, also, the liability to obstruction, without pain, from the small *vis à tergo* of the secreted fluid.

Admitting the accuracy of M. Gosselin's facts, we must conclude with him that the following additions to the pathology of gonorrhœal epididymitis are henceforth necessary:—1. The obstacle to the passage of the semen is generally at the tail or lower part of the epididymis, but may be at some other point. 2. This obstruction is not accompanied by pain. 3. It produces no change in the condition or function of the organs appreciable by the patient. 4. When the obstruction exists on both sides, it must destroy the fecundating power; so, too, if on one side, if the other testicle be otherwise diseased, atrophied, or wanting. In one case of single epididymitis, and varicocele on the opposite side, no spermatozoa were detected. 5. The duration of the obstructed condition may vary. It may be removed any time before eight months, and possibly after a much longer interval.

In the way of treatment, M. Gosselin recommends very active measures, by leeches, and especially purgatives, repeated every three or four days, in the earlier stages of the attack. These are to be followed by mercurial inunctions, and particularly by the free use of iodide of potassium, which he believes, from observation, to possess peculiar properties of resolving chronic inflammatory deposits in the testicle, even those which accompany tubercle or cancer. In gonorrhœal epididymitis it is very efficacious, especially when given early. The dose to be employed is not to exceed 1 gramme, equal to about $15\frac{1}{2}$ grains.

2. The case here to be considered was under the treatment of Dr. Kelburne King, of Hull, and well exemplifies the value of the urethral section in an old, obstinate, impenetrable, and complicated stricture.

The patient (aged 33 at the time of the operation) had suffered from urinary irritation before he was old enough to express his sensations; at school he had always had slow and painful micturition. At 7 or 8 years he was sounded, with great difficulty, for stone, but nothing was detected. From that time until his 12th year, small bougies were at intervals passed through the stricture; but after that period no instrument could be introduced into the bladder. Frequent, imperfect, and painful micturition were constant symptoms, but no blood had ever passed. At the age of 24, owing to more serious suffering and threatened retention, an attempt was made to force the stricture, without success, but with the ultimate result of causing perineal abscess and fistulæ. From the above-named period he became worse, and had continual incontinence of urine, with painful paroxysms of retention. In September, 1851, the patient, being then 32 years of age, came under Dr. King's care. Several fistulæ existed on the scrotum, and a hard knot could be felt at the seat of the stricture, midway between the pendulous portion and the bulb. The smallest sized bougie was arrested at four and a half inches from the orifice of the urethra, and the stricture could not be overcome. The urine was muco-purulent, and easily became alkaline. After continued suffering for some months, of a most severe kind, and having a serious effect on his health, another abscess formed in the perinæum, and when this was opened, the patient being under the influence of chloroform, an attempt was made to pass a bougie, by the aid of the finger, in the wound. The attempt failed, but a small calculus, about the size of a pea, was removed from the bottom of the abscess. The urine now flowed partly through the wound and partly by the natural passage, and of course considerable relief followed. But no instrument could even now be passed into the bladder; micturition was frequent, and accompanied with much straining. The stream was small, and mostly in drops, much passing by the wound. He had no power of propulsion, and the urine still threw down a copious muco-purulent deposit.

In October, 1852, a small grooved staff was passed as far as possible, getting it fairly into the stricture, an incision being then made through the skin, in *front* of the *scrotum*, and the induration felt with the finger; the urethra was opened in front of the stricture, and the knife pushed on through the firm substance to the end of the groove; here it struck against a calculus, which was immediately cut down upon, and then "four or five" calculi, weighing altogether ten grains, were turned out of a membranous sac. The staff, which had been held in its place, was then easily passed on into the bladder. A No. 6 catheter was afterwards introduced, and retained. The cone followed in the ordinary way. In a month's time only a few drops of urine passed by the wound; No. 9 catheter was easily introduced, and the patient could propel from him a full stream of urine, which he had not done from his early days. His general health became quite re-established. Dr. King informs us that the fistulæ all healed, and the wound gradually closed. A bougie was passed occasionally.

Dr. King discusses, at some length, the question, whether this was a case of early stricture, subsequently complicated by the calculi? or whether it originated in the formation of a calculus in early infancy or youth, and its subsequent imposition in the urethra? The former view is adopted first, because the symptoms were developed gradually, and not suddenly, and there was no blood in the urine; secondly, because instruments were passed with difficulty up to the age of 12 years, and no calculus had been felt then; thirdly, because the calculi were so small; and fourthly, because there was no occurrence of symptoms, either local or general, at any time, indicating the descent and impaction of the stones. If this view be correct, the case is a very rare one, but the author quotes another, occurring at four years of age, recorded by John Hunter. Certainly the *practical* impermeability of the stricture—an interesting fact—seems to have been due to the thickening and distortion of the canal produced by the local detention of the calculi within it.

The causes of so early a development of stricture are quite unknown.

It is not our intention to follow Dr. King in the discussion of a case of stricture, in which the urethral section was rendered a second time necessary, and which was treated in succession by Mr. Fergusson and Mr. Gay; nor do we here intend to examine minutely the merits of an operation which has undoubtedly been eminently successful.

In reference to the operation itself, it has only to be remarked, that as the grooved staff could not be got through the stricture, the next best course—viz., that of getting it as far as possible—was adopted. We must observe, also, that the section was made through "the front of the scrotum."

3. Two cases (xvii. and xxii.) of urethral section are recorded by Dr. Dobie. The first presents no very remarkable features; in the second case the seat of stricture was only one inch from the orifice of the urethra, and the narrowing was very great. An external incision, upon a grooved direction, was made, and a catheter retained for thirty-six hours. The wound healed on the fourth day.

4. Dr. Mussey, of Cincinnati, relates a successfully treated case of recto-vaginal fistula, large enough to admit two fingers. The sphincter having been divided purposely some days before, so as to facilitate the contraction of the fistula, the edges of this latter were pared and brought together by the clamp suture of Dr. Sims, secured by the wires and split shot. The principal feature in the subsequent treatment was the management of the diet, which consisted, for eighteen days, of two, or two and a half *crackers* (biscuits) a day—i.e., under five ounces of solid food. On the eighteenth day a gill of milk was allowed. An elastic catheter, removed and cleaned every second or third day, was kept in the bladder until the eighteenth day. On the seventh day the wires were cut, and the clamp was removed; the wound was united from end to end, On the twenty-fourth day the bowels acted, for the first time, from the use of castor oil. More food was then taken, and the patient left the hospital in the fifth week after the operation.

In commenting on this case, Dr. Mussey remarks that he prefers to use a thicker cylinder or clamp than Dr. Sims—viz., one and a half line diameter instead of one line; and to place his wires nearer—viz., one fifth of an inch apart, instead of one third. Moreover, he employs wire twice the thickness of a horse-hair, instead of that of equal thickness.

VI.—Affections of the Rectum.

Cause and Treatment of Prolapsus of the Rectum. By M. Duchaussay. (Archives Générales de Med., Sept.)

In a short but interesting memoir, M. Duchaussay reviews the circumstances attending this troublesome complaint, and fixes attention in particular upon the loss of power in the sphincter ani muscle as the chief cause of the descent of the bowel. Moreover, he endeavours to show that Dupuytren's operation, by excising the radiating folds of skin around the anus, and the operation by four touches with the actual cautery, practised by Guersant, act not by causing any subsequent retraction of the cellular tissue, skin, and mucous membrane, but rather by stimulating the sphincter muscle, so that it regains its contractility, and therefore its retentive character. How else, asks M. Duchaussay, do we explain the fact, that the prolapsus is often cured, or does not return after two days, or even after one day, or not at all, after the operation? He points out the fact, that in cases of this disease in infants, three fingers may sometimes be introduced without causing contraction of the sphincter, before the operation by cautery, whilst afterwards, if one be passed, a powerful contraction of the sphincter immediately ensues. As proof that this recovery of contractile power by the sphincter is the cause of cure, a case is mentioned in which M. Guersant had used the cautery too superficially, the sphincter failed to contract, and the disease returned. A second cauterization was followed, on the contrary, by return of the muscular contractility, and the cure was complete.

According to the author, the cautery acts as a stimulant to the paralyzed muscle, just as it will to the deltoid in a like condition. After pointing out the inconveniences and apparent severity of M. Guersant's method, M. Duchaussay suggests that a slighter cautery, or some other stimulant to muscular contractility, might act as well; and he suggests strychnine. This, with M. Guersant's permission, has been tried in the Hôpital des Enfants, in the case of a girl aged eleven years. The prolapsus here arose from obstinate constipation; it had lasted for four years; the bowel protruded at each evacuation about ten centimeters (= 4 inches). During the first month of her admission she was treated by laxatives only, with no other result than that of diminishing the length of the protruded portion of bowel to about four centimeters ($1\frac{1}{2}$ inches). Strychnia was then employed endermically near the region of the sphincter; the next day there was no evacuation; on the following day the bowels acted once, only a slight bulging of the rectum taking place; on the third day the protrusion was still less after an ordinary evacuation; and during the next thirteen days it did not occur again.

Blisters were made in the cleft between the nates, and on the right thigh close to that cleft; one-sixth of a grain of strychnia was applied the first day, one-third on the second, and one-third on the fourth day. On the fifth day, about half a grain of sulphate of strychnia was used, and this was repeated for the last time on the sixth day. In the case of a boy, it is recommended to be applied between the scrotum and anus, immediately over the anterior interlacement of the sphincter ani fibres. The remedy certainly deserves further trial.

QUARTERLY REPORT ON FORENSIC MEDICINE, TOXICOLOGY, &c.

By W. B. Kesteven, M.R.C.S.

Medical Jurisprudence in Great Britain has no distinct organ whereby to vindicate its condition and progress. In Paris, in Berlin, and in Vienna, journals devoted thereto preserve all the most important facts relating to continental forensic medicine. On this side of the Channel these topics must be sought throughout professional journals, or ordinary newspaper reports of trials, coroners' inquests, &c. It is not our object to trace the cause of this blank in British medical literature. Our aim will be (so far as the space can be spared for the purpose in this journal) to supply the deficiency, by selecting from other periodical publications all cases of importance or interest in a medico-legal point of view. We shall necessarily be restricted to brief abstracts of extended essays. It is hoped, however, that our quarterly record may supply the material for occasional retrospective reports, to mark the advance of this branch of medical science.*

Detection of Blood, and Blood-stains in Medico-legal Investigations. By Prof. Rose, of Berlin.†—The recognition of blood-stains when unmixed with other substances, or in sufficient quantity, is not difficult. The case, however, is different when the quantity is small and the blood is mixed with the fibres or tissue of clothes, &c., as in an instance mentioned by Prof. Rose, wherein the specks of blood could only be detected on a cloth coat by means of a lens and a strong light. The greatest care was required in their removal, whereby also the quantity was diminished, and they were intermingled with fibres of cloth. The traces thus obtained were macerated in a few drops of cold water, till the solution was of a reddish colour, it was then poured off the fibres of the cloth. The solution was then boiled, a coagulum formed, which on being treated by heat and caustic potash, presented a greenish solution presenting the phenomena of polarization, being of a green colour to transmitted, and red to incident light. Chlorine water produced white flocculi. Dilute nitric acid and tincture of gall have a feeble violet colour.

The discrimination of dried blood-spots from rust-spots on the surface of metallic iron is often still more difficult. Prof. Rose quotes the observations of Vauquelin and others, that iron-rust contains ammonia, applying this test in the case of iron-rust by comparison with the peculiar empyreumatic odour caused by burning blood. The presence of blood with the rust is further detected by the addition of muriatic acid, and the production of Prussian blue.

When blood is spilt on a polished steel surface, it dries, and is readily washed off again without leaving any mark on the surface; rust, on the contrary, cannot be removed without leaving a stain. A knife was submitted to examination by Rose. It had been found in a field where, some months before, a murder had been committed. The blade was rusted, but presented no trace of blood, which was, however, detected by chemical analysis in considerable quantity in the handle of the knife; it had been washed off the blade by rain. Professor Rose has observed that freshly precipitated oxide of iron has the property of combining with and dissolving the colouring matter of blood. The solution thus obtained is available for the application of other tests already mentioned. Hydrated silica possesses the same property. The researches upon these substances, by Professor Rose, and their application to the detection of blood in garden mould, are given in the article quoted from.

The subject of the detection of blood-stains was very fully investigated by Dr. Taylor (Guy's Hospital Reports, 1851, p. 371), on the occasion of the trial of Thomas Drory, for the murder of Jael Denny, at the Chelmsford assizes in 1851. A pamphlet containing Dr. Taylor's researches has been subsequently published.

A murder has recently been perpetrated near Windsor. At the coroner's inquest the opinion of Dr. Taylor respecting the date of certain blood-stains on the clothes of the accused, formed one of the most important links in the chain of circumstantial evidence which led to a verdict of "wilful murder." We reserve further notice of the facts in this case until the evidence to be adduced on the trial of the prisoner shall be before us.

* It may be stated that an unsuccessful attempt was made, about two years ago, to establish an English journal of Medical Jurisprudence. The *Legal Examiner and Medical Jurist* was set on foot by E. H. J. Crawfurd, Esq., M.P., Barrister-at-Law; the medical department having been superintended by G. W. Hastings, Esq., Barrister. Not having, however, the advantage of a medical editor, and its sale not having been promoted by the interests of a medical publisher, the hopes of its founder, that it would embrace all topics common to the two professions, were frustrated. The *Legal Examiner* still flourishes; the *Medical Jurist* has been some months defunct.

† Casper's Viertaljahrsschrift, Oct., p. 294.

*Diagnosis of Injuries by Sulphuric Acid from Burns by Red-hot Coals.**—The opinion of Dr. Masclika, of Prague, was required upon the cause of death in the case of a child apparently burnt by accident. On the examination of the body, indications appeared of the internal administration of strong sulphuric acid, and these were confirmed by chemical analysis. The questions were raised—whether the effects of a red-hot coal upon linen could be distinguished by chemical analysis from the results of the corrosion of concentrated sulphuric acid?—whether sulphuric acid would not be formed during the process of such combustion?—what would be the changes on the human body produced by the action of concentrated sulphuric acid?—and what from the effect of a red-hot body, as coal?

The experiments of Dr. Masclika showed that the moist, paste-like characters and colour of the edges of the holes burnt by sulphuric acid distinguish them from holes burnt by hot coals, so long as the texture is undisturbed: but that after washing in water these characteristics disappear. It was shown that in the combustion of a linen fabric with red-hot coal, sulphuric acid is formed. With regard to the effects of the two on the human body, it was stated that besides the differences in appearance of the two forms of corrosion, the action of a red-hot coal would give rise to phosphoric acid in greater quantity than sulphuric acid, which is not the case when linen is burnt. In the case under examination, it was made clear that death was caused by the internal administration of concentrated sulphuric acid, and that certain external burns were produced by fire, and were insufficient of themselves to have caused death.

Medico-legal Examination of suspected Spots on a Towel in a case of Infanticide.† By Dr. Wistrand.—A soiled towel having been found under circumstances supposed to throw some light upon a case of infanticide, it was submitted to examination by Dr. Wistrand. A stained portion was cut off and macerated, blood globules were discovered by the microscope and by chemical re-agents. Another portion, on examination, presented particles of bile, epithelium, and fatty matter. Epidermic cells and cutaneous glandular structure were also detected on portions of the towel. It was concluded that these several matters were stains from the blood of the umbilical cord, from meconium, and from the cutaneous secretion of infants known as *vernix caseosa*. The proof was that a new-born infant had been wrapped in the towel.

Burns from Phosphorus.‡—Two children in Havre having picked up some pieces of phosphorus in the street, and being ignorant of its dangerous properties, but attracted by its luminosity, one of them pocketed several pieces, whence his dress became ignited; when the flames were extinguished, it was found that very serious injury had been caused thereby.

Hysterical Monomania—Self-inflicted Wounds.§—M. Tolmouche relates the particulars of the case of a female who brought a charge of violence and rape against some persons unknown. On close investigation, it was discovered that a great number of punctured wounds, which were inflicted on various parts of her body, had been the work of her own hands, under the influence of a morbid love of notoriety.

Death from Flagellation.—The same writer records the case of a child, aged four years and a half, who suffered such severe and repeated floggings at the hand of a man who had adopted her, that congestion of the brain and death ensued.

Monomania—Book-stealing.‖—An Englishman in Paris having been convicted of stealing books from a stall, and condemned to two years' imprisonment, pleaded

* Schmidt's Jahrbücher, Oct. 1853, p. 89. † Schmidt's Jahrbücher, Oct., p. 94.
‡ Journal de Chimie Medicale, Octobre. § Annales d'Hygiène, &c. Oct.
‖ Annales Medico-Psychologique, Oct.

monomania, or an irresistible impulse, as ground of mitigation of sentence. From the previous history, however of the culprit, it was established that he must be held responsible for his acts; the plea was therefore negatived.

*Results of Wounds, Injuries, &c., as affecting capability for Labour.**—Dr. Böeker, of Bonn, has written an article extending over fifty pages of the journal, to discuss the subject of *arbeitsunfähigkeil, incapacité de travail personnel,* (Devergie). Dr. Böcker relates several cases in which difficulty was experienced in determining the fact and extent of "incapacity." The editor, Dr. Behrend, suggests that the duty of the medical jurist is confined to determining the persistence in any organs or limbs of the consequences of an injury, leaving incapacity for work to the determination of a jury. The French code affixes punishment to the inflicter of an injury which shall cause inability for labour beyond twenty days from the date of the infliction thereof. Other German medico-legal journals contain many elaborate articles upon the medico-legal bearings of wounds, injuries, &c.; the Prussian criminal laws having recently undergone revisal and modification. The Code Napoleon has been followed with regard to wounds, injuries, &c.

Fracture of the Cranium in an Infant at Birth.†—Dr. Swayne has placed on record the following case. A woman, aged 29, pregnant the third time, was delivered of a healthy male infant at the full period. Labour had been so rapid, that the child was born while the mother was standing upright, and the umbilical cord was torn. The head of the infant was much bruised; tumefaction followed; the case progressed favourably until the sixth day, when the child was seized with convulsions, and died in about four hours. On examination of the body after death, the left parietal bone was found fractured, a coagulum about the size of a shilling presenting itself on the surface of the membranes of the brain.

The points of medico-legal interest in this case are, the length of interval, six days, that occurred between the accident and the appearance of the symptoms: and the establishment of the fact of the possibility of the occurrence of such an injury under the circumstances mentioned, on which doubt has been thrown.‡ Dr. Swayne points out the importance, in all such medico-legal investigations, of bearing in mind that the direction of the child's body in the act of birth is obliquely downwards and forwards. This fact, as Dr. Cormack suggests,§ explains the discrepancies observed between the results of experiments by Chaussier and the cases collected by Klein.

Infantile Leucorrhœa.—In several numbers of the 'Medical Times and Gazette,' for September 10th to October 29th, will be found the *History of a recent Epidemic of Infantile Leucorrhœa, with an account of Five Cases of alleged Felonious Assaults.* By W. R. Wilde, F.R.C.S., &c.—"Considerable excitement," Mr. Wilde states, "has prevailed among all classes in Dublin during the last month, owing to the circumstance of no less than three cases of felonious assaults upon children under ten years of age having been brought forward by the Crown at the late Commission before the Chief Justices."

A correspondence has been published in the 'Freeman's Journal,' between Dr. Ireland, Physician to the Police, upon whose information the cases were sent for trial, and Mr. Wilde, who had one of the accused persons defended. Most of the leading members of the profession in Dublin gratuitously tendered their evidence in court, "in what they considered the cause of truth, science, and humanity." The occurrence of this form of *vaginitis,* in an epidemic form, as shown by Mr. Wilde, is perfectly well-known to most practical physicians and surgeons.

Mr. Wilde notices the delusion, which is extensively prevalent in Ireland, to the

* Henke's Zeitschrift, Oct. † Association Journal, Oct. 14th, p. 401.

‡ A very similar case is related by Casper, of Berlin, in his Gerichtliche Leichen-Offnungen.

§ Association Journal, Nov. 4.

effect that a man can get rid of an obstinate gonorrhœa by having connexion with a virgin; and as the easiest and surest mode of effecting that, a child of tender years is selected; hence the felonious assaults occasionally attempted, and for which men have been most justly convicted, and most righteously punished. But in all such cases it has been proved, that the men, after the commission of the crime, still laboured under gonorrhœa or venereal, although the popular opinion among the lower orders is, that the disease is not only completely, but instantaneously, transmitted from the male to the female. A similar superstition, it appears, is found to exist among the Arabs, as stated by M. Duchesne, in his recent work on 'Prostitution in Algeria.' It is the knowledge of this wide-spread superstition, which leads the mother at once, on the appearance of vaginal discharge, to jump to the conclusion that impure connexion has taken place, and possibly she may be confirmed in this idea by some medical man not conversant with the true nature of the affection.

Mr. Wilde dwells upon the suggestions, insinuations, and threatenings, which is usually had recourse to in these cases, in order to extort a confession of connexion, and very justly remarks that it is not likely that a child, who has neither passion nor love to influence her, will conceal the fact from her parents or near relatives, when hard pressed. In Mr. Wilde's contribution it is stated that one mother, on her own statement before the jury, actually threatened to cut her child's tongue out if she did not confess to the connexion. Besides the three cases already referred to as having become the subjects of trial at the late Dublin Commission, Mr. Wilde cites six other instances of epidemic leucorrhœa that have been brought under the notice of the profession, either at public institutions, or through the medium of the medical journals. For the details of these cases, and of the trials, we must refer our readers to the 'Medical Times and Gazette.'

*Duration of Pregnancy in the Human Female.**—On this subject Dr. James Reid gives the following as his conclusions, formed upon extensive researches :—

The duration of pregnancy is not altogether a fixed period. It varies somewhat in the human female as it does in the lower animals.

This deviation, however, is not to any great extent. The only *certain* data for calculation are those depending on the *known time of conception.*

The *average* duration of the pregnant state, when calculated from this event, is about 275 days, or it may have a range from the 270th to the 280th days.

There is no full or satisfactory evidence of gestation having been prolonged beyond 293 days.

The code of Napoleon, which allows 300 days, and the Prussian law, which fixes the *ultimum tempus* at 301 days, may be regarded as liberal.

Vagitus Uterinus.†—Dr. Braun, of Fürth, records an instance of this phenomena, of which, however, he has received the history from a midwife, a very intelligent and trustworthy woman. Dr. Braun refers to the opinions of various medico-legal writers, and concurs in that which he considers a perfect expansion of the lung, by the occurrence of vagitus uterinus, as impossible.

Deaths from Chloroform.‡—A patient, undergoing Mr. Syme's operation of perineal section, in the operating theatre of the Royal Infirmary, Edinburgh, suddenly expired. Every expedient for restoration of life was tried in vain. On examination of the body, every internal organ was found healthy. An anonymous writer in the same journal (p. 407), very properly suggests, that this case proves that the administration of chloroform is not so free from danger as "the Edinburgh professors would have the world believe." The deaths from chloroform have now been so numerous, that an accurate examination of the various after-death appearances becomes desirable.

* Lancet, Sept. 9 and 17. † Henke's Zeitschrift.

‡ Medical Times and Gazette, Oct. 15.

A woman aged forty, about to be operated upon for strangulated hernia,* at University College Hospital, had chloroform administered in the usual manner by Mr. Hillier, the resident medical officer, appointed to undertake this duty. The chloroform was administered on lint, and carefully held, at first, at a distance of three or four inches from the mouth. Care was taken that air should be inhaled with the chloroform. The whole quantity of the fluid employed was one drachm and forty minims. Convulsions occurred in about five minutes; these were followed by stertor, and, after a few inspirations, the patient ceased to breathe, and all means of resuscitation failed. On examination of the body the muscular structure of the heart was found to have undergone fatty degeneration.

Another death† from the inhalation of chloroform occurred at St. Bartholomew's Hospital, on the 20th inst. The subject was a patient, aged twenty-two years, in apparently good bodily health, about to have the actual cautery applied to a cancroid sore in the vagina; chloroform had been inhaled for a similar reason a fortnight before. On the fatal occasion symptoms of an alarming character suddenly appeared after the administration of about a drachm and a half, and death occurred within five minutes of their first manifestation.

Post-mortem examination could discover no deviation from health in any one organ, but the blood had lost its coagulability.

In these cases the most marked change was the poisoned condition of the blood, which rendered nugatory all the science and skill brought to bear upon the attempts at the restoration of life, as it had also superseded all the care and scientific precaution in the administration of the vapour. The warning herein conveyed is loud and impressive. Dr. Patrick Black, the medical officer by whom the chloroform was administered, has, in a letter to the Editor of the 'Medical Times and Gazette,' Nov. 25th, referred the death in this case, to the exhaustion produced by the struggles of the patient.

Deaths in two more instances are related in the 'Deutsche Zeitschrift für die Staatsarzneikunde.' In one the administrator is charged with culpable carelessness in the mode of its administration, and in the means had recourse to for resuscitation. In the other the quantity employed also was large, but due care was taken in its administration; every proper measure was adopted to arrest the fatal result.

Mode of Death from Inhalation of Chloroform.‡—Mr. E. R. Bickersteth, of Liverpool, lays down the following conclusions as the results of his experiments on animals:

1st. That in death from the inhalation of chloroform, the respiratory movements cease before the cardiac.

2nd. That the heart continues its action, uninfluenced by the chloroform, for a period longer or shorter after the cessation of respiration, and that its then failing may be considered as a natural consequence of the respiration having ceased, and as independent of the influence of chloroform.

3rd. That if, after the respiration has ceased, and while the heart is still in action, chloroform continues to be absorbed into the system, its movements become impaired or cease, the chloroform in such case acting directly upon the heart.

4th. That if artificial respiration be had recourse to before the cardiac contractions are seriously affected, and be properly maintained for a sufficient period, the respiratory function may be re-established.

A fatal case, already quoted from 'Medical Times and Gazette,' Oct. 15, is, however, related in the November number of the same journal, by Dr. Dunsmure, in which it is doubtfully stated that the pulse ceased to beat before respiration failed. This case seems to lend confirmation to the observation of Dr. P. Black, that the fatal result is, in some degree, attributable to exhaustion consequent on struggling, as the patient is described as having struggled violently during the process of inhalation.

* Medical Times and Gazette, Oct. 22. † Ibid. Oct. 29.
‡ Edinburgh Monthly Journal, September.

Oxygen an Antidote for Chloroform.—Dr. Abrahams, of New York,* has successfully employed the inhalation of pure oxygen gas in the case of a young man asphyxiated by the vapours of chloroform, and apparently *in articulo mortis*. Dr. Abrahams suggests the employment of this agent in suspension of life from drowning, from inhalation of the fumes of charcoal, from the fixed air of pits, mines, &c.

Poisoning with Nicotin.†—In 1851, the Count Bocarmi was charged with the murder of his brother-in-law Gustavus Fougnies, who was found dead under circumstances which attached suspicion to the accused. An examination of the body made it evident that death had been caused by some powerful irritant or narcotic-irritant poison. The question to be determined was, the nature of the agent employed. The following embrace the conclusions arrived at by M. Stas:

The substance which had been administered was nicotin, which, by chemical analysis, was separated and detected by its peculiar odour of tobacco. It was not possible to ascertain the quantity that had been swallowed, but it appeared, from the portion obtained by analysis, that a great deal more than would have sufficed to kill one man, had been taken. The effects observed in the body were similar to those that are produced in animals by the administration of nicotin. Some of the poison extracted from the stomach was given to two goldfinches and to a dove—the effects were quickly manifested. The chemical action of nicotin is that of a caustic alkali, and the lesions detected in the mouth, pharynx, and œsophagus, were those usually produced by caustic substances. The poison had been administered with vinegar, which, in the opinion of Orfila, would have somewhat moderated its activity.

Poisoning with Nux Vomica; Recovery.‡—The patient, a labourer aged twenty, was admitted into the Royal Free Hospital, under the care of Dr. Hassall. Three quarters of an hour before admission, he had taken about one drachm and a half of powdered nux vomica. When admitted he was in a profuse perspiration; the skin of the face, neck, and chest, was greatly congested, the eyes suffused, the pupils slightly contracted, the pulse hard and excited. A few minutes after admission a tetanic paroxysm came on suddenly, the man was thrown into a state of opisthotonos, all his muscles became rigid, and respiration was for a time suspended. The fit lasted about half a minute, when the muscles became relaxed, and he was again able to answer questions. He had had several of these fits before admission. The first occurred about ten minutes after he had swallowed the poison. They gradually decreased in severity; altogether he had five. On the following day crampy pain in his limbs were complained of. Emetics were given without effect. The stomach-pump was used, and removed some greyish powder. Aperient and saline medicine was given. On the second day he was discharged well.

The exact quantity taken could not be ascertained. Enough, however, evidently was swallowed to produce serious symptoms. Dr. Alfred Taylor§ quotes an instance where five grains were fatal: and another instance, of recovery after half an ounce had been taken.

Poisoning from Aconite and Belladonna.||—Dr. J. A. Easton relates the following case:—On the 8th of January, about half-past six o'clock in the evening, a young man, previously in good health, but suffering from slight headache, called at a drug establishment in Glasgow, to get advice from a medical student with whom he had been on friendly terms. The student administered a draught, containing twenty-five minims of tincture of aconite, twenty minims of tincture of belladonna, and a drachm

* Association Journal, Sept. 16, p. 819. † Henke's Zeitschrift, Oct. 1853.
‡ Lancet, Oct. 22. § 'On Poisons,' p. 775.
|| Poisoning by Aconite and Belladonna, quoted in Association Journal from the Glasgow Medical Journal, July, 1853.

of the tincture of musk. The tincture of aconite was prepared with sixteen ounces of the root of the aconitum napellus to thirty fluid ounces of spirit. Shortly after swallowing this mixture, the patient walked to a friend's house, about three quarters of a mile distant. On his arrival, at about twenty minutes past seven, he began to feel indisposed, and experienced a tingling sensation in his arms and hands. After remaining for a short time in the house, during which period the hands and arms became so benumbed and powerless that he could not keep them up, he walked again for about a quarter of a mile, and then was conveyed by an omnibus back to the place where he had swallowed the draught. He reached this about a quarter to eight; while there he vomited freely, his speech was thick, and he staggered like a person intoxicated, though there was neither stupor nor loss of consciousness. At half-past eight he was placed under the care of Dr. Lawson, when his gait and appearance were those of an intoxicated man; but when he spoke, which he did with some effort, his articulation was so distinct, and his conversation so intelligent, that any idea of alcoholic intoxication was dispelled. He complained of a general feeling of discomfort and illness, of tingling and numbness in the muscles of the face, and of dragging downwards, especially towards the left side. His face was not hot, neither was the skin cold, although the night was cold and stormy; his pulse was about ninety, of fair strength. He had an inclination to vomit, which was promoted by ipecacuanha. He was taken in a cab to his home about nine; on his arrival he presented the appearance of a person dead drunk; he vomited twice. Shortly afterwards he became convulsed, pale, and pulseless, and at length, retaining his consciousness to the last, he died about twenty minutes past nine, within three hours from the period at which he had swallowed the fatal draught.

A post-mortem examination was made by Drs. Macgregor and Easton, and the contents of the stomach were subjected to chemical analysis and microscopical examination. Congestions of the brain, lungs, and right side of the heart, were observed. Analysis of the contents of the stomach yielded no more definite proof than the production of appearances exactly similar to those present, by genuine aconitina, under the microscope. Drs. Macgregor and Easton declared that they were unable to determine from chemical analysis, the poisonous agency which overpowered the nervous system, and impeded the process of respiration and circulation.

Dr. Easton's opinion is doubtless correct, that death was owing entirely to the aconite which, as the more energetic poison, appears to have masked or superseded the effects of the belladonna. Of the signs of poisoning by the latter, only those common to narcotic poisons were observed, while all those characteristic of the noxious influence of belladonna were absent; the effects of the drug on the organs of vision were not produced, neither was delirium present.

From this case it is established that so small a dose as twenty-five minims of tincture of aconite of an ascertained strength, may take away life within three hours. The recorded doses of this poison which have proved fatal, are very various, as may be seen by a reference to works on toxicology and materia medica. This variety arises out of the want of an uniform formula for the preparation of the tincture.

In the narrative of the fatal case here quoted, we observe the omission of one or two important points. The treatment mentioned seems to have been inadequate to the danger of the case, supposing it to have been all that was had recourse to, if it were known that so powerful a poison had been swallowed. This, however, is not clearly stated, neither are we informed how soon after having swallowed the draught, it was discovered that the patient had accidentally taken such a poisonous dose.

Poisoning from Aconite, eaten in mistake for Horseradish.—The 'Times' of Nov. 10, contains the account of the death of a gentleman who partook of the fresh root of *aconitum Napellus,* which had been prepared for eating with beef at dinner. Soon after partaking thereof, a tingling sensation was felt in the hands and arms, and neck. Medical assistance failed to save his life, which was destroyed in about an hour after.

Similar symptoms appeared, with less severity, in another individual who had eaten in smaller quantities the poisonous root, and recovery was complete.

*Poisoning from Atropia.**—The 'Gazette des Hôpitaux' gives an account *of a case of poisoning with atropia*, remarkable from the small quantity employed, and the place of application, viz., the healthy conjunctiva. Three or four drops of a solution, containing about one grain of atropine to two ounces of water, acidulated with acetic acid, were instilled into each eye of a patient with double cataract. Half an hour afterwards the patient suffered from vertigo, &c.; three quarters of an hour later he manifested all the symptoms of poisoning by belladonna. The symptoms gradually passed off, but the patient did not recover his usual health for three or four days.

Poisoning from Laburnum Berries.†—Several children having eaten laburnum berries in the Hull Zoological Gardens, were found lying on the grass vomiting severely, and in a state of stupefaction. Medical assistance having been speedily rendered, they all recovered.

Poisoning from Fruit of Belladonna.‡—Two children, respectively seven and nine years of age, alarmed their parents by waking in the night, both in a state of violent maniacal delirium. On investigation it was discovered that they had eaten au unknown quantity of the berries of belladonna. Two adults had also eaten some of these—the one a man, who had swallowed about a dozen, became as if in a state of inebriation—the other a woman, having eaten only three, suffered severely from vertigo. Under medical care they all recovered.

Poisoning with Mushrooms.§—A family of five Italians residing in Paris, having partaken of mushrooms at a meal, were soon after seized with severe pain in the abdomen. Three out of the five (the wife and two children) died in horrible convulsions. Two dogs, which ate part of the food, speedily died in convulsions.

A labourer, who in disregard of caution ate considerable quantity of what he considered were edible mushrooms, died in convulsions after three days of extreme suffering.

Poisoning with Arsenic and Chromate of Lead—Recovery.‖—Dr. R. H. Thomson, of Liverpool, relates a case in which, as nearly as could be guessed, from half a drachm to two scruples of arsenious acid, and the same quantity of chrome yellow, were taken. Symptoms of poisoning did not appear until five or six hours afterwards. Calcined magnesia, to the extent of eight ounces in two hours, the stomach pump, mucilaginous and oily fluids were administered—subsequently opiates and external applications. Recovery was complete in about three weeks. Arsenic was detected in the urine, but no lead, which Dr. Thomson observes, was probably owing to the insoluble form in which the latter was taken.

Fatal poisoning from Arsenic taken to induce Abortion.¶—Mr. Brown, of Weston-on-Trent, relates the particulars of a case in which a young woman, twenty-four years of age, was supposed to have died from peritonitis after premature delivery. On exhumation of the body it was discovered that she had been the subject of ulceration of the stomach, produced by the action of arsenic, which was detected in that organ by chemical analysis.

* Association Journal, Sept. 16, p. 818.
† Morning Herald, Sept. 5, 1853.
‡ Journal de Chimie Medicale, Oct.
§ Ditto.
‖ Lancet, Nov. 29.
¶ Association Journal, Oct. 7th.

*Poisoning with Ink.**—A case is quoted from the Austrian 'Journal of Pharmacy,' in which severe symptoms of gastro-enteritis were caused by drinking a glass of ink by mistake for a glass of porter. The patient recovered, after having suffered for many days from weakness and trembling in the limbs, head-ache, &c.

Poisoning with Creosote.†—A woman having, in the act of applying some creosote to a decayed tooth, accidentally swallowed the piece of lint saturated therewith, in a few minutes (less than a quarter of an hour) experienced a sense of burning at the epigastrium, and became insensible; the pupils dilated, pulse regular and rather full—about 80. She had vomited the lint before the arrival of her medical attendant, Mr. Jeffery, of Worcester, who, besides other measures which he employed, extracted the tooth. On recovering, she did not know that her tooth had been removed.

Mr. Jeffery thinks the dose taken could not have been more than five or six drops, a quantity very little exceeding the dose usually prescribed for common autumnal diarrhœa. The severe effects produced in this case might probably, in some degree, depend upon the creosote having been taken into the stomach in an uncombined state.

Poisoning with Sulphuric Acid.‡—Mr. Dickenson related the following case at a meeting of the Western Medical and Surgical Society. Mrs. C——, aged fifty-two, took by mistake, on the 20th May, about an ounce of sulphuric acid, which, upon inquiry, was found to consist of equal parts of concentrated acid and water. Discovering her mistake, she immediately drank a large quantity of water, and applied for medical aid. Some carbonate of magnesia was administered, but was immediately rejected with much carbonic acid. No corrosion about the mouth or fauces was observed, nor were any laryngeal symptoms present, and the matter vomited had not any acid re-agency. She was then ordered to swallow some milk, which was immediately rejected in a coagulated state, with much grumous blood, and when seen at the end of two hours, she was suffering from soreness at the pit of the stomach: but as no violent pain was present, some castor-oil was administered, which caused the evacuation of much dark-coloured blood. Milk diet was enjoined, and in two days she considered herself recovered. Symptoms of injury to the pyloric end of the stomach, however, soon after appeared, and did not subside for three or four weeks. Mr. Dickenson explained the absence of corrosion about the mouth, by supposing an abundance of epithelium in the early part of the day. It was noticed also that salivation did not occur.

Poisonous Effects of Lobelia.—An inquest was held on the body of a man supposed to have died of the effects of lobelia, administered by an herbalist, agent to Dr. Coffin. Dr. Letheby stated in his evidence that the case had no doubt been one of cholera, accelerated by the exhibition of so powerful an irritant. Dr. Letheby stated that, to his personal knowledge, twenty-two deaths had occurred in this country from lobelia inflata. The surgeon, Mr. Champneys, who made the post-mortem examination, deposed to having found inflammation of the stomach and rectum. The coroner stated that he had twice sent this same individual (Stephens) before another tribunal for manslaughter for similar causes, and he had been discharged. A special verdict was returned, to the effect that death of deceased was accelerated by improper medicines, and that copies of the depositions be forwarded to the Secretary of State for the Home Department, that measures may be taken to suppress the illegal practice of medicine.

NOTICE.—On account of the demand on our space, the 'Therapeutical Record' is omitted: it will be inserted in our next number, with the Record for that quarter.

* Pharmaceutical Journal, Oct. † Association Journal, Oct. 21.
‡ Lancet, Nov. 25.

BOOKS RECEIVED FOR REVIEW.

A Practical Treatise on Sea Bathing and Sea Air. By George Hartwig, M.D. London, 1853. Small 8vo.

Elements of Psychological Medicine, an Introduction to the Practical Study of Insanity. By Daniel Noble, F.R.C.S. London, 1853. 8vo.

Sandgate as a Residence for Invalids. By George Moseley, M.R.C.S.E. London, 1853. 8vo.

On the Physiological Uses of the "Ganglionic Nervous System." By James George Davey, M.D., F.R A.S. 1853.

Clinical Reports on Continued Fever, &c. By Austin Flint, M.D., Professor of the Principles and Practice of Medicine in the University of Buffalo. Buffalo, 1852.

On Variations of Pitch in Percussion and Respiratory Sounds, and their application to Physical Diagnosis. By Austin Flint, M.D., of Buffalo, N.Y. (Reprinted from the Transactions of the American Medical Association). Buffalo, 1852.

Observations on the Remittent (so called) and Yellow Fevers of the West Indies. By David Lake Finlay, L.R.C.S.I. and L.A. Royal Mail Steam Packet Service.

Specielle Heilmittellehre, Chemisch, Physiologisch, und Klinisch bearheitet. Von Dr. W. J. A. Werber. Band. II. Abth. Erlangen, 1853.

Lehre der Nahrungsmittel, Für das Volk. Von Joc. Moleschott. Zr. Aufl. Erlangen, 1853.

Recueil des Travaux de la Société Médical de Genève. Première Année. Genève, 1853.

Notes worth Noticing relative to the Cholera. By Dr. Gillkrest, Inspector-General of Army Hospitals, W.P. London, 1853.

Medical Reform, being the Sketch of a Plan for a National Institute of Medicine. By Azygos. London, 1853.

Memoirs of John Abernethy, F.R.S. By George Macilwain, F.R.C.S., author of 'Medicine and Surgery one Inductive Science.' Vols. I. and II. London, 1853.

The Principles of Surgery. By James Miller, F.R.S.E., F.R.C.S.E., Surgeon in Ordinary to the Queen for Scotland, &c. Third Edition. Edinburgh, 1853.

Lehrbuch der Diagnostik. Von Dr. C. E. Bock, Professor du Pathol. Anat. in der Univ. Leipzig. Ze Abtheilung (Schluss) 1853.

An Introductory Lecture delivered at the Grant Medical College, at Bombay, on the 15th June, 1853. By C. Morehead, M.D., Principal, and Professor of Medicine. Bombay, 1853.

Cholera, its Nature, Treatment, and Sanitary Requirements. By S. Scott Alison, M.D. London, 1853.

Traité Therapeutique du Quinquina et de ses Préparations. Par P. Briquet, M.D., Médecin de l'Hôpital de la Charité. Paris, 1853.

The Pathology and Treatment of Pulmonary Tuberculosis, and on the Local Medication of Pharyngeal and Laryngeal Diseases, frequently mistaken for, or associated with, Phthisis. By John Hughes Bennett, M.D., F.R.S.E., Professor of the Institutes of Medicine and of Clinical Medicine in the University of Edinburgh. Edinburgh, 1853.

Das Blut in seinen Krankhaften Verhältnissen. Von Dr. F. Kehrer. Giessen, 1853.

A Text Book of Physiology. By Dr. G. Valentin, Professor in the University of Bonn. Translated and Edited from the third German Edition, by William Brinton, M.D., Physician to the Royal Free Hospital. Part Two. (Completion of the Work.)

The Elements of Materia Medica and Therapeutics. By Jonathan Pereira, M.D., F.R.S., and L.S. Third Edition. Vol. II., Part 2. (Completion of the work.) London, 1853.

The Science and Art of Surgery, being a Treatise on Surgical Injuries, Diseases, and Operations. By John Erichsen, Professor of Surgery in University College, and Surgeon to University College Hospital. London, 1853.

Der Typhus in Irland, beobachtet im Sommer, 1852. Von Dr. Joseph Lindwurm. Erlangen, 1853.

Beiträge zur Mikroscopischen Anatomie des Ganglien Nerven Systems, des Menschen und der Wirbelthierre. Von Carl Axmann, M.D. Berlin, 1853.

On the Advantages of the Starched Apparatus in the Treatment of Fractures and Diseases of Joints. By Joseph Sampson Gamjee. London, 1853.

Transactions of the Medical and Physical Society of Bombay. No. I. New Series. For the years 1851 and 1852. Bombay, 1853.

Asiatic Cholera, its Symptoms, Pathology, and Treatment. By Richard Barwell, F.R. C.S.E. London, 1853.

A Manual of Obstetrics. By Thomas F. Cock, M.D., Physician to the New York Lying-in Asylum. New York, 1853.

Popular Errors on the subject of Insanity, examined and exposed. By James F. Duncan, A.M., M.D. Dublin, 1853.

Transactions of the Pathological Society of London. Vol. IV. London, 1853.

Practical Pharmaceutical Chemistry, &c. By Dr. G. C. Wittstein. Translated and Edited from the second German Edition, by Stephen Darby, Practical Pharmaceutist. London, 1853.

Introductory Lecture to the Medical Session at the Charing Cross Hospital, London, Oct. 7th, 1853. By F. W. Headland, B.A., M.R. C.S., Lecturer on Botany. Printed by request. London, 1853.

Transactions of the Medical Association of Southern Central New York. Auburn, 1853.

Thoughts on Cholera. By Edwin Hearne, M.B. Lond. London, 1853.

Die Blatternkrankheit in Pathologischer und Sanitätspolizeilicher Beziehung. Von Chr. H. Eimer, M.D. Leipzig, 1853.

Handbook of Organic Analysis. By Justus Liebig, Ph.D. Edited by A. W. Hoffmann, Ph.D., F.R.S. London, 1853.

A History of British Crustacea. By Thomas Bell, F.R.S. (Part IX., completing the work.) London, 1853.

A Few Sober Words of Table Talk, about Table Spirits. By John Prichard, F.R.C.S. Eng.

On Diseases of the Rectum. By James Syme, F.R.S.E. Third Edition. Edinburgh, 1854.

Hooper's Physicians' Vade Mecum, or a Manual of the Principles and Practice of Physic. By W. A. Guy, M.B. Cantab., Physician to King's College Hospital, &c. Fourth Edition. London, 1854.

The Surgeon's Vade Mecum, a Manual of Modern Surgery. By Rob. Druitt, F.R.C.S. Eng. Sixth Edition. London, 1854.

Handbuch der Speciellen Pathologie und Therapie. Redigirt von Rud. Virchow, Professor der Medicin in Würzberg. Ersten Bandes erte Halfte. Erlangen, 1854.

Grundzüge der Pathologischen Histologie. Von Dr. Carl Wedl. Wien, 1853.

Clinical Lectures on Pulmonary Consumption. By Theophilus Thompson, M.D., F.R.S., Physician to the Hospital for Consumption and Diseases of the Chest. London, 1854.

Handbook of Chemistry, Theoretical, Practical, and Technical. By F. A. Abel, Professor of Chemistry at the Royal Military Academy, Woolwich, and C. L. Bloxam, formerly first Assistant to the Royal College of Chemistry. London, 1854.

On Fatty Degeneration. By the late W. F. Barlow, F.R.C.S. London, 1853.

The Pathology and Treatment of Stricture of the Urethra, both in the Male and Female Urethra; being the Treatise for which the Jacksonian Prize for the year 1852 was awarded by the College of Surgeons of England. By Henry Thompson, F.R.C.S., M.B. Lond., &c. London, 1854.

Surgical Anatomy. By Joseph Maclise, F.R.C.S. Second Edition. Fasciculi I. & II.

A Handbook of Inorganic Chemistry, for the Use of Students. By William Gregory, M.D., F.R.S.E., Professor of Chemistry in the University of Edinburgh. Third Edition, corrected and enlarged. London. 8vo, pp. 291.

The Remote Cause of Epidemic Diseases, or the Influence of Volcanic Action in the Production of General Pestilences. By John Parkin, M.D. Part II. London, 1853.

An Expository Lexicon of the Terms, Ancient and Modern, in Medical and General Science. By R. G. Mayne, M.D., Surgeon to the Leeds Lock Hospital. Part I. London. 8vo, pp. 152.

The Microscope in its special application to Vegetable Anatomy and Physiology. By Dr. Hermann Schacht. Translated by Frederick Currey, Esq., M.A. London. 8vo, pp. 131.

Anteckningar rörande de Förnämsta Medicinska Skolorna uti Italien, Frankrike, Holland, och England. Af Dr. Carl Santesson. Stockholm, 1853.

Handbook of Natural Philosophy and Astronomy. By Dionysius Lardner, D.C.L. Third Course. Meteorology, Astronomy. London, 1853.

A Treatise on the Venereal Disease. By John Hunter, F.R.S., with copious Additions by Dr. Philip Ricord. Edited, with Notes, by F. J. Bumstead, M.D. Philadelphia, 1853.

The Asylum Journal. Published by authority of the Association of Medical Officers of Asylums and Hospitals for the Insane. No. I. November 15, 1853.

Dysentery considered in a Pathologico-anatomical and practical point of view. By Dr. P. Bleeker, Netherland Medical Service. Translated by Dr. Theodore Cantor, Bengal Medical Service. Calcutta, 1853.

The Druggist's Handbook of Practical Receipts, a Manual for the Use of the Chemist and Medical Practitioner. By Thomas F. Branston. Liverpool, 1853.

The Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Second Series. Vol. 18. London, 1853.

Hufeland's Art of Prolonging Life. Edited by Erasmus Wilson, F.R.S. London, 1853.

The Positive Philosophy of Auguste Comte. Freely translated and condensed by Harriet Martineau. Two Vols. London, 1853.

The Diseases of the Heart and Aorta. By William Stokes, Regius Professor of Physic in the University of Dublin, &c. Dublin, 1853.

The Irish School of Medicine as it is, and as it ought to be; an Address Introductory to a Course on Pathological Anatomy. By Thomas S. Holland, M.D. Cork, 1853.

An Introductory Lecture at the opening of the Session of the Chatham Street School of Medicine, Manchester, October 3rd, 1853. By George Southam, F.R.C.S. Manchester, 1853.

On the Relation between Therapeutics and Pathology. A Course of Lectures delivered at the Royal College of Physicians in the Spring of 1853. By George Johnson, M.D. Londin. F.R.C.P. London, 1853.

APPENDIX.

Art. I.

The Outbreak of Cholera at Arbroath, in Scotland, in October, 1853. By Dr. T. Traill.

[The following communication and the appended remarks have been sent to us by Professor Alison, and are intended as additional evidence of the correctness of the opinions advocated in the review given in a former page. Our attention was also previously directed to the cholera in Arbroath (a town about 16 miles from Dundee) by Dr. David Anott, and the facts communicated by that gentleman are in entire accordance with those now forwarded by Dr. Traill to Professor Alison.

As this journal has taken an active part in the discussion on the causes of cholera, we may observe that Professor Alison, in his most able article, does not express any opinions which are at variance with those formerly advocated by us. We have never denied that cholera might be contagious; and we have expressly stated that it might be carried from place to place. We have contended, however, strongly, that the peculiarities of its spread render it impossible that its extension over the world can be accounted for by the limited hypothesis of contagion; we have argued that its spread by contagion is the rare exception, and its spread from other causes is the common rule. Dr. Alison has made a most lucid statement which approaches this opinion, except that he is evidently inclined to consider the spread by contagion to be more frequent than we had supposed. On such a point, strict and careful evidence, like that furnished in the Swedish report and by Dr. Traill, can alone suffice to show us the relative frequency of the different modes of transmission. The questions for discussion are, we believe, not whether cholera is contagious, or non-contagious? but how often it spreads by the agency of human bodies, and how often without such media?]—Editor.

1. The first case occurred at Arbroath, on October 29th, in the person of Mrs. Menmuir, a stout healthy woman, aged 55, residing in an open, airy locality, in the outskirts of the town, in a row of newly-built, one-story cottages, with small garden-plots behind. She had returned from Dundee on the previous day, where she had gone to wait on some members of her family, four of whom had died of cholera within four days. She had remained in Dundee two days. Died in 24 hours.

2. John Menmuir, husband of No. 1, had also been in Dundee. Attacked October 31; lived 24 hours.

3. Mrs. Christie, daughter of Nos. 1 and 2, residing in same row of cottages. Waited on No. 1: *had not been in Dundee.* Attacked Oct. 31; died in 30 hours.

4. James Forbes, residing in same street, nearly opposite to No. 1; had been repeatedly in house of No. 3, and assisted in taking care of her children. Attacked Nov. 1. Recovered.

5. Mr. George Winton, medical student, residing in a part of the town half a mile distant from the Menmuir's. Visited frequently Nos. 1, 2, and 3. Attacked Nov. 2: died after 13 hours' illness: had not been out of Arbroath for weeks before.

6. Christie, son of No. 3. Attacked Nov. 2. Recovered.

7. William Menmuir, son of No. 1. Attacked Nov. 3. Recovered.

8. Jane Christie, daughter of No. 3. Attacked Nov. 4. Recovered.

Nos. 6, 7, and 8, were attacked in the house of refuge to which they had been removed on Nov. 2. This completes the history of the Menmuir family, originally numbering sixteen, of whom ten were attacked by cholera—four in Dundee, all of whom died—six in Arbroath, of whom three died and three recovered; the seven deaths all occurring within eight days. All the other members had repeated attacks of diarrhœa and choleraic diarrhœa.

9. Harriet Coyle, æt. 2: Attacked Nov. 4. Died. This child was brought from Dundee two weeks previously, and had suffered from diarrhœa during that period.

10. William Frazer, half brother of No. 9. Attacked Nov.5, and died same day.

11. David Skygie, æt. 32. Attacked Nov. 5; died in seven hours. Remote from the other cases, and not known to have had communication with any of them.

12. John Smith. Remote locality. Not known to have had any communication. Attacked Nov. 7. Died in consecutive form.

13. Mr. Hutchen. Nearly in the same locality as No. 11, but not known to have had any communication. Died in consecutive form.

14. Alexander Nindle. New locality. No communication known.

15. Mrs. Hunter. Nurse in house of refuge, and also washed clothes in house of No. 11. Died within 24 hours. Nov. 10.

16. Mrs. John Smith, wife of No. 12. Nov. 10.

17. Mrs. Hovel. Nearly in same locality as No. 12 and 16, but no communication known. Died in 24 hours. Nov. 11.

18. George Rodger. New locality. No communication known. Nov. 11. Recovering.

19. Mrs. Foggo. Resident one-and-a-half mile out of town, in a solitary cottage. Went to Dundee on Saturday, remained there till following Monday, in her daughter's house in Dudhope-street (centre of cholera locality). Returned on Monday, was attacked while at work on Tuesday, and died on Wednesday, Nov. 16.

20. Edward Hutchen, husband of No. 13. Attacked Nov. 18; now in consecutive fever.

21. Anne Nash. New locality. Attacked Nov. 21. Died next day.

Number of cases in 23 days, from Oct. 29 to Nov. 21		21
Of whom have died	13	
Recovered	5	
Under treatment	3	21

Of these cases, four were probably imported from Dundee; ten were more or less in communication with one or other of these four; seven were *not known* to have had communication with any case of cholera. *Four* of these were attacked after removal to the house of refuge—a large, thoroughly ventilated modern house, previously used as an hotel. *All*, or nearly all the other inmates, suffered from diarrhœa, and several from choleraic diarrhœa—two of which ought probably to have been reported as cases of cholera. Several of the nurses also suffered from diarrhœa, and one of them, No. 15, died.

Localities in which the cases occurred, and sanitary state:

	Cases.
Crowgate Road (average)	7
East Wynd (bad)	2
Applegate (bad)	3
Crolls Wynd (very bad)	1
Guthrie Port (bad)	1
Hamilton Green (average)	2
Panmure Street (bad)	1
Marketgate (not good)	1
Panmure St. (bad)	1
Abbey Path (bad)	1
Denfield (country), average	1
11 Localities. Total	21.

Habits:

	Cases.
Dissipated	4
Very dissipated	2
Respectable	10
Children	4
Unknown	1
	21

We beg to fix the attention of our readers on the following facts, which are here completely ascertained, as to the introduction of the malignant cholera into Arbroath in October last, which exactly agree with what we have stated as to its introduction on former occasions into various towns in Scotland, considerably detached from others, and therefore affording satisfactory evidence on the subject. These facts, we confidently maintain, furnish *decisive evidence* of the disease having a contagious property, that is, of its being communicated *by intercourse of the healthy with the sick*.

1. The two first persons affected in the town of Arbroath (Nos. 1 and 2 of the above list, both of whom died) had just returned from Dundee, where they had visited persons suffering from cholera. The affection of these two persons of course proves nothing as to the contagious property of the disease, because it may be said that it was by inhabiting a "tainted district," not by intercourse with persons already sick, that they contracted the disease.

2. The *six* next persons who took the disease in Arbroath,—and the *six first* of the inhabitants of Arbroath who took it without having left the town (Nos. 3, 4, 5, 6, 7, and 8, of the above list), had repeated and close intercourse with the two first (Nos. 1 and 2), or with No. 3; that is, with persons already sick of the disease, before they took it.

3. All these six took the disease in the course of the week, between the 29th of October, when the first case, that of Mrs. Menmuir, occurred in Arbroath, and the 5th of November, when the case of David Skygie, the eighth in Arbroath who had not been in Dundee, and the first in Arbroath who had not had known intercourse with the sick, occurred.

Here, it will be observed, that in order to prove that the disease spreads by intercourse of the healthy with the sick, we do not only rely on the fact of a succession of cases, to the number of six, occurring among those who have had such intercourse, but on the fact of its occurring among them *only*, during a whole week, out of a population of more than 15,000 persons (the population of Arbroath having been 14,500 at the census of 1841), many of whom were undeniably more exposed to the action of every other external cause that has ever been assigned as influencing the disease, excepting only intercourse with the sick. This is exactly what we have shown to have happened in various other cases, when the disease has been introduced into a town in Scotland; and we maintain that a simple calculation of chances is enough to show that it could not have happened in any one of those succession of cases, if the circumstance of intercourse with the sick had not aided in determining the appearance.

W. P. Alison.

ART. II.

Observations on an Article in the 'Edinburgh Review,'* (*No.* 199), *entitled 'Quarantine, Small Pox, and Yellow Fever.' By J. O. M'WILLIAM, M.D., F.R.S., R.N., Medical Inspector to the Honourable the Board of Customs.*

THE subject of the epidemy on board H.M.S. *Eclair*, and on the island of Boà Vista, in the Cape de Verde, which has already on three distinct occasions been fully discussed in the leading medical journals of the United Kingdom, of France, and of America, has again been brought before the public in the 'Edinburgh Review,' for July, 1853, in an article entitled 'Quarantine, Small Pox, and Yellow Fever.'

On each of the three occasions to which I have alluded, the verdict of the press, in spite of all opposing efforts, has been uniformly and unanimously in favour of the views advanced in my 'Report on the Fever of Boa Vista,' presented to the House of Commons in February, 1846; and as it has been very generally admitted that the whole of this question has nowhere been so elaborately or so philosophically examined as in the pages of the 'British and Foreign Medico-Chirurgical Review,' (Nos. i., ii., iii., xx.,) I am desirous that the few observations I have to make on the article in the 'Edinburgh Review,' should obtain publicity through the same channel.

The Edinburgh Reviewer, to my mind, betrays a singular inaptitude for the important task he has undertaken, as his article throughout indicates a total ignorance regarding those epidemics whose nature and qualities he has presumed to determine. But even had he been well informed on these points, the recklessness of assertion, the disingenuousness in representing facts, the suppression of matters essential to the elucidation of the truth, and the onesidedness of argument which pervade the whole article, would of themselves be fatal to anything approaching to a fair, just, and impartial exposition of Small Pox and Yellow Fever, in their relation to the Quarantine Laws.

From a reviewer whose object is truth, and truth alone, and whose information, impartiality, and ability, render him equal to treat fairly and properly the subject of the quarantine laws, as applied to yellow fever, or any other epidemic disorder, we naturally look for a candid and unreserved statement of the evidence adduced on both sides of the question, and for due consideration of the results of those laborious and important investigations which quarantine has of late years undergone at the hands of competent inquirers; such as the committee appointed by the States of New York, and the 'International Sanitary Conference,' which

* We need scarcely remind our readers that on the outbreak of yellow fever at Boa Vista in 1845, Dr. M'William, an officer greatly distinguished in the unhappy Niger expedition, was sent out by the Government to investigate the origin of the disease. Dr. M'William went out to Boa Vista without bias, or, if anything, with a leaning towards the hypothesis of the non-contagion of yellow fever. On his arrival at the island, he examined personally every one who seemed able to afford information. Every single question and answer was printed in his Report, and from this evidence he proceeded to draw his conclusions

It is competent for every one to peruse and collate this evidence, without reference to Dr. M'William's conclusions. We have already done so, and have given in previous numbers the results of our inquiry. In coming to our conclusions, we were entirely free from personal bias, for we were quite unacquainted with Dr. M'William, and our opinions, from training and previous study of the subject, had been anti-contagionist. But we found the circumstances of this case so extraordinary, that after full collation and sifting of the evidence, we abandoned our previous opinions, and admitted of the importation of yellow fever into Boa Vista by the *Eclair* steamer. Nothing that has since appeared has altered our convictions, and we have little hesitation in affirming that few candid men will go over this controversy without agreeing with Dr. M'William.

It is with deep regret that we have seen an honest observer like Dr. M'William—a man who has deserved well of science and of his country—treated with obloquy and derision by the partisans of an opposite opinion. This is not the way in which a scientific question should be judged, nor is it the treatment which Dr. M'William should have received from those in authority. But personally the matter should be of little consequence to him. He has done his duty, and may safely commit his cause to the judgment of his profession.—EDITOR.

met at Paris in 1851, composed of representatives from the Governments of England, France, and ten other European powers.

In 1845 the House of Assembly of New York appointed a committee to report on the quarantine laws. The committee particularly investigated the question as to the necessity of yellow fever being guarded against by quarantine. The committee "sought information from all sources within their power," and interrogated executive and medical officers of the navy, commercial men, and physicians of reputation and experience in civil practice. In short, as is stated in the able report of the committee, presented to the House of Assembly in January, 1846, "every one in the city had an opportunity of being heard on the subject of quarantine, or public health." Here, from the genius and tone of commercial enterprise among the people, there could be no undue leaning in favour of restrictive measures, yet the committee, from the evidence brought before them, more especially from the fact that yellow fever had not appeared in New York for nearly a quarter of a century, and not since quarantine was so rigidly enforced, arrived at the conclusion "that a vessel arriving at New York from a port where the average temperature is about 80° Fah., may bring the yellow fever, if the yellow fever is prevailing at the port of her departure, or if she lays alongside a vessel infected with yellow fever, or if the crew or persons on board are otherwise exposed to it" (Report, House of Assembly, p. 44.) The House therefore, on the recommendation of the committee, imposed a quarantine of thirty days after arrival at New York, and at least twenty days after discharge of cargo, upon all vessels arriving (between the 31st of May and the 1st of October) direct "from a place where yellow fever, bilious, malignant, or other pestilential or infectious fever, existed at the time of their departure, or on board of which, during the voyage, any case of such fever shall have occurred."

The regulations of the 'International Sanitary Conference,' held at Paris in 1851, as regards quarantine upon vessels arriving at a European port, from any place where yellow fever is prevailing, are to the following effect:

1. "Traversée heureuse; minimum [of quarantine], cinq; maximum, sept jours.

"Le minimum de cinq jours pourrait être abaissé a trois jours, si la traversée avait duré plus de *trente* jours et si le bâtiment était dans de bonnes conditions d'hygiène."

2. "S'il y a eu des accidents pendant la traversée; minimum, sept jours; maximum, quinze jours."

3. "Pour les cas extraordinaires, d'une gravité exceptionnelle, et en dehors de toute prévision, les mesures seraient établies d'après ce principe supérieur à toute règle, *Salus populi suprema lex*—et laissées au jugement de l'autorité sanitaire, sous sa responsabilité."

In this country, also, the question of the contagiousness of yellow fever, and its capability of being imported, has recently been the subject of investigation by a board of army medical officers, convened by order of the late Duke of Wellington, and by a committee of the Royal College of Physicians of London, appointed by the College in consequence of a communication from the Lords of Her Majesty's Most Honourable Privy Council.

The Army Medical Board, by a majority of four to one, were of opinion that "yellow fever is a contagious disease, and may be imported." The Committee of the College also arrived at the conclusion "that it appears to the College to be sufficiently proved that yellow fever is, under certain circumstances, infectious, and, consequently, that it may be imported;" and added, "that the disease has been in some instances imported, *the history of the epidemic fever which occurred in Her Majesty's ship Eclair, and at Boa Vista, in* 1845, *affords conclusive evidence*."

It will scarcely be believed that, at the present day, an article professing to treat the subject of quarantine should appear in a prominent and influential journal without the slightest notice being taken of the special labours of the committee of the House of Assembly of New York, or of the International Sanitary Conference, or of the Board of Army medical officers. Why these able and important documents should have been unnoticed by the reviewer seems explicable only on the supposition, either that he was ignorant of their existence, or that they were laid aside by him because they were unsuitable to his purpose, as containing facts which, coming from sources so highly responsible and deserving of respect, would have stood in awkward juxtaposition with the opinions of those authorities to which he professes to give his faith.

In either case, the omission must be regarded as a signal proof of his disqualification to do justice to the subject of quarantine. The Report of the College of Physicians is indeed once alluded to; but then is only mentioned in order that the reviewer may indulge himself in a coarse sneer at the College. How often do we find ignorance and insolence going hand in hand!

In the same way that a just exposition of the general question of the Quarantine Laws requires that the facts which tell *for* as well as *against* their further maintenance should be fairly and unreservedly stated, so in like manner is it necessary to deal with individual cases, in which the endemic or foreign origin of an epidemy becomes the subject of discussion. In order, therefore, to test fairly the question, as to whether the Boa Vista yellow fever epidemy of 1845-6 was indigenous or imported, it would seem indispensable that every document (more especially every official document) relating to that outbreak, and written shortly after its occurrence, should be subjected to rigorous and searching investigation. This does not appear to have been the course adopted by the reviewer, as he discards altogether, for reasons best known to himself, Dr. King's official report, presented to the House of Commons in March, 1848. In like manner he omits all notice of the letters of Sir William Pym on the reports of Dr. M'William and of Dr. King, "on the fever of Boa Vista," both letters being addressed to the lords of the council, and presented to Parliament in April, 1847, and May, 1848, respectively. Nor does the reviewer once allude to my 'Remarks on Dr. King's Report,' published in April, 1848, or to my 'Observations on the Second Report on Quarantine, by the General Board of Health,' published shortly after the appearance of that official document. As regards the Boa Vista epidemy, the reviewer, in fact, has deemed it expedient to limit his sources of information to the 'Report of the General Board of Health on Quarantine,' and to a book by Dr. King, entitled, 'The Fever of Boa Vista unconnected with the visit of the *Eclair* to that Island,' published some time in the latter end of last year, that is, *six years after the event, and about five years after the publication of his official report.*

Upon these alone, and by a sweeping denunciation of my 'Report,' as well as by a careful avoidance of all approach to the chief and distinguishing features of the case, he endeavours to make it appear that the yellow fever at Boa Vista was not introduced by the sick crew of the *Eclair*.

It is doubtless in the recollection of those who have followed up the Boa Vista controversy, that the only effect of Dr. King's report was, by the statements it contained (although the conclusions were wholly opposed to mine), to confirm the main facts assumed in my own Report. That this was so, and that it was the opinion of the professional press here and elsewhere, will be evident to any one who will take the trouble to go over the Reports, and to consult the medical journals of the period. A glance at the following table will moreover show, that although Dr. King's inquiries at Boa Vista took place nine months after mine, there was a remarkable and important coincidence between our Reports, as to the periods at which the various districts of the island were invaded by fever.

Names of Places.	Dr. King.	Dr. M'William.
Fort on small island	Sept. 16th or 17th, 1845	Sept 16th or 17th, 1845.
Porto Sal Rey	About October 12th	Oct 12th.
Moradinha	Sept. 14th	Sept. 17th.
Rabil—Cabeçada	About October 14th	End of September.
Boaventura	Not mentioned	Nov. 15th, 1845.
Estacia Velha	Nov. 15th, 1845	About Nov. 15th.
Estacio de Baxio	Not mentioned	End of November.

Names of Places.	Dr. King.	Dr. M'William.
Eastern villages:		
Cabeça dos Tharafes	October 27th	End of October.
Fundo das Figueiras	October 31st	Nov. 19th.
Joao Gallego	Nov. 1st	Nov. 1st.

It thus appears that as regards the dates of these occurrences, there is a remarkable agreement between the two Reports. In his recent production, however, Dr. King makes several statements at variance with fact, and wholly irreconcilable with some of those contained in his Report. But as the errors and inconsistencies of Dr. King have elsewhere been exposed in detail, I need not, in the following remarks, do more than allude to them incidentally.

In discussing the objections professed to be raised by the author of the article in the 'Edinburgh Review,' against the importation of the yellow fever into Boa Vista by the sick crew of the *Eclair*, I shall first extract their substance as briefly as may be consistent with the employment of the reviewer's own words, and then endeavour to dispose of them *seriatim*, as nearly as possible in the order in which they occur in the review.

At page 208, the reviewer states that the *Eclair*, "on the 20th August, arrived at Boa Vista, which is in the yellow fever zone;" that some of the officers "and their servants were attacked with fever while living in the town; and that the captain was taken ill in the house of an English family, and the purser in that of Mr. Kenny;" "that the water for cooking and washing was of the worst description; the meat supplied to the fort was diseased and unwholesome; and that the cattle on the island were dying in great numbers—one of the recognised premonitory signs of an impending epidemic—and, "lastly," that "in the early part of September rain had begun to fall."

The inference evidently intended to be conveyed in the first sentence here quoted, is to the effect that as Boa Vista was in the "yellow fever zone," the occurrence of a yellow fever epidemy there was nothing remarkable. But there is another geographical fact, of some importance in this question, upon which the reviewer is silent—viz., that all the other islands of the group, nine in number, and within a few hours' sail of each other, are also in the "yellow fever zone."

This propinquity of the islands to each other is worthy of being borne in mind from the beginning to the end of the inquiry, from the very important and remarkable fact, that although they *all* possess the same geological structure, nearly the same kind of population, and are subject to the same meteorological influences, *none of them suffered from yellow fever except Boa Vista, the island at which H.M.S.* Eclair *had previously arrived with that disease prevailing among her crew.*

It was indeed asserted, in one of the authorities of the 'Review'—the Second Report on Quarantine "by the General Board of Health"—that "in the adjoining island of San Jago there was yellow fever while the *Eclair* was at Boa Vista," a statement which the reviewer has not ventured to repeat. After the refutation of this most erroneous statement of "the General Board of Health," by the evidence of his excellency, the governor-general, the late Mr. George Miller, of San Nicolas, Mr. Rendall, the English consul, Dr. Nunes, and Senhor Baptista, of San Jago, perhaps it is unnecessary to say more on the subject. But in case it should be again taken up, in the excess of zeal, by some imprudent partisan, it may be well here to state that Captain Simpson, of the *Rolla*, the main authority of the Board of Health on this part of the question, now admits, that "as he heard nothing of black vomit" at San Jago, his informant most probably meant merely severe cases of the remittent fever which annually prevails more or less at Porto Praga, and that Dr. Macfarlane, who was surgeon of the *Rolla* at the time alluded to (1845-6), also writes to me to say, "I am unable to conjecture how Captain Simpson could think that yellow fever prevailed at Porto Praga, at any time, as until the night of your communication *I never heard of a single case of yellow fever having occurred at any of the Cape de Verde Islands, except Boa Vista.*"

Dr. Stewart, deputy inspector of Malta Hospital, also writes me to say, "I do not know who could have told me about yellow fever at San Jago—possibly it was Captain Harston, of the *Eclair.*" Captain Harston, however, replies: "I never told Dr. Stewart, or any one else, that there was yellow fever at San Jago. I could not have done so, for I never heard of such an occurrence, nor do I believe it."

It is to be hoped that the question as to the alleged prevalence of yellow fever at San Jago, contemporaneously with the epidemic at Boa Vista, is now set at rest, and that the assertion of the General Board of Health must be abandoned, even by themselves. As respects Captain Estcourt and the other officers who were living in Porto Sal Rey, Boa Vista, as I have elsewhere stated, when the captain was taken ill there were black servants only in the house of Mr. Macaulay, and even against their being infected Captain Estcourt took precautions. The statement that "some of the officers and their servants were attacked with fever while in the town," immediately on its being seen by Mr. Macaulay, called forth from him the following observation: "This passage conveys a very erroneous impression. The rule laid down by Captain Estcourt, *and strictly followed in every instance*, was, that if any of the officers or servants living in the town were taken ill, they should *immediately* be conveyed to the hospital on the small island. This was the conduct actually pursued by Captain Estcourt, when he was attacked a short time afterwards."

Even supposing the statements of the 'Review' as to the attacks of the captain and officers to be correct, they could be regarded only in the light of mere negative evidence: but in the main they are wholly incorrect.

The water used by the *Eclair* crew at Boa Vista was procured from the same source as that from which the inhabitants had been supplied from time immemorial. The meat issued to the crew, which, on the authority of Dr. King (in his recent work, but not alluded to in his Report), is said by the reviewer to have been "diseased and unwholesome," was of the very same quality as that with which the tables of the governor-general, of the consul, Mr. Macaulay, and others, were provided; and in no case was it complained of. But to talk of a ship's company of an English man-of-war eating "*carrion,*" betrays either ignorance or wilful misrepresentation.

Unwholesome or tainted meat is, in every case, surveyed by the proper officers of the ship, and, if necessary, condemned. That the "cattle were dying in great numbers" is altogether untrue, and is contradicted by the evidence of those who had the best opportunity of judging. The cattle did not begin to die until the fever had in a great measure left the island; and even then, the consul says, "there was no unusual amount of illness among the cattle. It is the case, more or less, every year, at the end of the dry season, and after the new grass springs up." Moreover, will any sane person assert, that even had the water and the meat been of the worst possible description, either, or both, could have been the cause of yellow fever? For the state of the weather while the *Eclair* was at Boa Vista, I would only refer to the evidence of the consul, of Captain Buckle, and Dr. Carter, of the *Growler*, all of whom kept meteorological registers, which will be found to confirm what I had already heard from the late Mr. Macaulay, the Hon. Mr. Macaulay, Mr. Pettingall, and others—that the weather was never more beautiful than at the period in question. Even Dr. King did not invoke a vitiated and malarious atmosphere earlier than the "end of September or beginning of October," while by that time several persons are admitted to have been attacked, and it has never been denied that two Portuguese soldiers on the small island, engaged in guarding the sick, were both dead from yellow fever on the 21st of September.

The reviewer, at page 210, goes on to say: "All competent witnesses who actually saw the disease state that it was nothing more than an aggravated form of the common endemic fever of the African coast."

How does this affect the question at issue? There are many observers, of admitted competence, who consider yellow fever with black vomit an aggravated form of the endemic fever of the African coast. The report of survey held on the crew of the *Eclair*, by Dr Carter, Mr. Machonchy, and Dr. M'Clure, and dated at Boa Vista, Sept. 13th, 1845, specially states, "*the extremely malignant character of the fever, which has resisted the treatment usually found successful in the common endemic form of the coast.*"

Even Sir William Burnett assigns contagious properties to the fever at this period, for he thereby accounts for the fatal attack of the lamented Dr. M'Clure, which took place the very day on which the survey was held. (Admiralty Correspondence on *Eclair*, p. 55.)

At page 210-11 the reviewer observes: "The consul admits that, up to the 9th of October (when the fever began to show itself), extraordinary heat and the fall of a large quantity of rain had been experienced. The two Portuguese surgeons maintained that the fever arose from stagnant water, and held the same opinion until the 20th of November. The presence of a contagious epidemic was not found out until the 20th of November, whereupon the principal inhabitants, with the governor-general, quitted the island. The appearance of the earlier cases had excited neither surprise nor alarm, because they could not be distinguished from the ordinary autumnal fever of the island."

The substance of the first two sentences here quoted is taken from a despatch of the consul to the Earl of Aberdeen, dated at San Antonio, Dec. 22nd, 1845, and it may be well to take a few other extracts from the same despatch, to show how far the statements of the reviewer in the two last sentences are borne out by the facts there represented. The consul continues: "The anxiety which this vessel (*Eclair*) gave me, *during her lengthened stay here, was, I assure your lordship, of the most painful description.* The daily reports of deaths, the belief that the fever was of the worst description, and the fear of subsequent consequences, were all of the most distressing nature."

"The fears of the people had not subsided at the events already recorded, when it was reported *on the 20th September, seven days after the steamer had left, that one of the white Portuguese soldiers, who had been housed at the island with the crew of the* Eclair, *had died in the fort. The following day another also died, and the remaining soldier, a coloured man, was reported sick. Another coloured soldier was sent to assist his comrade, but who was also taken sick.*"

According to the reviewer, the epidemy at Boa Vista was not contagious, because it was not until the 20th November that the Portuguese surgeon declared it to be so. Conviction upon such ground is not likely to extend far beyond the reviewer, and he may, therefore, be safely left in the possession of his own opinion. I might, however, commend to his notice the *recorded opinions* of the Portuguese surgeons, the governor general, and others (who at first were inclined to blame the stagnant water), founded upon a calm and deliberate opinion of the whole case. Why has the reviewer so studiously avoided all allusion to the deaths of the Portuguese soldiers from yellow fever within a week after the departure of the *Eclair;* or to the illness of the coloured soldiers; or of Pathi's family, all in the month of September?

He can deal only in the most vague generalities; and these, even, are anything but faithfully stated. An appeal to individual cases, with the dates and order in which they occurred, would have caused him insuperable embarrassment.

At page 210, the reviewer says, 'Until November 20th, free intercourse with the other islands had subsisted without any communication of the disease to them; and even so late as the 27th January, 1846, the governor general informed the Portuguese consul general at Gibraltar, that the disease was perfectly endemic, for not one of those who emigrated to the different islands had the disease, or communicated it to others."

It is quite true, that until November 20th there was free intercourse between Boa Vista and the other islands of the group. But up to this time no sick person had left Boa Vista; and no sick person (with one exception, in which quarantine precautions were taken) was, at any time throughout the whole epidemy, landed on any of the other islands. It was asserted, it is true, in the 'Report of the General Board of Health,' that "sick" had fled from Boa Vista to the other islands of the archipelago. But the assertion was wholly unfounded. As regards the opinions of the governor general, his letters to Mr. George Miller, to Dr. King, and to myself, are extant, to prove that his excellency *felt convinced* that *the fever was contagious from its commencement, and during its progress on the island.*" (See Dr. M'William's Remarks on Dr. King's Report, p. 10.)

At page 211, the reviewer continues, "The examinations of the witnesses whom he (Dr. M'William) examined, are dated generally April 26th, seven months after the departure of the *Eclair*. We have already shown the enormous difficulty of tracing the progress of an epidemic from person to person, supposing it to be propagated in that way: but it is hardly possible to imagine conditions more unfavourable to the elucidation of the truth, the whole truth, and nothing but the truth, than those under which Dr. M'William's inquiry was conducted."

It is granted that it would have been better had the witnesses been interrogated at an earlier period after the departure of the *Eclair* from Boa Vista; but it must also be allowed, that in this respect my investigations were much less disadvantageously conducted than those by Dr. King, seeing that I preceded him on the island by eight or nine months, and it was no fault either of Sir William Burnett or myself, that I was not there at even an earlier period. It is, however, to be borne in mind, that when I reached Boa Vista the first outbreak was not yet finished, and that the main incidents of the epidemy were but too vividly in the recollection of the people who had witnessed them; and that my information was obtained not only from the "illiterate, semi-barbarous inhabitants" (as the reviewer is pleased to designate them), but also from intelligent and well-educated persons of the higher classes, including Portuguese and English, some of whom had moved in and not discredited the most intellectual society in London. Every unprejudiced person will, I imagine, differ with the reviewer as to the alleged unfavourableness of the conditions at Boa Vista for elucidating the truth. The salient points of the epidemy stand out in so prominent relief as hitherto to have defied the attempts of artifice to distort, or in any wise to disfigure them; and I still feel as forcibly as ever the justice of what I have elsewhere stated—"that throughout the whole history of yellow fever, not even at Ascension, have its properties been discussed on grounds in every respect so favourable for arriving at a positive result. In all former controversies regarding the importation of yellow fever, the battle-ground has been a large and populous city, generally a seaport town, where arguments, derived from doubts as to the previous absence or presence of disease, the varied types and modes of living of the inhabitants, the effect of tidal harbours, putrefying cargoes, and animal and vegetable matters of all kinds; in short, endless influences, real or imaginary, have been enlisted into the service of the discussion, and employed with an earnestness that gave evidence of the zeal if not always of the judgment of the parties engaged. Here, fortunately, we have no such complication. We have to deal with a narrow strip of rock within half a mile in length, and rarely tenanted by more than three individuals." (Dr. M'William's Remarks on Dr. King's Report, p. 6.)

The reviewer, at p. 211-12, states: "On this miserable African island there had originally been eighty-six Europeans, of whom, at the time of Dr. M'William's visit, thirty-two had died, and many others had left the island. The rest of the population were the most ignorant, wretched, and debased, of the human race. There was no register of deaths, burials, and other occurrences; in fact, no record of any kind upon which an able inquirer after truth could rely with certainty, and by which he could test the credibility of the statements made to him by the illiterate, semi-barbarous inhabitants, several months after the occurrence had taken place—people who had created all a common interest, a strong personal interest, in making out such a case as would further their common object, in securing ample damages from Great Britain."

The character of the inhabitants, as here drawn, is a fancy sketch of the reviewer—an exaggeration of the picture by which Dr. King, who saw little or nothing of these people, endeavoured to represent them. As a more faithful delineation of what they really are, I may here cite the published opinion of a lady of acknowledged attainments—literary and general— who lived several years on the island: 'They are a simple, gentle race, very susceptible of kindness, skilled in all the arts of peasant life, very apt in acquiring knowledge, (as she had many opportunities of observing in a school which she collected and taught among them,) and, according to their measure of instruction, unaffectedly influenced by religious feeling."—'Slavery, Past and Present,' by a Lady, pp. 15, 16.

As regards the allegation that the inhabitants tried to make out a case for damages from Great Britain, the leading features of the epidemy, so far as relates to the introduction of the fever into the island by the *Eclair*, is clearly made out, independently of the evidence of a single native. I need not even cite the testimony of the Portuguese, but I can appeal to the evidence of the English who were on the spot when the occurrence took place, in support of what I have stated. The English consul, Mr. Rendall, says, "In respect to the Boa Vista fever, I am ready to verify every statement you have made."

If, then, the inhabitants of Boa Vista leagued together to extort money, on false pretences, from Great Britain, we cannot exclude our own countrymen—the representatives of our own government, from being participators in the same combination.

The assertion that there was no register of deaths, &c., is simply incorrect. Had the reviewer deemed it expedient to consult my 'Report,' pp. 18, 19, or even Dr. King's 'Report,' p. 16, he would have found the dates of the deaths of eleven soldiers (nine of them European Portuguese), as extracted from the register kept in the office of Major Mascarenhas, the military commandant. He might there have discovered the dates of the deaths of the two first cases among the soldiers of Boa Vista—*the one being on the 20th, and the other on the 21st of September.*

Again: Is there not a very marked agreement between Dr. King and myself regarding the dates of the invasion by fever of Porto Sal Rey, Rabil, Moradinha, the Eastern villages, and other places on the Island? And surely the reviewer can hardly believe, that on determining these or any other points, there could be any collusion between the two reporters.

At page 212 the reviewer adds, "But this gentleman (Dr. M'William) laboured under still greater disadvantages; he was a perfect stranger to the place, ignorant alike of the habits, character, and peculiarities of the people. * * * Witnesses of the vilest class deposed, &c., &c. * * * To this was added—the only medical man of the place being dead—the testimony of a person, who kept a general store, and, among other things, sold a few drugs, and gave advice gratis. Such were the witnesses who convinced Dr. M'William that the epidemic which broke out at Boa Vista on November 20th, 1845, had been brought thither by the crew of the *Eclair*, in the preceding August."

It appears that the reviewer cannot be right, even by chance. It so happens that I was not ignorant either of the habits, character, or peculiarities of the people; but I trust the ignorance of the reviewer on those points has been already sufficiently demonstrated. The description here given of Dr. Almeida is, like that which the reviewer gives of the people generally, taken on the authority of Dr. King, who, in his 'Report,' cites the opinion of Dr. Almeida rather triumphantly, on more than one occasion, in support of his own statements. No sooner, however, did Dr. Almeida give an emphatic denial to what Dr. King had represented him as stating, than he shrunk down at once in Dr. King's estimation, into "a mere storekeeper and dealer in drugs." But I hold that, besides Dr. Almeida, there were many intelligent persons at Boa Vista while I was there, including Major Mascarenhas, Senhor Carvahal, Senhor Joào Baptista, Senhor Tavares, Senhor Librao Administrador do Concelho, and others. The evidence given to me by the lower classes, in every essential point, derives ample corroboration from that of the upper ranks—Portuguese, natives, and English. Among the last named, I may mention the consul, vice consul, the late Mr. Macaulay, the Hon. Mrs. Macaulay, and Mrs. Pettingall.

Such were the sources from which I obtained that certain information, which proves, beyond all reasonable doubt, that the history of the epidemic at Boa Vista fulfils every condition upon which evidence of the infectiousness of a disease is supposed to rest—viz.:

The healthiness of the island before the arrival there of the 'Eclair,' with yellow fever on board.

The outbreak of yellow fever among the inhabitants of the island (where that disease in the memory of man was unknown) speedily afterwards, while the other islands of the Cape de Verde remained unaffected.

The fact, that the first cases were in those persons who were brought into contact with the sick of the 'Eclair.'

The immunity from the disease of distant villages for long periods, until the arrival in them of infected persons, and the introduction of the disease in every district, from infected "foci."

The comparative immunity from the disease obtained by persons who adopted common, but partial precautionary measures against infection.

The absolute immunity from the disease, procured by persons who adopted strict measures of isolation and segregation, which measures must have failed, had the disease depended upon a general atmospherical cause.

According to the reviewer, the epidemic did not break out at Boa Vista until November 20, 1845; while the *Eclair* arrived there in the preceding August. In answer to this gross misstatement, I may adduce evidence to which the reviewer cannot reasonably demur—viz., Dr. King's Report, according to which the different parts of the island were invaded by fever at the following periods:

Fort on Small Island, off Boa Vista, Sept. 16th, or 17th, 1845.
Porto Sal Rey, on the Island of Vista, about October 12th, 1845.
Moradinha, on the Island of Vista, Sept. 14th, 1845.
Rabil—Cabeçada, about October 14th, 1845.
Estacia Velha, November 15th.

EASTERN VILLAGES:

Cabeça dos Tharafes. October 27th.
Fundo das Figueiras. October 31st.
Joào Gallego. November 1st.

In the name of all that is fair and honourable, I ask, how could the reviewer state that the epidemic did not commence until Nov. 20th, when, according to the testimony of Dr. King, as well as of myself, the fever had extended *over even the most distant parts of the island, in all directions, long before that period.*

Are we to believe that such an assertion was made in ignorance, or that it proceeds from a reckless desire to accomplish an end at all hazards, even to the sacrifice of truth?

At page 213 continues the reviewer: "Having succeeded in obtaining from Great Britain a grant of money and supplies of different kinds (in the distribution of which Dr. M'William appears to have played the popular part of almoner), in compensation for the losses inflicted upon them by the *Eclair*, the people of Boa Vista were encouraged to repeat an experiment which, on its first trial, had proved so successful, when the sickly season returned, after the departure of Dr. M'William in the following year. This gentleman addressed, in Nov., 1846, a letter to Sir William Burnett, announcing the reappearance of the yellow fever at Boa Vista, on the authority of a most respectable and intelligent merchant of the neighbouring island of San Nicolao, who had written to inform Dr. M'William that some persons had died, and others were sick of the disease. Dr. M'William suggested that the Director-General might possibly deem the case sufficiently urgent to recommend assistance being sent to Boa Vista, and offered his own services on the occasion, which Sir William Burnett declined, but "advised the Admiralty to send Dr. Gilbert King as medical inspector to Boa Vista. On the 23rd December the *Sphynx* man-of-war, with Dr. King on board, anchored at Boa Vista. Two boats approaching from the shore, were ordered to "lie off," and when questioned as to the sanitary condition of the island, the answer was to the effect "that the fever (the second epidemic) had carried off a great number of persons, some of them of respectable station. The disease was raging in Porto Sal Rey and the different villages throughout the island, and some were dying every day." The reviewer next proceeds to state that Dr. King, on landing, found that "besides a case of rheumatic fever," there was "*only one case of endemic fever in Porto Sal Rey*, and that in a few days after he had sufficient reason for believing that "*every other part of the island was equally healthy*."

Any one unacquainted with the case, upon reading these statements, might be led to suppose that there had been no outbreak of fever at all on the island after my departure, and that, in short, the letter written to me by Mr. George Miller, and the reports afterwards heard by Dr. King at Madeira, were wholly unfounded; nothing more than pure fabrications, devised for the purpose of extorting the sympathy and aid of the British Government, in the form of pecuniary compensation. Here, again, truth must dispel the illusion which these sentences of the reviewer convey. In three days after my departure (July 15th, 1846) fever again broke out in Porto Sal Rey. By Dr. King's own account, not a drop of rain fell until a month afterwards; and the first case occurred in one of the best and cleanest houses of the town (that in which Dr. King himself afterwards resided). The disease soon spread, attacking many persons in Porto Sal Rey; and long before Dr. King's arrival, had proved fatal to eleven persons in that town alone, including some of the principal inhabitants, viz., Mr. Macaulay, Senhor Hypolito Almeida, Senhor Francisco Spencer, Senhor Martines, and others. I have no means of ascertaining the number of attacks and deaths in the villages; but presumptively, they were as great in proportion as at Porto Sal Rey. Were these occurrences, coupled with the horrors already experienced by the people of Boa Vista in 1845, not sufficient to excite alarm? Could no complaint or appeal go forth from them, without their being subjected to the charge of fraud and dissimulation of the most degrading kind?

But in truth, the people of Boa Vista made no complaint except to their own governor-general, and then only for some food to relieve the necessities of the poorer and more distressed of their countrymen. Nor was it *till months after the second outbreak that they received any pecuniary compensation from the British Government;* so that the statement of the reviewer, that as they "had succeeded in obtaining a grant of money from Great Britain," on account of the epidemy of 1845, they were induced to repeat the experiment in 1846, is simply at variance with fact. No survivor among the English at Boa Vista will but feel ashamed that a calumny so cruel and so unfounded should have proceeded from one of his own countrymen against the inhabitants of that island. Had Mr. Macaulay been alive, no voice would have been louder or more indignant than his in refutation of so unjust an accusation.

With respect to the allegation that "John Jamieson exaggerated and misrepresented the state of sickness of the island when the *Sphynx* arrived at Boa Vista," I have to observe, that the man is now in this country, and denies that he gave the description of the then state of things as represented by Dr. King Jamieson's statement, that "the second outbreak had carried off a great number of persons, some of them of respectable station," cannot be controverted; and although Dr. King, in his recent work, says that he found the island *everywhere healthy a few days after his arrival*, it must not be forgotten that in his Report he talks of treating *fourteen or fifteen cases of fever* during his sojourn there of six weeks.

But supposing we are to grant that Jamieson made the statements which are imputed to him, what can they have to do with the sickness and mortality, which are not denied, even by the reviewer. And what are we to think of Dr. King's sense of public duty, when we find him afterwards employing, in the most confidential manner, in the exclusive investigation, in short, of the most important facts of his Report—a man whom, at the first interview, he had found guilty of exaggeration and falsehood!*

The course of conduct by which, according to the reviewer, a "clear-headed and candid inquirer" ought to be guided in investigations of this kind, remains yet to be noticed.

With a ludicrous gravity, equalled only by the ignorance which it discloses, he propounds the following as the "formula" of what my Report *ought to have been.* "All is uncertainty: unfortunately, the materials for a history of this epidemy do not exist. No facts come under my own observation: even my meteorological and other observations, carefully made during the healthy months of April, May, and June, during the year 1846, are utterly valueless for the purposes of this inquiry. Candour obliges me to warn the reader that they furnish no criterion whereby to judge of the meteorological or other causes of pestilence, which may have existed during the sickly season of 1845."

During the so-called *healthy months* of April and May, 1846, cases of yellow fever, several of them ending fatally, occurred at Porto Sal Rey, Rabil, and Joào Gallego; at the last place alone there were eight cases in April. There were also cases at Moradinha in June. Charity may here interpose for the reviewer, and plead his ignorance of these occurrences. But even then, it is incumbent upon him to invoke "meterological," *or* "*other causes*," to account for these attacks of, and deaths from, yellow fever during this alleged "*healthy season.*"

But the reviewer's knowledge of meteorology is evidently upon a par with his knowledge of the laws of epidemics, else he could not have committed himself to the absurdity of supposing that a meteorological cause (except in the case of a rotatory hurricane, the falling of a meteorolite, or some other wonderful and unusual phenomenon, which might give rise to accidental local mischief) could produce disease in one only of a group of islands lying in the sweep of the trade winds, and in sight of each other, without similarly influencing the others. A very slight acquaintance with the science of meteorology would have suggested to him that, in the absence of continuous observations for a lengthened period, observations carefully taken, and recorded during three months, at any given place, throw much light upon its general climate, during the same season every year, more especially in intertropical countries, where climacteric variations are less during a succession of years than elsewhere; and that, as regards the air temperature, a main element in the estimation of the climate of a place, most important information regarding its annual average mean, may be obtained in the course of twenty-four hours, or even less, by the meteorologist moderately skilled in the use of his instruments.

The personalities of the reviewer as to the mode in which I conducted the inquiry at Boa Vista, and general merits of my Report, as well as his allusion to the "*lenient* censure" passed upon the Report by the director-general, are scarcely worthy of notice. The best answer (were any needed) to them is to be found in the verdict returned by the professional press throughout Europe and America, after the Report had undergone the ordeal of criticism by that more enlarged, and certainly not less impartial tribunal.

It was then that commendation beyond the heart's content was substituted for *censure;* and that the *censurer*, in his turn, became the *censured.*

* In my "Observations" on the Second Quarantine Report by the General Board of Health, I stated (p. 22), "As respects Porto Sal Rey, the mode in which Dr. King obtained his evidence there was chiefly, if not wholly, thus:—His list of queries was written down, and John Jamieson, the consul's storekeeper, *alone* went the round of the houses, getting answers to those queries in the best manner he could, and then brought them to Dr. King, who *transcribed them thus obtained into his own journal or note-book.*" To this Dr. King, in his recent production (p. 101) thus replied: "It is quite correct; the information was obtained in that manner, and transcribed into my journal."

I feel it would be unjust to that profession of which I am a most humble member, did I conclude these remarks without declaring my belief that the article "On Quarantine, &c." which the editor of the 'Edinburgh Review' has unwarily published, would never have been allowed to appear in a medical journal, conducted with talent and respectability.

The ignorance of medical subjects, the careful evasion of the real merits of the question, and the paltry special pleading, everywhere apparent in the article, to say nothing of the tone of arrogance and disrespect assumed towards the College of Physicians and others, whose views do not suit his purpose, clearly indicate that the reviewer belongs to another profession than that of medicine.

The whole review, indeed, can only be regarded as another manifestation of the insolence and folly of that empiricism which of late years has endeavoured to usurp some of the most important functions of legitimate medicine.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1854.

PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

1. *First Report of the Commissioners appointed to inquire whether any, and what, Special Means may be requisite for the Improvement of the Health of the Metropolis.—London*, 1848. (Parliamentary Paper.)
2. *Second Report of the Commissioners, &c. &c.—London*, 1848. (Parliamentary Paper.)
3. *Report on Quarantine.* General Board of Health.—*London*, 1849. (Parliamentary Paper.)
4. *Second Report on Quarantine.—Yellow Fever.* General Board of Health.—*London*, 1852.
5. *Report on the Epidemic Cholera of* 1848-1849. General Board of Health.—*London*, 1850.
6. *Appendix to the Report of the General Board of Health on the Epidemic Cholera of* 1848-1849.—*London*, 1850.
7. *Minutes of Information, collected with reference to Works for the Sewerage and Cleansing of the Sites of Towns.* General Board of Health.—*London*, 1852. (Parliamentary Paper.)

(*Concluded from No.* 25, *p.* 25.)

WE shall now quit the subject of Cholera, and consider some of the other epidemic diseases.

In regard to the extension or propagation of the common Continued Fever of this country, we have obtained, merely by study of its history and external causes—certainly independently of any speculations as to its pathology—nearly all the information, bearing on the means of restraining it, which we can reasonably expect; and may almost say that our knowledge of the power we can exert in that way is reduced to the precision of the exact sciences. When a disease, entitled to this name, has really prevailed as an epidemic in this climate,—which has happened, during the last half century, much more frequently in Scotland and Ireland than in any part of England,—we regard it as an established fact, that on careful inquiry, it has very generally appeared to be *the circumstance of intercourse with the sick*, and no other circumstance, that can be pointed out as common to the persons becoming affected, which has determined the succession. And we think it equally certain, that when we have an effective poor law, from which families otherwise destitute can claim support; small but sufficiently numerous fever hospitals, *with seasoned nurses*—i. e., nurses who have passed through fever,—into which all fever cases among the poor may easily find admission, and by which they are *isolated*, even by a few yards, from the rest of the community; and, in large towns, *houses of refuge* for some of the destitute families of such patients, where they can be treated exactly as the families of cholera patients formerly mentioned (which purpose, however, will be fulfilled by ordinary poor-houses if the true interests of the ratepayers are duly explained to them, and understood by them); we can speak, almost with certainty, as to our power of restraining the extension of such fevers by contagion; even at the times, always to be expected to occur occasionally, and in the places, when the contagious effluvia may be supposed to be more virulent—certainly when the disease shows more tendency to spread—than is usual;—i. e., even during the existence of *epidemic constitutions* as to this disease.

Having watched the effect of increasing the number of fever wards in the Edinburgh Infirmary, in the two last great epidemics of Fever in Scotland, until they were more numerous than the wards for ordinary patients under the same roof (nine to six); and having seen that these numerous fever wards could be kept in operation many months, as long as the demand for them existed, without an instance of communication of the disease to the patients in the other wards within a few yards of them, under the same roof;—but that *unseasoned* clerks or nurses who officiated in the fever wards themselves, were *almost uniformly* and rapidly affected with fever; and that the reception of fever cases into the ordinary wards, when the disease was rife, was often followed by other cases in the adjoining beds; having observed, also, that a single case of continued fever, running its course in a small ill-aired room in any *close* in the Old Town of Edinburgh, was almost uniformly followed by others; but that such successions of cases rarely occurred, after single cases of fever, in the spacious well-aired rooms of the higher ranks in the New Town; and that there is no *close* in Edinburgh, however filthy and ill-drained, which may not remain, for many years together, quite free from fever, if there is no *importation* of single cases of fever into it; knowing, also, that all this is exactly in accordance with what has been observed in many other places, as is shown by the useful little collection of evidence on the use of fever wards, made fifty years ago by Dr. Clark, of Newcastle;—we feel entitled to give an opinion with confidence, both as to the contagious nature of the disease (varying in intensity from time to time), and as to the limits within which its operation may be anticipated; and the degree of ventilation, purification, and separation of the sick from the healthy, which is sufficient to restrain it; provided only, that the means are afforded of carrying into effect the measures which are obviously demanded for these purposes. On the other hand, the experience of some of the emigrant ships from the Mersey and the Clyde, within these two years, has afforded melancholy proof as to typhus fever, equally as to small-pox, that where the means of sufficient *separation of the sick from the healthy* do not exist, the extension of the disease may be as calamitous as ever. Still, there are some points as to the propagation of the poisons exciting this and other diseases which we think it right to state, as nearly put beyond doubt by observations made of late years; because they are considerably different from the views which were generally adopted only a few years since, and form a part of the science of prevention of epidemic disease, ascertained, as we think, by the present age, which may assuredly be highly important to the generations that succeed us, and are destined, we trust, never to be lost.

I. Although it was regarded, so lately—and by so judicious and experienced an observer as Dr. Bateman—as an important principle regarding Continued Fevers, to look on them all as constituting only a single malady, we think it now established, and important to be borne in mind, that there exist, or may be engendered, in this climate, several distinct poisons, known only by the febrile diseases which they excite, all coming under the head of Continued Fever, but differing from one another as to their dangerous effect as decidedly as small pox does from measles; and that this distinction is quite different from the distinction in the *degree* of the most urgent symptoms, which has been long noted in all epidemics of continued fever, and, indeed, in all epidemic diseases. We presume that there is now no difference of opinion, among medical men who have seen much of them, as to the *Relapsing fever*, with the very general yellow suffusion, but slight mortality, of 1843-4, in Edinburgh and Glasgow (now disappeared), having been the offspring of a distinct specific contagion from the *Typhus fever*, with papular eruption, no yellowness, less tendency to early crisis and relapse, and much greater mortality, which prevailed more generally a few years before and a few years after that slighter, but clearly characterized disease. We thought it certain, from many facts then presenting themselves, that these two forms of disease had this undeniable line of distinction between them—that passing through each furnished a security (almost complete) against its own return, but no security against an attack of the other; and this conviction was not formed without numerous opportunities of witnessing both diseases run their course in the same individuals, especially the same medical men. We cannot speak with the same certainty as to the existence of a similar essential distinction between the true Typhus fever and the Typhoid fever of Dr. Jenner and others, characterized by the longer and more indefinite duration, the more florid colour of the eruption, the uniform affection of the glands of Peyer, and less profound stupefaction of the brain. But we can bear witness to the accuracy of the description of many cases of continued fever given by these authors under this last head, and to the prevalence of cases of this description at certain times and places. We see nothing unreasonable in the supposition (only to be established, however, by watching the history and successions of cases showing those characters) that this typhoid or entero-mesenteric fever is specifically distinct from, and less contagious than, either of the other forms of continued fever which we have mentioned, but bears the same relation as they do to the local inflammations usually attending them—viz., that they are its complications and, in a certain degree, its consequences, not its cause. This conclusion would, in fact, be quite in conformity with what we consider to have been ascertained, as to the existence of distinct poisons exciting diseases so much alike, that we must give to them all the name of Continued Fever; and to which we must add, that any one of such poisons may acquire—under conditions, and therefore at times, which are yet unknown to us, whether by alteration of its own nature, or by alteration of the materials on which it is to act, an epidemic constitution, and a more urgent character;—may extend itself more generally, may threaten death both more frequently and in a different way, may require a separate study, and often demand, and bear, a very different treatment.

II. As we now admit of different diseases being ranked together as Continued Fevers, so it appears likewise necessary to admit that several, probably all, forms of continued fever may have, even at this day, *separate modes of origin.* To this we think it the more necessary to advert, on account of the efforts that have been made, no doubt with the best intentions, to trace all such diseases—at least the epidemic extension of all such diseases—to one source only—the pollution of the atmosphere by the decomposition of animal and vegetable matter. That continued fever, as well as intermittent, *may occasionally* originate and become *endemic* from this cause, particularly the typhoid or entero-mesenteric fever above mentioned, we think rendered very probable, if not absolutely proved, by a few instances put on record of successions of cases taking place within the same houses or neighbourhoods,—*showing no contagious property when removed from that locality,*—and in connexion with choked-up drains or badly-constructed cesspools, and offensive effluvia.* But this case is certainly a rare one; and when it is attempted to show that *all,* or the greater number of continued fevers originate in this way, and spread only from this cause,—still more, when it is attempted to *resolve the phenomenon* of the excitement of fever in a *living* body, *into* any modification of the *fermentation,* or decomposition excited by the operation of a ferment in *dead* matter, animal or vegetable, and simply transferred from thence to the living body, as it might be to another dead one,†—we think it necessary to remind our readers, that this theory is brought forward in the present age, in forgetfulness of the numerous observations by which our fathers in medical science were convinced, that the *origin* of the poison that excites any fever is still unknown; that the *action* of that poison on a living body, in exciting epidemic disease, can only be safely and scientifically viewed as a phenomenon *sui generis;* and the restoration or preservation of the purity of the atmosphere only as *one of the means,* by which the

* See, e. g., a case reported by Dr. Christison, in Edinburgh Monthly Journal, vol. viii. p. 1.

† "The *question concerning the nature of many contagions and miasms,*" says Liebig, "*may be reduced to the following:* Are there facts which prove that certain states of putrefaction in a substance are propagated to parts or constituents of the living animal body; that by contact with putrescent matter, *the same or a similar* condition is produced on such parts, as that in which the particles of the putrescent body are? This question must be answered *decidedly in the affirmative.*" (Liebig: Letters on Chemistry, p. 228.)

If by this it had been meant only to express the fact, that in the course of decomposition of certain dead bodies a poison is developed, which is capable of exciting a disease in another living body, and, as a result of that disease, to cause its own reproduction, as shown by the effect produced on other living animals—this is no doubt true, and has been long known; but it is further stated that the propagation of the chemical action of decomposition or putrefaction from a dead to a living body, solves the *question concerning the nature* of contagion or miasm; and that "substances which are in a state of transformation or decomposition are, *in virtue of that state*, to be regarded as the *proximate* causes of an epidemic or contagious disease." (Ibid., p. 230.) And to this doctrine, as hasty and inadequate to the explanation of the known leading phenomena, the observations in the text apply.

virus effecting this result may be diluted, and the human system strengthened against its influence.

(1.) We beg, first, to recal to the recollection of our readers the numerous observations made by many medical men of the last generation, as to the immense extent, and different modes, in which all kinds of putrescent effluvia may be applied to the human body, *without exciting any febrile disease* whatever. On this subject it is enough to quote two authors as coinciding in these statements, whose zealous opposition to one another as to the question, immediately connected with it, of the contagious property of Yellow Fever, may be held as ample security that there was no predisposition to coincide, and that they have agreed only because they had examined the question carefully, and saw that there was no room for difference of opinion as to it.

"Most writers," says Dr. Bancroft, "on the subject of Contagious Fever have believed that it might be generated—first, by an accumulation of those disgusting matters commonly called filth; secondly, by the offensive vapours emitted by putrefying dead bodies, or by other matters in a putrid state; and thirdly, by crowding persons, even when healthy, in ill-ventilated and unclean places. I hope that we shall always find within ourselves sufficient motives to remove or avoid filthiness, even when convinced that it does not produce contagious fever. Whence this belief of its doing so was derived I am unable to explain; but it has probably been confirmed by the frequent coincidence of such fever with nastiness and offensive smells, in the dwellings of indigent people. There is, however, no necessary or natural connexion between the former and the latter.

"Many writers of celebrity, and among them the great Lord Bacon, have thought that no effluvia were so infectious and pernicious to mankind as those which issued from putrefying human bodies; and although a century and a half have elapsed since Diemerbroeck attempted to convince physicians that at least such effluvia could not produce the plague, yet the old opinion has, in some measure, kept its ground. There are facts, however, on a large scale, which completely decide this question. Two of these deserve particular notice. The first relates to exhumations made in the churchyard of St. Eloi at Dunkirk, in the year 1783; and the other to those made three years afterwards, in the churchyard of the St. Innocents, at Paris. I shall, to avoid repetition, here describe only the latter, in which the corpses taken up probably constituted the greatest mass of putrefying animal matter of which we have any accurate information. The churchyard of the St. Innocents at Paris, situated in one of the most populous quarters of the city, had been made the depository of so many bodies, that the soil had been raised by them eight or ten feet higher than the level of the adjoining streets. Numerous complaints having been made concerning the offensive smells which arose from this spot and sometimes penetrated into the adjoining houses, and the public mind being greatly alarmed, it was at last determined to forbid all future burials there, and to remove so much of the superstratum as would reduce the surface to the level of the streets. This work was undertaken in 1786, under the superintendence of M. Thouret, a physician of eminence in Paris, and in *two years* he accomplished the removal of that superstratum, almost the whole of which was impregnated, or infected, as M. Thouret styles it, with the remains of carcases, and quantities of filth thrown upon it from the adjoining houses. 'The exhumations,' says this gentleman in the narrative of them which he published in the 'Journal de Physique' for 1791, p 253, 'were principally executed during the winter, but a considerable part of them was also carried on during the greatest heats of summer. They were begun with every possible care, and with every known precaution; but they were afterwards continued almost for the whole period of the operations, without employing, it may be said, any precaution whatever; yet no danger manifested itself in the whole course of our labours, and *no accident* occurred to disturb the public tranquillity.'

"It does not appear, after the fullest inquiry, that any febrile disorder was ever produced by this immense mass of corruption during the removals made in 1786-7, or while it was suffered to remain as a burying-ground. The grave-diggers were, indeed, sometimes thrown down suddenly, and *for a time deprived of sense and motion* (as in what is termed asphyxia), by the *concentrated vapours* which escaped upon accidentally breaking open by their spades the abdominal viscera of bodies in an early stage of putrefaction. These vapours, also, in a more diffused state, are said to have sometimes produced nausea, loss of appetite, and, in a course of years, paleness of countenance, debility, tremors, &c. But *fever* of any kind does not appear to have been ever noticed as resulting from the offensive or putrid matters of this churchyard, either to the grave-diggers or to the neighbouring inhabitants."*

We need not quote the evidence, adduced by Dr. Bancroft *in extenso*, of M. Berthe, professor in the School of Medicine at Montpelier, and two of his colleagues in that university, who were sent by the government of France into Spain to examine and report upon the nature of the yellow Fever, which had proved so fatal in several large towns of Andalusia in 1800, but had then ceased several months, and who reported that

"Through the imperfect manner in which the tombs and vaults pointed out to them had been closed—a defect which they had observed even in the churches that were most frequented—it appeared that the putrid emanations from the bodies of many thousand persons who had recently died of the yellow fever, did not produce any such disorder. If (he adds) the exhalations from piles of bodies destroyed by the plague itself, and corrupting in the open air, were incapable (as was ascertained at Smyrna by Mr. Howard) of generating the contagion either of fever or of plague, even during the prevalence of a pestilential constitution of the atmosphere (if any state of the atmosphere ever deserved that title), it may I think be safely affirmed, that there are no circumstances under which putrid animal matter can be supposed ever to produce febrile contagion." (Ib. p. 116-118.)

Now we think this last negative conclusion too strongly stated. It is quite possible, perhaps most probable, that under certain conditions, not yet fully understood, effluvia capable of exciting certain febrile diseases may be developed from these and other organized substances in a state of decomposition; but Dr. Bancroft's statements appear to us absolutely decisive against the proposition of Liebig, that "substances in a state of transformation or decomposition are, merely in *virtue of that state*, the *proximate cause* of epidemic or contagious disease."

We may pronounce nearly the same judgment on the evidence which he has adduced in regard to another supposed cause of epidemic fever.

"It appears to have long been an universal opinion, at least among those who have admitted the existence of any infectious fevers, that, to use the words of Dr. Cullen,† 'the effluvia constantly arising from the human body, if long retained in the same place without being diffused in the atmosphere, acquire a singular virulence; and in that state, being applied to the bodies of men, they become the cause of a fever, which is highly contagious.'

"If this were so," says Dr. Bancroft, "with what certainty would this not be effected in a variety of places which are entirely exempted from it? Take, for instance, those in which the natives of Kamstchatka dwell constantly during seven months of the year, and which are called 'yourts;' these are sunk seven or eight feet below the surface of the ground, and are covered with a thatched roof, in the form of a truncated cone, open at the top; they consist of one small apartment,

* See Bancroft, Essay on Yellow Fever, p. 102, et seq., and Annales de Chimie, tom. v. p. 154, &c.

† First Lines of the Practice of Physic, § lxxxi.

which usually contains six families, with their utensils and stock of provisions for the winter, the chief part of which is dried fish, almost putrefied.

"If the combination of personal filth with foul air were capable of creating the contagion of fever, every yourt would necessarily be a fomes of infection. But they never complain of the noxious air that prevails in these habitations. Instead of being generally attacked by contagious fever every winter, they seem to enjoy as good health during this season of confinement as any other people; and *fevers are not even mentioned in the list of diseases* which that respectable traveller, M. Lessep, either observed or heard of as existing among them.

"The people of the island of Oonalaska also 'inhabit yourts or subterraneous dwellings, each common to many families, in which they live in horrible filthiness.'* But these people, notwithstanding, are seldom attacked by any other disease than scurvy, for which they seem to possess a remedy in the allium ursinum, or wild garlic, and in the pinus Cembra.

"The Greenlanders and Esquimaux appear, by the accounts of those celebrated navigators, Davis, Frobisher, Baffin, Henry Ellis, &c., as well as of Bishop Egede, and Crantz, to live, during the greater part of the year, in very close, ill-ventilated, and crowded habitations (without chimneys), which, notwithstanding the great severity of the cold, they keep extremely warm, by their numbers and breath, assisted by a single burning lamp in each, and by excluding fresh air so completely, that any other people would think themselves in danger of being suffocated by the offensive vapours; and yet fever of any kind is a rare disease among these people; though, like those of Kamstchatka, &c., they are much disposed to scurvy.

"Having stated these facts," continues Dr. Bancroft, "in regard to the supposed effects of crowding human beings in small, unventilated habitations in northern countries, let us see what effects result from similar causes in the warmer regions. And here the African slave ships most obviously present themselves for examination. Until within a few years, these vessels notoriously conveyed human beings across the Atlantic, in a state of closer compression, and in an atmosphere more offensively impregnated with human exhalations and excretions, than could probably be found in any other place of confinement. I am fully convinced that fever of any kind rarely occurs on board these vessels, contagious fever never: though great mortality has frequently happened from other diseases, and more especially from dysentery. Dr. Trotter, who was formerly surgeon to a slave ship, after noticing what I have just stated from Dr. Lind, adds, 'The confinement of so many wretched creatures in a small space deservedly attracted the animadversion of a physician investigating the sources and progress of contagion. But *contagious fevers, we find, are not their diseases.*'†

"That this fever often exists in prisons cannot be denied; but this circumstance can afford no evidence of its having been generated therein, any more than the multiplication of vermin in such places could demonstrate the spontaneous generation of these and other insects, by the nastiness which favours the deposition and hatching of their eggs. For this purpose I will resort to the observations and testimony of Mr. Howard, than whom no man ever took more pains to ascertain the truth concerning prisons, or stated it with more exactness and candour: and the result of all that he either heard or saw is, that the jail distemper (i. e., continued fever) is not known in the prisons abroad.

"'If it were asked,' says Mr. Howard, 'what is the cause of the jail fever? it would, in general, be readily replied, the want of fresh air and cleanliness; but as I have found in some prisons abroad, cells and dungeons as offensive and dirty as any I have observed in this country, where, however, this distemper was *unknown*, I am obliged to look out for some additional cause for its production.' Mr. Howard's further experience, in his subsequent tour over a great part of Europe, and into Turkey (in 1785-7), being in conformity with his preceding statement, he repeated it in the same words in his work on 'Lazzarettos,' p. 231.

* Pennant's Arctic Zoology, vol. i. p. cliv.

† See Medicina Nautica, vol. i. p. 184.

"This 'additional cause,' which Mr. Howard thought it necessary to look for, in order to explain the production of jail fever, can be no other,' says Dr. Bancroft, than *the contagion thereof.*

"'The origin of the jail infection,' says Dr. Lind, 'is a point at present entirely unknown. No person has given us the least satisfactory account how or where it is generated. It does not seem to originate in air, and there are many prisons abounding with filth and impurities, perfectly free from it.

"'In ships, also, an infection is generally imported from the land, and many that have been long in a very dirty condition at sea bring their men quite healthy into the harbours.'

"From the preceding facts and considerations," Dr. Bancroft concludes, "I think it may be safely inferred, that filth, crowding, putrid human effluvia, and deficient ventilation, though favourable to the retention and accumulation of febrile contagion, where typhus fever exists, or has existed, and consequently to *its activity*, do not of *themselves* either *generate*, or enable the human body to generate, that contagion."*

From Dr. Chisholm we have an inquiry how far the effluvia from dead animal bodies passing through the natural process of putrefaction are efficient in the production of malignant pestilential fevers, and how far such effluvia are capable "of exciting a putrefactive emotion in other" living animal substances exposed to their action.†

"We are frequently told," he says, "by medical writers, more especially system writers, that the fevers which often desolate armies have their cause in the effluvia proceeding from the putrefaction of the unburied bodies of men and horses slain in battle. This, I am very much inclined to believe, is a mere theoretical idea, and I believe so, principally for two reasons:—1. We have innumerable instances of prodigious slaughter in battle, without this effect being the consequence to the living. 2. In all instances adduced in support of this opinion, we find most powerful *acknowledged* and indisputably *ascertained morbid causes* existing, fully sufficient to this effect, without resorting to a doubtful cause. If any direct unequivocal proofs had been given of the efficacy of these effluvia in producing the typhus gravior, I should be ready to submit to an opinion founded on such premises; but I believe there are no such direct unequivocal proofs on record.

"In the neighbourhood of Bitton, in Gloucestershire, about a mile from Willsbridge, which was my residence for nearly four years, there is what is called 'a bone manufactory,' in which animal bones, after the extraction of their medullary oil by boiling, are distilled, and yield the usual products, muriate of ammonia and sulphate of soda. From this manufactory a fœtor of the most offensive nauseating nature proceeds, and fills the atmosphere for nearly a mile around, diminishing in strength as it recedes from its source, and in proportion to its dilution. The country is thickly inhabited, and near the manufacture itself, is the village of Oldland, the population of which is very considerable; yet, in not one instance has this manufacture proved in the smallest degree injurious to health. This exemption from disease in the manufactory of sal ammoniac &c. has been noticed by Morveau and Chaptal."‡

"Between Bristol and Hanham, on the banks of the Avon, is Conham, remarkable for nothing but its having been chosen for the site of an extensive manufactory for the conversion of dead animals into a substance resembling spermaceti—a project which has been relinquished several years ago. This being also not very distant from Willsbridge, I made a good deal of inquiry into the result, as far as it affected the health of those immediately engaged in the process, and of the inhabitants of its thickly-peopled neighbourhood. The foreman, or superintendent, Richard Bolston, has been my principal informant, and his account was confirmed by that of other respectable persons. Bolston was two years employed constantly in this business; and during that time resided in the midst of

* Bancroft on Yellow Fever, p. 155.

† See Edinburgh Medical and Surgical Journal, vol. vi.

‡ Edinburgh Medical Journal, vol. vi. p. 395.

dead animal bodies, horses, asses, and dogs, many of which were left to pass through the natural process of putrefaction. He had three labourers under him, and he declares that neither himself nor any of these men suffered a moment's sickness. An idea may be formed of the immense volume of putrid animal effluvia enveloping continually the persons of Bolston and the labourers, by being informed that there were three hundred carcases of horses, and as many of asses and dogs, exhaling, in greater or less abundance, their miasms. Notwithstanding this, Bolston declares, that although the stench was offensive in the highest degree, yet he and those with him sustained no injury; and to this the inhabitants of the country around bear ample and angry testimony, both in relation to Bolston and themselves.

"Another remarkable fact is well known where the manufactory of refined sugar is extensively carried on,—butchers preserve the blood of the slaughtered animals in open tubs, kept in close, small, shut-up houses, sometimes for several weeks, until the quantity required is completed, or until there is a demand from the sugar-bakers for it. It is then, in a putrid state, conveyed through the public streets in carts or drays to the sugar-houses, emitting the most offensive effluvia, and extremely annoying to all those who pass it. It is seldom immediately used by the sugar-bakers, but kept by them in casks, in a putrid state, filling the air of the manufactory, and frequently of the vicinity, with its putrid miasms. But what is the result to the workmen, or to the inhabitants of the surrounding houses? Nothing inimical to health. This fact exists constantly in the city of Bristol, where, in general, the streets are extremely narrow, and the houses excessively crowded and ill ventilated; and yet the harmless nature of these exhalations may be daily verified.

"Mr. Newman, surgeon in Bristol, procured for me," adds Dr. Chisholm, "from friends, the following interesting particulars respecting the leather-dressing business:—

"Our men are generally healthy, the most so of the labouring poor. Many have been in our service and knowledge from fifteen to twenty years, and I do not recollect one case of fever occurring among them.

"The first process in dressing is to put the skins into a pit of water to soften them, which is often used two or three times, that is, for two or three parcels, before it is changed, until the stench is intolerable. After this process, the skins are struck out over a beam, and hung up side by side, as close as possible, in a small room, excluded from external air, which we term a stove. In this state they remain until they heat and slime, so that we can pull off the wool. The process of putrefaction is here so rapid, as to disengage large quantities of volatile ammonia, affecting the eyes of strangers with tears, and their noses with the most offensive smell. Our men always pulled the skins in the stove in cold weather from preference, and are occupied in it a whole day at a time, without injury.

"A gentleman concerned in the leather-dressing trade, in Bermondsey, informs me, that 'so far from our workmen being unhealthy, or particularly subject to fevers, the reverse is the fact; the men employed look generally robust and healthy. In a concern in this line of business, of fifty years' standing, in which fifty men are constantly employed, the men have been uniformly healthy; and those men who work upon the raw skins, from which there is a constant and profuse exhalation of putrid steams, and those employed at the lime and tan pits, are equally healthy.' Mr. Newman, the writer of the above, says there are about sixty leather dressers' and tanners' yards in Bermondsey, and in them about seven hundred men are constantly employed. (Ib. p. 402.)

"There are some remarkable instances of the savage cruelty of the natives of Guinea, given by Governor Dalzel, in his history of Dahomy,—in which, if pestilence could be the result of the putrefaction of animal bodies, we should expect to hear of the most direful epidemics; but in which no such result is even mentioned." (Ib. p. 407.)

"Mr. Forster, describing the Kalmuck Tartars, says, 'There is not perhaps on the face of the earth a human creature who lives on coarser fare, or to a civilized people more disgusting, than a Kalmuck Tartar. Raw putrid fish, or the flesh of carrion, horses, oxen, and camels, is the ordinary food of the Kalmucks; and they are more active, and less susceptible of the inclemency of the weather, than any race of men I have ever seen.'" *

"Cook,† Dixon,‡ and La Perouse,§ all agree in their description of the astonishing filth of the native Indians of Port St. Francis, on the north-west coast of America. La Perouse says their cabins possess a nastiness and stench to which the den of no known animal in the world can possibly be compared. Cook says they dry their fish within doors, gut them there, and these, with the fragments of their meals, and the addition of all other sorts of filth, lie everywhere in heaps, and are never carried away. In a word, their houses are as dirty as hog 'sties.' And yet these people seem to be acquainted with some of the more necessary arts of civilized nations, and *enjoy uninterrupted health.*

"Hence, then, as Spallanzani justly concludes, it appears that the various classes of animals, and man among the rest, in a healthy state, are endowed with the power, not only of checking the putrefaction of substances lodged in their stomachs, but also of correcting them when already putrid.

Dr. Chisholm concludes, on the whole:

"1. That the theory of ingenious chemists, founded on experiments or speculations to prove the pestilential influence of putrid animal effluvia, receives no support from practical knowledge or the known economy of nature.

"2. That in no known and well ascertained instance are putrid animal exhalations productive of pestilential fevers." (Ib. p. 418.)

Like other inquirers on this subject, he goes here somewhat farther, and carries his conclusions farther, than the facts which he has stated justify; but if we rest on the inference above stated, that there must be *something more* concerned in the diffusion of epidemics, than the mere decomposition of dead animal matter, and concentration of the effluvia from living, and propagation of the state of putrefaction, as a natural result of the application of the dead to the living,—we apprehend that our principle is unassailable, and must be held as established, whatever further information we may acquire on the subject.‖

(2.) In the next place, we beg it may be remembered, that in the case of Continued Fever in this climate, the most general fact stated in the last age, as pointing to the nature of this *additional condition,* thus shown to be requisite to make even the most putrid and concentrated effluvia effective as a cause of its extension, is, that a *case of such fever already exists;* from which, just as from the pustules of small pox, we have good reason to believe—almost ocular demonstration—that the *specific poison* may arise, and with which the next cases in succession may be shown to have had intercourse; and this was not assumed without careful inquiry. On careful examination of the early stage of such epidemics, particularly when occurring in the circumstances already stated, as affording the most decisive evidence—i. e., on a small scale, and in a new locality—we have no doubt this may be observed so generally as to leave no room for doubt as to the soundness of the principle.

* Journey from Bengal to England, p. 258. † Third Voyage, vol. ii.
‡ Voyage, p. 173. § Voyage, vol. ii. p. 134.

‖ If further evidence were required, we might refer to the case of the Voierie of Montfauçon, close to Paris, extending to many acres, existing many centuries, and where 40,000 animals are flayed annually, 9500 of them horses affected with glanders, carbuncle, or farcy. There is no drain. The committee of the Board of Health could not penetrate to the place on account of the effluvia. Yet the workmen and their families are stout, healthy, and long-lived; and Parent Duchatelet considered it clearly established that none of them had ever suffered from disease referable to diseased animal matter. (See Christison on Poisons, p. 582.)

We can adduce an example, which we think unexceptionable, in confirmation of it, drawn from a locality which we have had long under observation, and, indeed, have already noticed.

Burt's Close, Grassmarket, Edinburgh, has been already mentioned as one of the worst-drained parts of that poor and dirty district. There were in it, in 1826-8, two lodging-houses, elevated only a few steps above the pavement, both equally dirty and equally crowded, in general, by a succession of the lowest of the people. Into one of these, a wanderer from Glasgow (where fever prevailed at the time) was admitted in the winter 1826-7, who immediately fell ill of the common Continued Fever, and his case was followed by a succession of fourteen more fever cases *in that house;* the other lodging-house, distant from it only a few paces, remaining free from the disease. In the next winter the case was reversed. A case of fever occurred in the other lodging-house, and was speedily followed by eight more *in it;* the house first affected, although its inmates had been almost all changed, as well as the other houses in the Close, remaining at this time perfectly free from the disease. Any influence which could have resulted from filth, putrefying matter, crowding, and want of draining, should have acted equally on both houses, and in both winters, and in other houses of the Close; but these facts appeared to us then, and appear still, clearly to indicate, and co-operate with many others in indicating, as the main cause of the extension of that epidemic, intercourse with *persons already affected by it;* and we are not justified in saying more, as the result of that or other similar observations, as to the influence of any other cause, than that persons living in the vitiated air, or otherwise in the manner, of the inhabitants of these lodging-houses, are peculiarly liable, or are predisposed, to suffer from the action of that specific exciting cause. Whether those circumstances of predisposition are sufficient, in certain cases, and under conditions still unknown, to produce the disease, independently of any application of the specific poison, is another question, on which we shall say a few words presently; but such facts as we now state have convinced us, that if such cases occur they must be comparatively rare.

Another observation, made in the same Close twenty years later—in 1846-7—appears still more decisive as proof, that it is not mere want of draining, nor mere putrid effluvia, that have made it, on so many occasions, the seat of an epidemic influence. In this season, a succession of nearly twenty cases of fever in Burt's Close again occurred; but on examination it now appeared, that both the tenements which had been affected twenty years previously, and which, although still inhabited, are no longer occupied as lodging-houses, remained free from fever during almost the whole of this epidemic; one of them only being slightly affected towards its end, as the inhabitants believed in consequence of intercourse with the houses now infected. The succession of cases had now taken place on the third and fourth floors of two common stairs, at the entrance of the Close, unaffected on the former occasions, but now used as lodging-houses, and into which, as formerly, some of the wandering Irish from Glasgow had been admitted; the first and second floors even of those stairs remaining unaffected.

This fact, of the inhabitants of the third and fourth, nay, of the seventh or eighth, floors of a lofty tenement being affected with epidemic disease, while the lower stories of the same tenement, certainly much nearer to any impurities that drains could have removed, remain perfectly free from it—is one which we have witnessed dozens of times in Edinburgh; and this same Close furnished another illustration of it during the last epidemic Cholera. A number of cases of this epidemic were brought into hospital from this Close; but on inquiry it appeared that none of them were from the houses which had formerly furnished the fevers. They were all from the farthest common stair in the Close, which is usually inhabited by workmen in regular employment, and their families, and which had remained unaffected during all the epidemic fevers. On this occasion there had been a succession of cases of cholera there, two or three only on the first floor, *none on the second or third*, which are still inhabited by respectable artizans—all the rest from the highest story of that tenement, now inhabited by the same description of persons, of irregular and filthy habits, as had formerly inhabited the other parts of the Close which we have mentioned.

We think ourselves here justified in asserting, *on statistical evidence*, that although this is certainly a very low, dirty, and ill-drained Close, yet it is not the circumstance of dirt and defective drainage, common to the whole Close, and to the last thirty years, but other and more partial circumstances, in the condition of certain small and varying *portions of the population* there resident, which have rendered them repeatedly the seat of truly epidemic disease; and it will be observed, that in drawing this inference,—as to the influence of *intercourse with the sick*, as in the inquiries formerly stated as to cholera,—we trust to the *positive* evidence of successions of cases, as indicating this great external cause, only in so far as they are supported by a great body of *negative* evidence, excluding other causes; and are therefore unassailable by—as we were, indeed, all along perfectly aware of—the general objection to "the evidence on which Quarantine is based," stated by the Board of Health.

We have indeed always believed, that the habitual respiration of a polluted atmosphere is *one of the conditions* by which a part of the population of the poorer parts of towns is rendered peculiarly susceptible of epidemic diseases; but the pollution of the air is very generally effected much more by the crowding, and by the habits of the people crowded together, *within the rooms* which they inhabit, than by anything *external to those rooms*. We have seen many illustrations of a remark made by Sir Gilbert Blane, in the course of visitation of the poor in London, that garrets are generally worse ventilated and more offensive than cellars, inhabited by such families; because the source of the impurity being the air, which is heated in contact with the living inmates, and which rises upwards, finds its way outwards by the entrance necessarily left open in the latter case, but not in the former. This, of course, abates any sanguine expectation from draining; and, at all events, we have seen quite enough to convince us, that the mere draining and cleansing of such parts of our towns affords no protection against the extension of epidemic diseases through them, at those times when other causes, favouring the diffusion of such diseases, are suffered to prevail among the inhabitants; and this, it will be observed, is perfectly in accordance with what we formerly stated in discussing the "Predisposing Causes of Epidemics," which we showed to be various, and very generally, if not uniformly, to require the operation of a *specific poison*, in order to produce a specific disease, of local and temporary existence only.

(3.) In the next place, in considering the scientific question, how far the "propagation of decomposition and transformation" from dead animal matter to living bodies, can be looked on as an explanation of epidemic diseases, we must observe, that while, on the one hand, we have so strong evidence as has been stated, of putrescent matter being applied in enormous quantity, and in all different modes, to the different surfaces of the living body by which poisons usually enter, without exciting disease,—on the other hand, in those instances in which we can ascribe virulent disease with most probability, and on a large scale, to the action of poisons originating during the *decomposition* of organic matter, there is little or no perceptible *putrefaction*. We agree with most authors on the subject at the present day, in thinking it most probable that the malaria which excites intermittents and remittents, originates in this way, in places that have, in general, been flooded and afterwards dried; and it is this fact which gives the greatest plausibility to the doctrine of the poison of typhus or of cholera being occasionally evolved in like manner. But we must remember that in many places, where the poison of malaria is developed in full perfection, there is no *putrefying* matter; that the existence even of decomposition of previously organic matter is often only a matter of *inference;* and that the most fetid marshes, even in hot climates, are often the least dangerous. We see frequently in this country, marshes where all the conditions yet known for the evolution of malaria exist during a great part of the year—some of them in districts where, fifty years ago, agues were common—yet find no such disease now existing; and, on the other hand, we see many persons affected with ague, who have obviously contracted it by passing through districts where it still prevails, but are quite unable to point out those districts by any evidence of putrefaction which they there witnessed.

The observations of the late Dr. Ferguson, one of the most experienced of British medical officers, are of themselves sufficient to establish the proposition, as he himself states it, that "*putrefaction* and the matter of disease are distinct and independent elements."

"The first time I saw intermittent and remittent fever become epidemic in an army," says Dr. Ferguson, "was in 1794, when, after a very dry and hot summer, our troops in the month of August took up an encampment at Rosendaal, in South Holland. The soil was a level plain of sand, with perfectly dry surface, where no vegetation existed, or *could* exist, but stunted heath plants. On digging it was universally found percolated with water to within a few inches of the surface, which, so far from being at all putrid, was perfectly potable in all the wells of the camp."*

"The water in the gardens of Lisbon," he says farther, "being most precious, is husbanded with the utmost care for the three months absolute drought in the summer season. It falls, of course, into the *most concentrated state of foulness and putridity*. In the confined gardens of Lisbon these reservoirs may be seen in this state *close to the houses*, even to the sleeping places of the household; but *no one ever heard of fever* being generated among them from such a source; though the most ignorant native is well aware, that were he to cross the river, and sleep on the sandy shores of the Alentejo, where a particle of water, at that season, *had not been seen for months*, and where water, being absorbed into the sand as soon as

* Ferguson on Marsh Fever, in Edinburgh Philosophical Transactions, vol. ix. p. 274.

it falls, was *never known to be putrid*, he would run the greatest risk of being seized with remittent fever."*

"Ciudad Rodrigo is situated on the banks of the Agueda, a remarkably clear stream, but the approach to it, on the side of Portugal, is through a bare, open, hollow country, that has been likened to the dried-up bed of an extensive lake; and on more than one occasion, when this low land, after being flooded in the rainy season, had become as dry as a brick-ground, with the vegetation utterly burnt up, there arose fevers to our troops, which for malignity of type could only be matched by those on the Guadiana. . . . . At the town of Corea, in Spanish Estremadura, nearly similarly situated on the banks of the Alagon, a pure and limpid stream, our troops experienced similar results. It is difficult to conceive anything more deceptive than the appearance of these two towns, particularly the last, which might have been pitched on by the best instructed medical officer, if unacquainted with the nature of Malaria, as a place of refuge from disease; for the shores of the river seemed perfectly dry, and there was not an aquatic weed, nor speck, nor line of marsh, to be seen within miles of the town, nor anything but dry, bare, and clean savannah. It had, however, been so far the contrary in past times, that the ecclesiastics of its ancient cathedral had a dispensation from the Pope of no less than five months' leave of absence, to avoid the endemic fever."†

"The town of Point au Pitre, Guadaloupe, is situated among some of the most putrid marshes in the world, the smell of which is almost never absent from the streets; but strangers, however annoyed by the smell, often resorted to it with impunity. And the principal barrack in the island of Tobago has been placed immediately to leeward of the Bacoleta swamp, within the distance of less than half a mile, and the stench from its exhalations often pollutes the barracks; but these are so far from producing fevers at all times, that when I visited the white garrison there, they had been *more remarkably exempt from that form of disease*, for several years, than any other troops in the West Indies. . . But at the fort Fleur d'Epée, in Guadaloupe, the farthest outpost, at the extremity of the marshes, where they approach to the state of *terra firma*, where little or no water is to be seen on the surface, and *no smell exists*, there cannot be supposed a more deadly quarter, and all white troops consider their being sent there as equivalent to a sentence of death.

"I shall not multiply facts and illustrations to prove that *putrefaction* and the *matter of disease* are *distinct and independent elements*; the one *travels beyond the other*, without producing the smallest bad effects; and however frequently they may be found in company, they have no necessary connexion."‡

It may be said, and we are disposed to think justly, notwithstanding these facts, that although what we term *putrefaction* is not requisite, a certain stage or mode of *decomposition* of previously existing organized matter is really concerned in the evolution of *malaria*, and production of intermittent and remittent fevers; but let it further be remembered, that these diseases, at least in ordinary circumstances, are *not contagious*. It is of those epidemic fevers only, which have an origin, at this day as in the days of Dr. Lind, "entirely unknown,"—i. e., the typhus and other continued fevers,—that we assert with confidence, that, like small-pox or measles,—belonging, in fact, to the same great class of febrile exanthemata as themselves,—they spread by contagion—i. e., by the intercourse of the healthy with those already sick of them. And there are two important facts, in regard to the extension of all these epidemic diseases, known to us by observation only, which are of themselves enough to establish an essential distinction between their history and that of organic substances undergoing chemical decomposition:—1. The effect of what we call various,

* Ferguson on Marsh Poison: Edinburgh Philosophical Transactions, vol. ix. p. 269.
† Ibid., p. 279. ‡ Ibid., p. 282.

concurrent, or accessory causes, chiefly acting through the nervous system, particularly violent muscular exertions, acute sensations, or emotions and passions of mind, either in aiding or resisting their attacks, which assuredly possess no such power over the decomposition of matter which has lost its vitality; and, 2. The indemnity—varying in degree in different individuals, but certainly a general fact—from future attacks given by once passing through one of those diseases. And if it be said,—as no doubt it may be justly said,—that almost the only fact analogous to this last, known in nature, is the effect of the process of fermentation in vegetable fluids, in producing a state of their particles which is incapable of taking on the same action again, let us remember, that in the case of a living man who has passed through one of these diseases in infancy, and is fully exposed to the contagion or inoculated with the virus in advanced life, the living matter which then resists the influence of the poison had not been in existence—i. e., the elements composing it had not been grouped together so as to form organic principles—for many years after the living body to which it now belongs had gone through that process of protection.

We have entered on these illustrations, which we should hardly have thought necessary at this day, in order to show that, if we regard the subject scientifically, while we renounce the merely nervous pathology of Hoffman and Cullen, and willingly admit that their "solus spasmus et simplex atonia" are quite inadequate to explain the phenomena of febrile diseases, so, on the other hand, the refutation of the humoral pathology,—at least of that humoral pathology which would refer the excitement of an epidemic disease to a simply *chemical* change,—which was the work of the last century, remains as a part of the foundation of medical science; and that it is a step backwards, not forwards, in Pathology, to revert to the "putrefaction of organic atoms, propagated to the living animal body," as a principle which explains, or comprehends within itself, any of the most material of these phenomena;—and again, looking on the subject merely practically, that we must not trust to police regulations for the removal of putrescent matter and purification of the atmosphere—however useful as a *part* of the system to be adopted on such occasions—as adequate to meet the demands of an intelligent people threatened with an epidemic disease. The true scientific point of view in which we ought to regard the invisible cause of all such diseases, is simply as a part of the great science of Poisons—of those agents, inorganic, vegetable, and animal, which, by the mysterious dispensations of Providence, are destined to exercise an injurious or destructive agency on living structures. Knowing, as we now do, that many such poisons are continually formed, both by vegetables and animals, the effects of which, taken into the bodies of living animals, are very various, some of them truly *morbific*, i. e., exciting a long series of changes, constituting a true disease; and that the function of Excretion in animals, wherever carried on, is in fact the provision of nature for the expulsion of matter which, having lost its vitality, has come under the influence, in all parts of the body, of "destructive assimilation," and, if not expelled, will speedily and surely act as a poison;—knowing, also, that it is in the course of destructive assimilation and excretion that almost all animal, perhaps all organic, poisons are formed,—we cannot be surprised to find, and it is perfectly in accordance with what we have now stated to maintain, that although the chemical process of decomposition affords no explanation of febrile disease. yet, in the course of the decomposition continually going on in living bodies, in dead bodies, and in the excretions from living, under conditions not yet fully ascertained, compounds may be formed from time to time, which will not only act as Poisons, but exert peculiar *morbific* powers, and some of which may subsequently spread by contagion; and that it is only by inductive study of the history of each of the peculiar diseases thus engendered, that we can learn, either what are the peculiar conditions essential to its development or propagation, or what other powers exist in Nature by which it may be opposed. Accordingly, we can refer to various facts as to several such diseases, known only by observation, but which constitute of themselves, if we make no attempt to resolve them into others, an important and most useful body of Science.

The principle we have already stated, as applicable, certainly to several, perhaps to all such diseases, has a practical importance which justifies our dwelling on it a little farther,—that the poison exciting them may have, even at this day, more modes of origin than one; and therefore, that because we see such a disease spreading by contagion—i. e., by intercourse of the sick with the healthy—at one time and place, we are not entitled to infer that it must do so in all, or can have no other mode of propagation; or, *vice versâ*, to draw the opposite inference. Without recurring to the subject of Cholera in illustration of this, we have already stated that we have been satisfied by repeated personal observation, that Dysentery, commencing only in the usual way, from exposure to cold and wet, and independently of any intercourse with patients already affected with it, may in some seasons assume the character of a specific inflammation, and spread from the sick to the healthy, e.g., from patients to nurses and attendants, long previously healthy, and exposed to no other cause, in an hospital. In such cases, there is nothing unreasonable in supposing that a specific poison is developed under our observation. And we presume it is now pretty generally admitted that the Erythematic or diffuse inflammation may originate, particularly in certain seasons, in various ways, often in ways altogether obscure; that it may originate from the "cadaveric poison" from bodies dead of various diseases, and afterwards spread by contagion or inoculation; in which case inappreciably minute portions of matter from one living body are applied to another, particularly by nurses and medical attendants to patients receiving enemata, and to lying-in women, and that this is at least one, probably the chief, source of the Puerperal Fever, already recognised as an Erysipelatous Peritonitis. As the evidence of so important a fact may not be known to all our readers, and as we thus see a fair prospect of one of the most lamentable of epidemic diseases being permanently suppressed, we add here the numerical statement of the discovery made on this point by Dr. Semmelweiss at Vienna, as quoted by Liebig:

> "Since there have been, in the great Lying-in Hospital here, a division for the instruction of physicians, and another for midwives, the number of deaths on the physicians' side was constantly greater—in 1846 it was four times greater—than on that of the midwives; and in an equal number of puerperal cases in that year, the *excess* of deaths on the physicians' side was 400. After fully considering all the circumstances, Dr. Semmelweiss remarked, that both he and the students occupied themselves frequently with post-mortem examinations; that the cada-

veric smell on their hands, notwithstanding frequent washings, did not disappear for a considerable time; and that the pupils not unfrequently proceeded to the examination of a woman in labour immediately after dissecting a dead body. This was the only one of the probable causes of puerperal fever which either did not occur at all, or occurred only in a very limited degree, in the midwives' division. He now adopted the rule, that every one, before examining a patient, should wash his hands with chlorine water; and from the time when this rule was adopted, the patients on the physicians' side were not more frequently attacked with puerperal diseases than those on the midwives'. In 1848, of 3780 patients delivered on the physicians' side, 45 died, or 1·19 per cent.; and on that of the midwives, of 3219 cases, 33 died, or 1·33 per cent."*

It is, we believe, now generally admitted in this country, and we have seen abundant evidence of the facts—1. That the specific poison producing the true Puerperal Fever is usually communicated by accoucheurs and nurses, themselves healthy. 2. That it is often received by them from dead bodies, and communicated to women in childbed, although they are themselves unaffected, and although the bodies are not of those who have died of any such disease; and, 3. That this specific poison has often excited the true diffuse or erysipelatous inflammation, not only on the skin, but on the peritoneum, in the veins, in the synovial membranes of joints, in the cellular membrane generally, and other parts. We add with real satisfaction, that when the precautions obviously suggested by these facts, as to habitual ablutions with chlorides, and occasional separation and isolation of medical attendants and nurses, who have had intercourse either with the "cadaveric poison," or with patients labouring under this diffuse form of inflammation, have been taken, we have known repeated instances of apparently formidable epidemics of this kind being manifestly arrested.

The fact here stated as to the *cadaveric poison,* is quite justly connected, by Liebig, with the known facts, by which it has been proved, chiefly in Germany, that the use of various articles of food—cheese, ham, and particularly sausages—in a certain state of decomposition, may be followed, even in healthy persons, by dangerous symptoms; and if by this he means only to express the fact that matters evolved during this peculiar decomposition become the exciting causes of certain diseases, we apprehend that the analogy of the two cases is undoubted. But the most important facts which we understand to have been ascertained in regard to this kind of poison is, that "the sausages are poisonous only at a particular *stage of decay,* and cease to be so when putrefaction has advanced so far, that sulphuretted hydrogen is evolved, the central part being often poisonous when the surface is wholesome;" so that there is every reason to believe that "a peculiar poisonous principle is evolved" in the course of the changes which these matters undergo; and there is even strong evidence as to the chemical nature of this principle,—viz., it seems to consist of a peculiar vegetable acid, or modification of the acetic acid, combined with an acrid oil."† We have, therefore, good reason to expect that the poison existing in those articles of food, like that existing in the ergot of rye, and producing gangrenous inflammation,

* Liebig: Letters on Chemistry, App. p. 530.
† See Christison on Poisons, p. 585, et seq.

or like the cadaveric poison, producing puerperal fever, may be traced to a very limited quantity of matter, probably of short duration.

But if matter evolved at a certain stage, and during a certain kind of decomposition of dead animal substances, may become the exciting cause of a peculiar febrile, subsequently contagious disease, and specific inflammation, we cannot be surprised to find that a similar change may be effected, under conditions yet unknown, on a portion of that matter, existing in every human body, which is destined to excretion; and which we know to be excreted only because it has lost its vitality, and will be poisonous if retained.

There is, therefore, nothing unreasonable in the supposition that the poison of Typhus may be occasionally generated, under the influence, especially, of the causes known to give predisposition to the disease; and that it may afterwards, particularly when aided by those predisposing causes, spread by contagion. We have already stated it as a probable, although certainly not a common occurrence, that even in this climate a particular form of continued fever may originate nearly in like manner as intermittents do in dead matter; and there is also strong ground for suspicion that it originates occasionally in the living human body, simply under the influence of depressing passions. We have seen cases ourselves which seemed to us to be most probably of this description; and we add the following from an author whose experience and judgment will not be disputed:

"Some cases of the slow nervous Fever, which has been so accurately and well described by Huxham," says Sir H. Marsh, "have fallen under my observation in delicate and nervous females, which, as far as I could discover, were traceable to mental causes, and not at all to a contagious origin. I think, too, that in two instances in which I had an opportunity of observing the patients both before and during the illness, Typhus fever arose independently of contagion, from the combined action of depressed spirits, and of insufficient and unhealthy nutriment. In one of those instances the fever arose in an individual member of a detached family; from him all the others caught it in succession; the survivors became mendicants, and left fever and death in every house where they spent the night, in a line of country of many miles, along which no case of fever had previously existed."*

Sir H. Marsh says, farther,

"I have seen many severe cases of fever arise slowly and gradually, after long-continued anxiety of mind, and I was unable to discover any other cause for the disease. I am indebted to Dr. Cheyne for the following brief sketch of fever thus originating; I shall give it in his own words.

"'*Causes.*—Loss of property, of character; wounded pride; *invasion*, insidious, indistinct; patient generally unable to assign the date of the commencement of the attack; for some time before he has been complaining of bad nights, or has symptoms of a common cold, which almost insensibly degenerate into the proper symptoms of fever; when visited by a physician, he appears utterly unconscious of the formidable nature of his disease; but the symptoms are those of the typhus gravior of nosologists: a red suffusion of the eye, prostration of strength, subsultus tendinum, quick and weak pulse, hurried breathing, dun petechiæ, or a mottled state of the surface; of such patients a great proportion die. I have no recollection of a second case of this kind of fever occurring in a family, and I have never been able to discover that the patient had been exposed to contagion; it would seem to arise solely from mental causes.'"†

* Dublin Hospital Reports, 1827, pp. 525, 6.

† Ibid., pp. 533, 4.

Here we have evidence, we do not say more than probable, both of the febrile disease thus originating having subsequently a contagious property, and of that property being slight in comparison of other cases.

In the case of the truly *endemic fevers*, i.e., intermittents and remittents, which can be contracted only in particular places, and in the case of the true *epidemics*, as small-pox, or plague, and malignant cholera, which we know to have been absent from many millions of mankind, and for many centuries together, we may, indeed, always look for the operation of a single and truly specific cause; but in the case of the continued fevers of this climate,—diseases which are never absent from a large community for many months together, but prevail epidemically only in certain seasons, and show the contagious property much more in some seasons than in others,—as well as in the cases of dysentery and of diffuse inflammation in its different forms, we think the phenomena seem clearly to indicate *two modes of origin*.

After what we have stated here, and likewise in the paper on the 'Predisposing Causes of Epidemics' (January, 1853), in regard to the destructive assimilation continually going on in every living body, in the course of which Poisons are necessarily generated, and may be variously modified, we can see no difficulty in understanding how this may happen; and it can only be by careful observation of different epidemics, that we can learn whether, under what conditions, and to what extent, it does happen. We have given our reasons, founded only on such observation, for believing that the poison of Cholera, although certainly extending itself in ways not yet understood, independently of immediate intercourse of the sick with the healthy, in most parts of the world, does likewise, in this country, and in others, spread at times distinctly by contagion. In like manner, it is thus only that we can learn whether the Malaria arising from the earth's surface, and producing only intermittent or remittent fevers in general, may or may not, in certain circumstances, and at certain times, excite fevers which may take the continued form, and may propagate their kind, as small-pox or measles do. It has been repeatedly stated in this journal, that this seems by far the most probable supposition as to the extension of the Yellow Fever at Boa Vista, in 1845-6. The doctrine is the very same as has been repeatedly proposed by those who have studied the history of the various epidemics of yellow fever, as the only means of reconciling, not the conflicting opinions (which is a matter of little consequence), but the apparently contradictory facts presented by those calamities. The following were the conclusions drawn in the year 1826, from all the facts then known on the subject, by Dr. C. Matthei, author of an 'Essay on Yellow Fever,' to which a prize was adjudged by a very eminent committee in Germany.

"That Yellow Fever is a disease. *sui generis*, endemic only in low districts on the sea coast, but under certain circumstances sporadic in other places, never appearing beyond 48° of north latitude, nor without a temperature of 72°, somehow promoting its production and propagation,—depending, in part, on causes not yet known; but in circumstances favourable to its extension, *capable of being propagated by contagion*.*" It is substantially

* See Edinburgh Medical and Surgical Journal, vol. xxvi.

the same doctrine—as to the disease originating from other causes, and then spreading by infection—which was maintained in the last age by Sir Gilbert Blane, as well as other experienced observers.

"After laying together, and considering fully, all the facts relating to this subject, it appears to me that the yellow fever cannot be produced, but in a season or climate in which the heat of the atmosphere is pretty uniformly, for a length of time, above the 80th degree of Fahrenheit's thermometer; that under the influence of this heat, Europeans newly arrived, and more especially in circumstances of intemperance, or fatigue in the sun, may be subject to it in many instances; but that it has usually become general only either by the previous influence of that infection which produces the *jail, hospital, or ship fever*, or from the influence of *putrid exhalations;* and that when so produced, *it continues itself by infection.*"*

We add here a statement to the same purpose, of facts of recent occurrence in Brazil, which we think more important as coming from an author whose personal observation of the disease, like that of most of our army and navy surgeons, had afforded no indications of contagion. This is perfectly well stated—and the *limited* conclusion drawn has our perfect acquiescence—in the following passage:—

"Almost every person who joined the *Vestal*, during the prevalence of fever, was affected by it: but no person leaving her under the disease, communicated it to another in another place. And so it happens, if not universally, almost universally. Nearly every man who joins a ship in such a condition, has the prevalent disease, sooner or later; but no number of persons taken from such a ship, labouring under the disease in any stage, or in any form, and placed in a situation where the disease does not exist, though in the centre of a mass of healthy people, can excite it in a single instance. An accumulation of such facts, and there is a large accumulation, decides the question of the contagious power of the fever in the negative, absolutely *pro loco et tempore.*"†

That the conclusion should only be *pro loco et tempore*, appears from the following, *in the same paper*, which we quote in confirmation of the other, and we think the more reasonable doctrine.

"To show his contempt for infection, M. Lassere went on board the *Alcyon*, an infected ship, daily for several days, until he was laid up with fever, and died on the third day. His death created the greatest panic on shore. *Up to this period, the fatal cases had been confined to seamen, and new comers;* but M. Lassere was a man of robust constitution, had been resident in the country twenty-five years, and his wealth enabled him to obtain the first medical aid; notwithstanding all this, he died, and from that moment no one considered himself safe. He died at his country house, in the village, or rather district, of Capunga, where my cottage adjoined his place."

Of course his case proves nothing, but Dr. M'Kinlay adds:—

"Capunga had been *perfectly healthy up to this period, but within a week twenty cases of fever, and five deaths, had occurred there.*

"Another case, much relied on as an instance of direct contagious propagation, is thus told:—A wagoner, in the early part of the epidemic, was sent into the city, from Engenhio Carnana, an estate five leagues in the country, belonging to Dr. Domingos de Sonega. Having finished his business in the city, where he remained many hours, the wagoner sickened on his way home, and shortly after his arrival there, showed unequivocal symptoms of yellow fever, of which he soon died."

* Blane on the Diseases of Seamen, p. 607.

† Dr. M'Kinlay on Yellow Fever on the Coast of Brazil: Monthly Journal of Medical Science, Nov. 1852, p. 425.

His case, in like manner, proves nothing; but

"*Those who attended him were next attacked,* and the disease speedily spread over the whole estate. So great was the mortality, that Dr. Domingos was obliged to beg aid from the neighbouring estates, which was *refused upon the ground that the appearance of the disease,* on his own estate, *afforded so palpable an evidence of contagion.* Mr. Cowper *is certain there was no disease* on the estate before the return of the sick wagoner from the city."*

A similar fact is thus stated by Sir G. Blane:—

"On the 16th of May, 1795, the *Thetis* and *Hussar* frigates captured two French armed ships from Guadaloupe, on the coast of America. One of these had the yellow fever on board; and out of fourteen men sent from the *Hussar* to take care of her, nine died of this fever before she reached Halifax, on the 28th of the same month, and the five others were sent to the hospital, sick of the same distemper."

This, of course, proves nothing; but farther,

"Part of the prisoners were removed on board the *Hussar,* and though care was taken to select those seemingly in perfect health, the disease *spread rapidly in that ship* (formerly healthy), so that near one-third of the whole crew was more or less affected by it."†

In the face of such facts, it would be wrong to assert, on the breaking-out of Yellow Fever, or unusually virulent remittent fever, in any climate, known to be fitted for its extension, that it will not spread by contagion; but we are fully justified, by observation of that disease, in asserting on any such occasion, that whether it spreads by contagion within certain limits, or not, it will in all probability be confined to a *tainted district,* even in that climate; and that on careful inquiry in almost any such case, it will appear that *there are boundaries,* at no great distance from the spots first affected, beyond which the disease has not formerly spread, and need hardly now be apprehended. The limits of the yellow fever district were set in Philadelphia and New York half a century ago, and, as we understand, have never yet been passed; and, in like manner, the immunity of the Neutral Ground at Gibraltar, and the importance of forming encampments there, were ascertained in 1814,‡ and have been availed of in several epidemics since.

The exact conformity of the facts put on record at that time by Dr. Playfair, with the experience of many subsequent epidemics, appears from the following abstract of his observations.

The epidemic fever showed itself in Gibraltar in August, 1814; and soon began to prevail extensively in Dillon's regiment, occupying the same barracks in which it had been very fatal in another regiment, at the same season of the previous year. Some Spanish families, employed in the town, but living in the low sandy isthmus called the Neutral Ground, and likewise the inhabitants of the south side of the Rock, remained quite free from the disease, although communicating freely with the inhabitants of the town, where it now prevailed. On the 20th of September, after nearly a hundred men of Dillon's regiment had taken the fever, that regiment was removed from the barracks, and encamped on the Neutral Ground.

* Dr. M'Kinlay, loc. cit., Oct. 1852, p. 346.
† Blane on the Diseases of Seamen, p. 606.
‡ See Thesis de Fevre Flava Calpeuse, by Dr. Playfair, Edinburgh, 1819.

The first two days after this change twenty-seven more cases occurred in the regiment, no doubt having previously imbibed the poison. After the 22nd of September, and after the disease had affected a hundred and twenty men of the regiment in twenty-six days, it ceased entirely. A detachment of the 60th regiment, which had succeeded Dillon's in the barracks in the town, and been severely affected by the fever, was afterwards brought out to the Neutral Ground, and had not a case after the change. Dillon's was afterwards accommodated in transports in the bay, and remained perfectly healthy; but among the inhabitants and soldiers who remained in the town the fever raged violently for *nearly two months more;* and then, after heavy rains had fallen, gave place to the ordinary remittent fevers of the country. The south side of the Rock, as well as the Neutral Ground, remained during the whole time quite free from the disease.

The following proofs, on a scale of sufficient size, are given from Dr. Chervin, of the importance of similar *migration* from localities where yellow fever takes place:

"During the epidemic of 1800, at Cadiz, 14,000 persons left that city when the disease became suspected. These people fled to the country, where they remained free from the epidemic; while, of the 57,499 who remained, 48,520 were attacked, of whom 6884 lost their lives.

"On the 1st of September, 1803, the population of Malaga was 36,008, exclusive of the military garrison, prisoners, and other persons. Of these, 4548 fled, and thereby secured themselves from attacks; while, of the remaining 31,460, 18,787 were attacked, of whom 11,486 died.

"While these things occurred at Malaga, similar circumstances took place at Alicant, where the inhabitants amounted to 13,957; 2110 left for the neighbouring country, where they remained free from attacks; while, of 11,847 in the focus of the disease, 6971 suffered, and 2472 died.

"At twenty other places besides those which I have mentioned, similar occurrences took place, with similar results.

"On the 15th September, the superior authorities, civil and military, of Palma, in Minorca, left the town for Valdemoya, a village situated three leagues from Palma. The citizens, following their example, made a precipitate retreat to the country beyond the walls; and the migration was so considerable, that of 32,000, 12,000 only remained. It was necessary to raise funds, secure means of maintenance, and build huts in the open country; and two encampments were formed at the foot of Mount Belver, half a league from Palma.

"All the healthy poor were assembled, all who, for want of work, were starving, and all who had strength removed thither, and thenceforth *security was found from the disease;* or, as Dr. Pariset expressed it, 'they escaped the *contagion.*' The 12,000 who remained in Palma, after the emigration of the rest, had 7400 sick, and 5341 deaths.

"It was calculated that from Barcelona, in 1821, about 80,000 persons fled; and, except some who departed with the disease already upon them, or who were on the eve of being attacked, *all remained exempt* from the reigning malady; though subjected, as Dr. Lassis well knew, to great privations, for he says, 'These refugees from Barcelona experienced all sorts of vexations.' But in spite of all their sufferings, they remained free from the epidemic.

"On the necessity of migration on the outbreak of yellow fever, I shall now merely add a few words from the Baron Dupuytren, in his Report to the Academy of Sciences in Paris, 1825:

"'We should therefore regard as *incontestable* the principle which consists in evacuating immediately the places where Yellow Fever is declared to be; and everything for this purpose should be adopted. The utility of such a measure must always justify its rigorous execution.' "*

What we consider, then, as the most important general principle that has been ascertained in this department of practical medicine, is, that as to several of the most virulent of the epidemics which have been permitted to afflict mankind, the means of effectually restraining their diffusion are known, but cannot be carried into effect without both trouble and expense to the community that is to benefit by them; and that these means are *very considerably different* in the case of these different diseases—in the case, particularly, of small-pox, of erysipelas, of puerperal fever, of plague, of cholera, of continued fever, and of yellow fever.

As to the Yellow Fever,—regarding it as originating in an aggravated form of the remittent from malaria, and therefore affording illustrations of the natural history of that destructive agent in its most virulent form, —we can go somewhat farther. Recollecting what has been stated of the very limited origin of the Sausage poison, and of the cadaveric poison exciting erythematic inflammation,—not in putrefaction generally, but in a certain stage of the decomposition of certain matters only,—we shall be prepared to hope that a somewhat similar *limitation* may be ascertained for the development of this poison likewise; more particularly as we have already stated, that both in this climate, where it (in general at least) only excites ague, and in the hot climates, where, under a temperature of 80° or more, of some duration, it excites so much more formidable diseases, all the conditions which we can as yet point out as essential to its development may exist without its showing itself. On this point the experience of medical officers of the army and navy, for the reasons already given, being the result of observations on organized masses of men, is the most valuable; and facts are already known which may often be applied to practical use, and promise to lead hereafter to more minute and more uniformly applicable information. Not only is the general measure of removing the population of a district known to be malarious always to be recommended when the yellow fever shows itself, but *precise limits* may sometimes be assigned. It seems certain that the poison often originates in parts of the timber of certain ships, and is long confined to them.

"Notwithstanding," says Dr. M'Kinlay, "that the poison causing the disease, whatever it may have been, appeared to be so concentrated in Rio harbour, in the middle of March, [as to cause 45 cases out of a crew of 145 in the *Cormorant*, within a few days, and to cause not less than 15,000 deaths within nine months, in the city,] some ships escaped the disease. When H.M.S. *Cormorant* arrived in the harbour, on the 16th March, we found that H.M. brigantine *Spider* had been there for about a week before us. She sailed on the same day that we did. She was at anchor about half way, and in a direct line, between us and the city. So much was she supposed to be in a more dangerous position than we were, that the senior officer had serious thoughts of ordering her to move to a position more distant from the city. Communication between her and the shore was not at all interrupted. She was also in the harbour at various other times during the prevalence of the epidemic, and *never had a case of fever*.

"The United States ships of war, *Brandywine* and *St. Louis*, arrived from the Rio de la Plata at Rio de Janeiro in company, early in March. They remained about three days, sailed together for Monte Video, and had been at anchor in the

* Second Report on Quarantine, &c., p. 202.

same part of Rio harbour: the *Brandywine* lost three officers and six men; the *St. Louis did not get the disease at all.*"*

Such cases may lead us to suspect that it is always by some matter undergoing decomposition in certan ships, in those climates, rather than by malaria from ashore, that the ships' crews are affected; and of the frequency of malaria in hot climates, adequate to the production of the worst yellow fever, appearing as the result of decomposition of vegetable matter in ships, in certain parts of ships, and probably in certain stages of the decomposition, we have nearly unequivocal evidence.

"At the time of the battle of the 12th of April, 1782, there was not a sickly ship in our fleet; but many of those officers and men who were sent to take care of the French prizes were seized with yellow fever; and it was observed, that when at any time the holds of these ships, which were full of putrid matter, were stirred, *there was an evident increase of these fevers soon after.*"†

"In June, 1823, a brig named *Donastiana* sailed from the Havannah with a clean bill of health. She arrived at Passages on the 3rd August, with all on board healthy. The cargo, consisting chiefly of sugar and tobacco, was discharged, and for several days many people went aboard, without disease showing itself, either among them or the crew. On the 15th a custom-house officer, who had been several days on board, and said to have been much in the hold, looking after contraband goods, was taken ill, and died on the third day, with black vomit. On the 22nd, a man who had been in the hold, surveying the ship's timbers, likewise died. Some of the planks on one of the vessel's sides having been found greatly decayed, 12 carpenters were employed in removing them; and 6 of these men were attacked, in quick succession. This opening in the ship commenced on the 19th, and on the 23rd the disease began to appear, in an unequivocal form, in the houses close to where she was moored. The disease did *not extend beyond the houses opposite the ship:* where persons were attacked whose habitations were at a distance, *they had remained for some time within the space to which the malaria from the ship seems to have been limited.* The heat was 96°, and the course of the wind favoured the emanations from the ship to the houses."‡

"Official records at Somerset House show, that in the year 1794, yellow fever was not prevalent in Gibraltar, when the *Bedford* arrived, with her crew in health, on the 24th of August, from the Mediterranean. On the 6th of September, the crew having been mustered, every man answered to his name; but within a week after, 130 were sent to hospital, with fever possessing the characteristic symptoms; 11 died before the 24th of September, and others were left dangerously ill when the ship departed, on that day. The only feasible cause assigned was the shifting of the shingle ballast, with the object of trimming her. *The disease did not extend beyond the crew.*"§

"The navy," says Dr. M'Kinlay, "furnishes many instances of ships which appear to have been particularly haunted by fever; these it would be interesting to trace, were the materials available. The *Tweed* we have seen to have suffered severely in Brazil; she suffered severely, also, in a previous commission. The *Vestal* had much yellow fever in the West Indies, and in a subsequent commission in India. I remember she had much fatal fever at Trincomalee, when other ships present, and on the station, had none. The celebrated *Eclair,* now the *Rosamond,* furnishes us with a notable example. Her history in 1845 will not be easily forgotten; and now, after an interval of seven years, she has suffered so much from yellow fever in the West Indies, that the Admiralty have thought it necessary to order her to England. It would be easy to multiply instances, but they would be

* Dr. M'Kinlay on Yellow Fever on the Coast of Brazil: Monthly Journal of Medical Science, Oct. 1852, p. 339.

† Blane on the Diseases of Seamen, p. 608.

‡ Second Report of the Board of Health on Quarantine, p. 194.

§ Ibid., p. 202.

uninteresting and unprofitable, without a full detail of all the concurrent circumstances, which I am unable to give "*

But if it be supposed that it is the effect of the climate on any part of the materials of the ships, which is the *sole* cause of the evolution of the poison, this theory is again checked by the fact, frequently observed, that when the disease exists ashore vessels lying near the shore are thus affected, and that simply by standing out to sea, without change of climate, the succession of the fever cases is often checked.

"This was so often observed, and so clearly marked in the *Cormorant*, that several who at first ridiculed the idea of getting the disease by remaining a few days in harbour, and avoiding it by keeping at sea, ultimately became convinced of the propriety of the recommendations made to that effect; and the ships often anchored in the little bays along the coast, about the same time, had free communication with the villages and their inhabitants, without ever being suspected of either receiving or communicating the disease at those places."†

It is plain, from these facts, that the poison is developed occasionally—in the hot climates frequently—under conditions still imperfectly known, where *decomposition* of organized matter is going on, under a certain temperature, and we may add, from what has been already stated, often independently of perceptible *putrefaction*, both aboard ship and ashore, *but within narrow limits of space*, and in general its extension is bounded by narrow limits likewise; and these limits are the point which at this moment especially demands investigation.

"That sandy soils," says Dr. Ferguson, "should, in malarious climates, prove as productive of aggravated remittent fever as the swamp, has never been sufficiently explained. Certain it is, however, that they do so, in a marked and prominent degree. The Alentejo and Algarve of Portugal, regions, I may say, altogether of sand, are the most prolific of fever of any in the Peninsula."

"Bulama," says Dr. M'Culloch, "is said to be the original seat of the yellow fever. The city is well built, and the streets clean, but it is surrounded by sand-hills and ponds of stagnant water, which, within the Tropics, are quite enough to generate disease. The inhabitants and those accustomed to the climate are not subject to this formidable disease; but all strangers, even those from Havannah and the West India Islands, are liable to the infection. No precautions can prevent its attack, and many have died at Xalapa, on the road to Mexico, who merely passed through this pestilential spot."‡

It would seem to be nearly ascertained, by the observations chiefly of a *clerical* philanthropist, M. Billier, Archbishop of Chambery, that the origin of the poison producing goître and cretinism is nearly analogous to that of the malaria, and no doubt the special conditions necessary to its very partial development will one day be made out.§

This line of inquiry, although thus distinctly suggesting itself, has not yet been prosecuted to any decisive result. The malaria has been thought by some—e. g., by Dr. Wilson, in his 'Medical Sketches'—to be very often evolved from timber simply moistened and slowly dried, not only in ships but in logs of wood lying on the sea-shores in the hot climates. The dead wood of the mangroves, from which the living branches spring—giving to a grove of mangroves, at the mouth of an African river, the

* Loc. cit., p. 426. † Dr. M'Kinlay, loc. cit., pp. 287, 288.
‡ Second Report on Quarantine and Yellow Fever, 1852, pp. 108, 109.
§ See Actes de l'Académie de Savoie, published at Chambery, 1850.

aspect of green boughs stuck in basket work—has been accused by some; the simply newly turned up soil, particularly granite soil, in China, the mixture of salt with fresh water in marshes, by others. But we think it obvious, that by observations made on cases *of the disease itself*, especially when most intense and most partial, this grand problem may yet be solved; and it is quite possible that in this way the Malaria may be traced to as limited an origin as the "cadaveric poison" of the Vienna hospitals, and the most prolific cause of disease and of death by which the human race has been afflicted, may thus be disarmed of its powers, according to the strict maxim of science—*Natura non vincitur nisi parendo.*

The history of the yellow fever which originated last autumn in the vessel *Plata*, on the voyage from St. Thomas's, recorded by Dr. Harvey and Mr. Wiblin, of Southampton, is nearly enough to establish two important points:—1. That as one of the rapidly fatal cases originated (no doubt from the poison imbibed in the hot climates) after the arrival at Southampton, the effect of the colder climate in restraining the disease must be, not to disqualify the human body from taking it, but to modify the nature of the poison; and 2, That whether introduced into that vessel by invalids from others, or originating in itself, the poison was confined, in that as in other cases, to particular *parts of the ship;* no case having occurred in the cabins of any of the passengers, nor of several of the officers, although fourteen in all, of the officers and crew, suffered during the voyage.*

Whatever be the *origin* of the malaria, careful observations have already led to the knowledge of some facts as to its *diffusion* over the earth, which may often be turned to practical account. We are not aware that it has ever been suspected to be developed in ships in latitudes north of 48°; and we have facts to show, that in these latitudes at least, it cannot pass over a thousand yards of water without being deprived of its power. The following observation was made by Sir John Pringle on the fevers of Walcheren and South Beveland, in 1747:

"These epidemic fevers, by reason of the great heats of the season, not only began more early than usual, but were fully as fatal to the natives as to us. But Commodore Mitchell's squadron, which lay all this time at anchor in the channel between South Beveland and Walcheren, in both which places the distempers raged, was neither afflicted with fever nor flux, but amid all that sickness enjoyed perfect health; a proof," he says, "that the moist and putrid air of the marshes was dissipated or corrected before it could reach them."†

The very same observation was made at the very same spot, fifty-two years after, by Sir Gilbert Blane:

"I had, in the course of this service (at Walcheren, in 1809,) an opportunity of observing the extent to which the noxious exhalations extended, which was found to be less than I believe is generally known. Not only the crews of the ships in the Road of Flushing were entirely free from this endemic, but also the guardships stationed in the narrow channel between this island and South Beveland. The width of this channel is about six thousand feet, and although some of the ships lay much nearer to the one shore than the other, there was no instance of any of their officers or crew being taken ill with the same disorder as that with which the troops on shore were affected."‡

* See Lancet, Feb. 1853. † Diseases of the Army, p. 58.
‡ Medico-Chirurgical Transactions, vol. iii. p. 27.

Another property of the Malaria, which may be availed of in many cases for the prevention of disease, is its being distinctly less violent at any sensible elevation above the level of the sea, than on that level,—excepting only in the curious cases stated by Dr. Ferguson, where a height lies to leeward (in the region of Trade winds) of a malarious marsh—and the poison blowing over the marsh, and apparently resisted by the hill, becomes concentrated by ascending to its summit, and there, contrary to the usual case, is more virulent than below. That the windward side of a malarious plain is the least dangerous,—that the poison is dissipated in some degree by the heat of the sun, so that a marsh may be traversed with more safety at noonday than by night,—and that it would seem to attach itself to trees, so as to be most virulent immediately beneath their shade, and less apt to extend itself to houses which are protected by umbrageous trees intervening between them and the marsh, than to such as have no such protection,—are all facts which would seem to have been repeatedly observed. And it is equally certain, that the effect of *cultivation,* although slow, is always to diminish the intensity of the poison,—insomuch that it may probably be owing to gradually improved and extended cultivation that many parts of Britain have become free from ague; and owing to the reverse process that the Maremma of Italy has become pestiferous.

After the full discussion in a recent number, of the influence of predisposing causes on epidemics, we need not recur to that part of the subject; indeed, the effect at least of pollution of the air as a predisposing cause of all epidemics, has of late years attracted its full share of attention in this country; and our chief object on this occasion has been to show how the evidence of statistics may be applied, with good prospect of success, to inquiries into the limited and specific *exciting causes* which are assisted, but in general certainly not superseded, by any circumstances of predisposition, in producing these calamities.

One additional fact we must always keep in view, whatever mode of explanation we may devise: that as to all those destructive powers influencing the human race—just as in regard to analogous plagues, which effect the destruction of the lower animals and of vegetables—we must recognise epidemic changes;—i. e., causes of increase and of diminution—connected indeed, occasionally, with circumstances of weather and season, but recurring from time to time in a manner which we cannot connect with any such conditions,—of which we ought to be aware, and for which we ought to be on the watch,—but which we cannot pretend either to explain or to foretel. It is one of the most striking general facts which has presented itself in the course of our observation of disease, that such variations of the frequency, and of the general type, not merely of epidemics but of all other, and especially of inflammatory diseases—not referable to the influence of any known agent—has manifested itself gradually but unequivocally in the course of the last forty years; and convinced us that there must be powers appointed by Nature for thus influencing human life, which are not yet " dreamt of in our philosophy."

W. P. Alison.

Review II.

1. *Traité de Thérapeutique des Maladies des Articulations.* Par A. Bonnet, Professeur de Clinique Chirurgicale à l'Ecole de Médicine de Lyon, Membre Correspondant de l'Academie Impériale de Médicine.—*Paris*, 1853.

On the Treatment of Diseases of the Joints, &c.

2. *Beiträge zur Vergleichenden Pathologischen Anatomie der Gelenkkrankheiten.* Von Dr. E. Gurlt, Prakt-Artze und Assistenz-Artze der Chirurgish-Augenartzlichen Klinik der Universitäl zu Berlin. 1853.

Contributions to the Comparative Pathological Anatomy of Diseases of the Joints.

Since the time when Sir Benjamin Brodie first published his valuable work upon the diseases of joints, pathologists, both in this and in other countries, following the path which he pointed out, and enjoying the advantage of starting from a recognised point of inquiry, have contributed largely the results of their observations and experience. The employment of the microscope has established more correct ideas of the normal structure of articular tissues, and of the changes produced by disease; more patient and extended investigations have increased our reliance upon the natural powers of repair; while, to the surgeon, the discovery of anæsthetic agents has given the means of employing remedies which were heretofore inapplicable from the suffering which they caused. The subcutaneous section of tendons, established by Stromeyer, Dieffenbach, and others, has rendered contracted limbs again useful, and has arrested the pain attending chronic morbid processes by re-establishing the proper bearings of the bones. But yet we cannot boast of a very comprehensive survey into this interesting subject. Morbid anatomy, aided by both chemical analysis and microscopical research, does not expound the varieties of apparently the same affection, and hence, at every step, clinical observations become more and more necessary.

Taking the two works cited above as our text, we propose, in the present number, after offering a few remarks upon the normal condition of articular tissues, to review their general pathology.

The broad and expanded articular extremities of bones are composed of spongy texture, coated by a delicate lamella of compact tissue, about one line thick beyond the circumference of the cartilage, but thinner under this structure. Articular cartilage is composed of intercellular substance and of cells, the latter arranged, near to the bone, in vertical linear series, but towards the free surface flattened and spread out in a layer, incorrectly described as an epithelial coat of the synovial membrane. "These cartilages," observes Kölliker,* "are in the adult non-vascular, although vessels may spread over their surface from the neighbouring synovial membrane;" and that which Liston† describes as pathologically-developed cartilage vessels running into the tissue in parallel lines from the bone, and then returning after forming loops, is nothing more than the remains of fœtal structure, which may be persistent up to the eighteenth year.

* Handb. der Gewebl., § 223. † Med.-Chir. Trans., London, 1840, p. 94.

Therefore the term inflammation of the articular cartilages cannot now be used, although they may suffer morbid changes secondarily, in consequence of disease in the synovial membrane, or in the bone; or may undergo fibrous degeneration, or removal in various ways, so that the extremities of the bones become either partially or entirely denuded. Inter-articular cartilages and the elastic rims of bony cavities are composed of fibrous tissue combined with cartilage. They are found connected, in some situations, with the articular cartilages; in others, with the ligaments or with the periosteum; neither bloodvessels nor nerves have been traced into them. Fibro-cartilages have usually a superficial investment of epithelium.

Ligaments and capsules are strong, inelastic structures, composed of white fibrous tissue. Their bloodvessels are few, and widely separated, and nerve-filaments have not yet been satisfactorily traced. They are found strongest in situations where the movements are limited to certain definite directions, as in the hip. The thigh cannot be extended beyond a line drawn vertically through the long axis of the trunk without rupture of the capsular membrane. When bones are so mechanically fitted that their movements are regulated, as in the elbow or the ankle, no distinct fibrous capsule is requisite; parts become strongly developed to which the name of "lateral ligaments" is given, but the surrounding tendons constitute the general articular investment. Of the strength of these fibrous capsules an example occurred in March, 1853, in a strong countryman, who, supposed to be suffering from a recent dislocation of the hip, was twice subjected to extension with ropes aided by pulleys, the muscles having been previously relaxed by the administration of chloroform. Twice did the rope break, without effect. The patient was then sent to St. Bartholomew's Hospital, where it was found that the head of the femur had never left its natural position.

Until recently, the synovial membrane was regarded as a shut sac, and as allied to serous membranes. To this view we still adhere; although in the adult it is not continued over the articular extremities of the bones, and in a state of disease may become continuous with fistulous passages opening externally. Its pathological conditions bear a close analogy to those of serous membranes, and none to those of mucous membranes. Inflammation causes rapid effusion of watery serum, or of coagulable lymph, which, as in the pleura or peritoneum, may become organized. It is composed of vessels and nerves connected by a delicate areolar tissue. and covered by tesselated epithelium. There are neither secreting glands, nor papillæ, such as are in mucous membranes; but masses of fat, containing many bloodvessels and covered by the membrane, present themselves in certain situations, where they have received the name of synovial glands, ligamenta mucosa, &c. Kölliker remarks that they differ from the synovial membrane in other parts by their great vascularity, being made up of little else than a congeries of capillary arteries and veins. This statement has long since been proved by the elaborate preparations of Mr. Quekett, of the Royal College of Surgeons, London. Upon their free border are prolonged villous appendages of most varied form; sometimes, but not always, they contain bloodvessels; frequently there are seen in them cartilage-cells, and they are covered with a layer of epithelium.

They are important as taking an active part in many morbid changes. Nerve filaments are here comparatively numerous.

Every band and ligament has its peculiar function. Its direction, length, and attachments, have their proper signification; and the comprehension of this subject, much elucidated by the investigations of the Webers, throws important light upon some of the principles of treatment. Thus, in the hip, forced extension backwards must cease at the proper point; the humerus must not be expected to rotate freely in every direction. When the articular cartilages of the knee are diseased, the forced maintenance of the limb in an extended position is painful, because the tightened lateral ligaments hold the bones firmly pressed together. Rotation of the leg upon the thigh is permissible only when the knee is bent.

Bonnet,* desirous of learning which was the weakest part of the capsule, and where rupture or perforation would most readily take place, forcibly distended the different joints in the human subject with injection. The forcible filling of a joint with fluid throws the bones into that position in which the cavity is largest; the knee becomes flexed; in the hip, the thigh is bent upon the abdomen, abducted and everted; in the shoulder, the arm is abducted to 35° and slightly bent forward; when the elbow-joint is distended, the forearm is bent at a right angle, the hand midway between supination and pronation; in distension of the wrist-joint, the hand is straight; in distension of the ankle, the toes are pointed downwards. Fluid thus thrown into a joint effects a separation of the articular extremities of the bone; the capsule assumes a knobbed or lobulated appearance, from its unequal power of resistance. Forcible movements, under these circumstances, will cause laceration of the synovial membrane and rupture of the fibrous capsule in that spot where it is naturally weakest: as, for example, in the knee-joint, in front, under the quadriceps extensor cruris; in the hip, near the notch in the acetabulum, or near the trochanter minor; in the shoulder, by the insertion of the subscapularis tendon; in the elbow, either by the insertion of the triceps, or into the anterior intercondyloid fossa; in the ankle, the capsule gives way both in the front and the back of the limb. The fluid becomes infiltrated among the surrounding structures.

It has often been remarked how great is the difference in the frequency with which certain joints are attacked by disease, and how exempt others seem to be from the like affections. The former statement is usually illustrated by the affections of the hip and the knee, and the latter by those of the lower jaw, the sterno-clavicular articulations, and those of the heads of the ribs with the vertebræ. If it be from the greater or less exposure to external influences, why should the hip be more frequently attacked than the ankle-joint? if from the activity of their functions, why should the lower jaw escape?

No one hypothesis will suffice to explain the proximate causes of the many diseases, both constitutional and local, attacking every kind of tissue in joints of such different sizes and in such opposite relations. That parts, formed expressly for movement, should in a healthy individual discharge their functions uninterruptedly through life, need occasion no

* Traité des Maladies des Articulations, tom i. p. 50.

surprise; we must therefore seek, in every instance of exception, some specially acting influence. *Cæteris paribus,* the larger the joint the more prone is it to suffer from inflammatory diseases, especially when, as is the case with the knee, it is exposed to the action of atmospheric changes. But both the hip and the knee, through which the weight of the body is transmitted, are liable from constant pressure in an unhealthily organized individual, to an exciting cause of disturbance, which does not act in the shoulder or the elbow. The synovial membrane of the elbow, though of limited extent and attached to a segment of the body where the circulation is usually active, becomes inflamed, like the synovial membrane of the knee, in weakly girls, exposed to cold with insufficient dress and bare arms. The ankle-joint, removed to a distance from the centre of circulation, of moderate size and extent of membrane, rarely suffers from acute inflammation, unless of specific character, but is commonly the seat of more chronic morbid changes occurring in a scrofulous constitution, and probably connected with the deposit of tubercle; here, too, there is, among other phenomena, a marked indisposition to any effort at repair. Bony anchylosis is rare in the ankle as contrasted with the wrist. Of the former, we have secured but one specimen during nine years; of the latter, specimens are much more common.

The comparative exemption from disease of the costo-vertebral and temporo-maxillary articulations, shows the importance of size and extent of surface. In this list might be included the phalangeal articulations of the hand and foot; what joint is in more constant movement than the trapezio-metacarpal articulation of the thumb? what parts are more exposed to heat, to cold, to pressure, and to injury, than the joints of the great toe? and yet here idiopathic disease is rare. The temporo-maxillary articulation is one in which all circumstances combine to give it that immunity, without which the function of mastication would be attended with distress and suffering: its naturally limited cavity is divided by an interarticular fibro cartilage; it has two small synovial membranes, instead of one of large size. It has no weight to sustain, but is surrounded by highly-organized structures, which announce the earliest sensations of fatigue. Similar remarks apply in part to the sterno-clavicular articulation. In the horse, the fore-legs, which receive the weight of the body in springing, leaping, &c., are more commonly diseased than the posterior, the reverse of what happens in the human subject.

"I must confess," observes Sir Benjamin Brodie,* "in proportion as I have acquired a more extended experience in my profession, I have found more and more reason to believe that local diseases, in the strict sense of the word, are comparatively rare." To the gouty diathesis, in those cases in which there is an unusual quantity of lithic acid in the blood, as lately described by Dr. Garrod, he ascribes the most common cause of synovitis; and he mentions other cases, which resulted in the deposit of lithate of soda, both within the joint and external to it, presenting a mixed character of gout and rheumatism. Virchow, in writing upon affections of joints, enumerates rheumatism, gout, and podagra, among the exciting causes; and there are, doubtless, in these three diseases, varieties of

* Pathological and Surgical Observations on the Diseases of Joints, 1850.

character sufficient to impress its stamp upon the form which the articular affection will assume.

Diseases of the Synovial Membrane.—(1.) Acute inflammation of the synovial membrane may be artificially excited in animals, by injections of iodine and other irritating fluids; and the changes thus produced resemble those proceeding from internal causes. After the experiments of Bouley,* and of Rey,† there was noticed redness in the sub-synovial tissue, especially about those vascular structures called the synovial glands, but fainter where the synovial membrane was tighter, and more intimately connected with subjacent parts. Upon the free surface of the membrane, where the redness was intense, there was a continuous layer of plastic exudation, either thin and transparent, or thick, more opaque and rough. Shreds of lymph were noticed free in the joint, of light yellow and even dark-brown hue, and soaked in synovial fluid. The surrounding areolar tissue was infiltrated with a jelly-like substance, which often extended to a great distance among the muscles and tendons. MM. Richet and Lebert endeavoured to follow the same course of observation in man. Acute inflammation is characterised by rapid effusion of serum into the articular cavity; the vessels of the sub-synovial tissue are first congested, then those of the free surface of the membrane, which becomes thickened and bilious, varying in colour from a light rose tint to a brownish hue. Rokitansky has remarked how many synovial membranes have a very delicate structure, so much so that the reddening may be very slight, or even imperceptible in inflammation; the vascular fringes, or ligamenta mucosa, are in many joints so small and so concealed, as readily to elude observation: while the articular cartilages, over which not even the epithelial layer of the synovial membrane passes, retain their normal white colour. The effusion is serous, fibrinous, or purulent; the first being the usual product of increased arterial action; the second, occurring more rarely, forming a continuous, though shreddy, layer over the whole inner surface of the membrane: and uniting, in some cases, to constitute the organized bands of fibrous anchylosis; the third, occurring in severe rheumatic affections, in metastasis after parturition, &c., &c.

The fluid effused in the slighter cases resembles synovia, though turbid, and of more watery consistence; in severer cases it may contain, beside the flakes of lymph, some of the coloured elements of the blood. The more abundant the serous fluid, the more are the opposable surfaces of the bones kept apart, provided that position into which the limb naturally falls be unaltered; the surfaces of an adventitious membrane are thus often kept asunder, and prevented from contracting adhesions. The puriform fluid is usually diluted with synovia; it is rare to find it thick and cream-like within a joint. The secretion comes from the bloodvessels either of the synovial membrane, or of some organized adventitious structure, and cannot be referred, as Rokitansky avers, to degeneration of the plastic exudation, in consequence of some peculiar quality inherent in it. The changes, however, which ensue in cases of acute synovitis, often spread from the original seat of disease, and involve other parts.

* Recueil de Med. Vétèrin. Pract., tom. xxiv. p. 1, 1817.
† Journal de Med. Vétèrin., Ecole de Lyon, t. iii. p. 128.

The treatment of acute synovitis by mercury does not receive from Bonnet the same high praise accorded to it by the English surgeons:

> "Preparations of calomel, much used in England, are given in doses either purgative or alterative. In the first case, ten or twelve grains are given in a single dose; in the second, according to the practice of Law, a demi-centigramme (one-tenth of a grain) is given every hour, so as to excite salivation in the course of a few days. Mercurial frictions (one drachm to one ounce daily) are likewise employed for the same purpose. There is no accurate work which establishes the nature of the influence of the medicines. In the country where they are used they are sanctioned by tradition, but by no experience which has confirmed their effects in a vigorous manner." *

The doses here mentioned as customary in English practice are not quite accurate. About five grains are commonly administered as a purgative; while, for purposes of salivation, two grains (ten centigrammes) are given three or four times a day. Perhaps we may to a degree merit the reproof of a too careless veneration for tradition in the employment of this active remedy. We do not use it upon sufficiently fixed principles, and it might be a doubt whether it had occasioned more good or harm. We have seen it destroy a young girl suffering from acute synovitis, under the care of a most able and careful surgeon; and other cases illustrating serious results might easily be added. We do not think that mercury pushed to salivation should be given as the rule: it is unfit for the very young, the unhealthy, or the very old; its action is doubtful in specific inflammations, such as those of gout or rheumatism. Moreover, a healthy individual generally recovers, without treatment of this kind, from synovitis the result of accident. In cases, however, where inflammation of acute character becomes persistent in a strong subject, in spite of active treatment, then mercury may be administered so as to excite salivation with advantage.

In cases where the synovial membrane is much distended by its secretion, an exploratory puncture has been recommended with a grooved needle, that the fluid may be submitted to examination. "If it be serum, two or three more punctures may be made, and an exhausted cupping-glass be applied over them; if it be pus, a free opening should be made with a lancet in a depending position, so that the matter may run out easily."† We doubt whether the operation of puncturing inflamed joints can be often practised with safety to the patient. Although the sero-purulent fluid may be abstracted by the suction of the exhausted cupping glass, fresh secretion is poured forth from the inflamed membrane, and disease, moreover, may be propagated from its original seat to the articular cartilages and to the bones; the former becoming degenerated, the latter being denuded and roughened. "All operations," remarks Bonnet,‡ "by which the cavity of a joint is penetrated, such as extensive incisions for opening an abscess, or hydrarthrosis, or for the extraction of foreign bodies, or for resections, are extremely dangerous. . . . . They expose the patient to suppurations and to purulent resorptions, and are not uncommonly followed by death." As a rule, good effects do not ensue from the opening of a joint, whilst the articular cartilages retain their normal organiza-

* Bonnet, op. cit., p. 123. † Skey's Operative Surgery.
‡ Traité de Ther. des Malad. des Articul., p. 9.

tion; frequently most serious and destructive consequences have followed, when fortunate indeed is that patient who escapes with the preservation of the limb. A healthy man consulted a surgeon, in September, 1849, for stiffness of the right knee-joint, which, though able to support him, was swelled and painful, especially after exercise. A lancet was introduced into an elastic swelling near the edge of the patella, and there flowed from the opening a quantity of glairy fluid, like synovia. Within a very few hours the pain, which had heretofore been moderate, became insupportable; he was permanently confined to bed; abscesses formed about the condyle of the femur; the articular cartilages and some of the ligaments were destroyed; the bones were dislocated; and the patient, worn out by suffering, hectic fever, and diarrhœa, came to the hospital under the care of Mr. Lawrence, by whom the thigh was amputated.

The joints do not very frequently become affected in pyæmia. When such is the case, the surrounding structures are often infiltrated by pus. This disease occurs in connexion with the puerperal state; after infection by the poison of glanders; after surgical operations, or wounds of apparently trivial character. Mildner* has witnessed it in new-born infants. Valleix† describes a case in which it occurred in a child only twenty-three days old: there was separation of the epiphyses of several of the long bones. Arnott‡ saw purulent deposit in the knee-joint of a man who had shortly before been bled. Bouilland§ saw it in the shoulder-joint of a patient who had been similarly treated. Instances of its occurrence in the puerperal state are recorded by Dance, Beatty, Bonnet, Kiwish, Kennedy, and others; as a consequence of glanders, by Gintrac, Hutton, Gressent, Tardieu; after the acute exanthemata, by Ancell, Bonnet, Smith, Corrigan; and after violent dilatation of the urethra, by Moffait, Velpeau, and Coulson. Mr. Coulson found in the body of a man, æt. 70, who died after the operation of lithotrity, a thin chocolate-coloured fluid in the left knee-joint: the synovial membrane was injected, the articular cartilage covering the external condyle of the femur was softened and ulcerated, and the bone denuded. Pus was also found in the right knee; the cartilages were diseased, but to a less extent. The synovial membrane of the right hip was injected.‖ It is remarkable how slight may be the redness of the synovial membrane in some cases where the joint is thus distended, while in others the membrane becomes speedily absorbed, the articular cartilages disappear, and the pus, instead of preserving its usual yellow hue, assumes a variety of tints, from a light red to a dirty brown. In such cases the fibrous structures around become softened and infiltrated, and the muscles suffer in a similar way. In severer cases the ligaments are completely destroyed, and pus is found in he synovial thecæ of the tendons.

(2.) The term chronic inflammation is applied by Gurlt to that condition of the synovial membrane which leads to the production of a clear serous effusion, never of purulent character, and constitutes the hydrarthrosis of man and animals. It attacks synovial membranes of considerable extent, such as that of the knee, which frequently has its normal cavity enlarged

* Pragen Vierteljahrschrift, 1845.
† Arch. Gen. de Medecine, 1835.
‡ Medico-Chir. Trans., vol. xv.
§ Tr. Clin. du Rhum. Art., Obs. 35.
‖ The Lancet, 1852.

by communications with neighbouring bursæ mucosæ. Bonnet compares this fluid to the effusion in hydrocele, being transparent, yellow, and watery. It, however, coagulates when exposed to the action of heat. The synovial membrane is not at first much altered in texture: it is injected with blood, slightly thicker than natural, and adherent to subjacent tissues; but the articular cartilages are unaltered, although the synovial fringes, in time, become more prominent than usual. Rokitansky* has described these dendritic vegetations as dark-coloured prolongations covered with enlarged capillaries, and terminating in free extremities, round, club-shaped, or pointed. In many of them are found hard bodies, like lentil or melon seeds, sometimes in such numbers, that the whole villous appendage seems to be covered by them. There are also fat-cells mixed with fibrous tissue.

The disease may commence and run its course without violent symptoms; or it may come on as a lingering inflammation from previous acute disease, moderate in degree, but more acute from time to time. In such cases it furnishes a thin exudation, which is constantly augmenting in quantity; the membrane in time becomes more decidedly opaque, thick, and of a colour inclining to brown, and the surrounding tissues are infiltrated; or the membrane, thickened and highly vascular, yet secreting but a moderate amount of fluid, may project over the cartilages, causing their slow absorption. We do not, however, regard simple hydrarthrosis and pulpy degeneration of the synovial membrane as mere stages of one and the same disease. The former may be persistent without the development of the latter; while, in other cases, the thickening of the membrane is the character of the disease from the commencement. Upon this subject further investigations are required.

(3.) The conversion of the synovial membrane into an uniform thickened mass, of light yellowish-grey colour, intersected with white lines, has been improperly termed, by both French and German pathologists, "Fungus Articuli." It is apt to be thus confounded with cancerous diseases, with which it has no affinity. It may occur independent of any changes in the bones, ligaments, or other neighbouring tissues, and is most commonly seen in the knee. In one specimen which we examined, the synovial sheath, and the different bursæ mucosæ about the joint, were found affected with a similar morbid change. Its minute structure is characterized by a multitude of small round or oval corpuscles, each containing a nucleus and granular matter, supported in the meshes of a delicate fibrous stroma, in which are seen bloodvessels and oil-globules. We do not believe that the colour depends upon the number of the bloodvessels, except in so far as they may give a light pink tinge to the usual yellowish-brown hue. The consistence of the morbid structure varies according to the arrangement of the fibro-plastic element, which is effused into its tissue. Doubtless the change may extend into the neighbouring subsynovial fat. But Gurlt is in error, when he says that the thickness is due to the involving of the surrounding tissues. He affirms, that when the joint is superficial, even the subcutaneous areolar tissue becomes affected, and that the morbid structure may measure half an inch in thickness. We have dissected the synovial membrane of the knee, thickened to at least that extent, from

* Pathological Anatomy.

under the cruræus muscle, and have found no other structure involved. It is not uncommon to find small abscesses imbedded in the membranes. Nature seems to make an effort to remove these yellowish-brown corpuscles, which have so changed the characters of the synovial membrane. We have found them in abundance in the neighbouring absorbent glands, which had partially acquired a similar hue. In a limb removed by Mr. Lawrence in 1850, an opportunity was afforded of noticing more completely the process of repair. The tibia and fibula were drawn slightly forwards; the patella was movable, and rested against the external condyle of the femur. The knee-joint was divided into three distinct compartments, by adventitious bands and adhesions of the synovial membrane: one compartment being between the outer condyle of the femur and the outer articulating surface of the tibia; a second between the inner condyle of the femur and the internal articulating surface of the tibia; and a third between the patella and the front of the outer condyle of the femur. In two of these compartments, both the synovial membrane and the articular cartilages were in great part absorbed; the former, however, still presenting unmistakeable traces of the yellowish-brown disorganization; the latter being removed by absorption rather than by ulceration; organized fibrous bands extended between the opposed surfaces, limiting the movements of the bones. In the third compartment—namely, that between the inner condyle of the femur and the inner articulating surface of the tibia, that part where, in the healthy joint, the bones are held by their ligaments in the firmest opposition; and where the effects of friction would be most severely felt, the semilunar fibro cartilage, and the articular cartilages were entirely removed; the bony surfaces, rough and eroded, fitted into one another, so as partially to lock the joint; fragments of dead bone, nearly detached, were on the point of dropping into the articular cavity; and the remains of the synovial membrane, thickened, pulpy, and of light yellowish-brown colour, contained in its substance circumscribed collections of pus. Fistulous passages extended into this compartment of the joint, one being continued deeply into the popliteal space. The external lateral ligament was softened, elongated and entire; the internal was softened, pulpy, and spread out into a layer. The surrounding tendons were of their natural appearance; but the muscular fibre was pale, fatty, and immersed in a quantity of straw-coloured serum.

What was the history of this case? The patient had for some years been suffering from swelling of the knee produced by this pulpy degeneration of the synovial membrane; thickening had been followed by slow absorption of the membrane, by the formation of adventitious bands, and by the division of the large cavity into three separate compartments. In one of these, however, changes incompatible with recovery had taken place.

(4.) The treatment of hydrarthrosis by compression, and by the occasional employment of counter-irritants, has not received any important additions from modern discovery; and careful exercise is recommended in cases where it excites neither pain nor swelling. But of late, incision has been advocated, a practice against which experience has hitherto spoken. Boyer related the particulars of four cases in which the joint was opened for hydrarthrosis. In the first, referred to Lassus, there ensued suppuration of the knee, which demanded a counter opening; the patient recovered, but with a stiff joint. The same occurred in the second, operated upon by Warner. In the third, due to Schlichting, and the fourth, to Monro, there was purulent effusion and serious injury to the articular tissues. "Let it be remembered," says Bonnet,* "that in incision of the tunica vaginalis there is always suppuration of the membrane; and that the same occurs after incised wounds of the joint which do not unite by first intention."

It is not probable that hydrarthroses will be treated here by the introduction of a seton to excite adhesive inflammation, however such a practice may be recommended in other countries.† Nor will the system of rupturing the distended membrane by compression (l'écrasement) meet with greater favour, although it has been frequently effected upon distended bursæ. M. J. Guérin strongly recommends the subcutaneous puncture of distended joints;‡ and relates a case in which immediate cure followed the operation. The author adds, that he employs this method in all mono-articular hydrarthroses in which resorption does not go on after five or six days' treatment. Whatever success may attend such a practice, it is undoubtedly wrong to interfere with a recent effusion, as, in the majority of cases, it will be removed by the action of the absorbents. If a puncture be made, it is judicious to follow the evacuation of the fluid with firm compression, as recommended by M. Carrier, of the Saint Jean de Dieu Hospital, at Lyons.

M. Jules Guérin has attempted to cure hydrarthosis by the exhausting action of a syringe. He introduces a flat trochar to which a pipe admits of being accurately fitted; and having removed the fluid, irritates the inner surface of the membrane by rubbing against it the extremity of the instrument. Bonnet, who tried the practice, found that, although no inflammation ensued, the collection of fluid speedily returned.

The subcutaneous section of the cyst recommended by Professor Gerdy in the treatment of bursæ, and transplanted by M. Goyrand, of Aix, to that of hydrarthroses, may, from the experience of MM. Malgaigne and Bonnet, be practised with safety; but there is a great tendency to return of the disease.

But the treatment to which attention has of late been specially turned, is that of injection as performed by Gay in 1789, and by Jobert in 1830. The use of iodine was first tried by MM. Velpeau and Bonnet, and to them is due whatever merit the system of injecting a joint with an irritating fluid may acquire. Before we reject the practice, which we have been taught to regard with fear, let it be remembered that iodine may be safely injected into the cavity of a chronic abscess, the cyst of which is quite as vascular, perhaps even more so, than the synovial lining of a joint. M. Velpeau uses the tincture of iodine, diluted with one or two parts of water; Bonnet, in his first trials, used the tincture of iodine pure; of late he has tried the following formula:—Water, 16 parts; iodine, 2 parts; iodide of potassium, 4 parts. M. Barrier prefers equal parts of tincture of iodine and camphorated spirit. The fluid is allowed

* Traité des Maladies des Articul. † Gaz. des Hôpitaux, June 4, 1842.
‡ Gazette Médicale de Paris.

to remain in the joint about five minutes, when it may be wholly or only partly abstracted. MM. Bonnet and Barrier prefer leaving some in the sac. Bonnet confesses, although he recommends the stronger fluids, and especially that of M. Barrier, that severe inflammation may immediately ensue; and that once the consecutive tension became so great, that he feared he should be obliged to reopen the synovial cavity to let out the effused fluid. He denies, however, that there are any real dangers if the practice be followed with proper precautions; and MM. Velpeau, Jules Roux, Malgaigne, and Barrier have published cases supporting this favourable opinion. It has been tried, also, in cases of ulceration of the cartilages, and here with no success, the fluid remaining unabsorbed.

When the joint is sound, it appears, from the dissection of a knee in which this treatment had been successfully employed by M. J. Roux,* that the constituent parts retain their perfectly normal appearance. There were no fibrous bands of adhesion, as once presumed by both Bonnet and Velpeau. The iodine had, after momentarily exciting inflammation, altered the vitality of the membrane; fluid ceased to be effused in excess; and the limb regained its former freedom of movement.

(5.) The disorganization of the synovial membrane, and its conversion into a yellowish-brown, thick, and opaque structure, as before described, constitute a form of chronic inflammatory disease, which, when commenced, runs its course, and terminates either in fibrous anchylosis, or in inflammation and ulceration, and perhaps necrosis of the bones. The presence of pain does not of necessity indicate the progress of morbid action, as it may be severe during the stages of repair. During the whole period, the limb should be kept motionless, in an effective apparatus, and in that position which combines, with ease to the patient, the greatest possible amount of future usefulness in the event of anchylosis taking place. The usual forms of counter-irritation, such as blisters and issues, do not appear of sufficient force; the actual cautery is here especially applicable, and may be employed without pain. The practice, though fallen into disuse, is mentioned both by Hippocrates and by Celsus, and was followed by most of the Greek and Arabian physicians. The cauterization over such a superficial joint as the knee must act superficially, the rays of heat passing rapidly over the skin. The previous application of a bag of pounded ice for five or ten minutes so numbs the external parts that the patient may be rendered quite insensible to pain. The best instrument for the purpose is a long knife, the cutting part about three inches in length, and presenting a prominent convex border. If the joint be deeply seated, then heated buttons should be held sufficiently long to act upon the subcutaneous tissue. Bonnet praises this treatment. In the first form, which he designates " transcurrent cauterization," the surgeon should avoid completely dividing the skin; but if necessary, the knife may be carried, while heated, again and again along the same rays, so as to act as deeply as may be wished. Four to eight lines may be made over such a joint as the knee, each one being eight to ten inches in length. This is a more efficient method than the moxa, as introduced by Pouteau, and still occasionally used in modern surgery. The edges of the incisions being

* Gazette Médicale, 1849, p. 618.

charred, no immediate union can take place, and a discharge continues for many weeks.

Diseases of the Bursæ.—Acute and chronic inflammation occurs in bursæ, and in the synovial sheaths of tendons, producing effects similar to those observed in synovial membranes. Upon these subjects nothing need be here specially said. The treatment will be regulated by the common principles of surgery. It not uncommonly happens, however, that the synovial thecæ of the wrist, the large bursæ under the deltoid muscle, and other similar structures, become filled with a number of round, oval, or flattened bodies, some with a peduncle, others with none, the presence of which has given rise to much speculation. They are similar in their origin to one form of loose cartilage in joints, and we quote the description given by Hyrtl,* in illustration of their development.

"Hydatid swelling in the mucous bursæ of the flexor tendons of the carpus. The *external* fibrous layer was much thickened; 117 of these little bodies were contained within, some few having short slender peduncles. Between the inner serous and the outer fibrous layer of the membrane, the whole surface was studded with a continuous stratum of small knots, which gave it a glandular or warty appearance. Some of these bodies had become nearly detached, drawing over them a complete serous investment. They could be seen in all stages of development in the intervals between the tendons."

Gurlt† correctly considers them as hypertrophied parts of the villous appendages, already described in synovial membranes, and existing, though in a slighter degree, in the bursal sacs and synovial thecæ, as may be proved by minute injections. Mr. Stanley lately removed a number from the sub-deltoid bursa in a little girl; and a veterinary surgeon in Berlin found both loose bodies and calcareous masses in one of the subcutaneous bursæ of the horse.‡

It is remarkable that the synovial thecæ of the lower extremity should be less liable to disease than those of the upper, and yet so it is. An interesting case of this kind occurred in the practice of Mr. Stanley, and was reported in the 'Medical Times,' July 13, 1850. A young lady, aged 20, of slight frame, but in the enjoyment of fair health, greatly fatigued herself five years previously by a walk of extreme length, but she recovered in a few days, so as to go about as usual. From that time, however, the right foot and ankle were weak and painful, and swelled upon her taking the least exertion. About two years ago, the swelling became permanent, and the ankle, which was easy when at rest, felt so painful when she attempted to walk, that she was obliged to keep the horizontal position. Every variety of treatment was adopted without avail; the swelling about the joint increased, until the integuments gave way, and there was formed an ulcer, whence flowed a sero-purulent fluid. There was a large swelling between the tendo-Achillis and the os calcis, extending down to the sole of the foot. The swelling was partly firm, partly elastic; movement of the joint perfect. The limb was amputated below the knee. The morbid change was found to be in the synovial thecæ of the flexor tendons being strictly circumscribed and surrounded

* Med. Jahrb. d. k. k. osterr. Staates, Band xxxix.
† Beitrage zur pathol. Anatomie der Gelenkkrankheiten, § 52.
‡ Mus. d. Thierarznei-sch. zu Berlin, No. 1817.

by healthy structures. The thecæ were thickened; their usually bright synovial surface was converted into a dull, uneven, velvety structure, from which was produced a soft growth, composed of cytoblasts supported by a small quantity of delicate fibrous structure. The flexor tendons were unaltered in structure, but were surrounded by the soft friable growth above mentioned, and by a large quantity of thin synovial fluid, which seemed to be contained in more than one cavity; for behind the malleoli it was of pale-straw colour, and somewhat stringy; while in the sole of the foot it was of watery consistence, of red colour, and contained coagula of blood. The patient now enjoys good health; she has been married, and given birth to a healthy child.

We lately removed six bony masses, the size of large nuts, resembling in every respect the loose cartilages in joints, from the bursa of the semi-membranosus muscle.

Diseases of the Cartilages.—(1.) The experiments of Redfern* have shown that simple exposure of the articular cartilage, by incision, for a limited time, does not, in the lower animals, excite disease; that the application of heated iron is not followed of necessity by disorganization of this tissue; and that chemical agents cause rapid effusion into the joint, because it is not possible to limit their action to the cartilage, or to prevent their contact with the synovial membrane. Incised wounds of cartilage heal; partially detached portions become re-united, the deep cells being of normal appearance, those on the surface being granular, and the intercellular substance becoming obscurely fibrous. In some joints the cartilage-cells became loaded with earthy matter, and communicated to the knife a gritty sound, resembling the section of bone. The deep cells retained their normal vertical arrangement, differing from the healthy cells only in containing earthy matter. A precisely similar state was artificially produced in the patella of a rabbit, by the application of the actual cautery. Injured cartilage manifested a great disposition to heal by the formation of a thick fibrous membrane, or a thin osseous plate or scale. In no instance did the disease extend beyond the seat of injury into the sound cartilage further than was necessary for the formation of the cicatrix. There seems, therefore, reason to acquiesce in the opinion deduced from these experiments, that external irritation excites with difficulty disorganization of cartilage, and that inflammation, in the usual acceptation of the term, is impossible.

(2.) Hypertrophy is uncommon; it occurs in the form of knotty elevations, or of a more general thickening combined with softening. Under the microscope, the clear intercellular substance appears dark, opaque, and granular; the cells are larger than natural, and contain a number of smaller cells, which increase in size so as ultimately to burst through their capsule and become free. In the museum of St. Bartholomew's Hospital,† there is a preparation of the pelvis and the lower extremities of a young man, presented by Mr. Wormald. All the bones of the right side are atrophied; being shorter, less in circumference, softer, and lighter than those of the left limb. In compensation for this difference, the left foot is directed almost vertically, so that in the erect position the extremities

* Anormal Nutrition in the Human Articular Cartilages, with Experimental Researches on the Lower Animals.

† Series I. Sub-series A, p. 151.

of the toes of both limbs are at the same level; the neck of the femur is more oblique than on the right side. Both in the knee and in the hip joints, the articular cartilages had become hypertrophied, so as to constitute a layer more than twice the normal thickness between the articular extremities of the bones. Atrophy is common in bed-ridden subjects; the remains of the cartilage are thin, especially at the free borders, but adherent, and there may be isolated spots of abrasion resembling ulcers. In the museum of St. Bartholomew's Hospital is a specimen of symmetrical absorption of the articular cartilages in the ulnæ of an old woman, the space thus left on the surface of each being filled by a vascular growth, like a process of synovial membrane. The specimens are described by Mr. Paget in the 'Medico-Chirurgical Transactions,' vol. xxv.

(3.) The term "ulceration of cartilage" is applied by Sir B. Brodie to a process of disintegration of the cartilage-cells, in course of which a kind of fibrous degeneration ensues, which has attracted the attention of microscopic observers. Parts of the cartilage appear rough, abraded, and velvety; and present, in those situations where the change has gone on more deeply, a felted appearance, which closer inspection will show to result from the reduction of the cartilage to numerous fine filaments or fibres, attached by their deep, but free at their superficial extremity. In a yet more advanced stage is seen an excavation, at the base of which the bone appears either covered by a fibrous layer or altogether denuded. Fissures and radiating lines or grooves are also met with, generally corresponding with the axis of motion.

The cells first become enlarged to from five to six times their normal size; they lose their regular arrangement, and are scattered, instead of being collected into groups or columns; the nuclei disappear, granular matter fills the interior of the cell, or bursts through the wall, becoming free in the intercellular substance.

"This notion of fissiparous reproduction," observes Mr. Kingdon,* "is still further supported by the minute investigation of the disease known as 'ulceration of cartilage,' where the cells, and more especially the nuclei, divide and subdivide so rapidly as to break up the cartilage into a finely molecular condition, similar in appearance to fatty degeneration of other organs: from which we may reasonably infer that this power of reproduction is quite independent of growth, and seems to be inherited by the nucleus from the primitive germ-cell."

As the examination proceeds towards the spots of more active disease, we find the deep cells, thus split into two or three smaller cells, assume, from the direction of the fissure, a vertical linear arrangement; the larger superficial cells, yet more numerously subdivided, are almost filamentous in character, while others appear to have burst and discharged their contents. Thus "ulceration of cartilage," commencing from its free surface, seems to consist in a process of disarrangement of the cartilage-cells; their enlargement, accompanied with rapid subdivision and molecular disintegration of the nucleus; fission of the enlarged cells, and their arrangement in linear series resembling velvety tufts; finally, the rupture of the cells, the discharge of their granular contents, and their disappearance, leaving ultimately exposed the bone, which either participates in the disease, or throws out a plastic fluid to heal the breach of surface with a cicatrix.

* On the Development of Loose Cartilaginous Bodies: Trans. of Abernethian Society, 1851.

In the acute form of disease the intercellular substance yet remains in parts; but in the less active forms, according to Gurlt (op. cit.), the hyaline substance splits, so as to add to the fibrous appearance presented by the diseased cartilage. Sir B. Brodie observes upon this subject:

"Mr. Birkett, in 'Guy's Hospital Reports,' observes that it is now a well-established fact, that there is no such morbid process as ulceration of the cartilage. The destruction of this tissue depends on a loss of nutrition and disintegration, in which the development of fat forms a part. But," observes Sir Benjamin, "it may be a question whether ulceration, in whatever part of the body it occurs, is not a process of disintegration dependent upon want of nutrition."*

Between the filaments and fissures is found a gelatinous substance, adherent to the fibres; the arrangement of the cells at their point of continuity with the fissured cartilage, shows that they too partake in the vertical direction of the fibres. The membrane which heals these spots, or which covers the denuded extremity of the bones, presents the same fibrous character. Acetic acid first renders indistinct, and then dissolves this morbid structure, whence it is inferred that in the process of repair gelatine takes the place of chondrin.

(4.) The term fatty degeneration is sometimes applied to ulceration of cartilage, from the oily appearance of the intra-cellular granules into which the nucleus has divided. That this is not essentially connected with the whole process, says Gurlt, is proved by the circumstance that these oil-drops are only occasionally present, the granules being sometimes developed into nucleated corpuscles.

The direction of the superficial grooves and fissures would show that pressure and friction exercise some influence upon the morbid condition of cartilages. But we cannot believe that fluid in the joint ever can become an active agent. Gurlt thinks, however, such may be the case, because under such circumstances the cartilaginous surface is found dull and of a dirty yellow hue; and he refers to the experiments of Richet,* who noticed, both in living animals and in the dead subject, that coloured fluid would permeate the cartilage to a considerable extent. Mr. A. Kingdon, in the work referred to, attributes the growth of loose cartilaginous bodies within the synovial membrane to an assimilative power exercised by the cartilage cell upon the synovial secretion. This subject requires further investigation. In its present state it affords no sufficient argument for the practice of puncturing joints with the intent to evacuate fluid, supposed to be acting upon the cartilage. The evil may be hypothetical; while the operation is fraught with risk. With regard to the great pain experienced by patients suffering under this disease, Sir B. Brodie attributes the increased sensibility of these parts to the bony plate beneath the cartilage, rather than to the cartilage itself; and he remarks, that in inflammation of the synovial membrane there is effusion of fluid into the joint in the early stages: in the primary disease of the cartilage there is generally none.

(5.) Fibrous degeneration of the articular cartilage, with absorption, occurs independent of changes in any of the other tissues of the joint, and may pursue its course without pain. It is often consecutive upon inflammation of the synovial membrane, acute or chronic, or upon the pulpy

* Pathological and Surgical Observations on Diseases of the Joints.

† Annales de la Chirurgie franç. et étrang., 1844, tom. ix.

degeneration of that membrane. But when the articular extremities of the bones become inflamed, the cartilage separates in layers or flakes, leaving a vascular and superficially ulcerated surface, sometimes slightly covered by granulations. Of this affection we will speak more particularly in connexion with disease of the bones, with which it is in reality associated. Even in this case there is no continuation of the bloodvessels into the cartilage, nor is such an occurrence possible so long as the cartilage retains its normal structure. It may be discoloured by blood, or stained by secretion, but hitherto no true bloodvessels have been traced into its substance.

(6.) Ossification of the articular cartilages is of uncommon occurrence. Meyer* describes two forms. In the first, ossification commences at the line of junction of the cartilage with the bone, extends outwards in several directions, and ultimately spreads from isolated spots over the whole articular surface. If any cartilage remain it is either fibrous or split into its villous or velvety structure. There is never any cancellous texture in this ossified cartilage, which presents an even and compact lamella, said in pathological works to be "eburnated." The second form is characterized by the same kind of growth of bone as in fœtal life, not, however, over the whole articular surface, but in certain parts, producing a number of rough eminences, and giving a mulberry-like appearance. This change of structure, which interferes greatly with movement, is followed by an interlocking of the bony surfaces.

In gout, little masses of urate of soda are deposited in the substance of the cartilage, into which they seem to have been rubbed. The cartilage in many cases becomes absorbed. In some cases Sir B. Brodie has observed the cavity of the synovial membrane filled with a thick white fluid, being an admixture of lithate of soda and of pus; but suppuration is very uncommon in these cases. The lithate of soda has been observed by the same author underneath the synovial membrane, on the bone near the margin of the cartilage, in the cancelli of the bone, and in the cellular tissue external to the joint; and he believes that these deposits may have been much increased, if not absolutely produced, by patients taking large quantities of soda for the purpose of correcting the acidity of the stomach.

Diseases of the Articular Extremities of the Bones.—(1.) Inflammation of the articular extremities of the bones may commence either in the periosteum or in the cancellous texture; in the former situation it has been known to produce synovitis. The periosteum becomes injected with blood, thickened and softened; between it and the bone a plastic effusion is formed,—this becomes ossified, the ossifying process commencing from the external surface of the normal bone, which appears covered with a multitude of rough projections and spiculæ. In the museum of St. Bartholomew's Hospital there is the skeleton of a negro, whose long bones are affected in this way. During life he was much exposed to vicissitudes of temperature, and had suffered both from rheumatism and syphilis. Around the margin of the acetabulum of an old female subject in the dissecting rooms of the same hospital, there was found a multitude of bony projections, some firmly attached, others loosened, apparently from the movements of the limb. Pus may be

* Henle and Pfeiffer's Zeitschrift, 1851.

effused between the periosteum and the bone; in which case, the external surface of the bone may ulcerate or even become necrosed and exfoliated. So long as these changes do not affect the articular cavity they require no further treatment than would be necessary for similar changes in other parts of the limb: an incision through the swelled periosteum, and the discharge of the effused fluid at an early period, relieves pain, arrests inflammatory disturbance, and if it does not prevent the occurrence of necrosis, gives a ready passage to the dead bone.

The case, however, assumes a different aspect when the inflammation attacks the cancellous texture of the head of the bones. The vessels become full of blood, fluid is effused into the cancelli; the lamellæ are separated, and the outer wall, thin and distended, may become so soft as to yield to pressure under the finger or to the edge of the knife. Fragments of bone are detached and mix with the effused fluid, which undergoes decomposition, and stains the cancellous texture of a dark hue. Inflamed bone generally dies; it is usual in these cases to find necrosis of the interior of the bone, although it does not follow that the dead portion should be detached. The surgeon may find, upon trephining the outer wall, a mass of soft, dark, and crumbling bone, occupying the articular extremity, and requiring to be scooped out by a strong knife.

But a piece of bone may die, and become separated within the articular extremity. It may be minute, or as large, or even larger, than an almond. This sequestrum, also dark coloured, lies soaked in pus, contained in a well organised vascular cyst. The disease produces hardness of the surrounding bone, which is of whiter colour than natural, and in some instances enlargement by expansion of its tissue; the periosteum is generally thickened. In course of time the matter makes its way externally by a fistulous passage, opening usually upon the anterior part of the limb, and it is to the emptied cavity of an abscess of bone, the progress of which is characterised by most severe pain, that the term "spina ventosa" was given by the Arabian pathologists. Of this disease the best account is by Sir B. Brodie, who has recommended the practice of evacuating the abscess in the early stages, when indicated by continued and severe pain, by the employment of the trephine. The bursting of such an abscess into the cavity of the joint is followed by rapid suppuration, and by destruction of the articular cartilages; but it is not always by a fistulous passage that the pus finds its way into the articular cavity. The condensed bone, contiguous to the joint, between it and the abscess, perishes and separates, first causing a shedding of the articular cartilage, which is found free, and in flakes of different sizes. The denuded extremity of the inflamed bone is red, and covered by soft granulations; the detached bone is under these circumstances of yellowish white hue, and hard. To the space between two bones thus changed in structure, the term joint is scarcely applicable, as meaning an assemblage of soft and organised structures, uniting movable parts of a limb. There is scarce a trace of synovial membrane, the articular cartilages may be entirely removed, the ligaments are softened and often elongated, and in a thick, discoloured, and sometimes offensive pus, the exfoliated portions of dead bone, of various shapes and sizes, are found.

In this state of disorganisation, Mr. Gay, who condemns the speedy amputation of the limb, recommends the practice of laying the cavity of the joint open, removing the dead bone, giving issue to the pus, and allowing the extremities to unite by anchylosis, when the constitutional powers are equal to the task. He believes that in joints, where portions of cartilage have been removed without any apparent disorder of either synovial membrane or osseous surfaces, the portions so removed had first spontaneously degenerated, and then become absorbed.

When the disease originates in the bone, as is the case in by far the greater number of instances, the separation of the cartilage is effected not by ulceration, but by a process which he calls shedding, and the cartilage is then reduced to the condition of a foreign body within the joint. Shreds may be observed after months, and even after years of disease, and as its separation from the bone may be effected in an incredibly short space of time, it is fair to infer that the time thus passed must have been occupied by the process of its extrusion from the joint by disintegration and solution in the discharges of the latter. The bone, being diseased, adds its exfoliated or disintegrated particles to the cartilaginous débris, and the result is, to set up inflammation in the sound textures surrounding the joint, and to produce general systemic irritation. Sinuses form around the joint; the ligaments become ulcerated; the spongy tissues of the bone are infiltrated with pus and broken down; osteophytes form around the heads of the bones, and, in short, a climax is reached, at which the local mischief reacts dangerously upon the constitution. Mr. Gay considers that the exfoliated contents of a diseased joint have to be minutely broken up, and dissolved in the discharges of a joint, for their removal—processes of a very protracted order; and that even after extensive disease has been set up in other textures, the separated or shed portions of cartilage have only to be completely removed, that processes of repair may immediately commence. Resection finds little favour with Mr. Gay, the disease having actually extended itself in long bones no inconsiderable distance along the shaft. He recommends free and deep incisions along each side of the joint, so as to lay open its cavity freely, and to allow of no discharges being by any possibility retained within. They should be made of such a length, and so treated, that they do not heal into the form of sinuses; and they should include all abscesses and fistulous openings, when possible. If the bones be carious, the incisions should be carried deep into them, so as to allow the dead particles to escape; ligaments should be divided, if necessary; the wounds should be filled with pledgets of lint, and free suppuration encouraged.

(2.) Mr. Stanley believes that the bones are affected primarily in only a proportion of the scrofulous diseases of joints. He does not doubt the occurrence of two other forms of scrofulous disease in joints preceding, or independent of, scrofulous disease of the bones—one originating in inflammation of the synovial membrane, the other in inflammation of the cellular tissue around the joint.

"I have watched," he remarks, "the progress and termination of diseased hip and knee-joints, which had exhibited the well-marked local and constitutional characters of scrofula; yet, upon examination, inflammation was found in the synovial membrane, with tuberculous deposit in the joint and around it, or there were scrofulous abscesses around the joint; whilst in the bones no unsoundness existed."*

Care must be taken not to confound thickened pus with tuberculous deposit. Lebert† confesses the difficulty, perhaps the impossibility, of determining this point by the naked eye; in proof of which he relates cases where inspissated matter was taken from the vertebral region, as well as from the limbs. Attention was first directed to the similarity between tuberculous and inflammatory deposits by Reinhardt,‡ who proved that many of the cases described by Nélaton as tuberculous, were in truth instances of abscess. The possibility of making such a mistake is the greater, from the fact of tubercle first softening in its centre; while pus, deposited at first fluid, acquires a greater amount of consistency upon its surface, from the superficial absorption of the watery particles by the cyst. It is said, that by filtering serum from pus, the resemblance between the two deposits may be artificially produced. Again, there can be no doubt, as Gurlt observes, that encysted abscess of bone may undergo earthy or chalky degeneration, by which its original character will be further obscured.

The microscope often fails in enabling the observer to pronounce with certainty whether the object be a dried and altered pus-corpuscle, or one of the bodies constituting yellow tubercle; and we shall have again to allude to the great resemblance between all cells of recent formation. Malignant tumours involving joints generally proceed from one of the bones, of which the interior is the more common seat of primary deposit than the external surface or the periosteum.

(3.) In using the term "malignant," we do not limit it to the commonly-recognised medullary and hæmatoid growths, but include osteoid and pulsating tumours, and all which differ in structure from the normal tissues of the body. Upon this point it is quite possible that the microscope may lead the observer to erroneous conclusions; that he may be taught to speak of cancer elements and of cancer cells, as if there were something in them peculiar to the disease of which they are mentioned as types; whereas they are but the normal constituents of the body, endowed with some erratic developing force, probably inherent in them from the beginning, and transmitted from parent to offspring until the cells become exhausted and unfit for further propagation. Cancerous tumours, therefore, are but the product of cells which, separated from the blood instead of passing into some known normal tissue, multiply themselves by endogenous growth until they constitute a tumour of large size, the surface of which is composed of worn out and disintegrated particles forming a slough. The peculiarity, then, of a truly cancerous growth lies, not in the shape of the cells, but in their number in situations where cells should not properly exist; in their mode of growth; in their reproductive power; and in their size, so far as the latter circumstance indicates activity of development. Therefore the largest tumours generally are composed of mother-cells (mutter-zellen) supported in a sparing fibrous framework, and giving birth to young cells which, in their turn, become the parents of others. This process of degeneration spares no tissue with which it comes in contact; muscle and bone equally become involved, although fibrous

* On Diseases of Bones, p. 245. † Malad. Scroful. et Tuberc.
‡ Annalen des Charité-Krankenh.

tissue and cartilage resist the destructive process longer. If this view be correct, it seems difficult to believe that external injury can give rise to cancer in a subject not previously so disposed; or, in other words, in one whose tissues are, in every respect, healthy and sound.

There is no lack of cases which might be cited in opposition to this view. A man had his leg amputated for malignant disease of the tibia, said to be consequent upon a wound received thirty-three years before at the siege of Flushing. Since that time he had never been free from pain; subsequently there appeared a fungous growth with a soft flocculent surface, which at times bled profusely. Upon examination after the removal of the limb, it was found that the growth rested upon a firm grey-coloured base which sprang from the interior of the bone extending to the opposite wall. The cells composing the growth were large, long, and caudate. Other instances might be found in the origin of cancerous affections of the mouth, lip, and tongue; but still we do not explain how it happens that, out of a multitude of people equally exposed to the same exciting cause, this serious class of disease should manifest itself only in a very few; and the more the subject has been considered by us, the less are we disposed to view these affections as having a purely local origin.

The cells may be infiltrated through the substance of an organ or part, as in scirrhus of the breast or soft cancer of the bones; they may form a superficial growth, as fungus hæmatodes of the mammary gland or of the periosteum; and in many cases there is a tendency to hypertrophy of the normal tissue, the base being invested in a fibrous capsule, or containing osteophytes or fibrous lamellæ. In other and rarer cases, cells of equally rapid formation become elongated into fibres which enclose spaces occupied by a thick and gelatinous fluid; such tumours, generally deep and circumscribed, often between the muscles and the bone, are termed fibro-plastic—but should, perhaps, be more properly termed cellulo-fibrous; differing from cancer-growths in the fact of each cell assuming a definite fibrous form in which its reproductive power is lost, it resembles them in their rapid growth, indefinite increase, and in the liability to recurrence after removal. We are inclined to believe that all structures, whether healthy or morbid, are primitively developed from similar cells formed, at the moment of separation, from the blood; that extravasated blood, whether with or without blood-discs, once coagulated is but a foreign body, to be removed by absorption or discharged by inflammation; that the development of tumours is like the development of normal tissues under government of a fixed law, capable of being excited, but not brought into existence, by any external irritation.

Malignant diseases of bone do not so readily spread to internal organs as do similar affections in the softer structures—hence there is a greater chance that after amputation, when such an operation is possible, the remaining structures are sound. There is not even the same tendency to recurrence of disease; hence the removal of the limb may be recommended as the means both of getting rid of a source of suffering, and of giving to the patient an indefinite prolongation of life. Certain forms of malignant tumours which pulsate occasionally admit of treatment by the ligature of the main artery of the limb. To these, however, we shall refer on some future occasion.

Holmes Coote.

REVIEW III.

1. *First Annual Report of the Commissioners for Administering the Law for the Relief of the Poor, in Ireland, under the Medical Charities Act, 14 & 15 Vict., cap.* 68.*
2. *Report of the Commissioners of Health, Ireland, on the Epidemics of* 1846 *to* 1850.
3. *A Lecture on the Working of the Irish Medical Charities Act.* By ANDREW ELLIS, Fellow and late President of the Royal College of Surgeons in Ireland, &c. &c.—*Dublin,* 1853. 8vo, pp. 34.

OUR object in bringing these several publications under the notice of our readers is to inform them, as nearly as possible in the words of the Reports, of the origin, existence, and nature of the entire reorganization lately effected in medical relief for the sick poor throughout Ireland; and at the same time, to show the defects therein observed and experienced by those upon whom the development of the new system has devolved.

Events not very distant have, in Ireland, reminded us of what history has repeatedly recorded—viz., that pestilence certainly waits upon famine. Fevers, dysentery, scurvy, &c., have in all ages swollen the mortality of war, and as surely have they decimated the inhabitants of towns and countries when the vital energies of a people have been previously depressed by scarcity of food. In Ireland, more especially, owing in some measure to peculiarities in the moral and social condition of a great portion of its population, it has ever been noticed that insufficiency of diet has been followed by epidemic fever. We add one more instance of this coincidence in the prevalence of epidemic typhus in 1847–8–9, succeeding closely upon the famine of 1845–6, produced by the failure of the potato crops. The order of these events may be seen in the following table, constructed from data furnished by the Report of the Irish Board of Health:

	Average Prices of Provisions.								
	Potatoes, per cwt.		Wheat, per barrel, of 20 stone.		Oats, per barrel, of 14 stone.		Oatmeal, per cwt.		Admissions into temporary fever hospitals.
	s.	d.	s.	d.	s.	d.	s.	d.	
1845	2	2	27	0	11	0	12	0	
1846	4	10	31	0	14	0	16	0	
1847	8	4	40	0	20	0	20	0	95,800
1848	7	0	30	0	11	0	12	0	110,381
1849	6	11	21	0	10	0	11	0	87,135
1850	4	4	21	0	10	0	10	0	39,056

The peasantry being dependent for their wages, not on money, but on potatoes, would necessarily feel most severely the effect of the high prices

* The Commissioners appointed under the Medical Charities Act are—Messrs. A. Power, John Young, Thomas A. Larcom, J. M'Donnell, M.D., and E. Senior; assisted by five medical inspectors—viz., John F. Purcell, M.D.; William J. Geary, M.D.; John Hill, M.B.; Alexander Knox, M.D.; and Thomas Dillon, M.D. The names of Crampton, Marsh, and Corrigan will be received as a guarantee of the correctness of the statement put forth by the Board of Health which they themselves constituted.

of provisions; hence typhus, small-pox, and cholera, speedily prevailed among the poor in Ireland, to an extent that had not been known for many years.

In this emergency, a more efficient system of medical relief was required than was available for the sick poor at the outset of the pestilence. The then existing state of the medical institutions of Ireland, the Commissioners remark, was unfortunately such as peculiarly unfitted them to afford the required medical aid on the breaking out of an epidemic. The County Infirmaries had not provision for the accommodation of fever patients; the County Fever Hospitals were destitute of sufficient funds; and Dispensaries, established for the purpose of affording only out-door relief, could, of course, afford no efficient attendance on the numbers of destitute persons suffering from acute contagious diseases in their own miserable abodes, often scattered over districts several miles in extent.

A more inadequate provision for medical relief we could not expect to find in the United Kingdom, in the nineteenth century. Under such circumstances, it excites no surprise that applications for the intervention of the Board of Health, constituted by the Temporary Fevers Act, should flow in abundantly. These were freely responded to; in the three years, from February, 1847, to September, 1849, they amounted to the number of five hundred and sixty-seven. A close investigation by the medical inspectors was made into the cases of all these applications. In some few instances they were refused, because it did not appear to the inspectors that the relief was called for by the extent of the disease and the existing amount of provision.

We learn from the Report of the Board of Health, that the total number of cases of fever treated at hospitals, provided under requisitions to the Board of Health, during the above period, was 332,462. Of these there died 34,622, showing a mortality of $10\frac{2}{5}$ per cent. on the number received into the hospitals.

It has often occurred that the recipients of charity have perversely misrepresented the motives and the means put in action for their benefit. Many instances might be adduced; we have, however, one at hand in the opinion then generally entertained in Ireland, that the mortality was higher in the hospitals—that the lives of the poor sufferers were more endangered by entering those benevolent institutions, than by their being permitted to lie in temporary sheds, in dry ditches, or in their own wretched cabins. The activity, however, and perseverance of the inspectors dispelled the fallacy by the discovery of deaths among the victims of pestilence in their own abodes, by the road-side, or in ditches, frequently unknown or unheeded until discovered by the vigilance of the inspectors.

It became painfully obvious, that the infectious maladies thus spreading epidemically, manifested also distinctly contagious characters, by their extension to members of the upper classes who were brought into daily contact with the sick. The mortality consequent thereon included clergymen, members of relief committees, inspectors, and medical officers. Our readers will not need to be reminded that among the last-mentioned, the profession had to deplore the loss of many who, in this trying hour, sacrificed their lives to a sense of duty. Of 473 medical officers appointed under the authority of the Board, 36 died. How many more medical men, not thus officially attached, fell victims to the fever, has not been placed on record.

With the close of 1850, the epidemic having subsided, the occasion for the Temporary Fever Act ceased, and the arrangements under the Board of Health were superseded by the establishment of permanent Dispensary Districts under the Medical Charities Act, to which we desire now more especially to direct attention.

As early as possible after the dissolution of the Board of Health, under the Temporary Fevers Act, the Medical Charities Act for Ireland having been passed, the Commission was constituted for carrying into operation its intentions. On the 12th November, the Commission having been formed, operations were at once commenced, by the issue of circulars to all boards of guardians in Ireland, requiring them to proceed, with all possible dispatch, to divide their respective unions into districts, for the establishment of dispensaries therein, according as the extent and population of such districts might to them appear to be necessary. The instructions of the board having been promptly and energetically acted upon by the guardians of a considerable number of unions, several dispensary districts were officially declared before the end of December of the same year, and the last dispensary district was declared by the end of May, 1852. It will thus be evident, from the dates here stated, that the report before us does not embrace one entire year of the existence of these dispensary districts, but it contains, nevertheless, sufficient information to enable us in some degree to appreciate their utility, and to estimate the degree of completeness, as well as the defects of their arrangements.

Previous to the enactment under which the new system has been introduced, there existed in Ireland a system of district dispensary relief, which, so far as we understand the report in question, appears to have been distributed according to the electoral divisions. These were partly dependent upon voluntary subscriptions, and partly upon grants to a like amount from the grand juries. It was found that many of these dispensaries were without funds, their subscribers having withdrawn; and as a consequence, the grand jury grants having fallen to the ground on the passing of the Medical Charities Act. The Commissioners did not in all cases supersede these existing institutions, but where they deemed it expedient, incorporated them in the new districts. The medical officers in possession of previous appointments, were offered the option of retaining them under the new system, although they had not in every instance the qualifications required by the Commissioners, from candidates for new appointments to the dispensary districts:—these are, the diploma of one of the recognised medical or surgical colleges of England or Ireland, and a certificate, from some known body, of the possession of a competent knowledge of midwifery. When the Commissioners exercised their discretion in dispensing with these qualifications, they did so only after a rigid scrutiny into professional reputation and length of service of the officers in whose favour the regulations were suspended. Strictness in applying their rules was indispensable, the paramount intention contemplated in the Medical Charities Act being, the providing the sick poor throughout Ireland with medical officers undoubtedly competent to practise medicine, surgery, and midwifery, and resident at such distances as should render their services available within the period that they may be useful, and before the lapse of the opportunity of affording benefit. In the actual operation of the system, it appears that a false economy has, in many instances, defeated the object in view.

The whole number of districts is 723; of dispensaries therein, 960. The Commissioners assert that they were at great pains to adjust the number of districts to the size of the unions. When they considered a district, as proposed by a board of guardians, to have been of too great extent, they have subdivided it; and, on the other hand, they have sometimes thought it advisable to amalgamate smaller districts. In some few instances, they have endeavoured to secure a proportionate amount of medical aid, by providing for the appointment of additional officers.

The management and more immediate superintendence of these dispensaries has been entrusted by the Commissioners, under their own supervision, to local committees, consisting of guardians resident in the districts, together with those who possess or occupy property therein, as *ex-officio* members. In the event of the number of these falling short, so many persons as may be necessary to make up the number, are elected from amongst the rate-payers resident in the district, and liable to pay poor-rates in respect of property therein, of the net annual value of 30*l.* at the least. The objects aimed at in this plan are stated to be, the selection of individuals who have a direct interest in promoting the economical and efficient working of the institutions; and through whose local knowledge, and authority to refuse tickets, the charity is saved from abuse. The extent to which frauds are constantly committed upon medical charities, by persons not properly objects of charity, and by individuals far removed from need of their aid, are familiar to the profession. The medical officer of the Irish dispensary is authorized and encouraged, according to the report before us, to report to the committee of management upon all cases not deemed entitled to come within the limits of their legitimate operation. It is thus made to appear, that the medical officer has the power to protect himself from the abuse of a public charity, in the working of which he would otherwise victimize himself.

The correspondence published in Mr. Ellis's pamphlet sets before us a different picture. We find therein statements which prove, that the "direct interest of the members of committee of management, rather lead them to practise economy as far as their own private purses are concerned, than to watch the economical expenditure of the public funds, by strictly defending their dispensaries from the admission of patients well able to remunerate their medical attendant out of their own earnings."

The Commissioners in Dublin, exercising a general control and supervision over the system, and issuing general rules and regulations for the guidance of local committees and medical officers, profess, in fulfilling their responsibilities, to manifest a strong desire to promote and protect the interests of their medical officers.

If we were to judge only from the tone of the Commissioners' Report (and we give them credit for sincerity), we might be led to suppose that the medical profession in Ireland would hail the new dispensary system as a great boon sent straight down from heaven, to relieve them at once from degradation and unprofitable patients; conferring upon the medical officer a degree of independence and protection not to be thought of in England; and leaving him free to go about doing good, conscious that he is worthy of his hire, and confident of its receipt. But the reports of those who have been obliged to undertake the performance of those duties do not so fully confirm this impression. It would seem that many regard their appointments as conveying little more than the privilege of working hard for insufficient remuneration. It is true that the dispensary officer escapes the ordeal of a degrading personal canvass of a miscellaneous list of tradesmen and other subscribers to a public charity, few of whom are in any degree competent to judge of the qualifications of a medical officer. He has, nevertheless, to submit his application and credentials to the approbation of a board of parochial guardians, including, probably, a majority of persons equally incompetent to form a judgment in such matters. At the same time he has the advantage of their limitation, by the definite instructions of the Commissioners, as to what shall constitute the required qualifications.

How far the protection from the admission of improper objects to these dispensaries is to be relied upon, under the existing arrangements, may be gathered from the following extracts from the letters of Mr. Ellis's correspondents:

"There should be some control over the members of the 'committee of management,' to prevent them from recommending unfit objects for dispensary medical relief. I have got some struck off who had been recommended, but before I could get this done, I was obliged to attend to them, and give the necessary medicines. . . . . One landlord told me that 'all his tenants were dispensary objects.' . . . .

"The new Medical Charities Act is shamelessly abused in this district, for I find that tickets of recommendation are issued for advice and medicine, and '*immediate*' visits at the homes of undeserving objects, not poor, but snug farmers, having from five to twenty acres of land, well stocked; some of them getting 18*l.* or 20*l.* the same week for a horse; national schoolmasters and schoolmistresses (schools founded by the late Erasmus Smith), having not only good salaries, but houses, gardens, and lands attached; gentlemen's stewards, servants, gardeners, &c., to whom tickets are issued by my Lord This, and Sir That, and by Rev. Mr. So and So, and by Messrs. innumerable Members of the committee of management, who consider the foregoing recipients fit and proper objects to be placed under the fostering care of 'the doctor,' who may be found (in some places) reluctant, for reasons best known to himself, to expose such flagrant abuses!!!"

These two extracts suffice to prove that a measure which, if properly administered by a fair proportion of professional to lay commissioners, is every way calculated to benefit both the medical officer and the objects of the charity, has actually resulted in great injustice to the former, upon whom entirely the successful operation of the system is dependent.

Thanks are due to those of our professional brethren who, now making a decisive stand against such perversion of the intentions of the legislature, have not hesitated openly to expose the abuses. We are convinced that the purpose of the framers of the act was, that the profession should be protected; but the provisions for such protection, at present in force, are utterly futile. An order once issued, the patient must be received, until struck off the list by the committee! The committee meet once a month, sometimes once in three months. It is perfectly clear that such protection is no protection at all.

The duties of the medical officers of these district dispensaries are, as they must necessarily be in an institution of great public benefit, onerous. They are as follow:

"1. He shall attend at the dispensary of the district or division of a district under his charge, on such days, and at such hours, as the committee of management may direct, for the purpose of affording medical relief for any poor person applying for the same, and presenting a ticket as hereinafter provided.

"2. He shall duly and punctually attend upon, either at the dispensary or at the home of the party on whose behalf the application is made, as the case may require, and supply all requisite medical and surgical advice and assistance to every poor person in the district or division of a district under his charge, whom he may be required to attend as medical officer, by a ticket in the Form E 1, or E 2, annexed to this order, signed by a member of the committee of management of the dispensary district, or by a relieving officer or warden acting for any electoral division or part of any electoral division included in such district or division thereof.

"3. He shall give a certificate under his hand to the committee of management, or the board of guardians, or the relieving officer, when called upon to do so, of the state of health of any poor person whom he shall have been required to attend.

"4. He shall keep and duly enter up the dispensary register, according to the Form F, hereunto annexed, and lay the same before the committee of management at each ordinary meeting.

"5. He shall vaccinate all persons who may come to him for that purpose, at the times and places at which he shall be required to attend for the purpose of affording medical relief, such persons not having before been successfully vaccinated; and shall do and perform all such other acts and things as may be necessary for the purpose of causing such vaccination to be successfully terminated; and keep a register, in the annexed Form G, of all cases of vaccination performed by him; and submit the same to the committee of management at each ordinary meeting.

"6. He shall keep and duly enter in the Form I, hereunto annexed, an alphabetical index to the medical relief and vaccination registers, and to a case book, with which he shall be provided (Form H), and in which he shall enter such particulars as he may deem necessary as to any of the cases attended by him.

"7. He shall lay before the committee, at each ordinary meeting, a return, in the annexed Form K, of the number of patients attended by him during the previous fortnight, and shall forward a monthly return of the same to the medical inspector of the district.

"8. He shall lay before the committee, at each ordinary meeting, his report book (Form L), in which he shall enter any matter which he may deem it necessary to bring under the notice of the committee; and he shall submit to their consideration any entry which shall have been made therein since the last meeting.

"9. He shall, at the first ordinary meeting of the committee in each month, deliver or forward to the chairman or vice-chairman of the committee a statement (in the annexed Form M 1) of the medicines and medical appliances used during the past month, and of those remaining on hand, with an estimate of the quantities required for the ensuing month.

"10. He shall furnish to the committee, at each ordinary meeting, a separate account, in the Form N hereunto annexed, of medicines supplied to prisoners or inmates in any bridewell or house of correction within his district.

"11. He shall make such returns, appertaining to the duties of his office, as shall be from time to time required of him by the committee of management, the board of guardians, or by us, the commissioners for administering the laws for the relief of the poor in Ireland."

The medical officers are also called upon to furnish such other occasional returns as may be required by the board of commissioners or committees of management.

We regard the last as an extremely important portion of the duties of the medical officers. They might supply an accurate knowledge of the extent of the operations of each district; and they might constitute a storehouse of pathological data of the greatest value in estimating the *morbility* of the country, or parts thereof. It is not possible to overrate the importance of the information that may be thence furnished on sanitary matters or on the history and progress of epidemic diseases, and with regard to the measures to be adopted for their arrest or extinction. The amount of statistical information of this kind that is daily wasted at our own dispensaries and other medical institutions is beyond all means of estimation.

The salaries which have been in many instances awarded for the onerous and multifarious duties above described have given rise to very general dissatisfaction among the medical officers of these district dispensaries. With a view to the possession of facts bearing upon this point, Mr. Ellis addressed to many of the dispensary medical officers throughout Ireland, a circular letter containing the following queries:

"1. *Q.* When was your 'dispensary district' defined under the new Act?

"2. *Q.* What is the probable number of square miles it comprises?

"3. *Q.* What number of persons reside in it who may be entitled to claim medical relief?

"4. *Q.* Are you satisfied with the 'rules and regulations' framed by the commissioners for your observance, according to the 12th and 13th clauses of the Act? and if not, state your objections to them.

"5. *Q.* How many members does the 'Committee of Management' consist of?

"6. *Q.* Do you think that the members of the committee of management and the relieving officers act fairly and impartially in issuing tickets for 'advice and medicine;' and that none but deserving objects obtain them?

"7. *Q.* How often has the medical inspector visited you since his appointment?

"8. *Q.* If you have no objection, be so good as to state the amount of your salary; and if you consider it a fair remuneration for the duties you perform.

"9. *Q.* Did you sustain any pecuniary loss after the old system became 'illegal,' and before the new Act was put in force?

"10. *Q.* How were the sick poor attended to in your neighbourhood during *that* period?"

To these, four hundred replies were received. All tend to exhibit the inadequacy of the remuneration to the amount of duty involved; many districts being too large for efficient attendance upon the sick poor. These answers also contain reiterated complaints of the heavy additional work imposed by the various books of returns, reports, registers, &c., required to be kept, over and above visiting the sick and dispensing medicines.

To the same effect has been the opinion expressed at a meeting of more than three hundred medical officers of the dispensaries on the 7th June, at the Royal College of Surgeons, in Dublin.* Almost the whole burden of the speakers was the inadequacy of their salaries as compared with their duties. Not without reason did those complain who are compelled to give attendance at one or more dispensary stations, and to visit the sick poor among a population spread over districts extending fifteen or twenty

* See Report in Medical Times and Gazette, June 11.

miles in one direction by ten or fifteen in another—for 50*l.* per annum! but it must not be concealed that there are many who receive double this amount.

The average salary of the medical officer of the Irish dispensary is 71*l.* per annum (not 50*l.* as stated by a weekly contemporary).* The average salary of the English Poor-Law Union medical officer is 50*l.* The average extent and population of the districts in the two countries varies, however, considerably, as may be seen in the subjoined table, prepared from the reports before us, and from the reports of the English Poor-Law Board:

Comparative Estimate of Poor-Law Medical Relief in Ireland, and England and Wales.

	Ireland.		England and Wales.
Total population of unions and dispensary districts	6,552,055	...	16,137,136
Average population to each medical officer (in round numbers)	8,000	...	5,000
Average area of districts and unions in acres (round numbers)	26,000	...	16,000†
Number of unions and dispensary districts	960	...	607
Number of medical officers	805	...	3,156
Average salaries of ditto	71*l.*	...	50*l.*
Total expenditure per annum for medical relief	100,000*l.*	...	212,050*l.*
Average cost per head, whole population, for medical relief	3*s.* 7*d.*	...	3*s.* 1*d.*

From this table it will be seen that the inhabitants are more widely scattered over Irish than over English districts; that the proportion of the population to the area in acres being smaller, the districts are in Ireland necessarily much more extensive. In England we have a greater number of unions, consisting of populations aggregated into towns, than are to be found in Ireland. Therefore, small as is the remuneration of the Irish dispensary medical officer, the medical officer of the English union (bearing in mind the much higher rate of living in England) is worse paid on an average salary of 50*l.* per annum, for which he has, in most instances, to find his own medicines, whereas these are provided at the Irish dispensary stations. It is to be observed, also, that buildings, specially appropriated to the purposes of dispensaries, have been erected under the directions of the commissioners in all the Irish dispensary districts; at these stations, all requisite medical and surgical means and appliances are provided. The one great deficiency, however, is urged in many quarters, of the want of a dispenser to prepare the medicines which time scarcely serves for the medical officer himself to compound, after several hours devoted to travelling over a wide district in his attendance on the sick. The extent of these duties may be gathered from the following and preceding tables, prepared from the report of the commissioners of the Irish Medical Charities Act.

* Medical Times and Gazette, May 14.

† This estimate is calculated from the extent of each union in England and Wales as stated in the British Medical Directory, 1853.

Return of Duties of Medical Officers of Irish Dispensaries, for the half-year ending September, 1852.

Number of cases admitted	311,666
Average number at each dispensary	431·07
Ditto ditto for each medical officer	401·63
Ditto ditto of tickets cancelled by each medical officer	3·97
Ditto ditto of attendances of each medical officer at dispensaries	65·26
Ditto ditto ditto at patients' homes	187·79

It is worthy of note, that this table bears out the statement of many medical officers, that the present arrangements offer but slight protection against the abuse of the charities; since, out of upwards of three hundred thousand patients, the tickets of only three individual patients have been cancelled. The circumstances which should constitute a proper object of the charity require to be distinctly defined; at present great abuse is perpetrated.

The estimated annual expense of working the district dispensary system is 102,700*l.*; having regard, however, to the diminution of pauperism and the improving circumstances of the country, it is not expected that the annual charge will exceed 100,000*l.* per annum, amounting to an average poundage of 2*d.* and a small fraction, on the rateable property in Ireland. Of this sum, the salaries of medical officers consume only one-half, while the remainder is expended in medicines and medical appliances, rent, salaries to non-medical officers, stationery, and incidental expenses. The cost per head for medical relief in Ireland is rather more than a halfpenny over what is expended for the same purpose in England—see table at page 367. The expenditure for 628 dispensaries was 76,000*l.* per annum under the former system; there are now 960, therefore any excess of cost may be considered as more than compensated by the augmentation of medical aid placed within reach of the sick poor—the consequent diminution of distress among the labouring classes, and the saving of human life, will eventually effect a clearance of poor rates. These should be indirect results of the new system, but it will also operate to the production of an immediate gain by the introduction of medical relief in districts where, from the absence of resident gentry, there was formerly a total want of medical aid otherwise than by resort to the workhouse. At the same time, it is observed by the commissioners that the tax now falls equally on the property of absentees and of resident proprietors, and is no longer a double charge on the benevolent; first in the shape of voluntary subscription, and next in the shape of county cess. The source whence the salaries of the medical officers are drawn has been strongly objected to by those gentlemen as "tending to bring them into collision with boards of guardians, and thus interrupt the good feeling and unanimity which are so essential to the effective working of the Act." It is desired that the payment should be transferred from the poor's rate to the consolidated fund, as in England.

It will afford great satisfaction to the profession, both in Ireland and in England, if the expectations, expressed at the meeting in Dublin, shall be realized, in the co-operation of the commissioners with the medical officers in their efforts to procure increase of salary, and remedy of several other defects which have been discovered in the course of the short time during which the Act has been in operation. The generous intentions of the legislature and of the framers of this Act we may now confidently hope will not be frustrated by an ill-judged economy. A fair and liberal remuneration for the arduous labours on which the success of the enactment is solely dependent, may surely be obtained by firmness and unanimity on the part of the profession. The medical officers present at the meeting above mentioned resolved themselves into a medical association, for the purpose of promoting the primary ends of the Irish Medical Charities Act by securing to its officers such payment for their services as shall enable them efficiently to perform their duties; also for the purpose of promoting other objects arising out of the requirements of the sick poor, not one of the least of which is the establishment of district hospitals, as essential to the perfection of the district dispensary system of medical relief.

We cannot conclude without expressing a wish (not likely to be realized, we admit) that a system so well devised, and so admirably adapted, under proper administration, to answer its design, could be introduced on this side the channel, and could be brought to supersede the present unsystematic and heterogeneous mixture of medical relief by Poor-Law officers, sick clubs, dispensaries, &c. &c.; institutions which are too often in debt, and compelled to have recourse to humiliating modes of raising funds, while their medical officers must submit to the indignity of personally canvassing for permission to spend some of the best years of their lives in gratuitous and often thankless toil. The dignity of the profession would be greatly enhanced by a national system of medical relief for the sick poor; and much waste of funds, as well as great imposition on the benevolent, would be averted by a well-organized system on the basis of the Irish Medical Charities Act.

W. B. Kesteven.

Review IV.

1. *A Manual of Pathological Anatomy.* By Carl Rokitansky, M.D. ('Sydenham Society's Translation.')
2. *An Anatomical Description of the Diseases of the Organs of Circulation and Respiration.* By C. E. Hasse, M.D. ('Sydenham Society's Translation.')
3. *Précis d'Anatomie Pathologique.* Par G. Andral, M.D.
4. *Pathological Anatomy.* By Robert Carswell, M.D.
5. *On Cancerous and Cancroid Growths.* By J. H. Bennett, M.D.
6. *On White Patches on the Pericardium.* By James Paget, F.R.S. ('Med. Chir. Trans.,' vol. xxiii.)
7. *On the Nervous Centres, &c.* By Robert B. Todd, M.D., F.R.S. ('Cyclopædia of Anatomy and Physiology.')
8. *On Cirrhosis of the Lungs.* By D. J. Corrigan, M.D. ('Dublin Journal,' vol. xiii.)

We feel that the subject from which we have taken the above title, is one of no little difficulty to review in a satisfactory manner, as having ill-defined limits, and being in many respects obscure. It seems to us, however, quite worth while to break ground upon it, as it has at present scarcely been made the topic of special inquiry; most of the phenomena which we shall have to notice being usually set down rather vaguely as the result of inflammation. That changes of the kind in question may be the result of undoubted inflammation is tolerably certain, but it seems to be a point of very great importance, interesting alike to the pathologist. and the bedside observer, to ascertain how far inflammation is concerned in the production of fibroid and similar degenerations, whether any of them seem to occur independently of any recognisable phenomena of inflammation: and whether, in most cases, an inflammatory action of any kind or degree is to be considered as of the essence of the disease. We confess that we incline, as our observation has led us, to the view that many of the most important fibroid degenerations are not essentially dependent on anything that can be justly called inflammation, and that the real cause is rather to be sought in an abnormal state of the blood. We purpose to proceed as we did in the case of fatty degeneration, taking first the most marked examples of fibroid and similar changes that we can find in the different organs, and then considering what is the general bearing and import of the facts examined. In the outset, however, it is quite necessary to give some definition of the descriptive terms we have used, lest we should appear to our readers to include in our review changes which they may consider essentially different. Under the name of fibroid and allied degenerations, we shall describe all such changes as increase in a notable manner the firmness and toughness of the affected tissue, whether they are attended with apparent hypertrophy, or with atrophy; not, therefore, confining the use of the terms to tranformations of the higher tissues into fibre, nor again refusing it to many instances where the fibre formation is exceedingly imperfect, though the effused blastema evidently tends towards this kind of development in its morphological and physical qualities. From this it is manifest that we do not use the term fibroid degeneration in the same sense as the term fatty, at least, if the latter be restricted to what is distinguished as true fatty degeneration, the conversion of the elementary parts of a tissue into oily matter. We expressly wish not to limit the application of the idea of fibroid change to the marked typical instances, for we think they pass by very gradual shades into those of a less definite character; and it seems more likely to be of practical advantage to recognise affinities than to multiply differences. We shall freely avail ourselves of the recorded experience of others, which in several respects must be more satisfactory than our own. We commence with the serous membranes. Dr. Todd thus describes thickening of the arachnoid.*

"An opaque condition of the arachnoid, varying both in degree and extent, is a very common appearance of this membrane, especially at the middle and advanced periods of life. This occurs sometimes in patches, at other times it is more generally diffused over the whole membrane. It is most conspicuous on the convex surface of the brain, especially towards the great longitudinal fissure, and it is frequently associated with large and numerous Pacchionian bodies. It occurs, however, very commonly at the base, and frequently opposite the confluxes of the

* Art. on Nervous Centres: Cyclopædia of Anatomy and Physiology, p. 716.

subarachnoid fluid. The opacity of the arachnoid is commonly attributed to a former acute inflammation of the membrane, or to a chronic inflammation going on up to the time of death. But this state of the membrane is of such frequent occurrence, and is so often found in persons who evinced no sign of important organic change during life, that it seems scarcely correct to attribute it to such a cause. It is not meant to deny that previous inflammation, or chronic inflammation, is capable of causing these opaque spots, but undoubtedly other causes may produce them as well. The friction of two opposed surfaces may do it, and deposits upon the free surface of the membrane, an altered condition of the epithelium, may have the same effect. Some recent microscopical examinations convince me that morbid deposits, similar to those which are formed on the coats of arteries, may be found here, and occur in those morbid states of the blood, and consequently of the whole system, which are favourable to the deposition of a morbid material throughout the arterial system, or in the substance of viscera. In confirmation of this view, it may be stated that opacities of the arachnoid are most common after the middle period of life, and that they are then almost uniformly associated with a morbid state of the arteries of the brain, and of other portions of the arterial system."

The absence of adhesions in the very great majority of cases over the thickened patches, is worth noticing with regard to the question of their inflammatory origin. Rokitansky thinks that the changes in the membrane "can be attributed only to congestion, or to slight and passing attacks of inflammation." To see any such congestion, he admits is very rare. His description of the change in the membrane is very graphic: he says "it is opaque, dull, like whey or milk, tumid and white, and it has the appearance and density of tendon." After the middle of life, he believes that opacity and thickening of the arachnoid are absent only in exceptional cases: he classes them, together with the Pacchionian bodies, as being products of the same cause. Dr. Todd remarks that,

"Cartilaginous spots are by no means unfrequently found on the (spinal) arachnoid, chiefly in connexion with its visceral layer. They are generally small detached laminæ, thoroughly incorporated with the membrane, rarely exceeding in size the flat surface of a split pea, more frequently much smaller. They generally occur on the posterior surface of the arachnoid. I have seen such deposits in cases where there were no previous symptoms to denote any affection of the central nervous system."

We have observed one case of some interest, in which there was considerable thickening of the arachnoid. An unemployed stableman, aged between 50 and 60, was brought into St. Mary's hospital in a state of insensibility; he had three epileptic attacks, and died in twenty-four hours. The calvarium was much thickened, especially at the points of ossification; the vessels of the dura mater were gorged with blood. On the left side of the superior longitudinal sinus, about its middle, there were two spots of calcareous deposit, about the size of a sixpence; on the right there was a patch of false membrane, which probably was the remains of extravasated blood. The visceral arachnoid, covering the convex surface of the brain, was much thickened and very opaque. The fornix was softened, as well as a spot in the arterior lobe of the left hemisphere, and another on the posterior part of the right optic thalamus. The brain was pale and wet, and there was much subarachnoid effusion. There was some thickening at the base of the mitral and of the aortic valves. The liver was firm and dense, there was a puckered depression on the posterior part of the right lobe, and one or two subserous white patches; the Glissonian sheaths were much thickened. The kidneys were in an early stage of granular disease. Microscopic examination showed that the thickening of the arachnoid depended on the formation of new and tolerably perfect white fibrous tissue; this was rendered more translucent by acetic acid, and there were then seen numerous oval and circular rather small-sized nuclei everywhere in its substance, and in some parts molecular oily deposits, as well as concrete oily masses, and oil-containing vesicles. It is not at all clear to our mind that there had been, or was, any chronic arachnitis in this case; the bones of the cranium were evidently hypertrophying, and might cause some irritation and increased flow of blood to the meninges; at the same time the blood was in an unhealthy state, in consequence of the renal degeneration; and there were thickening processes taking place in the cardiac valves and in the liver. It seems to us most probable that the diseased state of the blood was the essential cause of these thickenings, and of that of the arachnoid. We are inclined to look on them as secondary phenomena of unhealthy nutrition, rather than as distinct inflammations.

The Pacchionian bodies, whose connexion with the arachnoid membrane is so intimate, are very fitly included in our investigation. We have carefully examined them ourselves, but have really nothing to say which is not confirmatory of the excellent description given by Dr. Todd (*loc. cit.*), to which we refer for further details than our space will allow to introduce here.

The Peritoneum is very frequently affected with chronic thickening; this is sometimes extensively diffused, almost general; more often it occurs in distinct localities, and forms patches of various size and thickness. The capsular coverings of the spleen and of the liver are its most frequent seats. The former is perhaps the more liable to this change from the peculiar reservoir function of the organ, which might easily give rise to a greater amount of exudation than takes place elsewhere. On the surface of the liver the thickening process gives rise to milky-looking spots and patches, which are sometimes well-defined, and at others shade off gradually into the healthy tissue; some of the patches are elongated, and show some traces of branching,—this depends on the Glissonian sheaths of the superficial portal canals being evolved, which thus appear in this form on the surface. The thickening of the capsule of the spleen sometimes takes place to a great degree; we have seen it amounting to a third of an inch or more. It often extends over a considerable part of the surface, and though this remains perfectly smooth and polished, it is often irregularly thickened, so that while over a large space the dark-red colour of the parenchyma is completely concealed by the dead white of the altered capsule, it faintly appears in a few less affected spots. This chronic thickening, in its more advanced degrees, produces very much the appearance of cartilage, so that it has been termed *cartilaginification.* It is needless to say that there is no real resemblance between the altered tissue and cartilage.

There does not appear to exist any relation between the condition we are considering and bands of adhesion; they may both be present together, or either may exist in a marked manner separately. When the peritoneum is thickened more generally we think it does not take place to the same degree that it does when it is partial. In cases of ascites the membrane looks dull and opaque, and as it were, sodden, and is found to be somewhat thickened; one specimen of this kind which we examined microscopically exhibited very little except dense and rather granular fibroid tissue, traversed by a few ramifying injected vessels. All the partial thickenings are produced, we believe, by slow and continuous exudation taking place into the subserous tissue, and the substance of the membrane. It does not appear to us that the process advances *saltatim*, but gradually and continuously; we have never observed anything like stages of transformation of the exudation. In one very marked instance of thickening of the capsule of the spleen, it was very well seen how the successive deposits had taken place and been organised on the detached surface: instead of there being any prominence on the free surface this was quite even with the surrounding part, and all the encroachment had taken place at the expense of the parenchyma. The new tissue in this case consisted of reticulated fibres, with a solid basic stratum filling up the interstices. As far as we are aware no symptoms whatever accompany the thickening process; it seems to be completely latent.

We have observed one or two instances of a rare morbid condition which seems to us in some measure analogous to the foregoing, consisting in the formation of a complete capsule of false membrane all over the surface of the liver, as well as of layers lining the peritoneum in other parts. The false membrane, as it shrinks, causes considerable contraction; we have seen the liver and both lungs remarkably diminished from this cause. We subjoin the following illustrative case:

W. Kearns, æt. 45, admitted August 23rd into St. George's Hospital. Was attacked with dropsy on the 12th, which commenced in the ankles, and afterwards extended to the abdomen. He had enjoyed good health, but for some time back had been subject to cough and shortness of breath; never had rheumatic fever; the abdomen was distended by gas in the intestines, and was also fluctuating; the œdema of the legs was but slight; countenance somewhat dusky; lungs congested posteriorly; heart's dulness extended, and its action increased; urine free, of low specific gravity, albuminous. After having improved for a short time the urine became scanty, the dropsy increased, the respiration became daily more laboured; he sank gradually, and died semi-comatose on October 25th. Hæmoptysis took place the day before his death. At the post-mortem examination there was found a large quantity of serum in both pleural cavities; blood-stained in the right, more clear in the left. Both pleuræ had undergone chronic thickening, as evidenced by the membrane being of a whitish opaque aspect, and thicker than natural; the thickening seemed to depend on the formation of a firmly adhering thin layer of false membrane; when this was dissected off the pleura remained, presenting a puckered surface. Both lungs were contracted, and their tissue condensed by pressure; their margins were rounded, and they had evidently been compressed by the shrinking of the false membrane investing them. The lower lobe of the right was in great measure consolidated by extravasation of blood. There was some serum in the pericardium, and chronic thickening of the visceral portion of the membrane, especially of that covering the right auricle. Heart much enlarged and dilated; valves healthy, except a single vegetation on one of the aortic flaps. The capsule of the liver had undergone chronic thickening, just like the pleuræ, only to a greater extent; the thin edges of the organ were rounded, and its size perhaps rather diminished. The parietal peritoneum was thickened in some places, and remarkably affected with black discoloration; it contained an enormous quantity of serous fluid. The spleen was rather enlarged, very dark, its capsule considerably thickened, and adhered to the diaphragm. The kidneys were shrunken, granular, cysted. The structure of the liver was tolerably healthy, but the Glissonian sheaths were somewhat thickened.

It seems pretty certain in this case that the formation of the adventitious coverings to the lungs and liver must have been going on a long while, without giving rise to any symptoms; it is impossible that it could have taken place during the time he was consciously ill. There was chronic thickening of the pericardium, and of the capsule of the spleen, as well as of the parietal peritoneum. All these changes may be ascribed to so many chronic inflammations set up by the diseased state of the kidneys. To us it seems a simpler and truer statement to say that they were the result of slow exudation of unhealthy plasma in different parts, which, containing much fibrine, had given rise to these layers of false membrane. Inflammation may have been concerned in some degree in the production of these changes, but there was no evidence of it; and there is much to lead one to think that mere inflammation could not have been the principal cause. We have seen a few times the surface of the capsule of the liver covered with minute firm particles, much like little grains of sand; they occur sometimes together with chronic thickening, sometimes without it, the membrane around them being quite transparent. They are, beyond doubt, small nodules of fibrine, and can hardly be conceived to be of inflammatory origin.

In the *Pleura* we think that the common bands of adhesion, the result of inflammatory exudation, are of much more frequent occurrence than patches of chronic thickening. Certainly, we have not seen them nearly so often in this situation as in the peritoneum. Rokitansky distinguishes the chronic thickenings from the exudations seated on the inside of the pleura, and describes them as follows:

"In the other case the fibroid and cartilaginous tissues are developed independently of inflammation, and merely in consequence of an hyperæmic condition in the subserous areolar and fibrous tissues, and in the tissue of the serous membrane itself. We first observe a whitish, more or less circumscribed opacity and condensation of the serous tissue; there is here a development of tissue, resulting in the formation of a smooth or nodular elastic plate, or of a group of granulations of fibrous or fibro-cartilaginous tissue, or even of irregularly shaped masses, which vary from the size of a pea to that of a nut, and finally ossify. These are always situated under or on the outer side of the serous membrane, and are invested by it. . . . Subserous adventitious products occur almost exclusively on the costal and diaphragmatic pleura, and their most common seat is in the intercostal spaces. They sometimes become liberated, and are found free in the cavity of the thorax in the form of round nodular masses of (false) cartilage."

We do not quite agree with Rokitansky, that chronic thickening is so very much confined to the parietal pleura; we think it is seen quite as often in some parts of the pulmonary pleura. In a case we recently examined, in which death occurred from pneumonia, the kidneys being in an advanced stage of granular disease, the apices of both lungs presented a large patch of subpleural thickening, but were almost entirely free from adhesions. The pulmonary tissue beneath them was indurated and disorganised, as the result of former tubercular deposits; and it was remarkable that these should not have given rise to the usual adhesions which so often form a cap to the lung, but to the gradual exudation which produced the dense white patch. The thickness of this was so great that it was evident it could not have been formed otherwise than by a slow process. There was also a white pericardial patch, and nodular thickening of the capsule of the spleen. Dr. Walshe notices this thickening as occurring both at the base and apex of the lung, and remarks how the puckering which it often occasions was mistaken by Laennec for evidence of cicatrization of tuberculous cavities.

The white patches which are so common on the *pericardium* are considered by most pathologists as the result of inflammation, partial and circumscribed. Rokitansky says that of this there can scarcely be a question. Mr. Paget, in his paper in the twenty-third volume of the 'Medico-Chirurgical Transactions,' has adduced evidence in favour of the same view, from the observation that in almost all cases in which white spots exist there are also found, on careful examination, traces of adhesions between the two layers of the serous membrane. Hasse distinguishes milk spots of inflammatory from those of non-inflammatory origin, stating that the former are of somewhat rarer occurrence than the latter. He describes them (the former) as

"Of various thickness; of exceedingly irregular form and distribution—now sharply defined, now blending almost insensibly with the healthy serous membrane; their surface is sometimes smooth, sometimes rough or villous. They occupy the free surface of the pericardium, and though cohering by means of bloodvessels, can be easily peeled off without detriment to the serous basement membrane. They are more frequent at the anterior than at the posterior surface of the heart; most of all, along the course of the coronary vessels, which are spotted, striped, or dotted with them. This variety of milk spot is doubtless met with in persons who had never obviously suffered from thoracic inflammation, and occasionally even in infants. Nevertheless, it must be looked upon even then as the consequence of inflammatory irritation. In some instances I have ascertained the inflammatory origin of these milk patches, from the co-existence of partial cellular adhesion, and the formation of distinct filamentous bands between the heart and pericardium."

Rokitansky speaks of them as

"Pale, bluish-white, tendinous-looking spots, or *plaques*, appearing, when closely investigated, to be glued or soldered to the adjacent tissue; on being torn or detached the pericardium is brought into view, and is almost normal in its character,—not perfectly smooth, but having a dense, and sometimes even an opaque tissue. They must be distinguished from many other diffuse opacities of common occurrence on the pericardium, which consist in an inconsiderable excess of structure, hypertrophy, a slight thickening and condensation of the serous investment of the heart."

Mr. Paget does not think that the question, whether these spots are seated in or on the pericardial membrane, can or need be definitely answered. He says:

"According to the depth and degree to which the cellular or adipose tissue round the heart is inflamed, the lymph will be effused, either on its surface (after the removal of the epithelium), or in its areolæ, or in both situations; and in any case where it becomes organized, it will form a new epithelium on its free surface, and may, therefore, be said to be beneath the serous membrane. The spots are generally easily stripped off, but in no case after they are organized can they be separated from the subjacent tissue without dividing numerous connecting filaments, and leaving the surface from which they are removed flocculent and shreddy. The little patches of soft, transparent, and very easily separable lymph, which M. Bizot mentions, are, I believe, only an early stage of the same affection, in which the inflammation is chiefly superficial, and lymph is effused on the surface of the membrane; at first it of course adheres but slightly, but subsequently, when vessels pass into it, it undergoes all the usual changes of effused lymph, and becomes opaque and adherent."

In some comparatively rare cases, a band of adhesion has been found passing from the white patch to the opposite serous surface; but in the great majority, the latter presents no trace of similar alteration. We subjoin some details of our own examination of the white patch. In the case of a man, aged 49, dying with cirrhosis, and having baconny deposit in the liver, similar deposit in the spleen, necrosis of the tibia, pleuritis, and albuminuria; there were several very well-marked patches of moderate size, and very various shape, on different parts of the visceral pericardium; they were sometimes sharply defined, at others shaded off gradually into the serous membrane around, which, in the greater part of its extent, was perfectly natural. The white patches appeared most clearly to be chiefly beneath the serous membrane, but attached to and involving its tissue; they were not separable from it, but were removed quite readily along with the serous membrane, when the latter was pulled off. On making a vertical section of a patch, the cut surface showed the serous membrane passing on to and over it, or blending with it, while the whitish thickening substance became everted beneath. Microscopical examination showed the patch to be produced by the presence of fibroid tissue, with varying quantities of obscure granulous and oily infiltration, together with numerous elongated nuclear corpuscles. The subserous tissue, in another, was shown to be involved in the exudation, by the circumstance that several groups of fat-cells were seen imbedded in the patch. One patch, of elongated shape, was remarkable from the circumstance, that a portion of it near the middle, about one-third of an inch long, with sharply-defined edges, was raised and detached from the serous membrane so far that the eye of a needle could be passed for some way on either side beneath it. It seemed that the exudation had gradually contracted, and drawn together the serous membrane, raising up, at the same time, a fold of it, which had thus become, in great part, severed from the rest of the surface. The greater part of the patch was subjacent to, and continuous with, the serous membrane. This separation of a thickened part reminds us of the setting free of subsynovial formations (loose cartilages) in the cavities of joints. In connexion with the milk spots, we may notice certain little granular-like bodies, which are occasionally met with in the pericardium and endocardium. Mr. Paget seems to allude to these under the name of pearly granules, which is tolerably descriptive of their aspect. They vary from the magnitude of small granules to that of a minute seed, and have a kind of translucency. They lie in the subserous tissue, but cause slight prominence of the membrane covering them. They consist of an homogeneous basic matter, imbedding elongated and spherical nuclear corpuscles, and seem quite analogous to the Pacchionian bodies. There are found in some cases, we cannot say in what proportion, a number of small fibrous processes, much like villi, fringing the appendices of the auricles; some of them are simple, others ramified. They consist of fibroid tissue, imbedding some granular matter, and rather numerous nuclear corpuscles; they taper more or less gradually to their free extremities, and have smooth, well-defined margins. Some good observers regard them as of the nature of adhesions, as evidences of by-past inflammation. Mr. Paget remarks, very correctly, that the adhesions which often co-exist with white patches are generally situate upon the great vessels which pass to or from the base of the heart; and he explains the great frequency of their occurrence in this situation, by the consideration that these vessels are fixed and motionless in comparison with the walls of the heart, and so "present every facility for the adhesion and organization of the lymph that is effused, and which either gravitates to them in the recumbent position of the body, or is impelled thither by the currents which the action of the heart excites in the fluid around it." With regard to the kind and degree of inflammation producing these spots, Mr. Paget speaks in the following terms:

"In all other cases (except where there exist adhesions) I should regard them as the effects of local and defined inflammation, which has been prevented from producing adhesion, by the fluid simultaneously effused separating the serous surfaces, and permitting part of the lymph to sink down to the great vessels, while the rest remained on the surface, and in the substance of the inflamed part."

The inflammation, then, which produces the white patch, causes not only the effusion of lymph in some quantity, but of fluid enough to separate the serous surfaces. We confess it seems to us difficult to believe that this can occur commonly without symptoms; at least such symptoms as would be more frequently noticed than they are among the carefully observed patients of our hospitals. Again we must object, that in the more numerous cases where no symptoms occur, where there is no illness, the subjects are not in the recumbent position the greater part of the time, but following their usual avocations. The effused lymph ought, therefore, to gravitate rather towards the apex and under-surface of the heart, and not to adhere to the great vessels. Indeed, even in the recumbent position, one would rather expect to find it between the heart and the diaphragm, than in the more anterior and higher position. Further, if the white patch were produced by inflammation, causing some noticeable effusion of lymph and serum, we should surely find much more manifest trace than we do of progressive transformation of this effused lymph. On the contrary, it appears to us, that the white patch *is always a white patch*. It varies extremely in degree and size, from the slight streaky opacity with which it commences, to the dead-white, well-defined *plaque;* but we do not find it passing through stages of transformation. Had the origin of these spots been such as Mr. Paget describes, we should surely have had no more doubt upon the matter at present than we have about the formation of common pleural adhesions. We would also refer to analogy: it seems scarce doubtful that the white patches of the pericardium are analogous to the Pacchionian bodies, and the thickening of the splenic capsule, &c. Now, as we do not say that these are the result of arachnitis, or peritonitis, why should we put down the former to pericarditis? With regard to the adhesions, it seems that in many cases they are very slight, and we almost doubt how far the effusion of a very small quantity of lymph, which we should suppose to take place on the spot, can be considered certain proof of the existence of actual inflammation. Might not a little congestion produce the same result? Even in the passive exudation of ascites, fibrine is sometimes present in some quantity, and it does not seem at all improbable that a spot of active congestion stopping short of inflammation, might cause the transudation of fibrinous fluid through the thin serous tissue. Such little spots of congestion, though they might in strict language be said to border on inflammation, would be at a prodigious distance from any condition that we could justly call pericarditis. We do not think it warrantable to conclude, from the frequency of their occurrence, that pericarditis is a frequent disease, such pericarditis, at least, as the physician has to regard. However, even if the adhesions could be regarded as proofs of slight inflammation having existed, we still think their coincidence with the white patches is by no means a sufficient proof that these are the results of pericarditis. It is decidedly an indication of a greater strain upon the bloodvessels, or of more excited action when effusions take place into a serous cavity, than when they are simply deposited in the interstices of the bloodvessels of the subjacent areolar tissue. They traverse two membranes in the one case, the vascular wall (far the thinner) in the other. The case of the Pacchionian glands shows, it seems to us conclusively, that fibrine may exude and collect in the subserous tissue, independently of inflammation certainly, and probably even of much congestion. We incline, therefore, to the belief, that though inflammation may give rise to the white patches, yet that, in the majority of cases, they are the result either of the simple exudation of unhealthy plasma, or of such plasma effused from vessels in a moderate state of congestion. With regard to the filamentous processes fringing the margin of the appendices auricularum, we are led to conclude, from their microscopic characters, that they are outgrowths from the serous membrane, and not mere remnants of adhesions. The number and appearance of the corpuscles they contain, and the fat-cells which are occasionally present in them, seem to show this.

The endocardium is not uncommonly observed in a state of slight thickening and opacity, more particularly, we think, in cases where the valves are puckered and stiffened, and the chordæ tendineæ shortened. The change occurs in a diffused manner, overspreading the general surface, and also in the form of patches. In these parts, microscopic examination shows infiltration of the fibrous tissue with granulous and oily matter, as well as more or less increased formation of fibroid tissue. A peculiarly interesting condition, which seems only an extension of the same process, is that which we have noticed in the review on fatty degeneration, as a fibroid degeneration of the apices of the columnæ papillares, which causes secondary fatty transformation of the muscular fibre. In a specimen we lately examined, the apicial part of one columna papillaris of the mitral valve was converted, for about the space of half an inch, into a whitish, firm, opaque tissue. This terminated abruptly where it came into contact with the muscular fibre, and had evidently replaced it. Microscopically examined, it consisted solely of fibroid tissue, the atrophy of the muscular fibres was complete, and even their oily remains had quite disappeared. This is an excellent instance of fibroid degeneration; it may not unfrequently be observed in various degrees, but never presents that we have seen any traces of hyperæmia or exudation undergoing change. The fibroid substance invades the muscular column gradually, and creeps on by a gradual transformation of blastema into its own tissue.

We proceed to consider chronic changes of a fibroid nature in the *cardiac valves*, which certainly are yet far from being fully understood as regards their causative origin. Most observers are inclined to regard them as in some way connected with endocarditis, of which, however, there is often no history whatever. Had we space, we would quote at length from Dr. Latham's work on 'Diseases of the Heart,' Lect. xxiv., where the obscurity of the nature and commencement of these changes is fully recognised. We must content ourselves, however, with a mere reference to it, and proceed to quote Rokitansky's excellent description of the morbid condition in question, which accords very much with the result of our own observation.

"Hypertrophy of the valves affects either their fibrous texture, or their investment of endocardium. We have already observed that hypertrophy of the fibrous basis of the valves is occasionally associated with their general hypertrophied condition. We moreover frequently notice in the auriculo-ventricular valves, and especially the mitral, both in individuals of advanced life and in young persons, a pale white, yellowish white bulging, or the thickening of the valve towards its free edge, or a series of bulgings at the insertion of the papillary tendons, which, however, do not interfere with the function of the valve. No osseous concretions are ever developed in this hypertrophied tissue of the valves. In young persons we occasionally meet with a condition of this portion of the valvular structure, which very probably indicates incipient hypertrophy of the fibrous texture, the free edge appearing swollen, more especially at the insertions of the papillary tendons. This bulging is produced by a pale red, translucent, more or less gelatinous substance, effused into the texture of the valves, from which, as from a blastema, the fibrous tissue is developed. This substance is very commonly found to consist of a translucent, partly homogeneous, and partly indistinctly fibrous mass, in which are imbedded numerous cell nuclei, and the so-called nucleated fibres.

"Hypertrophy of the aortic valves, more especially of their nodules, is not of very rare occurrence. Hypertrophy of the endocardium is, on the other hand, both more frequent and more intense in the arterial valves, where it more especially affects the aortic valves, as might be expected from the greater tendency of the left side of the heart, and of the trunk of the aorta, to a similar condition of excess of growth in the endocardium and the lining arterial membrane. The valves become thicker in consequence of the deposition of new layers, and the aortic valves, more especially at their nodules and free margin, present an appearance of bulging; the protuberance being roundish or cylindrical in form, even and nodular, and having occasionally a somewhat prismatic or facetted character, from the pressure which they mutually exert on one another. The valves thus coalesce with one another, and with the walls of the arteries, by means of prolonged depositions from their lateral insertions. A shrivelling process, similar to that by which the arteries are analogously diseased, now affects the valves, which become thicker, full, and rigid, and degenerate into a cylindrically-formed swelling, and by this means, on the one hand contract the ostium, and on the other become insufficient."

Calcareous deposit may take place in the thickened structure, or it may undergo atheromatous disintegration; and in either case fibrinous vegetations may form upon it. This form of hypertrophy of the valves, in Rokitansky's opinion, is not the result of endocarditis. While we agree completely with the first part of Rokitansky's account, we cannot but entertain much doubt whether there is any real difference between the two varieties of change which he distinguishes: whether the hypertrophy of the endocardium is not produced in the same way as that of the interposed fibrous tissue—viz., by exudation undergoing fibroid development. We cannot think the change which produces stiff and puckered valves is at all of the same kind as that which we described in the review on fatty degeneration, as essentially depending on formation of fresh layers of fenestrated membrane on the inner surface of the arteries. The difference seems to us rather to lie in the circumstance, that in one case a small mass of exudation is thrown out, which gradually undergoes fibroid change; while in the other the exudation is never more than can be at once converted into fibroid endocardial tissue, which thus goes on steadily hypertrophying. Two of our own observations show how latent these changes may be. The heart of a man who committed suicide appeared perfectly healthy, with the exception of some thickening of the mitral curtains. This was caused by the presence of a reddish succulent substance between the layers of the endocardium, consisting of a finely mottled blastema, imbedding multitudes of fibre forming nuclei. In a healthy child, aged 5, dying soon after a severe burn, the larger curtain of the mitral valve presented, near its free edge, close to the attachment of the cordæ tendinæ, a circumscribed oval dark reddish soft-looking patch of thickening. This exhibited, under the microscope, only fibro-homogeneous stuff, with an infiltrating reddish fluid, and some granulous corpuscles little larger than nuclei; some patches of oily-looking matter were also seen, which proved to be of earthy nature; nuclei and celloid corpuscles were dispersed scantily throughout the homogeneous-fibroid substance. Deposits of this kind are by no means uncommon in the mitral valves: and it is manifest that if they are extensive, invading the greater part of the valve, they wili at a later period, when they begin to contract, cause puckering of the tissue, and contraction of the orifice. Not the least doubt remains on our mind that these changes may be of a gradual and latent kind, that they may arise and increase, and produce no symptoms whatever, until their secondary effects begin to manifest themselves.

This, we think, is most frequently the case, but there can be no doubt that the same ultimate effects may result from endocarditis, rheumatic, or of other kind. The more usual issue of such attacks is, however, the deposition of vegetations on the valves, rather than thickening of their substance. We are much inclined to believe that the quality of the exudation has much influence on the contraction of the valve affected; sometimes we find a valve much thickened, without much narrowing of the orifice, at others the narrowing is very considerable. This seems to depend not only on the age of the exudation, but also on the circumstance that some kinds of exudation have a greater tendency to contract than others. In a valve greatly thickened and indurated, with contracted orifice, we found the fibroid tissue consisting of an homogeneous basic substance, with mingled granulous and oily matter, the latter in small quantity. Through this there were visible, more or less distinct traces of white and elastic fibre, but scarce any of nuclei, which had probably undergone change into fibre; acetic acid rendered the whole much more transparent. Hasse, after giving an excellent description of the thickened, indurated, and contracted condition of the valves, says this condition, in the advanced degree which he describes, "is always a result of endocarditis of variable intensity. An inflammatory origin," he proceeds, "cannot be assigned to every instance of thickening and puckering of the internal surface of the heart." He directs attention particularly to the slighter grades, the first traces of organic changes, and their gradual development: and remarks most justly on the advantage to be gained from studying cases where there had been no marked symptoms of heart-disease. The two cases we have cited above show how small are the beginnings of chronic valvular changes, and how little they have of any inflammatory appearance. It may also be remarked that, although we meet with the most various degrees of valvular thickening, we never, so far as we are aware, find any great variation in the condition of the part, such as would indicate the transitional stages of an inflammatory effusion. There is at first slight thickening, which is added to, and so the process goes on, but it is very much of the same kind throughout. With respect to the circumstances under which chronic valvular changes take place, we may gather some information from Dr. Barclay's papers in the 'Medico-Chirurgical Transactions' for 1848 and 1852. He states that "about a third of the cases of valvular lesion are associated with a rheumatic history," and that "in more than half the cases of valvular lesion the kidney was not quite healthy." On looking over the tables he gives, we find that in that attached to his first paper, there were 56 cases in which the valves, aortic or mitral, were more or less thickened. In 9 of these there was a previous history of rheumatism, and no renal disease; in 24 there was no rheumatic history, but renal degeneration far more frequently producing the granular kidney; in 3 there was a concurrence of rheumatism and renal degeneration; and in 19 there was neither. In the table attached to the second paper, there are 55 cases of greater or less amount of thickening of the cardial valves, of these 10 coincided with a previous attack of acute rheumatism, 19 with renal disease, in 9 there was a history of acute rheumatism, and the post-mortem showed renal disease, and in 3 neither rheumatism nor renal disease occurred. If we combine the two tables, we get 96 cases of thickening, of which 19 coincide with acute rheumatism, 43 with renal degeneration, 12 with both, and 22 with neither. We are much inclined to think that if all the kidneys had been microscopically examined, a still larger number would have been found in a state of degenerative disease. Still, the proportion is such as to show that thickening of the valves is associated much more often with renal disease than with acute rheumatism. The influence of advancing years on the production of valvular thickening seems unquestionably great; in the 55 cases of the first table there were only 8 below the age of 30; and in the 41 of the second, only 9. Considerable thickening had, however, occurred in several cases before the age of 40.

Dr. Ormerod, in his 'Gulstonian Lectures,' arrives at an opposite conclusion. It is true that his tables show that the number of valvular affections coinciding with rheumatism as their cause, exactly equals that which coincides with renal disease; but he believes that the independent influence of renal disease, apart from rheumatism, in inducing valvular disease, is not of any very great importance. He regards it rather as a modifying than as an original cause: and remarks that sub-acute rheumatism is so frequent a complication of renal disease, that its occurrence may well have been overlooked. To want and intemperance he attributes almost as much causative influence as to rheumatism or renal disease; but remarks, as before, that the occurrence of rheumatism may easily have been overlooked. To our own mind there is quite as much reason to think that renal disease may have been unnoticed as rheumatism, and still more for thinking that renal disease was, rather than acute rheumatism at least, the cause of the thickened conditions which we have particularly in view. However, it appears from Dr. Ormerod's tables, that out of 132 cases of valvular disease, 83 were connected with renal disease, or with want and intemperance, both being causes which would induce serious deterioration of the blood, and predispose strongly to unhealthy nutritive changes. Perhaps chronic rheumatism itself may act in the same way, and induce slow valvular changes of the nature of thickening, distinct from the vegetations deposited in the acute attack.

The arterial *semi-cartilaginous patches*, as they are termed by Hasse, seem to require some notice here, though they do not correspond exactly with the fibroid changes we have been considering. Hasse says—

"They occur, throughout the whole of the arterial system, on the free surface of the internal membrane. They, in all probability, originate from a semi-fluid, almost gelatinous substance, of a pale yellowish or reddish colour, forming a layer, of greater or less extent, upon the inner surface of the artery. In the aorta and larger vessels, this layer is mostly scanty, but in the smaller vessels often more abundant. Thus Bizot found a considerable portion of the tibialis anticus almost replete with it. It is doubtful whether the layer be the result of morbid exudation from the internal membrane, or be deposited immediately from the blood. The latter assumption appears more satisfactory, if it be considered that neither the internal nor the other arterial coats suffer any change at first; that in the large vessels the gelatinous layers mostly form at the points where branches are given off; that they are more frequent and numerous in the smaller arteries at a distance from the heart, and where the circulating current has less power; that they originate on the free surface of arteries only, and never (as some anatomists assert) between the membranes. Bizot regards them as a morbid secretion of the inner membrane engendered by inflammation. Conjointly with these soft deposits are found, occasionally in the same subject, others more solid, and at the same time more firmly adherent to the internal membrane; others of still greater consistency, being whitish and more opaque; others, again, closely resembling boiled white of egg; and, finally, membranaceous patches, almost cartilaginous. Thus have we the means of tracing, though not always in the same individual, the progressive transition from the gelatinous layer to the semi-cartilaginous patch, proving, almost beyond a doubt, the development of the latter from the former. This is, moreover, corroborated by the experience of Bizot, who met with the softer deposits more frequently in young subjects, and the harder in old ones. It is seldom possible to discover any trace of internal membrane beneath these patches; for, although with care we may succeed in separating this membrane along with its gelatinous investment, it usually tears off at the confines of the cartilaginous patches, proving the latter to be in immediate contact with the middle tunic of the artery. In some preparations, however, I have distinctly observed the internal membrane remaining unchanged beneath the patches. These semi-cartilaginous plates occasionally become partially loosened from their base, and project into the calibre of the artery, thereby promoting the deposition of fibrinous coagule upon their prominent margin. I have observed this twice in the abdominal aorta. These semi-cartilaginous patches undergo hardly any change in the sequel, appearing only to acquire greater thickness. They never ossify, although Andral and others have maintained the contrary. Where several patches are grouped together, the whole surface of the artery, for a considerable extent, becomes coated with a thin, pellucid false membrane, giving it the semblance as if the inner membrane consisted of several layers, and was prolonged over the semi-cartilaginous patches. The arterial walls then acquire an unusual thickness, become stiffer and less elastic, and their free surface, without losing its polish, assumes a delicately puckered character."

We must say, that our experience of this deposit leads us totally to disagree with Hasse as to its being situated on the internal surface of the artery. To us it has always appeared to consist manifestly in a fibrinous exudation taking place in the internal layers of the wall, and there solidifying, so as to produce the various sized, more or less rounded elevations by which it is distinguished. The process seems to us exactly analogous at its outset to that of thickening of the valves, differing in its further progress chiefly in the circumstance, that the primarily homogeneous fibrinous deposit is not so prone to undergo fibroid change as that which is exuded in the substance of the valves. Andral states the seat of the deposit to be between the internal and middle coats; and Hasse himself says, in one part, that the patch is in immediate contact with the middle tunic. The tendency of the arterial deposit to contract is very marked, and occasions both the wrinkling and puckering of the coats, and sometimes, when the exudation surrounds the orifice of a branch, considerable narrowing or even closure of it. Rokitansky's account of this state seems to us to be an inextricable confusion of three different conditions—the atheromatous deposit, the hypertrophy of the fenestrated membrane, and the true fibrinous exudation. He says—

"We find, on a close examination of the deposit, that it has nothing in common with cartilage or fibro-cartilage, with which it is ordinarily compared and even confounded; and that it actually consists of structures analogous to the layers which constitute the lining membrane of the vessel, the epithelium, fenestrated membrane, and longitudinal fibrous coat. At its commencement it is a delicate, soft, succulent membrane, exhibiting a vitreous transparency; it continually increases in thickness by the addition of new strata, and thus gradually passes from the condition of succulence and transparency, characteristic of recent formations, to that state in which it appears opaque, resembling coagulated albumen; and finally presents a ligamentous appearance, having a dull wrinkled surface."

He proceeds to speak of the formation of canals within the deposit opening on the internal surface of the artery, and penetrating as far, in many instances, as the circular fibrous coat, which is soft, brittle, cleft, of a faded dirty brownish colour, and passes into a state of fatty degeneration. By these channels the blood of the artery enters into the deposit. Rokitansky regards these canals as the result of a partial resorption of the deposit, similar to that which Stilling observed in the thrombus of tied arteries, and he himself in a fibrinous coagulum within the heart. We have not observed these canals ourselves in the arterial fibrinous deposit, but we have seen a deposit of lymph on the surface of an inflamed pleura channelled much in the same way, and we have no doubt that Rokitansky's view, of the change being due to absorption, is correct. This circumstance, therefore, seems to be a further argument to show that the deposit is a true fibrinous exudation. Out of the cases of which we have notes, there is only 1 in which there was thickening and stiffening of the cardiac valves. In 3 the kidneys were more or less degenerated; and in 3 there was more or less indication of fibroid change in the Glissonian sheaths, or in the capsule of the liver. In 1 the capsule of the spleen was thickened, and there were granular lymphy deposits on the peri-endocardium. Rokitansky believes, and our own experience, so far as it goes, is confirmatory, that this diseased state of the vascular coats is the most frequent cause of spontaneous aneurisms.

Handfield Jones.

(To be continued.)

Review V.

1. *Die Verdauungssäfte und der Stoffwechsel.* Von Dr. F. Bidder und Dr. C. Schmidt. Zweite Abtheilung—*Der Stoffwechsel.—Mitau und Leipzig*, 1852. pp. 418.

The Digestive Fluids and the Metamorphosis of Tissue. By Professors Bidder and Schmidt. Second Part—*The Metamorphosis of Tissue.*

2. *Der Harnstoff als Maass des Stoffwechsels.* Von Dr. T. L. W. Bischoff, Professor der Anat. und Phys. in Giessen.—*Giessen*, 1853. pp. 188.

Urea as a Measure of the Metamorphosis of Tissue. By Dr. Bischoff.

3. *On the Composition of Food in relation to Respiration, and the Feeding of Animals.* By J. B. Lawes, Esq, and J. H. Gilbert, Ph. D. ('Transactions of the British Association for the Advancement of Science, for 1852.')—*London*, 1853. p. 323.

In a previous volume we have given the latest researches on the Chemistry of the Digestive Fluids, as described in the elaborate work of Bidder and Schmidt; we are now, therefore, in a position advantageously to consider the second part of the work above named, which treats of the most important and complex subject of the 'Metamorphosis of Tissue.' A correct knowledge of those changes which go on in that potent laboratory, the human frame, is the aim alike of the physiologist and the physician, and without this knowledge neither physiology nor pathology can be considered as other than a crude and imperfect science.

In considering the late publications on this subject, we shall direct our attention principally to the second part of the volume by Bidder and Schmidt,—a treatise which is justly characterized by Lehmann as distinguished alike for its accuracy of method, and for the value of its discoveries. We shall then consider the elaborate and most practical treatise of Professor Bischoff; and shall, finally, analyze the excellent paper which has been published by Messrs. Lawes and Gilbert.

But before entering on the immediate consideration of these works, it is necessary to notice some material differences of opinion between Bidder and Schmidt on the one hand, and Bischoff on the other. In fact, at the very outset of the inquiry on the metamorphosis of tissue, a question presents itself, which it is absolutely necessary to solve, and yet which is perhaps at present incapable of solution. This question refers to the formation of urea and the elimination of the nitrogen of the system.

After the discovery of urea in the urine, and the demonstration by Prevost and Dumas, and others, that it is not formed by the kidneys out of the constituents of the blood, but that it exists preformed in that fluid, the question naturally arose as to whether it was derived solely from decomposed previously developed animal matters, or whether it was also a product of the excess of nitrogenous food, which was thus got rid of without conversion into tissue. The fact of its being found in the urine of starving animals convinced Müller, March, and others, that the former was its source; and Berzelius, likewise, was of opinion that it was the product of the gradual metamorphosis of a portion of the living solids of the body. Liebig believes it to be "impossible that urea can have any other origin than the metamorphosis of the nitrogenous tissues;" and he considers that "the quantity of tissue metamorphosed in a given time is measurable by the amount of nitrogen in the urine." The close dependence of the quantity of urea on the nourishment taken, and the extent to which its formation is increased by the free use of animal food, convinced Lehmann, on the contrary, that in this case the excess of protein does not first enter into the substance of the tissues, but that urea is directly formed from it, and that nature thus gets rid of the superabundant plastic material along with that which has become unfit for use. Frerichs holds a similar view, believing that urea is formed in the blood as a secondary product by the oxidation of the nitrogenous constituents of that fluid. Bidder and Schmidt, while they hold and recognise the importance of the distinction between plastic and respiratory aliments, also believe that animal food taken in excess undergoes, immediately after its arrival in the mass of the blood, oxidation and separation into urea, carbonic acid, and water; and they maintain, with Frerichs, that, consequently, only a part of the urea excreted under these circumstances is the product of the metamorphosis of the nitrogenous parts of the body, the greater part being formed, as we have observed, directly in the blood, by the oxidation of its albumen. These views, if confirmed, would be fatal to the establishment of a measure of the metamorphosis of tissue.

To these latter opinions Professor Bischoff objects, first, that there is no positive proof of the formation of a part of the urea directly at the expense of the albumen of the blood; secondly, that all are agreed that the urea secreted during starvation is and can be only a product of the metamorphosis of the nitrogenous elements of the body, and that it is on all hands admitted that the same is true where an exactly sufficient amount of nitrogenous nutriment is taken, and, provided the individual grows and increases by it, even where the quantity of such nutriment is more than is necessary for his support; and he argues, that it is then unreasonable abruptly to assume another and wholly different mode of formation, in instances in which still more food is consumed and actually digested, and when proportionally more urea is excreted. "Where is," he asks, "the boundary between the two modes of the formation of urea?"

But he further argues, that it is both physiologically and chemically impossible that urea should be derived directly from the metamorphosis of the albuminates of the blood—i.e., from the excess of food. If the oxygen in the blood could make the direct formation of urea from albumen possible, its constant presence, which has been established, would prevent the persistence of albumen in that fluid, and would render nutrition through the agency of this principle impossible. It would be only in case of the existence of a great excess of albumen that any could escape metamorphosis, and remain for the purpose of nutrition. But it is only when present in excess that it is said to undergo the transformation into urea.

Chemically, too, this direct change of the albumen of the blood must, in the present state of our knowledge, be considered an impossibility. Urea is essentially the end of a series of oxidating processes of the albumen, the realization of which is as yet possible only in the living body. The possibility of a direct change of the albumen in the blood into urea would exclude these intermediate stages, and should then undoubtedly be capable of artificial accomplishment external to the system.

For these and other reasons Professor Bischoff is of opinion that "urea is solely a product of metamorphosis, and that the albumen of the blood must necessarily pass through other changes in the vital process before, with continued phenomena of oxidation, it is transformed into urea."

Without wishing to offer a decided opinion on a subject in reference to which such distinguished investigators, who have directed so much attention to the point, are at issue, we would observe, that we cannot think the mere rapidity with which an increase of the quantity of urea in the urine takes place after the ingestion of animal food affords sufficient ground for assuming the direct formation of urea from the constituents of the blood. So intimate is the sympathy by which the organism is bound into an harmonious whole, that it appears to us to be an admissible hypothesis, that the arrival of fresh supplies may be the signal for the instant commencement of increased metamorphosis in the most distant parts of the body, and for the consequent augmentation of urea in the urine; or the same effects may be produced by the heightened vascular action which accompanies the early stages of digestion.

With regard to the other point of difference we have alluded to, Bidder and Schmidt maintain that the elimination of nitrogen from the body takes place, as nearly as possible, solely through the kidneys; and they attribute the discrepancies between their observations and those of others to the ease with which the decomposition of urea takes place, which can, they remark, only be obviated by the instant addition of mineral acids to the urine: any loss by the evolution of carbonate of ammonia &c. would be calculated as nitrogen expired from the lungs. Bischoff, on the other hand, usually found that about one-third of the nitrogen is eliminated in some other way than as a component of urea, although it is difficult to decide in what form it is removed. It does not appear to be in the intestinal excrement, as the quantity got rid of in that mode is, comparatively speaking, extremely small; and the researches of Regnault and Reisset have shown that but a very minute portion is evolved in respiration, while nothing is known of any more considerable escape of nitrogen by the skin. Professor Bischoff suggests that it may be given off as carbonate of ammonia by the skin and lungs.

Professor Bischoff points out the great facilities afforded for these investigations by the discovery of Liebig's mode of estimating the urea by means of a solution of nitrate of mercury—a mode devised by its author in compliance with a request of Bischoff himself, who had long felt the want of such an instrument and its effect in limiting observations on the urine, and who has since always employed this method in his researches.*

Having premised these observations, we shall now take up the work of Professors Bidder and Schmidt; and to enable us to understand clearly the general principles on which their labours have been conducted, a translation of the remarks introductory to the portion of the volume we are about to consider will be necessary.

"The circulation in the animal body of the substances pre-formed in vegetables and in the external world, from their entrance as carbo-hydrates, hydro-carbons, albuminates, water, oxygen, and salts, to their exit in the form of carbonic acid, water, urea, &c., embraces a series of intermediate steps, the chemical constitution and physiological importance of which have already been discussed in the former part of this work (Saliva, Gastric Juice, Bile, Pancreatic and Intestinal Juices). In each of these intermediate circulations, water combined with organic and inorganic constituents, characteristic of the particular glandular secretion, traverses the capillaries of the intestinal tube, the compound so formed being again taken up into the general circulation, after having, in a more or less altered condition, fulfilled certain mechanical or chemical functions, to undergo further changes by splitting and oxidation of its complex atoms into simpler combinations. It then appeared, that for each kilogramme of an animal's weight from 150 to 160 grammes of the secretions enumerated are daily subjected to intermediate circulation, are separated and again absorbed, to prevent stagnation and consequent abnormal splitting-processes of those matters (putrefaction), and to bring the normally-formed products of metamorphosis (taurin, glycocoll, &c.), in a rapid stream, under the influence of oxygen, and to restore them, after the employment of the caloric combined with them, in the highest oxydized form to the external world.

"A clear view of the entire metamorphosis of matter indispensable for the maintenance of life—i. e., *of the lowest possible measure of movement*, in the intermediate and general circulation, at which existence can continue, as well as the magnitude of the arbitrary increase of the same by the ingestion of nutriment, can only be obtained by simultaneous observations of the whole ingesta and egesta of the same animal, compared with the intermediate intestinal circulation of animals of the same constitution under circumstances in other respects similar. The *typical minimum* of the metamorphosis necessary for a particular species of animal is obtained by observation of the state of starvation—with greater certainty in carnivorous animals, as cats, which endure prolonged fasting without essential dis-

* With respect to the accuracy of this mode of determining urea, besides the evidence adduced by Liebig himself, Bischoff and Vogel affirm that it is extremely correct. Heintz, in his late masterly work (Lehrbuch der Zoochemie, p. 1058), speaks of it as equal in accuracy to the best methods. We shall, therefore, assume that Bischoff is justified in the extended use he has made of this plan.

turbance of the several organic functions, than in herbivorous animals, as rabbits, which can at most last only from three to five days without food. The excess above this indispensable measure of typical change may be regarded as *superfluous consumption* (Luxus Consumption), although the health and the energy of all the vital functions are, to a certain degree, improved by an increase of the circulation of matter. The amount of this excess may be in like manner ascertained by general observation of animals more or less largely fed, the elementary composition of the food daily weighed out to each being determined by analysis specially for each series of experiments." (p. 291.)

To this view of the "*typical minimum*" Bischoff objects that the state of starvation can never be regarded as a normal condition, and that the normal condition must be that in which an individual with a definite proportion between the nitrogenous and unazotized parts of the body maintains an uniform weight. The term "luxus-consumption" appears to us to be unhappily chosen; a man, for example, ought to endeavour to use exactly that amount of food which will maintain both body and mind in the most efficient and energetic state, and it is only when his consumption exceeds this limit that any part of it can, in strict language, be called superfluous.

The portion of the work we are now considering is subdivided into three parts: the first treating of the circulation at large; the second, of the factors or agents of metamorphosis, under the heads of the respiratory process and of the formation of urine; and the third, of the elementary circulation, considered both absolutely and with reference to the weight of the body as unity. Some idea of the extent of the authors' investigations may be formed from their own statement, that during four weeks occupied in the first series of observations, one of them and the assistants employed with him rested but three or four hours in the twenty-four, the remainder of the time being given uninterruptedly to experiment. They express their regret at being obliged, in the present instance, to depart from their principle of stating all the direct results of their analyses; the portion of their volume now under our consideration is, in fact, for the most part, a tabular compendium of their experiments. In attempting an analysis of it, we must therefore content ourselves with giving our readers a general idea of their mode of investigation, and a *resumé* of the more useful of their conclusions.

In order to ascertain the extent of the formation of carbonic acid and urea during the freest use of albuminates and hydro-carbons, compared with the lowest possible amount of change of material during prolonged inanition, a healthy, nearly full-grown cat was selected, and fed during the first eight days of observation with as much chopped fresh beef as she could eat, five per cent. of the meat, when carefully mixed, being retained for analysis. The cat was kept in a tin box, 0·5 cubic metre (273·5 cubic inches) in size, furnished with three pairs of windows in the sides. The floor of the box was concave, and had a central drainage-tube for the urine; after each evacuation of urine the cat was set free for some hours in the room, and carefully watched, and was then used for the experiments on respiration. The capacity of the glass bell used for the latter was 39·43 litres (or 2155·821 cubic inches), from 15 to 20 litres of the air being drawn off within an hour, by means of the aspirator, so as to prevent any injurious accumulation of carbonic acid. The proportion of carbonic acid in the atmosphere of the apartment was ascertained in each experiment, and that of the air remaining in the bell was determined by volume, with all possible precautions, in carefully graduated tubes.

The urine of cats is merely a solution of urea with salts; it contains no trace of uric acid, and but very small quantities of other organic matters, which it is easy to prove by determining the carbon on the one hand, and the nitrogen on the other. The former was accomplished in the usual manner, by combustion with oxide of copper and metallic copper; an accurate determination of the hydrogen is impossible, because urine cannot be obtained perfectly anhydrous without decomposition; to avoid which we are obliged to dry the entire quantity with a weighed amount of freshly ignited powdered silica—only so far, however, as may be necessary to prevent loss when the residuum is mixed with the oxide of copper. In like manner the determination of the nitrogen is effected by ignition with oxide of copper in a stream of carbonic acid. Most of the determinations of urea were accomplished by means of sulphuric acid and chloride of platinum; in some, Bunsen's method was used, by heating a weighed quantity of urine with chloride of barium in an hermetically sealed glass tube, at 180° C. (356° F.); in others, the mode just described, with oxide of copper in a stream of carbonic acid, was employed.* The inorganic matters were ascertained by drying, carbonization, exhaustion with water, and incineration of the residual carbonaceous matter, the sulphuric and phosphoric acids being quantitatively determined directly from the urine by the ordinary method, with chloride of barium.

During the eight days of *ad libitum* feeding, the cat, which at the commencement of the experiment weighed 2·177 kilogrammes, or 4·8 pounds avoirdupois, increased 0·337 kilogramme, or nearly three-quarters (·7432 exactly) of a pound. The authors calculate that this increase may have been caused by—

1. Retention and deposition as adipose tissue, of the fat consumed, the albuminates and the gelatinous matters (Collagen) undergoing oxidation and separation into urea, carbonic acid, and water, sooner than the fats; or—

2. Through morphological metamorphosis of the organo-plastic portion of the food, the fats, previously oxidized, being made subservient to the process of respiration; or—

3. Through equal assimilation of the smaller part of both, the greater portion undergoing metamorphosis under the influence of oxygen.

The authors conclude from their experiments, to enter into the details of which would occupy far too much of our space, that—

> "The meat eaten and taken up in a soluble form into the circulation of the animal subjected to experiment, is, immediately after its arrival in the mass of the blood, oxidized and separated into urea, carbonic acid, and water, and that it does not undergo previous morphological change into other histological tissues, (muscle, areolar tissue, &c.)"

* Prof. Draper, of the University of New York, has recently proposed a method of determining the quantity of urea in urine, founded on the fact that this substance is immediately decomposed by nitroso-nitric acid, carbonic acid gas escaping with brisk effervescence. By means of a suitable apparatus the gas is made to pass through baryta water, and the urea is estimated from the quantity of carbonate of baryta formed. See London, Edinburgh and Dublin Philosophical Magazine, Oct. 1853, p. 290.

And that—

"The separation of the albuminates into urea, and carbon and hydrogen to be used for respiration, must precede the oxidation of the fats, since the urea leaves the body sooner than do the latter in the form of their oxides, carbonic acid, and water."

It appears to us unfortunate that a pregnant animal was selected on this occasion, as was proved by the expulsion of two dead fœtuses on the ninth, and of one on the tenth day of the subsequent "inanition experiment." Much of the increase of weight must, of course, have been owing to the natural growth of the young animals, and the corresponding development of the uterine system.

From table V. it appears that the ingesta during twenty-four hours, were composed of 358·012 grammes of meat, containing fat, water, and oxygen—viz., 221·491 grammes of water; 40·920 of carbon; 5·645 of hydrogen; 8·604 of nitrogen; 78·016 of oxygen; 2·804 total of inorganic ingredients; 0·532 of sulphur; 0·9·0 of phosphoric acid.

Of these were assimilated 39·939 of fat and albuminates, consisting of 16·729 grammes of water; 16·667 of carbon; 2·457 of hydrogen; 0·809 of nitrogen; 2·964 of oxygen; 0·129 total of inorganic ingredients; 0·144 of sulphur; 0·071 of phosphoric acid.

The difference constituted the egesta, as urine, fæces, and expired carbonic acid and watery vapour. The amount of urea passed in twenty-four hours was 16·682 grammes; and the quantity of carbonic acid expired during the same time was 74·375 grammes.

The converse investigation, the "inanition experiment" was continued during nearly nineteen days, the animal having been kept without food for 442¼ hours, when death ensued; on seven occasions during this period, a little water was given. To estimate the metamorphosis of tissue which took place in the absence of external supply, the carbonic acid, urea, water, and fæces, excreted in a given time (every twenty-four hours), were compared with the animal's loss of weight in the same period, the nitrogen was reduced to its equivalent of fresh muscular substance, and the surplus of expired carbon calculated as fat. No difficulty attended the estimation of the carbonic acid contained in the pulmonary and cutaneous exhalations.

Deducting the weight of the fœtuses, as not properly belonging to the mother's body, the cat weighed at the commencement of the experiment 2·464 kilogrammes; on the day of its death, it had sunk to 1·267. During the first eight days the exhaled carbon was proportionate to the daily loss of weight:—i.e., an animal of equal weight would on each day have expired the same quantity of carbon. From the eighth to the sixteenth day, the loss of weight exceeded in proportion the diminution of the amount of exhaled carbon; during the last two days the diminution of carbon suddenly increased, so as to exceed in proportion the daily loss of weight. The renal secretion (urea, sulphuric acid, phosphoric acid, and salts) sank on the first three days rapidly, as compared with the integral loss of weight, then continued proportional to the latter until the sixteenth day, and on the last two fell, like the carbonic acid, with disproportionate rapidity. The hepatic secretion sank suddenly on the first two days, and subsequently it diminished slowly, but regularly. The sulphuric and phosphoric acids of the inorganic constituents of the urine maintained their relative proportion to each other; they increased considerably during the period of inanition, the urine becoming consequently more acid. The chlorides rapidly disappeared. At the commencement of the experiment, about 12 per cent. of the bile appeared in the fæces; the quantity regularly increased till, at the tenth day, all the bile was excretion.

The authors found that in every period of the inanition, the taking of water apparently increased the secretion of all the constituents of the urine, while it did not increase the quantity of carbonic acid expired; from which latter fact they argue that the apparent increase is not to be ascribed to a more abundant formation of urea, but merely to a greater mechanical facility of transudation through the kidneys of the ingredients of the urine already formed in the circulation; and elsewhere they even draw the conclusion, by comparing this with another experiment, *that a free use of water diminishes the metamorphosis of tissue.* Professor Bischoff, while he agrees that a freer ingestion of water increases the flow of urine, and with this the amount of urea voided by the kidneys, (which latter circumstance he attributes to the greater quantity of water removing the urea more rapidly from the body, according to the law of masses, and before it has time to be partially converted into carbonic acid and ammonia,) dissents from the latter view, observing, that if it were the case, a starving animal should drink largely to limit the metamorphosis, which is contrary to his experience, as his dog never drank less than during complete abstinence from food. He observes, however, that while the absolute quantity of urea is thus increased, its per-centage in the urine is diminished.

At all periods of the inanition, the loss of weight was much more considerable during the day than at night. This difference was, however, less during the last three days, in consequence of the animal becoming blind. The influence of the daylight having been thus removed, there remained only the frequent removal of the cat from the respiration-bell to the scales or urinary recipient, to accelerate the breathing. The loss of weight in eighteen days was 1264·8 grammes, of which 337·18 were anhydrous fat, blood, muscular and areolar matter, and consequently 927·62 were water.

The authors draw the following further conclusions from their experiments on inanition:

1. The daily loss of weight of the animal, excluding on the one hand the intestinal and renal excretions, and on the other, the water taken in, sinks constantly and regularly during the entire course of inanition. This is also true of the sum of the carbon, hydrogen, pre-formed water, and of the excess of oxygen of the metamorphosed fats and albuminates over the amount contained in the excretions from the bowels and kidneys, separated through the lungs, and, though in infinitely smaller quantity, through the skin.

2. The quantity of albuminates subjected, after separation of the urea, to oxidation, sinks very considerably in the first forty-eight hours (almost 50 per cent.), then continues fixed until the ninth day, sinks very slowly from the ninth to the sixteenth day, and falls with exceeding rapidity on the last two days.

3. The quantity of fat oxidised daily is nearly the same from the beginning to the end of the inanition.

4. The carbonic acid expired at first scarcely amounts to double, in the middle of the period it reaches two and one third times, at the end it is even three times that of the bodily substance subjected to metamorphosis.

5. The quantity of watery vapour expired sinks constantly to the close; rapidly on the first three days, then slowly and regularly to the thirteenth; finally, somewhat more rapidly, until the death of the animal.

6. The absorption of oxygen likewise diminishes until the close of the experiment: at first more rapidly, then slowly and regularly, and again somewhat more rapidly until death.

7. The quantity of oxygen combined in respiration, not again expelled as carbonic acid, but chiefly employed in oxidizing hydrogen, increases throughout the period, at first with more rapidity, subsequently more slowly.

8. The proportion of the separation of water in the fluid state by the bowels and kidneys, to that in the gaseous form through the lungs and skin, is constant to the death of the animal, and is on the average as 10 to 7.

9. The quantity of watery vapour daily expired sinks more quickly than that of the carbonic acid; and therefore diminishes, when referred to the latter as unity, at first more rapidly, then more slowly, but constantly, until three days before the death of the animal, when it appears to be again somewhat increased: without, however, attaining the original relative height.

During the last three days the pulse, respiration, and temperature of the body sank steadily and rapidly. The two former were, eighty-seven hours before death, respectively 160 and 28 in a minute; the latter, as estimated by a thermometer in the rectum, was 38° 4 C. (= 101·12 F.) Seven hours before death the pulse was 88, the respirations 16, the temperature 32° 4 C. (90° 32 F.)

From the authors' further experiments on temperature, it would appear that for two and a half hours next after death, the cooling of the body goes on much more slowly than would be in accordance with the laws of the normal radiation and communication of heat; but that after that time it corresponds to those laws. The authors give the results of other experiments connected with this series, which our space will not admit of our quoting.

In the following table we have the results of four series of experiments instituted under different conditions as to food, but otherwise under circumstances perfectly identical.

A full-grown cat, for each kilogramme of its weight in 24 hours.	Expired.		Excreted by kidneys and bowels.				
	Carbonic acid.	Water.	Water.	Urea.	Inorganic ingredients.	Sulphur.	Phosphoric acid.
Inanition, with full allowance of water	16·30	15·60	55·47	1·237	0·225	0·041	0·071
Normal animal food, without water	21·32	17·08	23·49	3·050	0·461	0·090	0·178
Normal animal food with water	20·32	15·36	50·59	2·958	0·441	0·086	Not determined.
The greatest possible amount of nourishment, with water *ad libitum*	34·88	14·70	67·72	5·152	0·760	0·149	

The period of perfectly uniform normal feeding with animal food, appeared to be particularly well adapted for determining the changes of the temperature of the body at various hours of the day and night. These were ascertained by introducing a thermometer into the intestine to the depth of 0·08 of a metre (rather more than three inches), due precautions being taken to prevent its expulsion. The heat of the body increased steadily from 7 A.M. to 10 P.M., and sank during the night as regularly, until 7 in the morning, when it reached its lowest point, the difference between the extremes being 1·3° C. (2·34° F.)

The loss of weight of the animal, excluding the urine and fæces, gives the simplest measure of the extent of the respiration, representing the sum of the expired water, carbon, hydrogen, and oxygen of the food, or of the substance of the body subjected to metamorphosis. The authors present us with tables exhibiting parallels of the processes of respiration, and of the development of heat, founded on these data.

In the second subdivision of this portion of their volume, the authors pass to the consideration of the *factors* of the metamorphosis of tissue, and enter first on the examination of the process of respiration. The respiration-capacity, like every other constituent element of the metamorphosis of tissue, is to be regarded as partly variable, according to the actual amount of food taken; and partly fixed, according to the species, age, and sex of the animal. The typical respiratory capacity of a given species is as constant and characteristic as its anatomical structure, and depends essentially on the animal's consumption of heat. This typical capacity is, in carnivorous animals, to be estimated from a fasting individual; in animals which do not bear abstinence so well, it must be decided by giving the typical minimum of nourishment—i. e., the quantity of assimilable matters by taking which the weight of the body remains for a time constant, but would fall if even a slight diminution were made in the daily rations. In a fasting animal the capacity must from day to day diminish in direct proportion to the decrease of the weight of the body, because so much less substance has to be raised to 38° ·5 C. (97·3° F.), and besides, the specific heat of this bodily substance itself diminishes daily, for the specific heat of the osseous system,—which takes scarcely any part in the metamorphosis, and the relative weight of which to the rest of the body is consequently always increasing,—is less than that of the other parts; a smaller absorption of oxygen, and a more limited oxidation of carbon and hydrogen, will therefore be required to maintain the normal temperature. But as the times of cooling are inversely as the volume of the body, three cases may arise—either the loss of heat by the more rapid cooling may be less than the gain by diminution of the specific heat; or they may balance one another; or the increase of loss may exceed the gain, and the necessary amount of oxidation may be raised, in proportion to the actual weight of the body, from day to day, and in the experiment shown in Table XII. the last was the case. But it would require much more extended data to admit of the deduction of a general rule in reference to this increase, and therefore the authors give a table of the diminished amounts of oxygen required at different degrees of diminution of weight, which may be used approximately for animals of the same form and heat at nearly equal temperatures of the surrounding atmosphere, and between which there is not too great variety of bodily weight.

The second factor, the influence of the quantity and quality of the nourishment on the capacity of respiration, is, in fasting animals, of course = 0; in purely carnivorous animals, as cats, it can be determined with accuracy: so much of the hydro-carbons which are deficient in oxygen (fats) as is not voided with the fæces in the form of soaps of lime and magnesia, is completely oxidized to form carbonic acid and water; almost all the nitrogen of the albuminates is, with the corresponding proportion of carbon, hydrogen, and oxygen, separated as urea; more than half the sulphur is given off in the urine as sulphuric acid, a very small portion as derivatives of the bile (cholic acid, taurin, &c.); the remainder, about 46·1 per cent. of carbon, 4·7 of hydrogen, and 13·7 of oxygen of the anhydrous albuminates oxidized and eliminated in the lungs as carbonic acid and water, serves to the production of heat. 100 grammes of albuminates are accordingly, (*a*) with reference to the amount of carbon alone, equivalent to 59 grammes of fat; (*b*) in reference to the quantity of oxygen required for perfect oxidation, to 50·4 grammes of fat; (*c*) in reference to the amount of heat developed by perfect oxidation to form carbonic acid and water (the true respiratory equivalent), supposing the oxygen present in the substance to be already combined with its equivalent of hydrogen (the correctness of which assumption is very doubtful), and therefore excluding it, they will be equal to 47·1 grammes of fat.

If we compare the quantity of food taken by a carnivorous animal with that required for the production of warmth, we find a doubly superfluous consumption, on the one hand by superfluous oxidation and development of warmth, which must be compensated by a corresponding increase of aqueous evaporation—i.e., abstraction of heat; on the other, by the separation of nearly one-eighth of the carbon, and more than one-third of the hydrogen of the albuminates, as urea. Of this formation of urea by far the smallest part is necessary for the existence of the animal; it is derived from the histologically-formed albuminates of the body themselves, which are subjected to the metamorphosis of tissue chiefly to remove the nitrogen united to the carbon, hydrogen, and oxygen remaining from the albuminates designed for expiration from the body, in a form not calculated to disarrange the functions of the economy. This superfluous consumption of carnivorous animals is less in proportion as their food is more abundant in fat; in herbivorous animals it is for the most part very small, because in them the albuminates taken up with a superabundant quantity of carbo-hydrates and hydro-carbons serve almost exclusively to replace the histologically-formed albuminates which have been necessarily subjected to metamorphosis.

The superfluous consumption of young animals using the largest possible amount of nourishment, is less than that of full-grown individuals of the same species; while their metamorphosis of tissue, referred to the weight of the body as unity, exceeds that of the latter. This fact is explained simply by the constant diminution of the relative typical warmth—i.e., the need of respiration, which attends increase of bodily weight.

The exact estimation of the quantity of fat which is subjected, in feeding on fatty matters, to oxidation, and which is finally expired as carbonic acid and water, is attended with difficulties, partly on account of its tedious absorption through the intestinal villi, partly on account of the expulsion of a portion with the fæces, either unchanged, or with the separation of the glycerin set free by the formation of soaps of lime and magnesia by the alkaline, pancreatic, and intestinal juices. The authors' experiments on this point make it probable, that during the exclusive feeding of purely carnivorous animals with fat, the histogenetic and histological structures of the body (the albuminates) themselves undergo the metamorphosis; while the fats which have been taken in, being previously wholly or for the most part deposited in the body, perhaps in consequence of the necessity of passing through a number of intermediate stages, are not oxidized until a later period.

"The process of respiration in purely herbivorous animals must differ essentially from that in the carnivorous in the proportion of oxygen taken in to form carbonic acid. While in the latter class a considerable part of the oxygen inspired is employed in oxidizing the free hydrogen of the food, the carbo-hydrates, as the chief representatives of vegetable aliment, contain an amount of oxygen exactly sufficient to oxidize the hydrogen, and the pectin group, and the most widely diffused vegetable acids (the oxalic, tartaric, citric, and malic) even contain this principle in excess. The result of experiments on respiration, therefore, supposing the constitution of the food to be known, may be with certainty determined beforehand. An herbivorous animal living on vegetables abounding in starch will, in normal or excessive feeding, completely separate again as carbonic acid the oxygen he has taken in in breathing; if the food abound in vegetable acids, more oxygen will be expelled in the carbonic acid than has been taken in in the same time; if it be rich in albuminates, more oxygen will be combined than is contained in the expired carbonic acid; and this will be still more the case if the food contain a large quantity of vegetable fats (oily seeds, flax seed, rape, &c.). This state of things is but little altered by the typical conversion of the albuminates in full feeding; whilst the typical respiration process of the fasting herbivorous animal, who creates the warmth required for the performance of his organic functions by the oxidation of the substance of his body, will, as in the case of fasting or normally fed carnivorous animals, differ from that of the abundantly fed only in the amount of the superfluous consumption, while, qualitatively, the processes will be identical."

To make what has been said clear, the authors subjoin the following table·

Material.	Carbon.	Hydrogen.	Oxygen.	To form carbonic acid and water, there is still required, in addition to what is present, of oxygen,	Of 100 grammes of oxygen taken in, the expired carbonic acid contains
100 grammes albuminates and gelatinous matters	46·10	4·72	13·66	147·04	83·60
100 ditto fat	78·13	11·74	10·13	292·14	71·32
100 ditto starch	44·45	6·17	49·38	118·52	100·00

"The cerealia contain with starch disproportionably more albuminates than vegetable acids; consequently, a part of the oxygen absorbed will be employed in the oxidation of hydrogen, the quantity of oxygen absorbed in respiration will somewhat exceed that again expelled in the expired carbonic acid; the same must, for a similar reason, hold good in the case of fresh roots and bulbs (carrots, turnips, potatoes, &c.)."

The authors next proceed to ascertain experimentally the effect of the exclusion of bile on the metamorphosis of tissue. In the first part of their work they discussed the question how far the carbon and hydrogen serving to maintain the animal heat passes through the intermediate step of the formation of bile. It then appeared, that in the typical metamorphosis (that of a fasting animal) but a small part (about 5 per cent.) of the expired carbon passes through this stage, and that the proportion in normal feeding with animal food is not essentially increased. But the exclusion of bile has a vast effect upon the metamorphosis of tissue by greatly diminishing the amount of fat taken up from the intestinal tube, and also by the great development of gas from the large intestine to which it gives rise, by which a certain loss of material adapted for respiration (sulphur and carburetted hydrogen) takes place; while the greatest part of the bile, which in the normal condition is again taken up by intestinal resorption and is completely oxidized to form carbonic acid and water, is of course likewise withdrawn from contributing to the production of heat. In order, therefore, to cover this triple loss, there must be a corresponding increase of the amount of food taken; if this be not supplied, the substance of the body itself will yield the necessary material; the calorigenetic deposit of fat will be consumed; the histogenetic and histologically-formed structures of the body will disappear, as in a starving animal, in proportion as they can be best spared from ministering to the most important of the vital functions.

If the increased quantity of food required be unlimitedly supplied (overfeeding), three cases are possible:—(*a*) This increased quantity will exceed the animal's power of assimilation, and it will perish; (*b*) both will be equal, in which case the typical metamorphosis will be maintained; (*c*) the power of assimilation will exceed the necessary consumption, and, with the typical, superfluous consumption will take place. In the authors' experiments the last occurred: thus a dog in which a biliary fistula was established, and which was fed on meat and butter without any compensating increase, diminished from 5·8 kilogrammes on the 22nd October to 3·418 on the 24th November, the day of his death; while another dog, likewise furnished with a fistula and made the subject of experiments from the 15th February to the 11th April, increased in weight by 423 grammes in eight days, during which he was abundantly supplied with meat.

In the former of these animals, about 20 per cent. more of carbonic acid was expired than is the case in typical respiration. This is explained by the decomposition of the fats necessary for the formation of bile, one product of which (fatty acid = cholic acid) is separated by the liver only in a partially oxidized form, while the other (glycerin) is subjected as a material of superfluous respiration to oxidation. If the bile arises from the decomposition of the albuminates alone, whose absorption is not impaired by the deviation of the former from the intestinal canal, an increase of them equivalent to the quantity of the abstracted bile only would be required to maintain the normal weight of the animal. The typical demand for respiration may be met by the equivalent quantity of these substances after the separation of urea, as well as by fats. If, on the other hand, the bile arises from the simultaneous decomposition of the albuminates and the fats, so that products of decomposition of both (glycocoll, taurin of the former, oxidized fatty acids = cholic acid of the latter) combine to form the united acids of the bile, the deficiency of fats cannot be supplied by the albuminates present—the entire quantity necessary for the formation of bile must be supplied from without, or, if this be not possible, from the adipose tissues of the body until the latter are exhausted, and the further formation of bile necessarily arrested. While pure fat is, when the bile is excluded, taken up from the intestinal tube only in very small quantities, it may—when so finely mixed with much flesh that, no longer visible to the naked eye, its presence can only be demonstrated through the agency of ether—reach the circulation in quantities corresponding to the amount of albuminates in the food; if, then, the quantity of meat given and digested be very considerable, the amount of fat introduced may be wholly, or, at least, nearly equal to that required for the formation of bile: and that this is the case appears from the authors' subsequent experiments.

By the constant discharge of bile outwardly, a simultaneous loss of soda occurs, which, if not replaced, produces an essential disturbance in the system at large. The acid arriving from the stomach in the intestine, if not neutralized by the soda of the bile, will combine with the weakly-united alkali of the pancreatic and intestinal juices and prevent them discharging their functions; the small portion of fat, the passage of which is normally accomplished by means of the alkalescence of these fluids, remains unabsorbed, and the disturbed equilibrium between the acids and bases in the body can only be restored by means of the separation of an equivalent quantity of the former by the urine, and a correspondingly strong acid reaction of this fluid, unless this reaction be counteracted by a sufficient formation of ammonia.

In one of the authors' experiments, 0·247 grammes of soda were eliminated with the bile. To replace this the consumption of 110·4 grammes of meat would be necessary, a quantity which is raised to about 155 grammes by the coincident occurrence in the hepatic secretion of chloride and phosphate of soda. This fluid, in both fasting and fed animals, contains but very small quantities of potash; and besides, a substitution of one base for the other cannot take place in the formation of bile, as soda is incapable of being replaced in this secretion by any other base.

In two series of observations on a dog with biliary fistula, and supplied with abundant food, the consumption of oxygen and the formation of carbonic acid far exceeded the typical respiratory requirement.

The next great agent in the metamorphosis of tissue brought under our notice is the formation of urine, which appears as a function of two magnitudes—one variable, depending on the quantity of histo-plastic (albuminous) food assimilated by the normally, insufficiently, or superfluously fed animal; the other constant, depending on the necessary consumption of albuminates of the fasting animal. The former can be estimated directly by the elementary analysis of the food; the latter conversely, by ascertaining the total amount of nitrogen in the excretions.

Of this process we have in carnivorous animals, whether well fed or fasting, the simplest form. Almost all the nitrogen of the compounds of albumen and gelatinous matters separates in order to form, with the necessary equivalents of carbon, hydrogen, and oxygen, the complex atom urea; while the remainder of the latter elements, amounting to about five-sixths of the whole quantity of warmth-creating material, is subjected to oxidation, with the formation of carbonic acid and water, and, after fulfilling its calorimetric functions, is separated by the lungs in the gaseous form. The quantity of bodily substance which undergoes metamorphosis is in this instance directly proportional to that of the excreted urea: for every 100 grammes of the former in the anhydrous condition, 32·1 grammes of urea appear in the renal secretion; or, conversely, 100 grammes of excreted urea correspond to at least 311·1 grammes of anhydrous bodily substance. The excretion of nitrogen in other ways is exceedingly small; actual expiration of this gas scarcely takes place, or only as an unessential and secondary consequence of decomposition.

Circumstances are quite different with herbivorous animals, whose multifarious nourishment comprises, besides the more easily oxidizable unazotized compounds (as sugar, gum, starch, most vegetable acids, &c.), such as pass through the circulation more or less unaltered, whether in the isolated state or united to intermediate products of metamorphosis. Oxalic acid owes its frequent occurrence in the urine to vegetable food. The authors never found a trace of it under the most different circumstances in the urine of the pure carnivora. Its wide diffusion in the vegetable kingdom sufficiently explains its occurrence in the urine of herbivorous animals; it is more difficult to understand the conditions under which it is completely oxidized to form carbonic acid. That this takes place is proved by its frequent absence from the urine after the free use of vegetables abounding in it, as, for example, sorrel; while again, it can, in other cases, be plainly discovered in the urine in apparently undisturbed health, although not in quantity equal to the amount ingested.

Uric acid is undoubtedly formed from albuminous or gelatinous matters, for it is found in large quantity in the urine of birds of prey fed on meat alone. Its absence from the urine of many carnivorous mammalia is striking, especially from that of the cat family. No trace of it is to be found under any circumstances in the urine of cats or dogs, while in man, animal diet increases its quantity. Neither can creatine or creatinine be discovered in the urine of those animals.

The subjoined schemes show the possibility of the formation of urea from either pure albumen or pure gelatine, without the absorption of additional oxygen, the intermediate stages of metamorphosis being omitted.

A. Albumen.	Carbon.	Hydrogen.	Nitrogen.	Oxygen.	Sulphur.
100 parts of albumen contain ...	53·5	7·0	15·5	22·4	1·6
33·2 — urea — ...	6·6	2·2	15·5	8·9	—
66·8 parts remain for the formation of bile and for respiration ...	46·9	4·8	—	13·5	1·6
B. Gelatine.					
100 parts of gelatine contain ...	50·5	6·8	18·3	24·4	—
39·2 — urea — ...	7·8	2·6	18·3	10·5	—
60·8 parts remain for the formation of bile and for respiration ...	42·7	4·2	—	13·9	—

It is not likely that taurin and glycocoll, the intermediate products of separation and oxidation which albuminous and gelatinous matters contribute to the formation of bile, are to be regarded as stages in the production of urea, but rather as corresponding modes of decomposition in a different direction, since, when reduced to an equal amount of nitrogen, they contain more oxygen; but an oxidation and subsequent reduction is an inadmissible hypothesis. It is more probable that after previous oxidation they enter as intermediate points of chemical changes into the histoplastic complex atom, as they contain the elements of urea and a carbohydrate, as sugar, lactic acid, &c.

Creatine, creatinine, and uric acid cannot be regarded as intermediate steps in the formation of urea, as they themselves traverse the renal capillaries undecomposed.

In the present uncertain state of our knowledge as to the true constitution of the albuminates, we cannot hope to establish a well-founded theory of the formation of urea.

The authors subjoin some experiments, from which it appears that whether in the normal, insufficient, or superabundant use of animal food, nearly the entire quantity of nitrogen contained in it, and consequently the quantity of urea equivalent to a given weight of histo-plastic material, is separated by the kidneys. The animal heat is maintained chiefly by oxidation of the adipose deposits of the body, for a comparison of the carbonic acid exhaled from the lungs, and of the urea excreted in the urine, in equal times, reduced to an equal amount of nitrogen, shows a much greater proportion of carbon than is present in the albuminates. This excess can only be derived from oxidation of the fats, a conclusion which is verified by the disappearance of fat from all the organs of animals subjected to inanition.

The last subdivision of this second part of the elaborate volume we have now in our two articles studied as fully as our limited space would admit of, is devoted to the consideration of the movement of matter in the animal body, viewed under the heads of the circulation of diffusion, and the circulation of metamorphosis. To the former category belong the circulation of the pre-formed water and of the undecomposed diffusible salts, to the latter that of the carbon, hydrogen, nitrogen, oxygen, sulphur, phosphoric acid, chlorine, alkalies, alkaline earths, and iron. As this portion of the work, however, consists almost entirely of a series of tables, we must recommend the reader who is anxious for information on the subject to inspect them for himself, as we would refer him to the portion of the work we have gone through for many important points which we have unavoidably omitted. These tables exhibit, among other things, a view of the quantities of each element voided by the lungs, kidneys, and bowels, compared with those circulating intermediately in the bile, saliva, gastric, pancreatic, and intestinal juices. Appended to the volume are some most beautiful charts, executed by Dr. Schmidt, affording synoptical *resumés* of several of the most important subjects treated of in the body of the work.

We have already had occasion to mention some of the opinions held by Professor Bischoff in reference to the subject of which we have been treating; it will therefore now be sufficient to place the reader in possession of the principal results of his experiments, and of the conclusions he has drawn from his observations. In doing this we shall, as far as possible, avoid returning to the points we have already spoken of. We may observe, that the experiments on animals are extremely numerous, and rival, in minuteness and extent, those of Bidder and Schmidt. The most elaborate calculations are based upon them, but these it would be impossible to give without occupying the whole of our review with tables.

In the first place, Bischoff holds that urea is the azotized product of the metamorphosis of the nitrogenous parts of the body, and that it never is a product of the direct metamorphosis of the albuminous constituents of the blood. Gelatine, however, differs from all other nitrogenous aliments, for it is directly changed in the blood into urea. Still it never is a natural aliment, nor is it ever found as a normal ingredient of the blood. The amount of urea is greatly influenced by the nature of the nourishment, being greatly increased when the aliment is abundant and highly nitrogenous. The limit to this increase, in fact, appears to be the power of the individual to dissolve and digest nitrogenous food. The amount of nitrogen which escapes otherwise than by the kidneys (we beg the reader to refer to page 387 for Bischoff's opinions on this point) does not become greater in consequence of the increase of azotized aliment, while it *is lessened by the use of non-nitrogenous food* (fat), a fact of the highest importance, if it be confirmed.

Non-nitrogenous aliment, especially fat, in all cases limits the metamorphosis of the nitrogenous parts of the body; but a difference exists in reference to the separation of nitrogen in the form of urea according to other coexistent circumstances.

In *starvation*, fat diminishes both the metamorphosis and the quantity of urea.

In cases where nitrogenous food is given in quantity *exactly sufficient* to maintain the weight of the body, *fat likewise limits the metamorphosis; the body increases in nitrogenous elements, which does not take place where the same food is given without fat:* but the amount of urea increases, because the excretion of nitrogen in any other form is diminished.

In *superabundant* nitrogenous aliment, *fat also favours the deposition and limits the metamorphosis of nitrogenous elements;* but, at the same time, the quantity of urea is less than it would be with the same diet without fat, because the factor of the limitation of the excretion of nitrogen in any other form is, in this case, too insignificant to produce a perceptible increase of the quantity of urea, which is, in spite of the restriction of metamorphosis in general, considerable, although the fat in this case, also, undoubtedly exercises the same influence. The effect, therefore, of the use of fat as an article of diet will be to increase the weight of the animal consuming it.

For the next inference we shall allude to, we must confess, the title of Professor Bischoff's work did not prepare us, although his expressed opinions have done so;—it is, that urea cannot as yet be used as a direct measure of the metamorphosis of tissue. Before this point can be attained, it will be necessary, he observes, to determine the quantity of nitrogen which escapes in other forms, as well as the influence exercised by certain aliments and by water on its evolution both in these modes and in urea. This view is, he points out, opposed to that held by Schmidt, who, as we have seen, maintains that the quantity of the nitrogenous substance of the body subjected to metamorphosis is directly proportional to the amount of the excreted urea; and that all the nitrogen contained in the food is separated by the kidneys in this form. Professor Bischoff shows that the former of these two opinions of Schmidt is entirely opposed to that of the direct transition of a portion of the albuminates of the blood into urea; and considers that much remains to be done before such a *coup d'œil* of the complicated vital processes as Professor Schmidt has attempted, can be obtained.

The author next devotes some pages to a defence of Liebig's theory of food, in which he shows that it was never intended to be absolute or exclusive, and that Liebig never meant to assert that in the animal body any substance plays an exclusive part. Want of space, however, forbids our entering on this topic; we shall therefore conclude our notice of Professor Bischoff's volume by quoting one or two points of practical importance.

Fat given alone appeared to be less capable of digestion than when it was consumed with other matters: thus, a dog perfectly digested a mixture of one pound of meat, one pound of potatoes, and half a pound of fat, in twenty-four hours; while half a pound of fat eaten alone was not digested.

Common salt was found to increase the amount of urea in the urine, an effect which was due to augmented metamorphosis, as was proved by a diminution of the animal's weight.

Concentrated azotized food not only did not increase the absolute amount of urea in the urine of herbivorous animals, but diminished it; while the per-centage increased equally with the concentration of the food, which seems to be another instance of the great influence of water on this excretion.

Starch in bread appeared to limit the general metamorphosis of the nitrogenous parts of the body.

Potatoes seemed to be badly adapted to form flesh or impart strength, but to be well suited to favour the formation of fat. The condition of the Irish peasantry, who formerly were strong and muscular on a diet which, during great part of the year, consisted exclusively of potatoes, appears to contradict this deduction.

The experiments in the third work we have before us are almost as numerous as those of Bidder and Schmidt, and are as carefully recorded. The whole strength of the well-known laboratory at Rothamstead has apparently been put upon them; and Mr. Lawes' position as a practical agriculturist has enabled him to extend his observations on the feeding of animals to a greater extent than any other observer in this country could have done.

Most writers and experimenters on the subject of the chemistry of the food, observe Messrs. Lawes and Gilbert, agree on two main points—viz., the connexion of the nitrogenous constituents of the food with the formation in the animal body of compounds containing nitrogen, and with the exercise of force; and, on the other hand, the general relationship of its non-nitrogenous constituents with respiration, and with the deposition of animal fat; but as it has been generally assumed that our current food-stuffs are measurable rather by their flesh-forming than by their more specially respiratory and fat-forming capacities, the per-centage of nitrogen has for the most part been taken as the standard of nutrition. This opinion appears, from their experiments, to be erroneous.

The first question to which the authors call attention is,

> "Whether, in the use of our current foods, under ordinary circumstances, the amount consumed, and that of increase produced, have a closer relationship to the supplies in such foods of the nitrogenous or of the non-nitrogenous constituents?"

Of the experiments tried in reference to this and other questions,

> "Sheep and pigs," they inform us, "have been the subjects; and the general plan has been to select several different descriptions of food, containing respectively various amounts of nitrogenous and non-nitrogenous constituents, the proportions of which were ascertained by analysis. To one or more sets of animals to be compared, a fixed and limited amount of food, of a high or low per-centage of nitrogen as the case might be (as, for example, oil-cake, linseed, barley, malt, oats, clover, chaff, &c.) was allotted, and they were then allowed to take *ad libitum* of another or complementary food (as Swedish turnips, clover, chaff, mangold-wurtzel, bean meal, bran, Indian meal, &c.). In this way, in obedience to the instinctive demands of the system, the animals were enabled to fix for themselves, according to the composition of the respective foods, the quantities of each class of constituents which they required."

The authors present us with tables, showing—1st. The amounts, respectively, of the nitrogenous and the non-nitrogenous constituents consumed weekly per 100 lbs. live weight of animal; 2ndly. The amounts consumed of each of these classes of constituents to produce 100 lbs. increase in live weight. In the former, the mean weights are taken for calculation—i. e., those obtained by adding together the weights at the commencement and at the conclusion of the experiment, and dividing by 2. The quantity of nitrogenous substances consumed is obtained by multiplying the amount of nitrogen by 6·3, on the assumption that they all exist as protein compounds. This mode, though frequently far from accurate, and especially when applied to succulent vegetable substances, is considered by the authors to be sufficient for their purpose.

Our limits will, of course, only admit of our dealing with the results.

The first proposition deduced by the authors from their experiments is,

"That, in all comparable cases, there is much more of uniformity of amount in the total of *non*-nitrogenous than in that of nitrogenous substance, both as to the quantities consumed to a given weight of animal within a given time, and to those required to produce a given weight of increase. With this general uniformity as to the amount of *non*-nitrogenous substance consumed under given circumstances, or for a given result, those of the *nitrogenous* constituents are found to vary, under the same circumstances, in the proportion of from 1 to 2 or 3."

The experiments with sheep showed that "the amount of food consumed would seem to be regulated by the quantities which it supplied of the non-nitrogenous, rather than by those of the nitrogenous constituents." In the case of pigs, too, it appeared to be the non-nitrogenous, rather than the nitrogenous, which fixed the limits to consumption.

The second question considered by the authors is the relationship of the increase in live weight produced, to the consumption of nitrogenous and non-nitrogenous constituents in the food. In reference to this point, it appeared that the amounts of available non-nitrogenous substance consumed to produce a given weight of increase, are at any rate much more nearly uniform than are those of the nitrogenous constituents. It was further found, in comparable cases, that a less amount of the non-nitrogenous constituents which contained more oil, was required to produce the same resulting increase in the animal, than of those in which the starch series of compounds was predominant; and again, less of the starchy series was required than of some of the peculiar products of the root crops. These results are highly interesting, and would seem to corroborate Professor Bischoff's observations, as to the effect of fat and starch in limiting metamorphosis.

The per-centage of nitrogen appeared to be wholly inapplicable as a measure of feeding value: as, in fact, did any analytical method, unless a detailed determination of the proximate compounds, when succulent products, as roots, were the subjects of the experiment. Indeed, there is reason to believe, that in succulent produce an unusually high per-centage of nitrogen is frequently a pretty sure indication of immaturity and innutritious principles. On the whole, the experiments with sheep proved,

"That beyond a limit, below which few, if any, of our current food stuffs are found to go, it is their available non-nitrogenous constituents, rather than their richness in the nitrogenous ones, that measure both the amount consumed to a given weight of animal, within a given time, and the increase in weight obtained."

Similar results were obtained with pigs. The following experiment was undertaken to ascertain the composition of the increase. Two pigs were selected as nearly as possible similar. One was killed at once, and its composition determined; the other was killed after eight weeks' fattening on weighed quantities of food, and subjected to the same method of preparation and analysis as the former one; it was thus found that about eight-ninths of the entire dry increase was pure fat. Other animals, however, as fed for the butcher, will generally contain more flesh and less fat than the pig.

"The fact that fat is in so much a larger proportion than lean in the animals fed for the butcher, would seem not only to be consistent with the results of our experiments, as to the great influence of the non-nitrogenous constituents of the food of these animals in the production of increase during the fattening process: but it indicates also the predominance of this non-nitrogenous character in that description of human food (butcher's meat), which is generally spoken of as the most nitrogenous, and therefore the most nutritive."

The authors found, in a further experiment, that there was nearly four times as much fat stored up in the animal, the subject of the experiment, as there was of fatty matter ready formed in the food; showing that a considerable formation of fat took place in the animal's body, and exemplifying Professor Bischoff's remark as to the suitability of potatoes for promoting the development of fat. They believe the formation of fat in the animal body, even to a considerable and practically important extent, and most probably from the starch series of compounds, to be now clearly proved.

The experiments of Messrs. Lawes and Gilbert, to which we have thus briefly alluded, will be found to be practically important and interesting. Of their concluding remarks we may quote the following:

"A somewhat concentrated supply of nitrogen does, however, in some cases, seem to be required when the system is over taxed; as, for instance, when day by day more labour is demanded of the animal body than it is competent, without deterioration, to keep up; and perhaps also, in the human body, when under excitement or excessive mental exercise. It must be remembered, however, that it is in butcher's meat, to which is attributed such high flesh-forming capacity, that we have also, in the fat which it contains, a large proportion of respiratory material of the most concentrated kind. It is found, too, that of the dry substance of the egg, forty per cent. is pure fat."

"In conclusion, it must by no means be understood that we would in any way depreciate the value of even a somewhat liberal amount of nitrogen in food. We believe, however, that on the current views too high a relative importance is attached to it; and that it would conduce to farther progress in this most important field of inquiry, if the prevailing opinions on the subject were somewhat modified."

We must now conclude our notice of the present state of the "Chemistry of Digestion;" and while in doing so we congratulate ourselves on the progress made during the last few years in a knowledge of the subject, and on the advances it is likely to receive at the hands of the able, zealous, and untiring investigators, whose works we have been reviewing, we feel that the present is a fitting occasion to bring before our readers the caution given by Professor Lehmann, that in connexion with the process of digestion, in which we think we have to do with the most direct effects of chemical action, we must not be surprised to find the very point which may to-day appear to be established by the most direct experiments and the most accurate observations, rendered doubtful to-morrow by the results of other experiments and other observations; a circumstance which should, he continues, more than anything else, lead to diffidence in the enunciation of our opinions, even though the latter should be based upon what seem to be the most exact investigations. In an equally philosophic spirit does this writer deprecate the selfish obstinacy with which, in spite of contravening facts, and in contempt of all true science, some attempt, often with the shallowest arguments, to defend opinions once expressed. Professor Lehmann, like all who have learned much, evidently feels that what is known, bears, and probably must ever bear, but a small proportion to what is unknown: that "our knowledge is piecework;" still (to apply to physiological, the sentiments he expresses with more especial reference to pathological, chemistry), he does not despair for the future of his favourite science; but, knowing that her watchword must ever be "forward," he adopts as her emblem the opening season of hope and progress, and concludes his elaborate volumes in the words of the poet,

"Und es muss doch Frühling werden."

William D. Moore.

Review VI.

Statistical Reports on the Sickness, Mortality, and Invaliding among the Troops serving in the United Kingdom, Mediterranean, and British America. Compiled by Lieut.-Col. Tulloch & Dr. Graham Balfour, and presented to Parliament.—1853. Folio, pp. 259.

The Statistical Reports on the health of the army have been so frequently noticed in this journal, and are so well known to the profession, that it seems hardly necessary to refer to the history of their origin. In 1835, Lord Howick, then Secretary at War, instructed Mr. Marshall, Deputy Inspector General of Hospitals, and Captain Tulloch, 45th regiment, now Lieut.-Colonel, and Military Superintendent of Pensioners, to prepare from the records of the army medical department and the War-office returns, a report on the health of the troops in the West Indies. In the following year Mr. Marshall retired from the active labours of the investigation, but continued to render his advice and assistance in it. His place at the War-office was supplied by Dr. Balfour, of the Guards, now of the Royal Military Asylum at Chelsea, who continued to assist Captain Tulloch in the preparation of four volumes, which were presented to Parliament between the years 1838 and 1841, and comprised the sanitary details of the troops in all the British colonies and dependencies, except India.

After a lapse of eleven or twelve years another volume of Reports has been presented to Parliament, prepared by the same parties, with the exception of Mr. Marshall, of whose valuable aid they have been deprived by death. It is the first of a new series, in continuation of those formerly published, and comprises the sanitary details of the army serving in the United Kingdom, the Mediterranean, and British America, during the ten years from April, 1837, to April, 1847. We shall endeavour in the following pages to lay before our readers a comprehensive summary of the leading facts contained in the Report.

Before entering upon the health details of the troops in the United Kingdom, a brief sketch is given of the measures which have been adopted, since the date of the former Reports, with a view to improve the condition of the army. These have consisted chiefly in providing additional means of active amusement and occupation by the erection of ball and racket courts, and the formation of cricket grounds, in the vicinity of the principal barracks; the establishment of barrack libraries as a counter-attraction to the canteen; improved ventilation, and increased space for the men in barracks, with a more liberal provision of the means of ablution; the reduction in the amount of corporal punishment; the establishment of military prisons and of barrack cells, to supersede the necessity of sending the military offender to herd with criminals in the civil jail; and the improvement of the soldiers' diet, by the general introduction of an evening meal. To these may be added the promulgation of the Good-conduct Warrant, which made the increase of pay to depend upon continuous good conduct, instead of mere length of service; the establishment of Regimental Savings' Banks, where the soldier can deposit in safety any small amount he may be able to save or earn, and which previously was too often squandered on intemperance; and the increased facilities afforded to men, when tired or discontented with the service, to purchase their discharge. At a period subsequent to the date to which this Report extends, two very important measures were carried out, for which the highest praise is due to their author, Lord Panmure, then Secretary at War—the introduction of limited enlistment into the army instead of service for life, and the abolition of the sale of spirits in the barrack canteens.

Such are the principal ameliorations which have taken place during the last fifteen years in the condition of the soldiers, and which have doubtless exerted an important influence on their health. There yet remains, however, much to be done, and we trust the authorities will not rest content with what has been accomplished. An improvement in the quality of the education of the soldier, considerable modifications in his dress, and the introduction of greater variety in his diet, and especially in the cooking of it, are desiderata well worthy the attention of the military reformer.

The troops serving in the United Kingdom may be divided into four classes. First, the Household Cavalry, whose service, in time of peace at least, is confined to London and Windsor. Second, the Dragoon Guards, and Dragoons, who are quartered throughout Great Britain and Ireland. Third, the Foot Guards, whose duties are chiefly confined to the metropolis, with one battalion at Windsor, and one in country quarters; and fourth, the Infantry of the Line, scattered throughout the kingdom, but chiefly in the larger towns. From the great number of regiments of the Line required for colonial service, it is obvious that a large proportion of the men in them must have served abroad, where they have probably been exposed to those morbific agencies with which tropical climates especially abound, and are likely, from the liability to disease thus contracted, to raise the admissions into hospital above the average, after the return of the corps to this country. With a view to get rid of this source of error, no regiment has been included in the Report until six months had elapsed from the date of its landing at home, during which time most of the men whose constitutions had been impaired by colonial service would probably be discharged and replaced by healthy recruits. With this explanation, we submit the following summary, showing the proportion of admissions into hospital and deaths in 1000 of each of these classes of troops, by the principal diseases, during the ten years reported upon; and annexed to it, for the purpose of comparison, is the mortality of a town population at the same period of life as the military, compiled from the Registrar General's Reports.

—	Annual ratio per 1000 of mean strength.								Town population: deaths of males betw. ages 20 & 40.
	Household Cavalry.		Dragoon Guards and Dragoons.		Foot Guards.		Infantry of the Line.		
Aggregate strength	12,083		54,374		40,120		160,103		
	Admit.	Died.	Admit.	Died.	Admit.	Died.	Admit.	Died.	
Fevers	Not stated.	1·4	53	1·4	78	2·4	73	2·5	1·2
Eruptive fevers		·15	5	·1	8	·3	7	·4	·3
Diseases of the lungs		6·55	152	7·3	161	13·8	171	10·2	6·3
Diseases of the liver		·25	6	·3	6	·2	8	·4	·2
Diseases of the stomach & bowels		·4	78	·4	71	·5	63	·8	·4
Diseases of the brain		·6	8	·8	7	·6	7	·8	·6
Dropsies		·1	1	·5	1	·3	2	·3	·4
Rheumatic affections			54		39		54		
Venereal affections			206		250		277		
Abscesses and ulcers			165		91		124		
Wounds and injuries		1·5	124	1·6	56	1·7	58	1·4	1·2
Corporal punishment			3		1		5		
Diseases of the eyes			18		15		48		
Diseases of the skin			37		33		95		
All other diseases			51		45		52		
Suicide, accidents, violence, &c.		·15	...	1·2	...	·6	...	1·1	1·3
Total	...	11·1	961	13·6	862	20·4	1044	17·9	11·9

As regards the amount of sickness in each class of troops, it will be seen that it is lowest among the Foot Guards; but the difference will be found chiefly in those diseases which are not likely to give rise to much mortality. Thus, the excess in the Cavalry is principally in abscesses and ulcers, and wounds and injuries to which they are more exposed from the nature of their duties than the Infantry; while in the regiments of the Line it is from venereal, abscesses and ulcers, and diseases of the skin, the latter doubtless arising from the larger proportion of recruits in them, who

"Being often taken from the lowest class of the population, and subject to much privation and the want of personal cleanliness prior to enlistment, are frequently found to be affected with itch on joining the regiment; the instances in which it occurs among older soldiers are comparatively few."

The mortality is lowest in the Household Cavalry, and highest in the Foot Guards. Among the former, indeed, it is lower than among a town population at the same period of life. This is attributed by the reporters to the advantages enjoyed by the Life Guards of superior pay, more extensive barrack accommodation, and a considerable exemption from exposure on night duty. There is also a much greater facility in these regiments for discharging men of short service, when they begin to exhibit traces of constitutional disease, and for getting rid of men who are injuring their health or bringing discredit on the corps by dissipation and irregular conduct. The men thus discharged have averaged nearly 2 per cent. of the strength annually.

"Such a weeding of the corps cannot fail to have a very beneficial effect both on their moral and physical condition, and, if practicable, would be of vast benefit also in other branches of the service; but dismissal, which in this force would be a severe punishment, might, in the Line, be looked upon as a boon. The pay and other advantages in the former being considerably above the income of the labouring classes, no difficulty is experienced in filling up vacancies; but it is not so with the rest of the army, owing to the lower rate of pay, the less attractive character of the service, their prolonged absence on foreign stations, and the risk of life and health which such service usually involves."

The high rate of mortality among the Foot Guards is chiefly from disease of the lungs,

"The cause of which must, we believe, be sought rather in a combination of circumstances than in one alone. The most prominent among these are—defective barrack accommodation; the great amount of night duty; the deteriorating influence of residence in a large town; and the greater facilities for and temptations to dissipation."

This view is corroborated by the circumstance, that when a regiment of the Line was quartered in the Tower of London in 1838–9, to replace one of the battalions of Guards sent to Canada, the mortality in it was as high as $27\frac{8}{10}$ per 1000 of the strength, being considerably above the usual loss of the Foot Guards in the same quarters. At the same time the deaths among the Foot Guards in Canada were fewer, in the proportion of 14·5 to 16·5, than in the regiments of the Line serving during the same period in the same colony. Great improvements have, within the last few years, taken place in the barracks in the Tower and Hyde Park, which it is anticipated will exert a favourable influence on the health of the troops. Indeed, we understand that since the new barracks in the Tower were occupied, the men have not suffered from those epidemics of severe typhus to which they were formerly subject.

We shall now notice a few of the more striking points connected with the different classes of diseases.

Fevers.—The mortality by fever has been identical in the Life Guards and Cavalry of the line, and in the Foot Guards and Infantry of the line; in the former amounting to 1·4, and in the latter to 2·4 and 2·5 per 1000 annually. The excess in the Infantry, compared with the Cavalry, depends probably on their being more crowded in barracks, quartered chiefly in large towns, and more exposed on night duty. Intermittents have been scarcely known, and the few cases that have occurred were chiefly in men who had suffered from that form of disease in the colonies. Fevers of the continued type caused twice as great a mortality among the regiments of the line as in the Cavalry or Foot Guards; while the latter force suffered to a much greater extent from typhus than the others. This appears chiefly to have been attributable to local causes connected with their quarters in the Tower of London, where that disease prevailed epidemically among them seven times in ten years. Happily these have now been removed or greatly ameliorated. The proportion of deaths to cases of typhus was nearly the same in all, having been 1 in $3\frac{1}{4}$ among the Cavalry, 1 in $3\frac{1}{2}$ among the Foot Guards, and 1 in 4 in the regiments of the line.

Diseases of the Lungs.—This class of diseases has been most fatal in the Foot Guards, the ratio of deaths having been double that of the Household Cavalry, and a third above that of the regiments of the line. The difference lies entirely in the chronic affections, the proportion from pneumonia, pleurisy, and acute catarrh, having been exactly the same in the Foot Guards and Infantry. The extremely fatal character of phthisis is illustrated by the statement, that in the Cavalry 307 cases were traced to a fatal termination out of 386 admitted; in the Foot Guards 479 out of 654; and in the regiments of the line 1241 out of 1657; the remainder must not be classed as recoveries, many of them having probably been discharged as invalids, and died on the pension list, but the reporters had not the means of ascertaining to what extent this was the case.

Diseases of the Stomach and Bowels.—These prevailed to nearly the same extent in all classes of troops, but the mortality was double in the regiments of the line, a result doubtless attributable to former colonial service.

Diseases of the Brain also present a striking similarity, both as regards admissions and deaths. If one may judge from the relative prevalence of *delirium tremens*, the Cavalry are the most dissipated, and the Foot Guards the least given to intemperance; but the reporters explain that the apparent difference may probably arise from spirits being the favourite drink of the regiments serving in Scotland and Ireland, while beer is the ordinary beverage of the Guards.

Rheumatic Affections.—This is a class which must be looked upon with considerable suspicion in military returns. As it is necessary that some cause of unfitness for duty should be assigned before a soldier can be discharged, however long and faithfully he may have served: and as rheumatism is one of those diseases, the existence of which in a chronic form it is almost impossible to disprove, so it is the one most frequently selected by the old soldier on which to lay up for his discharge. In all comparisons, therefore, with the proportion of cases occurring in civil life, it must be borne in mind that rheumatism in the army is often a disease feigned, or at least aggravated, for a specific purpose; and even in comparing one branch of the service, or one colony, with another, the results are liable to be affected by the relative proportion of *old* soldiers serving in each.

Diseases of the Eyes appear to have been a source of considerable inefficiency in the regiments of the line, in consequence of the prevalence of ophthalmia. The reporters give a tabular statement of the regiments in which it prevailed to a great extent in each year, and state that there can be little doubt the majority of the cases were of foreign origin. They then observe that

"After a careful study of the history of the disease, as it appeared in each of these corps, we are compelled to admit that the causes of its origin, prevalence, and cessation, are still unknown. It has usually manifested itself in the form of catarrhal ophthalmia, and though a source of great inefficiency, has not often given rise to permanent loss of vision."

Corporal Punishment.—The extent to which this kind of punishment has been carried, has of late years been undergoing a steady progressive reduction both in frequency and severity, so that during the last year included in the report, it was inflicted upon only 1 in every 1000 of the Foot Guards, 3·4 in every 1000 of the Dragoon Guards and Dragoons, and 4·8 in a like number of the soldiers of the line. This, with the limit of the number of lashes to fifty, ordered in 1847 by the late commander-in-chief, has materially lessened the responsibility of the regimental medical officer, in the disagreeable duty of superintending such punishments. It is very satisfactory to learn that this amelioration has not been attended with any bad consequences as regards discipline, for it is shown by a return from the adjutant-general, that there has been a corresponding progressive reduction in the number of courts-martial, and especially in general courts-martial, by which all offences of a very serious character would be tried.

From an interesting comparison of the amount of crime in the French and English armies, it would appear that after deducting certain offences which are not tried by courts-martial in the French service, the proportion brought to trial of the voluntarily enlisted men, who are obviously the only fair subjects of comparison with our army, approximates so nearly as 56 to 60.

Deaths from Suicide, Accidents, and Violence.—The proportion of deaths from these causes is almost the same among the Cavalry and regiments of the line as in the civil population, while it is only half as high among the Foot Guards; the difference being principally in the infrequency of suicide. In a former report it was stated that suicide was much more frequent among the military than civilians; but on this point the reporters now observe,

"It is remarkable that the ratio of deaths from accidents, violence, and suicide, is almost the same, both among civil and military; thus negativing the conclusions in our previous Report, as to the greater tendency to suicide in the army. The absence of accurate information, at the commencement of these statistical inquiries, gave rise to this error. At that time, the relative frequency of suicide in the army and in civil life could only be estimated by comparing the cases among a given number of soldiers, and a like number of the civil population at all ages; for though it was known that the tendency to suicide is less common among females, men of advanced age, and boys, than among males at the military period of life, no data existed to show the extent to which that peculiarity was likely to influence the results."

These remarks are instructive, as showing the necessity of ascertaining, before making any comparison, that the conditions essential to accuracy exist: and that we are not placing in juxta-position groups of facts, which in some material points are wholly dissimilar.

The stations occupied by our troops in the MEDITERRANEAN, are divided into three military commands, Gibraltar, Malta, and the Ionian Islands. In these the same ameliorations in the condition of the soldiers have been carried out as at home; and several local improvements and changes have been effected, which it may be well to notice briefly. At Gibraltar, in consequence of additions to the fortifications, it became necessary to carry out the drains beyond low-water mark, where their contents are at once removed by the tide, instead of being left exposed, as formerly, to the action of the sun. The mouth of the sewers, also, being now always under water, the houses are free from the draught of foul air which ascended into them through the drains at ebb tide, whenever the wind was in the west, and to which the outbreaks of epidemic fever were by many attributed. In the Ionian Islands the barracks were, in 1837, transferred from the control of the local government to the care of the ordnance department, by whom several new ones have been erected, and the old repaired and put in habitable condition. The islands which seem to have benefited most are Corfu and Zante. In the latter, the old barracks were completely destroyed by an earthquake in 1840, and new ones consequently erected, which came into occupation in 1844.

A very important change, affecting the whole of the force employed in the Mediterranean, was introduced in 1837, and is known among military men as the "Rotation system of Reliefs." Instead of serving as formerly for a continuous period of ten years in the Mediterranean, a regiment proceeds to the West Indies at the end of three or four years: and, after serving there for a similar period, completes its term of foreign service in British America. In consequence of this, the average age of the men serving in the Mediterranean has been lower than under the former system.

Instead of entering upon an examination of the health of the troops in each of the commands separately, as has been done in the report, we have condensed the results into one general table, and shall offer a few remarks on the relative prevalence of the different classes of diseases in each.

	Annual ratio per 1000 of mean strength.					
	Gibraltar.		Malta.		Ionian Islands.	
Aggregate strength (1837—46) ...	33,131		21,172		26,201	
	Admit.	Died.	Admit.	Died.	Admit.	Died.
Fevers	81	1·96	210	1·79	255	5·84
Eruptive fevers	2	...	2		1	
Diseases of the lungs	132	5·82	140	7·93	134	6·22
Diseases of the liver	10	·09	30	·76	15	·38
Diseases of the stomach and bowels	203	1·87	212	2·79	154	1·64
Epidemic cholera	...	...	10	2·22	—	—
Diseases of the brain	14	1·06	10	·61	10	1·45
Dropsies	1	·18	1	·38	2	·27
Rheumatic affections	44	1·54	46	1·46	40	·84
Venereal affections	79		99		85	
Abscesses and ulcers	126		116		158	
Wounds and injuries	101		100		110	
Corporal punishment	5		11		9	
Diseases of the eyes	71		66		94	
Diseases of the skin	23		17		19	
Other diseases	47		50		53	
Accidents, violence, &c.	...	1·06	...	1·42	...	1·30
Annual ratio per 1000 of mean strength	939	13·58	1120	19·36	1139	17·94

From this it will be seen that, both as regards admissions and deaths, Gibraltar has been more healthy than the other two commands, in which, if epidemic cholera be omitted, the results are almost identical. The excess in the admissions over those at Gibraltar has been chiefly by fevers; in the deaths, it has arisen in the Ionian Islands from the same cause; and at Malta, from diseases of the lungs, and of the stomach and bowels.

Among the deaths recorded in the preceding summary are those of invalids on their passage home. As this information was not given in the former reports, it is necessary in any comparison of the health of the troops during the two periods, to deduct these, and include only such as occurred within the limits of the command—on which principle, accordingly, the following statement has been prepared:

—	Annual ratio per 1000 of mean strength.			
	1817—36.		1837—46.	
	Admit.	Died.	Admit.	Died.
Gibraltar	966	22·	939	12·1
Ditto, omitting 1828 and 1834	923	12·7	—	—
Malta	1142	18·7	1120	16·7
Ionian Islands	1201	28·3	1139	16·8

Thus it appears, that in all the commands there has been a marked reduction in the mortality. At Gibraltar, even if the years 1828, when yellow fever, and 1834, when epidemic cholera prevailed, be deducted, the ratio of deaths by the ordinary diseases of the command is lower than formerly. In Malta, notwithstanding an outbreak of cholera in 1837, the proportion is 2 per 1000 less; and in the Ionian Islands it is not much more than half what occurred during the previous twenty years;—results which bear most favourable testimony to the beneficial character of the measures adopted to improve the health of the troops.

We shall now notice a few of the most striking facts connected with the different classes of diseases.

Fevers.—As already stated, the principal difference in the admissions into hospital at Gibraltar and the other two commands has consisted in the much greater prevalence of this class of diseases in the latter. There is, however, another point worthy of remark, that, while in Gibraltar and Malta paroxysmal fevers are almost unknown, in the Ionian Islands they constitute nearly a third of this class. At Malta, common continued fever prevailed to a great extent in 1840-41 and 1844; indeed, the admissions in these three years nearly equalled those of the remaining seven. It was at the same time very prevalent among the civil population, and was attributed, though apparently on very insufficient grounds, to the excessive heat and drought. Gibraltar escaped epidemic yellow fever, which formerly created such havoc in the garrison, but it is difficult to assign any satisfactory reason for the immunity on the present occasion.

Small-pox prevailed to a great extent among the civil population of Malta in 1838 and 1843-4, and proved very fatal; but, as is generally the case with this disease, the military almost entirely escaped—a result to be attributed to the care taken in the army to see that every soldier has been vaccinated.

Diseases of the Lungs.—This class of diseases has proved more fatal in Malta than either at Gibraltar or the Ionian Islands; the difference being chiefly in phthisis and chronic catarrh. A continued residence at Malta, indeed, would appear to be very unsuitable to persons having a predisposition to phthisis. The proportion of cases of hæmoptysis and phthisis occurring among the troops in that island, remarkable for its equable climate, compared with those in Canada where the winter cold is intense, and the vicissitudes of temperature sudden and extreme, is shown in the Report to be as 9·8 to 8·4 per 1000 of the strength annually; and the deaths by these diseases to be as 4·3 to 3·8. It does not necessarily follow, however, from these results, that Malta is not a suitable residence for invalids: the difference should not be lost sight of between the permanent residence of a soldier doing duty in garrison, and the temporary stay of an invalid who selects the proper season, and regulates his exercise and exposure according to the state of the weather.

Influenza prevailed as an epidemic in Gibraltar in 1837, and in the other commands in the same year, and also in 1840 and '41; but did not occasion many deaths. An interesting account is given of the progress of the epidemic at Gibraltar, from which it would appear, that it ran its course in fourteen days, during which time 417 cases occurred, and one died from the supervention of pneumonia. In detailing its progress through the various regiments in the garrison, it is stated, that the 82nd in the Town Range Barracks and King's Bastion escaped altogether:

"It is worthy of remark, however, that the 82nd had suffered from epidemic catarrh in a mild form, a very short time previously, on their passage from Cork to Gibraltar; and that among 100 men of the 33rd who came out in the same vessel, only 2 cases occurred during its prevalence in Gibraltar, though they were quartered in the same barrack with corps which suffered considerably."

Diseases of the Liver—Are thrice as prevalent in Malta as in the other two commands. This has been supposed by some to depend upon the continuous high temperature and extreme aridity of the island during nearly half the year; but this explanation admits of doubt, as the number of cases seems but little influenced by the season, and in 1840, which was remarkable for heat and drought, they were much below the average amount on the island.

Diseases of the Stomach and Bowels.—This class of diseases proves a much greater source of inefficiency and mortality in the Mediterranean than in the army at home—the difference being chiefly in dysentery. There has, however, been a marked diminution in the severity of this disease of late years, particularly in the Ionian Islands. With reference to it, the reporters observe—

"That there is no disease, in the prevalence of which so remarkable an increase takes place among British soldiers when removed to a foreign station, as dysentery, and none which is so likely, by repeated attacks, to have a debilitating effect on the constitution."

Epidemic Cholera.—Malta was the only one of the Mediterranean stations in which this disease prevailed during the period reported on.

It broke out in the Poor-House on the 9th of June, rapidly spread throughout the island, "not following any regular course, but breaking out in many and distant places at the same time," attained its maximum about the middle of July, and finally ceased in October. It appears to have raged with great intensity among the inmates of the Poor-House, for 549 cases and 451 deaths occurred between the 9th June and 2nd September among them, although their number was only 878 at the period of the outbreak. Among the civil population of Malta and Gozo, amounting to 121,187 persons, 8379 cases and 4180 deaths occurred, being at the rate of 69 and 34½ per 1000 living. It did not prove so fatal to the soldiers, among whom the cases amounted to 98 and the deaths to 22 per 1000 of the strength. In every instance, cholera was ushered in by premonitory diarrhœa.

"The medical officers in Malta were decidedly of opinion that the disease was not contagious; a conclusion strikingly corroborated by the facts that, although necessarily much exposed to its influence, from their constant attendance on the sick, these officers did not suffer in a greater proportion than the others; and that of the numerous orderlies in hospital, two only were attacked, both of whom recovered."

In a subsequent part of the Report, when treating of the relative prevalence of this disease among the seamen and soldiers serving in the Mediterranean, we find the following remark:

"It is also worthy of notice, as bearing strongly on the question of contagion, that not one of the residents in the Naval Hospital at Malta, in 1837, whether medical officers, patients, or nurses, was attacked, although upwards of 70 cases were admitted into that establishment for treatment. It has also been shown, in our former Report, that in the military hospitals at Gibraltar, during the epidemic of 1834, neither the orderlies nor other persons employed suffered in a greater proportion than the troops in barracks."

Diseases of the Brain.—In all the Mediterranean commands there has been a very marked increase in the prevalence of diseases of this class—chiefly, we regret to say, of *delirium tremens.* It is difficult to ascertain the cause of this, as there is no reason to believe that any material change has taken place in the habits of the soldiers. It may, perhaps, be in a slight degree owing to the larger number of men employed on the public works than formerly, for which they receive additional pay, and which, combined with the low price of spirits in the colonies, of course affords the opportunity of getting drunk when they are so disposed. It appears to be upon this supposition that it is suggested—

"With the view of checking this vice as much as possible, so long as working-pay continues to be issued to the troops, the messing should be improved to the extent of the additional allowance thus drawn by them monthly, except so far as it may be required for keeping up their supply of necessaries. This might be done, either by the addition of an extra quantity of meat, or the occasional issue of fish and other varieties of food, which would diminish the means of intemperance, and, at the same time, improve the diet of the soldier."

It is to be hoped that the establishment of regimental savings'-banks may also exert, in this respect, a beneficial influence, by affording to the well-disposed an opportunity of accumulating in safety their earnings, till they require them for some useful purpose.

We cannot give more briefly than in the words of the Report, an account of an epidemic which broke out at Gibraltar in 1844:

"Between January and June, 1844, there appear in the returns 5 admissions of inflammation of the brain, all of which proved fatal, and deserve notice because the disease prevailed at the same time among the civil population in an epidemic form. It first showed itself in the early part of January, and did not cease till the end of May, during which time about 450 cases occurred in a population of 16,000, and 42 of these terminated fatally. A similar epidemic prevailed in various places in France between 1837 and 1842, but, contrary to what happened in Gibraltar, it affected the military to a much greater extent than the civil population, who, in many of the garrison towns, appear to have escaped the disease altogether. There were 1062 cases reported to the Council of Health of the French army, during the five years above noted, of which 605 terminated fatally. When it extended to the civil population, the loss was in the same proportion; for in 1841, of 150 cases reported at Strasburg, 90 died.* No cause could be assigned for its prevalence."

Of the other classes of diseases comprehended in the table, there has been an increase in the admissions from abscesses and ulcers in all the commands, arising probably from the greater number of men employed upon the public works. At Malta, there has been a marked reduction in the amount of venereal, attributed to the continued vigilance of the police in enforcing the regulations regarding prostitutes. Both at Gibraltar and Malta there has been a most satisfactory diminution in the cases of ophthalmia, which seems, perhaps, to have been in some measure due to an arrangement whereby each soldier was provided with separate means of ablution. In the Ionian Islands, however, this disease has more than doubled in amount during the last ten years. There is no reason assigned for this increase, nor are we informed whether the same arrangement for ablution has been introduced as at Malta; it is, however, a matter deserving careful investigation, the admissions having amounted to 10 per cent. of the strength.

The reduction in corporal punishment has been quite as great in these commands as among the troops at home, so that we may presume secondary punishments have been found sufficient for the maintenance of discipline, except in the case of a few hardened offenders, who are, and probably ever will be, found in most regiments, though it is likely they are not more numerous than among the same class of persons in civil life.

No cases of Plague have occurred among the troops in these Commands during the ten years included in the Report, notwithstanding the increased intercourse with the eastern shores of the Mediterranean, where that disease is so generally prevalent, the relaxation in the stringency of the Quarantine regulations, and the reduction in the duration of the voyage by the employment of steam-vessels.

In the preceding observations we have confined ourselves to the results obtained from the Ionian Islands command generally, but the relative salubrity of the various islands constituting this varies materially. With a view to illustrate this, we have prepared the following table, and have added to it a column compiled from the former Statistical Reports, showing the mortality in each island during the twenty years prior to 1836:

* An abstract of Dr. Casimir Broussais' account of this epidemic in the French army was given in the British and Foreign Medical Review for January 1847, p. 82.

	1837—46.		1817—36.
	Annual ratio per 1000 of strength.		Annual ratio of deaths per 1000.
	Admitted.	Died.	
Corfu and Vido	1086	15·8	20·1
Paxo	973	24·	Not stated.
Santa Maura	1307	19·9	46·
Cephalonia	1114	15·	30·5
Zante	1494	19·6	32·
Ithaca	1143	21·	26·1
Cerigo	1153	19·6	20·1

Thus the admissions range from 973 to 1494, and the deaths from 15 to 24 per 1000 of the strength; but if we omit the three smaller islands, where the force consists only of from 40 to 50 men, the difference is much less, being from 1086 to 1494 on the admissions, and 15 to 19·9 on the deaths. In all the islands there is a most marked reduction in the mortality of the last ten years, compared with the preceding twenty, amounting, in Santa Maura and Cephalonia, to upwards of one-half. The reporters find themselves at a loss to account for this; they remark that—

"The improvements which have taken place in barrack accommodation, or in the drainage of the country, are scarcely sufficient to account for it, as the amelioration has extended to islands where there has been comparatively little change in these respects."

And again, in noticing an epidemic of remittent fever which occurred at Santa Maura, they observe:

"When calling attention in a former Report to the improvement in draining and bringing under cultivation the ground in the vicinity of the barracks in this island, we remarked, 'There is so little certainty, however, regarding the real cause of remittent fever, that it is extremely difficult to state with any degree of accuracy what the effect of such improvements may have been.' Unfortunately these remarks were fully confirmed in 1837, when, notwithstanding these improvements, remittent fever broke out, and the admissions amounted to nearly half the strength, while one in eight of the cases terminated fatally. At the same time, fevers of the intermittent and continued types were more than usually prevalent and fatal."

In referring to the reduction in the mortality by fever at Cephalonia, they furnish us with a salutary caution, observing, that—

"Though it may be attributed in some measure to the improved ventilation of the barracks, yet so little is known of the real causes of fever, that it is premature to hazard any positive conclusions, as the events of a single year might bring back the ratio to the former standard, though the ventilation and all other improvements remained the same."

We have dwelt thus fully upon the subject because we think it ought to convey a useful lesson to all of us, as there can be no doubt we are but too apt to jump at a conclusion, and to frame our theories on imperfect observations and insufficient data.

In this portion of the Report is a long and interesting chapter on the relative health of seamen and soldiers serving in the Mediterranean, founded on the returns of twelve years. On this subject we do not propose to enter, because we formerly* discussed it very fully; and although our observations were then founded on the returns of seven years only, the general conclusions at which we arrived, and the principal facts we laid before our readers, correspond so closely with those in the Report, that it is unnecessary to repeat them on the present occasion.

The 'Report on the Health of the Troops in British America' is divided into four sections, corresponding with the four military commands—viz., the Bermudas, Nova Scotia and New Brunswick, Canada, and Newfoundland.

The Bermudas.—Since 1840 the force in these islands has been materially increased, in consequence of the number of convicts sent there. The additional troops have been chiefly quartered in Hamilton, situated at the western extremity of the Bermuda group, from which three companies have always been detached for duty on Ireland Island. The barrack accommodation at St. George's was increased, while in the other islands houses were hired and fitted up as temporary barracks. The issue of salt meat to the troops was reduced in 1840 to thrice a week in summer, and four times a week in winter; and, as in the other colonies, an evening meal of bread and coffee was established. The average force in the islands during the ten years amounted to 1122 men, among whom the admissions and deaths by the various classes of diseases were as follow:

——	Annual ratio per 1000 of strength.	
	Admitted.	Died.
Fevers / Eruptive fevers	168	16·
Diseases of the lungs	134	9·4
Diseases of the liver	12	·5
Diseases of the stomach and bowels	289	2·8
Diseases of the brain	12	·9
Dropsies	1	·1
Rheumatic affections	44	1·7 (Rheumatic affections to Other diseases)
Venereal affections	80	
Abscesses and ulcers	142	
Wounds and injuries	77	
Corporal punishment	6	
Diseases of the eyes	155	
Diseases of the skin	19	
Other diseases	50	
Accidental, violent, and sudden deaths	...	2·4
Total	1189	33·8

Thus it appears that the chief cause of mortality in the Bermudas has been fever, which occasioned one-half of the deaths during these ten years. This arose from an outbreak of yellow fever as an epidemic in July, 1843, when so fatal did it prove, that, before the end of the year, 159 men died by it out of a force of 1402. The officers suffered to even a greater extent, 10 having died out of an average strength of 41. Exclusive of that year, the deaths by fever among the soldiers amounted only to 21,

* British and Foreign Medical Review, April, 1844.

or in the proportion of 2·1 per 1000 of the strength, being a fraction lower than among the infantry serving at home. It is much to be regretted that "the information contained in the Medical Reports regarding the origin and progress of the epidemic is very meagre and unsatisfactory." From the statements in the volume now before us, it would appear to have broken out first at St. George's, and to have been more severe there than in the other islands, 2 cases in 9 having proved fatal; while at Hamilton 1 in 11, and at Ireland Island only 1 in 14, died. Of the deaths, 1 occurred in July, 90 in August, 56 in September, 8 in October, and 4 in November. Referring to the question which was raised, whether the disease was true yellow fever, the reporters observe:

"This is almost invariably the period at which yellow fever prevails in New York, New Orleans, and the southern parts of the United States. Whether the disease, therefore, is identical with the yellow fever of the West Indies or not, this striking difference is manifest in its operation, that there, no period of the year is exempt from it, while in the Bermudas and other places just referred to, it only appears at that season when the influence of fever is generally at its maximum."

"There was nothing either in the temperature, moisture, fall of rain, or other meteorological phenomena at this period, to account in any way for the appearance and violent character of the epidemic."*

Next in point of mortality are diseases of the lungs, which have caused rather above half the deaths, exclusive of those from fever. We cannot give the observations on this class more briefly than in the words of the Report:

"There is still the remarkable feature of this command, with its comparatively uniform temperature, having a much higher mortality by diseases of the respiratory organs than the North American colonies, with their extremes of heat and cold, and their long-continued winters.

"Nor is this confined to the Bermudas alone, for it will be found in a subsequent part of this Report, that the admissions by consumption in the United States army serving in East Florida, under nearly the same latitude, and with a corresponding temperature, amount to $8\frac{7}{10}$ per thousand of the mean strength annually, while in the posts beyond the 40th degree of north latitude, including all those where the winter is most severe, the proportion admitted into hospital for the same disease is but 7 per thousand annually. It is very satisfactory thus to find the results obtained in our own army corroborated by the observations made in another, particularly on a question of so much importance, and which but for such corroboration might be considered as doubtful."

There has been a great reduction in the amount of diseases of the stomach and bowels: and the difference would have been still more strongly marked but for the prevalence of diarrhœa in the Rifle Brigade, on landing after a long and boisterous voyage, during which they were much exposed to the weather. It seems also to have been partially augmented from the overcrowded state of the barracks, after the increase

* In August last, epidemic yellow fever again attacked the troops, breaking out, as on former occasions, at St. George's, and raging there with greater severity than in the other islands. In the short space of three months, about a third of the whole force was cut off, being a higher proportion than in the two previous epidemics. The officers suffered to even a greater extent than the men, two-fifths of them having fallen victims to the disease. We trust that on this occasion the necessary measures have been taken by the Army Medical Board to obtain accurate information relative to the origin and progress of the epidemic, and that in the next volume of reports there may be no complaints of it being "meagre and unsatisfactory."

to the force, and before adequate arrangements were made for their accommodation.

We are happy to observe a reduction in the amount of diseases of the brain, and particularly apoplexy and delirium tremens, a circumstance which justifies the hope that there has been an improvement in the habits of the troops.

There has been an increase in the amount of ophthalmia, attributable chiefly to its prevalence in the 70th, which brought it with them from the West Indies, and in the 30th regiments. It does not appear from the Report whether the system of providing means of separate ablution for each soldier was adopted, as in Malta, but it seems highly advisable that such should be the case in all colonies where ophthalmia is a source of much inefficiency among the troops.

Corporal punishment, which appears formerly to have been in high favour here, having extended, in 1822, to one half of the garrison, has been reduced to less than 4 per 1000 of the force.

Nova Scotia and New Brunswick, and Canada.—Two circumstances require especial notice, as having been likely to affect the health of the troops in these commands during the period reported upon. The first is, the rotation system of reliefs, already noticed in our remarks upon the Mediterranean Report, by which a large proportion of the regiments came from the West Indies, and necessarily brought with them many men labouring under chronic disease, and with constitutions deteriorated by residence in a tropical climate. It was also, unfortunately, necessary, in two or three instances, to move them in the depth of winter, and before the requisite provision of warm clothing could be made.

The other circumstance was the necessity for concentrating in Lower Canada a large force, to put down the insurrection in the latter end of 1837. To accomplish this, several regiments were marched, in the depth of winter, across the high lands separating the waters of the St. John from those of the St. Lawrence, while others were brought from Home, the Mediterranean, and the West Indies. A considerable portion of this force was on service in the field during two winters, and exposed to much hardship, being frequently moved about and billeted wherever accommodation could be procured. The rest were quartered in the towns, in hired buildings, or temporary barracks, which were too often over-crowded, defective in ventilation, and destitute of almost every convenience and comfort. Several of the posts thus temporarily occupied were in situations where the causes of fever abounded; and some of them had to be abandoned on this account.

Notwithstanding these unfavourable circumstances, the health of the troops in both commands appears to have been satisfactory, and the mortality lower than during the preceding twenty years. The following summary shows the proportion of admissions and deaths in each:

	Annual ratio per 1000 of mean strength.			
	Nova Scotia and New Brunswick.		Canada.	
	Admitted.	Died.	Admitted.	Died.
Fevers	60	1·1	103	2·1
Eruptive fevers			3	·2
Diseases of the lungs	180	7·7	157	7·4
Diseases of the liver	6	·3	10	·3
Diseases of the stomach and bowels	109	1·6	138	1·1
Diseases of the brain	15	1·3	17	1·3
Dropsies	1	·3	1	·3
Rheumatic affections	44	1·5	44	1·4
Venereal affections	98		117	
Abscesses and ulcers	101		103	
Wounds and injuries	119		134	
Corporal punishment	7		9	
Diseases of the eyes	81		79	
Diseases of the skin	20		18	
Other diseases	59		49	
Suicide, accidents, violence, &c.	...	2·2	...	3·3
Total	900	16·0	982	17·4

Compared with the preceding twenty years there has been a reduction in the mortality of $2\frac{1}{2}$ per 1000 in Nova Scotia, &c., and of $3\frac{1}{2}$ in Canada. This has arisen chiefly from the absence of epidemic cholera. With the exception of a slight diminution in the deaths from fevers the results are in other respects nearly identical. We shall now extract a few of the more striking facts regarding the principal diseases.

Fevers.—In Nova Scotia and New Brunswick there has been apparently a great increase in the prevalence of intermittents, but the cases have, with very few exceptions, occurred in corps recently arrived from the West Indies or Ionian Islands, where this type is very prevalent. By a tabular statement, showing the regiments from which the admissions into hospital occurred in each year, it appears that

"In 12 regiments which came from stations where intermittent fever is usually prevalent, 409 cases occurred; while, in five battalions and the Ordnance corps, which came from stations where this disease is comparatively rare, but 18 cases were recorded in the whole ten years under review.

"The table also shows how rapidly the liability to attacks of intermittent fever yields to the influence of residence in this Command, the proportion of cases in the second year being rarely above a fifth of that in the first, and before the third year of service has elapsed it altogether disappears."

On these results the reporters found a suggestion for the removal from the West Indies of invalids who have suffered from repeated attacks of this type of fever, attaching them to some corps in Nova Scotia till the arrival of their regiment in its regular turn of service, and anticipate that many soldiers might thereby be saved from being irremediably broken down in constitution, and would pass their time in the exercise of their duty, instead of lingering out a miserable existence in hospital. In Canada there has been a marked reduction in the amount of intermittents, notwithstanding the necessity for quartering troops, during part of the time, in localities where they were very prevalent. For instance, at one of these temporary stations—Chatham—39 cases of intermittent, and 26 of remittent fever, occurred in less than three months, in a garrison of 104 men; and at another—Sandwich—269 cases of intermittent fever occurred in 1839, out of a force of 179 men. This diminution of paroxysmal fevers, which has been observed also among the civil population, is probably attributable to the extension of cultivation, and the improvement in drainage.

Diseases of the Lungs.—This class of diseases has given rise in both Commands to nearly half the mortality, but notwithstanding the severity of the climate, the proportion both of admissions and deaths by them has been lower than among the Infantry in the United Kingdom. There has been an increase in the cases of acute catarrh in Nova Scotia, &c., during the last ten years; a result which cannot excite much surprise, when we find that two regiments landed in that Command from the West Indies in December, one in January, two in February, and one in March, some of them wholly unprovided with winter clothing. This increase appears to have been confined to catarrh, the admissions from other diseases of the lungs having been fewer than formerly.

The Report shows that two regiments, which came direct from home, had, in the first twelve months of their service, on the average, 48 cases of catarrh in each; one from the Mediterranean had 98; three from the West Indies had an average of 134 each; and three from Jamaica 138 each. It is stated that in later years, when better preparations were made, the disparity became less obvious.

Adverting to the extent of pulmonary disease in America, the reporters observe:

"To those who have been accustomed to regard low temperature and severity of climate as the chief exciting causes, it will no doubt seem remarkable, that the proportion of deaths by diseases of the lungs during the same ten years in Malta was 7·9; while in Canada it only amounted to 7·4 per 1000 of the strength. In the Mediterranean Report we have already shown that the same holds good even as regards consumption and spitting of blood, the admissions in Malta to those in Canada being relatively as 9·8 to 8·4, and the deaths in hospital as 4·3 to 3·8."

These remarks are strikingly corroborated by the results in the Report on the Health of the United States Army, which show the admissions into hospital for consumption to have been 7 per 1000 of the strength at the posts in the Northern States, $11\frac{1}{10}$ per 1000 at those between New York and Savannah, and $8\frac{7}{10}$ at the Southern stations on the Lower Mississipi and in East Florida. As regards the civil population:

"In the principal towns of the United States, the proportion of deaths annually by these diseases is as follows:

—	Annual ratio of deaths per 1000 living.	
	By Diseases of the Lungs.	By Consumption.
Boston	4·99	4·03
Baltimore	5·84	4·12
Philadelphia	5·47	4·20
New York	6·34	4·96
New Orleans	8·26	5·61

continent where we have troops, affords another striking instance how little that disease appears to be influenced by those atmospheric causes which were at one time supposed to be most active in inducing it. So far as regards consumption, the proportion attacked annually of the whole Force has only been about 4 per thousand. In the Mediterranean, out of an equal number, the proportion attacked would have been $5\frac{6}{10}$, and among Infantry, in the United Kingdom, $10\frac{3}{10}$. Even in the mild and equable climate of the Bermudas, the proportion attacked by the same disease would have been 9 per thousand of the strength. This island has entirely escaped epidemics of cholera and influenza, the causes of so much sickness and mortality elsewhere."

In the preceding pages we have confined our observations to the amount of sickness and mortality among the troops; there are, however, other subjects treated of in the Report, to which we shall now very briefly advert. The first of these is the extent of

Invaliding, the term applied to discharging men as unfit for military service. So long as the system, before alluded to, continues, of requiring some specific disability to be assigned before a soldier is discharged, no deductions of any value can be drawn from the numbers invalided, as it is impossible to distinguish between cases where real disqualification for duty exists, and those where some disability is assigned merely as an excuse to pension off an old soldier. When the limited enlistment act is in full operation, the returns on this head will be more satisfactory. We may observe, however, the large proportion of men invalided from the Mediterranean commands on account of pulmonic affections, amounting to more than one-third of the whole number discharged, corroborates the previous conclusions as to the influence of the climate of the Mediterranean on this class of diseases. There is also a remarkable similarity in the proportion invalided on this account in the Mediterranean and British America, notwithstanding the great difference of climate. For instance,

"The number invalided for pulmonic disease is almost precisely the same in Gibraltar, where the temperature ranges from 45° to 86°, as in Canada, where it ranges between 23° below, and 87° above zero; and in Malta, an island remarkable for its uniformity of temperature, the results are identical with those in Nova Scotia and New Brunswick, where the thermometer has been known to fall 52° in twenty-four hours; while in the Ionian islands, in which the alternations of temperature are greater and more sudden than in the other Mediterranean commands, the proportion is lower than in the Bermudas, the climate of which has been described as perpetual summer."

In the Mediterranean the proportion discharged on account of diseases of the eyes—at one time a source of great inefficiency and expense—has been reduced more than one-half, and amounts now to only 0·56 per 1000 of the strength; but in the American commands there has been a slight increase in the numbers invalided for this cause.

Average number constantly sick.—The proportion constantly non-effective from sickness appears to vary but little in the different commands whose health details we have been investigating. Thus, it has been 40 per 1000 among the cavalry at home, and the troops in Canada; 43 among the foot guards, and the troops at Gibraltar and Malta; 44 in the Ionian Islands; and 56 at Bermuda, the excess in the latter depending upon the large amount of diseases of the bowels, and of fevers. In Nova Scotia and New Brunswick it has only amounted to 35 per

1000, but there seems reason to believe that the sick of the detachments were not reported with the same regularity as in the other commands.

There is a section on the *influence of age and length of residence on the mortality* in each of the commands, but the results being given in a tabular form, are so condensed that we must refer our readers to the Report itself for information on this interesting topic.

The next section relates to the *Mortality of the Officers*. This, however, is so much affected by the opportunities they enjoy of selling out, or retiring on half-pay when their health becomes impaired, that it would be difficult to institute a fair comparison between the relative mortality among them and the soldiers.

There yet remains one subject to be noticed,—*the influence of the seasons in producing sickness and mortality among the troops.* We shall reserve this topic, however, for another occasion, when the subject of medical meteorology comes under review.

We have endeavoured, in the preceding pages, to give a general view of the leading facts contained in the Report; but in doing so, we have of course been obliged to pass unnoticed many minor points of interest. For these we must refer our readers to the Blue Book itself, with an assurance that a perusal of it will amply repay any who are interested in the geographical distribution of diseases, or the study of their influence upon masses of men. Upon one point we must give the authors every credit—while for the purpose of more easy comparison they have grouped the diseases into classes, they have always, in the appendix, given detailed numerical abstracts of the admissions and deaths: and thus afforded an opportunity to individuals to make use of the facts in their investigations into the prevalence and mortality of any particular disease.

We have only further to express our conviction of the immense value of the information contained in this Report, as well as of the skill with which it has been so lucidly arranged.

Review VII.

Vestiges of the Natural History of Creation. Tenth Edition. *London*, 1853.

In the mind of any one at all practically acquainted with science, the appearance of a new edition of the 'Vestiges' at the present day, has much the effect that the inconvenient pertinacity of *Banquo* had upon *Macbeth*. "Time was, that when the brains were out, the man would die." So time was, that when a book had been shown to be a mass of pretentious nonsense, it, too, quietly sunk into its proper limbo. But these days appear, unhappily, to have gone by, and the same utter ignorance of the public mind as to the methods of science and the criterion of truth, which were evidenced to a Faraday by the greedy reception of the table-turning folly, have encouraged the author of the 'Vestiges' to venture upon a *tenth* edition, "with extensive additions and emendations." We doubt not that this edition—very pretty and well got up it is—will be as greedily swallowed by those to whom it is offered, as any of the other nine, to the great glory and no small profit, of its modest and ingenious author. We grudge no man either the glory or the profit to be obtained from charlatanerie, and we can hardly expect that those who are so ignorant of science as to be misled by the 'Vestiges,' will read what we have to say upon the subject; but a book may, like a weed, acquire an importance by neglect, which it could have attained in no other mode; and, therefore, it becomes our somewhat unpleasant duty to devote a few of our pages to an examination of some of the leading points of this once attractive and still notorious work of fiction: indeed we feel the more called upon to undertake this criticism at present, since, as we shall see, the 'Vestigiarian' has bolstered up his case by the use of names and authorities, which, were it justifiable, might give a certain value to his statements.

It would be no less wearisome than unprofitable to go into a detailed examination of all the blunders and mis-statements of the 'Vestiges'—to drag to light all the suggestions of the false and suppressions of the true, which abound in almost every page, and which, in a work of such pretension, of such long elaboration, and so filled with whining assertions of sincerity, are almost as culpable, if they proceed from ignorance, as if they were the result of intention. We propose, therefore, to confine our attention to the fundamental proposition of the book and to some one or two of those matters connected with the biological sciences, which come more particularly within the province of this review.

The Vestigiarian modestly tells us, that none of his critics have rightly conceived "the fundamental proposition of the work" (Proofs, &c., lviii); and in answer to the reproach of superficiality, he says, that "to be a superficial book, it has been remarkably hard to understand" (Ibid.). We really must suggest that difficulty of comprehension is by no means a test of depth of thought, and that it would be well to leave out, in the eleventh edition, the confession, that this book, so popular with the mob, is incomprehensible to the thinker. Obscurity is more often the result of the muddiness than of the depth of the stream of thought. This, by the way, however: for, lest we should fall under like condemnation, we quote here the author's own words, that "The actual proposition of the 'Vestiges' is 'creation in the manner of law,' that is, the Creator working in a natural course, or by natural means." (Proofs, lix.) Here then is the idea of the book, and if the author has not demonstrated this, it is so much waste paper. There is, however, one preliminary which must be settled before passing to the question of demonstration—namely, has this potent proposition, as it is here expressed, any intelligible meaning at all?

What is the sense of the terms "Law" and "Nature"? Nature is, of course, the totality of all laws, and therefore our inquiry is limited to the question of the meaning of "Law." As we prefer not to adopt any definition not admitted by our author, we will turn to his own pages, though there really are authorities (Sir John Herschel, or Mr. John Mill, for instance), to whom we should give a preference—especially as the Vestigiarian does not pursue that pleasant and useful habit of being consistent with himself, but has one theoretical meaning, which he defines and writes about, and one practical, which he acts upon. The former is brought forward *explicitly* in controversy; the latter *implicitly* in the body of the book. The former may be met with in the controversy with Dr. Hitchcock.

"Law, I have over and over again said, is merely a term of human convenience to express the orderly manner in which the will of God is worked out in external nature; and He must be ever present in the arrangements of the universe, as the only means by which they could be even for a moment sustained." (Proofs, lxii.)

We perfectly agree with this definition of Law; but suppose that we substitute it for the word law in the portentous formula we have cited above, thus—"Creation took place," "in the orderly manner in which the will of God is worked out in external nature," "he being immediately present;" and, stripped of unnecessary verbiage, it comes to this, that "Creation took place in an orderly manner, by the direct agency of the Deity." A proposition which is as old as the Book of Genesis.

As exhibited in controversy *explicitly*, then, the proposition of the 'Vestiges' is, as a scientific explanation of Creation, not a whit in advance of the notions entertained by our grandmothers; but the tacit, *implied* proposition which the author, whenever pressed, denies, but always acts upon, is very different. The Vestigiarian, in fact, with no small pride, contrasts this his own private notion—1. With the allowing of the existence of any "dominion of marvel;" 2. With the notion of creative fiats, interferences or interpositions of creative energy—with the need of a "special attention" "whenever a new family of organisms is introduced;" 3. With the "undignified" notion that the nodulosities and corrugations of a cephalopod are "worthy of the particular care of that same Almighty who willed at once the whole means by which infinity was replenished with its worlds."

What then is this real proposition of the 'Vestiges?' It is simply, exhibited in all its naked crudeness, the belief *that a law is an entity*—a Logos intermediate between the Creator and his works—which is entertained by the Vestigiarian in common with the great mass of those who, like himself, indulge in science at second-hand and dispense totally with logic. To use a phrase of M. Comte's—the mind of the Vestigiarian is in the metaphysic stage, and confounds its own abstraction with objective fact.

A few citations will abundantly bear out what we have said. Thus at p. 307, "natural laws" are said "to produce winds," and are "sometimes unusually concentrated in space or in time, so as to produce storms and hurricanes, by which much damage is done."

Again, at p. 307, we find the following mysterious passage:—"The laws presiding over meteorology, life, and mind, are necessarily less definite, as they *have to produce* a great variety of mutually-related results."

"*Indefinite* laws"! "*which produce results*"*!!*

Once more—

"The Creator, then, is seen to have formed our earth, and effected upon it a long and complicated series of changes, in the same manner in which we find that he conducts the affairs of nature before our living eyes: that is, *in the manner of natural law*. This is no rash or unauthorized affirmation. . . . . It is a point of stupendous importance in human knowledge; here at once is the whole region of the inorganic taken out of the dominion of marvel, and placed under an idea of divine regulation, which we may endlessly admire and trust in." (p. 113.)

And again (p. 114) with regard to the modes of origin of organic bodies.

"The Eternal Sovereign arranges a solar or an astral system, by dispositions imparted primordially to matter; he causes, by the same majestic means, vast oceans to form and continents to rise, and all the grand meteoric agencies to proceed in ceaseless alternation, so as to fit the earth for a residence of organic beings. But when, in the course of these operations, fuci and corals are to be for the first time placed in those oceans, a change in his plan of administration is required. It is not easy to say what is presumed to be the mode of his operations. The ignorant believe the very hand of the Deity to be at work. Amongst the learned, we hear of 'creative fiats,' 'interferences,' 'interpositions of the creative energy,' all of them very obscure phrases, apparently not susceptible of a scientific explanation, but all tending simply to this,—that the work was done in a marvellous way, and not in the way of nature. Let the contrast between the two propositions be well marked. According to the first, all is done by the continuous energy of the divine will,—a power which has no regard to great or small: according to the second, there is a procedure strictly resembling that of a human being in the management of his affairs. And not only on this one occasion, but all along the stretch of geological time, this special attention is needed whenever a new family of organisms is to be introduced: a new fiat for fishes, another for reptiles, a third for birds; nay, taking up the present views of geologists as to species, such an event as the commencement of a certain cephalopod, one with a few new nodulosities and corrugations upon its shell, would, on this theory, require the particular care of that same Almighty who willed at once the whole means by which INFINITY was replenished with its worlds!"

If the author of the 'Vestiges' really means by law, simply the *mode* in which the "Will of God"—who is ever present in the arrangements of the universe—takes effect, as he says he does, what meaning is there in the passages we have just quoted? If everything is the direct result of the Will of God, what does his theory differ from that of the "learned," at whom he sneers? If the Deity be ever present and phenomena are the manifestation of his will—law being simply a name for the order in which these occur—what is every phenomenon but the effect of a "creative fiat," an "interference," an "interposition of creative energy?" If everything be the expression of the will of a present Deity—as the Vestigiarian affirms when it suits him—the introduction of every "new family or organism,' *must* be an act of "special attention;" and upon his own showing, the Vestigiarian should believe, that the "corrugations and nodulosities" upon a cephalopod's shell, as much require "the particular care of the Almighty," as "the replenishment of infinity with worlds."

But, truly, what an entirely false and mean view of Nature is revealed in this very phrase, what utter snobbism and *philisterei* of conception. What is great and what is small in nature, from whose bountiful hand all things are poured out equally complete and equally perfect? Why should not the chambers of a cephalopod's shell be as worthy of the particular care of the Almighty in their production, as our Vestigiarian himself, or any of the other nebulæ? What are Alps and Andes but "corrugations and nodulosities" upon the mother earth, whose majesty he slanders? What are the worlds, whose magnitude excites his admiration, but "corrugations and nodulosities" upon the bosom of the infinite universe? We are half inclined to doubt whether our author has progressed even so far as the "metaphysic" condition of mind, or whether he has yet emerged from that Fetish worship, where reverence is proportionate to the bigness of the idol.

Totally inconsistent with itself—the product of coarse feeling operating in a crude intellect—the boasted fundamental conception of the 'Vestiges' turns out to be unworthy of serious attention, and might be well left to find it own level, were it not that the Vestigiarian has mixed up and confounded together with his supposed explanation of creation, this "creation" in the manner of law, the totally independent idea, which took its origin in far other heads—that the past may be interpreted by the present; and that the succession of phenomena in past times, took place in a manner analogous to that which occurs at the *present day*. Such a proposition is the base of the modern science of history, whether natural or civil: its truth or falsehood is a perfectly legitimate subject of inquiry, but the result neither increases nor diminishes the "region of marvel."

If with Sir Charles Lyell we affirm that the physical forces at present at work are sufficient to account for the changes undergone by the earth's surface in past ages, we do not render those changes either more or less wonderful than they were before—nor do we in any way account for them —we merely state them in a readily conceivable form.

So, if with the Progressionists, we conceive that species of living beings undergo transmutation at the present day; that this transmutation is from a lower to a higher type; and that all the kinds of living beings which have ever existed upon the earth's surface, have originated in this way; the idea is a perfectly legitimate one, and must be admitted or rejected according to the evidence attainable; but if fully proved, it would not be, in any intelligible sense, an *explanation* of creation; such "creation in the manner of natural law," would, in fact, simply be an orderly miracle.

In truth, every one who possesses the least real knowledge of the methods of science, is perfectly aware that "natural laws" are nothing but an epitome of the observed history of the phenomena of the universe; and to assert that the Creator, from whom these phenomena proceeded, worked in the manner of natural law and that, therefore, there is no scope for wonder, is as if one should say that, in ancient Greece, he worked in the manner of Grote's History, and that, therefore, there is nothing remarkable in Greek civilization—that is to say, the phrase is simply ridiculous and unmeaning.

On the other hand, if by the expression, "creation took place in the manner of law," we mean only that the new phenomena were correlated together, or succeeded one another in a manner analogous to that in which certain phenomena are correlated or succeed one another at the present day; if we assert that the civilization of ancient Greece was developed in the same manner as the civilization of a new community at the present day, we have a scientific proposition which is intelligible and is capable of proof or disproof; but the demonstration of the analogy of two sets of phenomena, each of which is marvellous, does not, so far as we know, diminish the marvellousness of either. The production of Goethe and Schiller by German civilization, is analogous to that of Shakspeare and Milton by English civilization; but we do not perceive that the *fact* of the origin in either case is thereby rendered less wonderful, or in any way explained.

Whether true or false, then, the scientific basis of the 'Vestiges' cannot bear out its speculative conclusions; for the progression theory, if true, would be no explanation of creation. But has the progression theory any real foundation in the facts of palæontology? We believe it has *none*, and for the following reasons.

In the first place, with respect to plants. We must altogether demur to the assumption at p. 54 of the 'Vestiges,' that "the numerous fungi and other lowly forms, could scarcely have left clear memorials of themselves in the rocks, or in the masses of coal." Lichens, at any rate, are hard and indestructible enough, and had there been a "cryptogamic age," in which the flora was composed of fungi, algæ, and lichens, we see no reason why the two latter should not have been preserved. But so far from there being reason to believe in the absence of higher plants in the early ages, the fact is, as even the author of the 'Vestiges' admits, at p. 59, that in Portugal, and in America, in the lower Devonian and even in the Silurian —that is, the lowest fossiliferous—rocks, not only ferns, but lepidodendra, which are among the highest cryptogamic forms, have been discovered. Even supposing then that the ludicrous classification of plants, quoted with apparent approbation, italics and all, from an article in the 'Quarterly Review,' by our Vestigiarian,* were correct, the first plants would still be the very highest cryptogamia, and not low forms, as they ought to be.

There are two points which should be carefully remembered by every one who would understand the total inefficiency of the progression theory as applied to plants, but which are not mentioned by the Vestigiarian—the first is, that during the carboniferous epoch, ferns existed, so closely resembling those of the present day, that it is doubtful whether they are generically different (Lyell, 'Manual,' p. 310); and secondly, that the lycopodiaceæ and equisetaceæ of those days, were much more highly organized plants than any of their present representatives; so that we can definitely say, as regards the cryptogamia, that since the carboniferous epoch, there has been no advance in some respects, and a very decided falling off in others.

Precisely similar arguments apply to the lowest discovered remains of animals. These have been found in the Llandeilo flags, at the bottom of the Silurian system, and are cystidæ, graptolites, trilobites, and lingulæ—the latter being the oldest and lowest. Lingulæ, however, are anything but the lowest in the scale of organization of their class; they have a well-developed intestine and well-developed hearts, a nervous system, and long, peculiarly organized arms. So far from the brachiopoda being, as our author states (p. 199), "the first animals we meet with in this line, having parts capable of commemorating their existence," there lies beneath them, in the zoological scale, the vast series of the polyzoa, the great majority of which possess hard parts, eminently preservable; to say nothing of the tunicata, which the Vestigiarian, guided by his second-hand information, supposes to be unpreservable (ibid.); while, in fact, their integuments are always woody in composition and often so in hardness.

As to the graptolites, the assertion of the Vestigiarian, at p. 34, that

* "Now we suppose it will be admitted the Cryptogamia, Phænogamia, Gymnosperms, and Dicotyledonous Angiosperms, *constitute a succession, and a progressive one*."

they are "a humble polypian family," is untrue. All the evidence that we have leads us to believe, that they were either pennatulidæ—which belong to the more highly-organized helianthoid polypes—or polyzoa, which are higher still. Here, again, the lower forms of polypes, the sertularians, are eminently preservable; so that had they first existed, there would have been no difficulty in finding them; and as if to spite the progressionists, those forms of animal life which lie below them, the sponges and foraminifera are the most easily preserved of all, from their calcareous spicula and shells; but of these hardly a trace has been found in the lowest strata. This fact is indeed adverted to in the Proofs and Illustrations (p. xii. *et seq.*), where the author, treating of the forms of animals in ascending order, "illustrates" his own geological lore by placing foramenifera (*sic*) *after* polypiaria.

Had the first crustaceans been low forms, we should have had daphnidæ and cypridæ in the Llandeilo flags. What is the fact? The first crustaceans are trilobites, which, there is every reason to believe, resembled the limulus, the highest of the entomostracous crustaceans. However, supposing that the trilobites are very low crustacea, still, in the series of annular animals, annelids are below them, and should have been found earlier. The reverse is the case.

As respects the cystideans, we must remark, that the absence of any living type which at all resembles them, should lead us to be excessively cautious in drawing conclusions as to their real nature, particularly if we consider the very extraordinary facts which Professor Müller's researches have recently revealed to us, with regard to the relation of the adult forms of echinoderms to their larvæ; and which are a sufficient answer to Agassiz's dreams upon this subject, quoted at p. xiii. of the Illustrations. However, the author of the 'Vestiges,' in what he has to say about the crinoids and cystideans, exhibits a more dense ignorance as to the facts of comparative anatomy than is even usual with him. The crinoidea, says he, "might be compared to a lowly kind of star-fish fixed on the top of a flexible stalk, arising from the sea bottom. . . . . It is a very humble animal, only, indeed, a stomach, with arms wherewith to supply itself with food." We have a sort of notion, on the other hand, that some ten or fifteen years ago, one Johannes Müller—of whom our erudite Vestigiarian may, perhaps, have heard, in the course of his laborious and conscientious zoological studies—wrote a long essay upon the organization of the Pentacrinus Europœus, in which he showed that the crinoids have a spiral intestine, provided with a distinct anus,—thus standing higher than some asteridæ, —a very distinct water-vascular system, with ambulatory feet, and a blood-vascular system. In truth, the crinoids are as highly organized as the asteridæ; and the prevalent notion that they resemble the larval forms of other echinoderms, has been long since upset by the discoveries of Müller, Busch, &c., which demonstrate that all echinoderms, so far from being fixed, are locomotive active swimmers in their youngest stage; and that the larvæ of crinoids (comatula) resemble those of the highest holothuriadæ.

As for the other animals of the lower Silurian period, the Annelids and the Molluscs, the assertion at p. 41, that the palæontology of the lower Silurian period exhibits families, "generally speaking, low in their respective lines of gradation," is as little borne out for them, as for the others to which we have referred. The often-repeated conclusion drawn from the nautilus-like form of the shell of Lituites and other lower Silurian genera, that these were tetrabranciate cephalopods, ceases, as Mr. Austen has well shown, to have much weight, when we consider, that if we did not happen to be acquainted with the animal, the same thing would be (and indeed was) said of Spirula; and again, it is more than probable that the shell of Bellerophon is not that of a pteropod, but of a heteropod, the most highly-organized among the mollusca cephalophora.

Such is the Fauna of the lower Silurian strata. It contains animals which are, to use the weakest phrase, far above the lowest in their respective lines, and of the very lowest classes of animals, sponges, foraminifera, and sertularian polypes—all of which are very easy of preservation—it offers few traces.

The facts which we have stated are notorious; they have been insisted upon by Sir Charles Lyell; they are taught every year by Professor E. Forbes, in his public lectures at the School of Mines; they are denied by no one; and it was, therefore, with a feeling closely allied to disgust, that we perused pages 140 and 141 of the 'Vestiges,' in which we find the bare-faced assertion, that the doctrine of the progression of animal forms in time, is "only feebly disputed by one or two geologists;" that "it can be asserted, on the authority of the first naturalists of the age, that, in all the conspicuous *orders* of animals, there have been, in the progress of time, strong appearances of a progress of forms, from the more simple to the more complex;" that "the general fact of a progress in all the orders is not to be doubted;" while there is not the slightest reference to the explanation of the appearance of progression in some groups, afforded by the known laws of bathymetrical distribution, so admirably developed by Professor Forbes. It may be, however, that Forbes and Lyell are the "one or two geologists," whose opinions are treated with so much contempt by our Vestigiarian.

The most prominent argument made use of by those advocates of the progression theory, from whom the Vestigiarian derives his information, is drawn from the nature of the palæozoic fishes. Agassiz, the great investigator of these animals, whose lively fancy has done at least as much harm to natural science as his genius has assisted its progress, maintains that the Ganoid and Placoid fishes of the Devonian epoch, represent the embryonic stages of osseous, or, as he calls them, "more perfect" fishes, in their heterocercal tail, cartilaginous skeleton, and more or less persistent chorda dorsalis. The Vestigiarian, who parades Agassiz on all occasions as *the* philosophical naturalist of the day,—a circumstance in itself sufficiently indicative of his own scientific knowledge and judgment,—greedily seizes upon this notion, re-enforcing it in the Proofs and Illustrations of the present edition by the authority of the writer in the 'Quarterly,' from whom he quotes the following passage:

"It is no argument against the views that naturally arise out of the summary of the facts of Palæontology, as they are now known, to urge that 'the fish and reptiles of the secondary rocks are as fully developed in their organization, as those now living.'—(*Sir Charles Lyell.*) . . . . One of the leading distinctions amongst animals is the position of the skeleton; the great binary division of Lamarck into vertebrata and invertebrata was based upon this distinction; and Cuvier's supplementary labours, which made us better acquainted with the real nature and value of the invertebrate groups, have served in the main to confirm the reality of the great characteristic manifested in the internal or external position of the skeleton.

"We have already adverted to the fact, that no completely ossified vertebrata of a fish had been discovered in the strata of the Silurian and Devonian period. Those strata are of enormous extent, and have been most extensively investigated. As regards the internal skeleton, these primeval fishes were less fully developed than those of the tertiary and existing seas.

"[Their external or dermal skeleton] was not only developed in excess, as compared with the great majority of recent fishes, but presented in its form and structure a closer analogy to the exo-skeletons of invertebrata than that of any known fish which possesses the same system of hard parts well calcified. In Pterichthys, Pamphractus, and Coccosteus—e. g., of the Old Red Sandstone rocks of Scotland, the exo-skeleton presents the form of large plates, either symmetrical, or articulated symmetrically by straight sutures, like the shell of the lobster. The large calcified dermal shield which protected the head of the Cephalaspis, has often been mistaken for that of a trilobite of the division Asaphus." (pp. xxiii.—iv.)

All this is ascribed by the Vestigiarian to Professor Owen, but we really must, however unauthorizedly, interpose to save the learned Professor's reputation, and to protect him against the ascription of supposititious writings, with which his known and published opinions are totally at variance. Is it conceivable that a man who ventures to write upon matters of comparative anatomy should be unacquainted with Professor Owen's Hunterian Lectures upon Fishes? But the whole of Chapter VI. in that excellent work is devoted to a most successful demonstration of the non-embryonic nature of cartilaginous fishes, and the author speaks, not without some contempt, of the progressionists:

"Yet there are some who would shut out, by easily comprehended but quite gratuitous systems of progressive transmutation and self-creative forces, the soul-expanding appreciations of the final purposes of the fecund varieties of the animal structures, by which we are drawn nearer to the great First Cause. They see nothing more in this modification of the skeleton, which is so beautifully adapted to the exigencies of the *highest organized of fishes*, than a foreshadowing of the cartilaginous condition of the reptilian embryo in an enormous tadpole, arrested at an incomplete stage of typical development. But they have been deceived by the common name given to the plagiostomous fishes: the animal basis of the shark's skeleton is not cartilage. . . . .

In like manner the modifications of the dermal skeleton of fishes have been viewed too exclusively in a retrospective relation with the prevalent character of the skeleton of the invertebrate animals." (p. 147.)

And again at p. 148:

"These teleological interpretations of the dermal bony plates may give some insight into the habits and conditions of existence of those Ganoid and heavily protected Placoid fishes which so predominated in the earlier periods of animal life in our planet; whereas these Ganoids and Placoids have hitherto been viewed almost exclusively by the light of the analogy of an embryonic 'Age of Fishes,' or explained by the hypothesis of transmuted Crustacea. Some have gone so far as to affirm that in all those solid parts that cover and shield the exterior of the body of the sturgeon and analogous fishes, 'there is nothing in the least analogous to any part of the internal articulated skeleton of the vertebrata,' but that 'it is entirely a remnant of the superficial shells of the invertebrata.' You would hardly suppose, from these exaggerated expressions, that both Ganoid and Placoid plates are as richly organized and permeated by nutrient vessels as the bones within; and that they present the same microscopic structure as the ossified parts of the endo-skeleton which they serve to protect."

And in addition we find, in the arrangement of fishes "in the ascending series," at p. 47, that the Ganoid fishes are placed *above* the Cycloid and Ctenoid fishes, and the Placoid fishes again *above* them.*

We cannot but think that any man who is acquainted with these published opinions of the Hunterian Professor, and can ascribe the article in the 'Quarterly' to him—or, on the other hand, who, not being acquainted with them, can dare to write on the Palæontology of Fishes, must have a superhuman allowance of the "æs triplex" about his conscience;—perhaps, however, as the incognito of the Vestigiarian does away for the necessity of the presence of this "exo-skeleton" in his countenance, the principle of "*balancement*" may account for its over-development in the other region.

Our critical arm, however, is really weary of smiting this straw giant, and it will be some relief to ourselves and our readers to digress for a short space on to the general question of the organization and position of the Ganoid tribes, inasmuch as they seem to us to form the key of the position of both the progressionists and their adversaries.

The arguments of those who maintain the low position of the Ganoid and Placoid fishes of the Palæozoic period, are the following:

(*a.*) The imperfect development of their vertebral column, and its non-ossification.
(*b.*) The existence of an extensive exo skeleton.
(*c.*) The heterocercal tail.
(*d.*) The tadpole-like appearance of some genera, such as Cephalaspis, &c.; the position of the viscera, anus, &c., in these genera.

(*a.*) To say nothing of Professor Owen's argument as to the possible teleological meaning of the comparatively soft state of the vertebral column—of the fact that it is ossified to a very considerable extent in a manner (*loc. cit.*, p. 147) totally different from that of an embryo†—and of its histological difference from the embryonic tissue, we find that the amount of cartilage in the vertebræ, or the incompleteness of the ossifying process, *bears no relation whatsoever to the position of the fish in the scale, or to the rest of its organization.* This is, we know, a bold assertion, but the facts are open to every one. If we compare, for instance, the vertebra of a shark with that of a pike or salmon, we shall find the amount of osseous matter and its arrangement to differ very little (see Williamson, *loc. cit.*); while, on the other hand, in the Helmichthydæ—a highly interesting class of fishes, to which Professor Kölliker has lately directed particular attention—the vertebral column contains a complete chorda; and though this is surrounded by flexible thickened portions which represent the bodies of vertebra, these are not true vertebræ, but mere thickened and slightly calcified portions of the sheath of the chorda. But these Helmichthydæ unquestionably belong to the division of the Murœnidæ, and, as Kölliker says, "are osseous fishes, almost without bones, with a chorda extending to the skull, and almost avertebrate."

* See, also, Owen, loc. cit., p. 13. "Sharks and rays, called amphibia nantes by Linnæus," in explanation of the phrase "higher fishes," in the text.

† See Professor Williamson: Structure and Development of Scales and Bones of Fishes. Phil. Trans., 1851.

Again, "the skeleton of the Lepidosiren is said by Professor Owen to manifest," upon the whole, the highest grade which is attained in the class of fishes (*loc. cit.* p. 83); but in this very fish the embryonic state of the bodies of the vertebræ as a continuous chondro-gelatinous chord remains, though the neur-and par-apophyses, many cranial bones, and the maxillary, mandibular, hyoidean, and scapular arches are well ossified (*loc. cit.* p. 57). Well worthy of the attention of palæontologists, especially as contrasted with the paragraph from the 'Quarterly,' above quoted, is this important passage, at p. 57 of the same work:

"The fact of many fossil Ganoid fishes showing the same parts of the skeleton petrified and undisturbed, but without a trace of the central elements of the vertebræ, shows *that the transitorial condition of the Lepidosirens skeleton was not uncommon in the primæval members of the class.* So far as the observations of M. Agassiz have extended, not one of the fossil fishes hitherto discovered in the Silurian and Devonian rocks, the most ancient in which remains of that class have been found, manifest a vertebral centrum; and not many have shown neural and hæmal arches and spines."

Here we have the highest authority for believing that the Lepidosiren is the highest member of its class, that it has an unossified vertebral column, and that the Silurian and Devonian tribes resembled it in this respect. What becomes, then, of the reiterated argument as to the inferiority of the Ganoid fishes, drawn from this very fact?

(*b*) It is amusing to find writers who argue to the low position of the Ganoid fishes from their resemblance to vertebrate embryos, in the same breath urging the excessive development of the exo-skeleton as confirmatory of their views. Is it not perfectly clear that so far as the Ganoid fishes have an exo-skeleton, they depart from the organization of a vertebrate embryo, and that, therefore, the two trains of reasoning are inconsistent? So far as their exo-skeleton approaches an invertebrate development, to precisely that extent they diverge from embryos, which have no exo-skeleton. But is it true that the Ganoid exo-skeleton in any way approximates that of the invertebrata, or that it is more developed than that of osseous fishes? We totally deny either statement. The arrangement of the plates upon a Pterichthys, or Holoptychus, or a Coccosteus, is totally unlike anything articulate—unless that notable analogy brought forward by the author of the article in the 'Quarterly,' that they have straight sutures "like the shell of a lobster" ("an M in Macedon and an M in Monmouth"), is to have any weight; or that still more forcible evidence that somebody has "mistaken" the head of a Cephalaspis for that of an Asaphus. The author of the 'Vestiges' has "mistaken" the pen of the 'Quarterly' reviewer for Professor Owen's—would a sane man take this as evidence of the fact?

In the second place, it is not true that the Ganoid exo-skeleton is either more extensive or better developed than that of osseous fishes, unless mere thickness is to be called better development. It seems to be forgotten that a perch has as complete an osseous covering as any extinct Ganoid fish; every scale being as truly a bony structure as the bones of its endo-skeleton; and that the scales of the Gurard and Trunk and File fishes are as thick and as strong as those of any extinct Ganoid.

(*c*) Similar arguments apply to the heterocercal tail-fin; the Lepidosiren, "the highest fish," has no tail-fin at all: a particular in which it is exactly resembled by Coccosteus, Pterichthys, and Cephalaspis; which have been on this ground, among others, relegated to the lowest division. Again, the "Salamandroid Lepidosteus, with its lung-like air bladder," is, among the Ganoid fishes, accounted the highest by reason of the ball and socket structure of its vertebræ, but it has a heterocercal tail; while the Amia, which has ordinary vertebræ, has the tail homocercal. Surely criteria like these, which can be shown to fail in obvious instances, should hardly be applied to so obscure a subject as Palæontology.

(*d*) Perhaps the most singular mode of proving the inferiority of organization of Cephalaspis, &c., however, is by comparing them to tadpoles of Batrachia (Agassiz). Surely, if this argues anything, it is that they are higher than other fishes. The grounds of the comparison are worth noting; they are these: the large head, undistinguished from the thorax; the aggregation of the viscera anteriorly; the position of the anal fin and vent immediately behind the cephalo-thoracic expansion; and the appropriation of the rest of the trunk for locomotion. Any one who will go into the market and buy a sole, may satisfy himself that on these grounds that unhappy fish has hitherto been raised beyond his proper position, and is no better than an upstart tadpole; and the Gymnotus and the Amblyopsis, will no no less learn to "begin with shame to take a lower place."

We must hold, then, until altogether new evidence is brought to bear upon the point, that there is no evidence whatever to show that the extinct Ganoid fishes were not as highly organized as the recent Lepidosiren: that the strong presumption is that they *were*, and therefore that so far from resting content with Sir Charles Lyell's modest supposition, that "the fish of the secondary strata are as fully developed in their organization as those now living," we might very reasonably assume that they were *more highly organized*, inasmuch as the Lepidosiren is more highly organized than any other fish.

It may be readily comprehended what validity there is in the whole argument of the 'Vestiges,' as regards the successive development of life upon our planet, when its foundation appears to be thus baseless and rotten. We have submitted this portion of the work to a more detailed criticism, because its positions are maintained by some whose opinions are entitled to respect; the rest has no such claims upon upon us, and neither space nor inclination allow us to do more than lay before our readers specimens of a farrago, of whose value they may thence judge. We must remind them that this is the *tenth* edition of the book, "with extensive additions and emendations."

The Physiology of the Vestigiarian:—

"Nutriment is converted into these (nucleated cells) before being assimilated by the system." (p. 127.)

"The *volvox globator* can hardly be distinguished from the germ which, after passing through a long fœtal progress, becomes a complete mammifer, an animal of the highest class." [! ! !] (p. 128.)

"The globules of the blood are reproduced by the expansion of contained granules." [!] (p. 128.)

Pages 133—138 are occupied by all the old nonsense about the Entozoa and Mr. Crosse's Acarus.

At p. 144, it is stated that the mammalia in the fœtal state "have a branchial apparatus. Afterwards this goes back, and the lungs are developed from a different portion of the organism." The bronchial clefts are here absurdly mistaken for a branchial apparatus.

Page 145. "Amongst phanerogamous plants, a certain number of organs are always present, either in a developed or rudimentary state." [!] We should be glad to be informed in the next edition what these organs are, and what their number.

At page 166, the Vestigiarian quotes Dr. Carpenter, to the effect that each germ must have a certain peculiar definite capacity of development, and makes the following sapient commentary:

"I would venture to remark that, without seeing the germ of a particular being maintain the tendency to the parental form in the nidus of an animal specifically different from its parent, we are not entitled to assume that it has 'a certain capacity of development peculiar to itself.' Its capacity of development may be quite indefinite, and only bound down to the attainment of the parental form by being kept and nourished by the parent." (p. 166.)

Has this profound naturalist ever heard of a cuckoo, or of an ichneumon fly?

The knowledge of the Literature of the subject possessed by the Vestigiarian. At page 147 we are told:

"Embryonic Development, first surmised by the illustrious Harvey, afterwards illustrated by Hunter in his wonderful collection at the Royal College of Surgeons, Embryonic Development has latterly become a science in the hands of Tiedemann, St. Hilaire, and Serres."

If the Vestigiarian had ever read a page of Harvey, he would find him quoting his predecessor, Fabricius, who like himself by no means "surmised," but worked out development in the chick. No mention of Wolff, or Von Baer!

At p. 171, the theory of the Alternation of Generations, one of the best established and most notorious scientific generalizations of the day, is talked of as a "late curious investigation by a Danish naturalist;" and we are told, patronizingly, that "Such matters are as yet obscure, however highly they may promise in time to illustrate this question." This ignorance is the more unpardonable, as though the author of the Vestiges is totally unacquainted with foreign literature, the perusal of Professor Owen's 'Parthenogenesis,' published years ago, would have been amply sufficient to give him more just ideas. But we suppose it was pleasanter to generalize than to learn.

The Comparative Anatomy of the Author of the Vestiges.—

"The Tunicata are similar in all essential respects [to the Brachiopoda and Lamellibranchiata] except in being of humbler organization." [!] (p. 199.)

"Between the invertebrate animals and the fishes, the junction is tolerably clear at one point. This is where the cephalopodous mollusks connect with such fishes as the myxine or hag, and the lamprey. . . . . *The affinity to the cephalopods is fully admitted.* It is seen in the nature of the skeleton, in the character of the investing skin which ejects a copious secretion whenever the animal considers itself in danger, in the power of respiring through the gill apertures without any dependence for that function on the mouth, and in the eight free filaments seen in some species extending forward from around the mouth." (p. 208.)

This choice morsel of zoological reasoning may be left to itself, especially as a few lines further on, we have the old nonsense about the cephalopod being a vertebrate folded upon itself (which was exposed and set aside twenty years ago by Cuvier) formally reproduced. The assertion which we have signalized by italics is quite an effort of genius as a piece of effrontery.

At p. 210, the humbler forms of fish are said to approach the annelides, which is equally untrue; and a little further on we find it said that the fistularidæ approximate the vertebrata. [!]

Surely we have waded far enough through this lumber-room of second-hand scientific furniture, this attempt to build a tower of Babel heaven-high with half-burnt bricks; at any rate, far enough to convince the reader that however the Vestigiarian may wince under the remark, Professor Sedgwick was quite justified in asserting that he is "not only unacquainted with the severe lessons of inductive knowledge, but possesses a mind apparently incapable of comprehending them."

We look for evidence of knowledge, and we find—what might be picked up by reading 'Chambers's Journal' or the 'Penny Magazine.' We look for original research, and we find reason to doubt if the author ever performed an experiment or made an observation in any one branch of science. We seek for acuteness of thought, and we find nothing but confusion of ideas, and an ignorance of the first outlines of speculation. A spurious, glib eloquence, an affectation of reverence for truth and of scientific modesty, are not wanting to remind the curious observer all the more strongly of the total absence of that careful research and fair representation of both sides of a question, which should be the first-fruits of the latter qualities.

The author of the 'Vestiges' plumes himself greatly, and is much praised by others, for the calm and philosophic style of his book; and he complains bitterly of the opposite tone adopted by certain of his reviewers, especially by Professor Sedgwick. The handling of the Woodwardian Professor may have been a little more rough than should beseem a Cambridge Don; but to a thorough, an earnest, and above all, a genial man, who has made truth the search of his life, and knows the difficulties of the road and the stern practical discipline required for success—to such a man, a Hotspur resting after the extreme toil of the fight, there is a source of wrath, such as the author of the 'Vestiges' is obviously quite unable to understand, in the cool interposition of a mere sciolist with his "hypotheses" in a neat pouncet box, who would have been an astronomer—but for sitting up at night; a geologist—but for soiling his fingers; a physiologist—save for "the dirty and unhandsome corses;" attempting to divide the spoil he was incompetent to win, and cutting his fingers with the weapon he is unable to handle.

But truly, as a man sensitive to criticism, and particular about the preservation of his own incognito, our Vestigiarian conducts himself somewhat oddly. In a note (p. lix., Appendix) to the present addition, forgetting the garb of meekness assumed throughout the text, he ventures to sneer at Professor Sedgwick and the "mechanical department of the one science in which his name has a place," and at Dr. Clark, of Cambridge. It is needless to justify the reputation of either of these gentlemen against such attacks; but we dare venture to predict, that should the author of the 'Vestiges' ever be so ill advised as to let his name be known, it will be found prominent neither in the mechanical nor any other department of even *one* science—unless the science of a Mechanics' Institute, which is about the calibre of our author's, is to be called a mechanical department of science. And however proud our Vestigiarian may be of his notoriety, we may remind him, that had Dr. Clark, of Cambridge, whose extensive knowledge and sound judgment are well known, been so misguided as to write the 'Vestiges' in his student days (as any sharp, careless lad might), he would simply have burnt it subsequently; and yet, though he had thereby escaped being known 'outside the walls of Cambridge,' his scientific reputation, in the mind of every one conversant with these matters, would have stood incomparably higher than if he had published it. Any man of science of ordinary judgment has considered and rejected the notions which the author of the 'Vestiges' advances as great facts.

Not less remarkable than the infelicitousness of his sarcasm is the want of knowledge of the etiquette usual among authors displayed by this unfortunate scientific *parvenu.* An article which appeared in the 'Quarterly,' and to which we have already made frequent reference, is repeatedly quoted, and attributed to Professor Owen, obviously without authority. In any case the attribution of anonymous writings without very good grounds is a proceeding in very questionable taste, and in the present instance it is particularly so; for, to say nothing of that wonderful classification of plants into Cryptogamia. Phænogamia, Gymnosperms, and Dicotyledonous Angiosperms which the Hunterian Professor must feel truly gratified to have laid to his account, the paper in question contains a most unjust and unworthy reference to a gentleman whose scientific zeal, extensive information and kindly readiness in communicating it, have won him the good-will and respect of every one but the writer of that article—we mean Professor Quekett. To ascribe to that gentleman's nearest colleague this underhanded attack upon him, is a most marvellous *bêtise*, not less remarkable than the critical sagacity which would fain make Professor Owen express opinions which are in direct contradiction, as we have shown, to his published works.

In conclusion, we cannot address to our Vestigiarian a peroration so condescendingly benevolent as that with which he leaves Professor Sedgwick, (Proofs, lix.) We do *not* "part with him in perfect good humour," but in a very bad humour. We desire too much to have some value set upon our praise, not to speak boldly where great demerit calls loudly for censure. In the popular mind the foolish fancies of the 'Vestiges' are confounded with science, to the incalculable diminution of that reverence in which true philosophy should be held; and we should be unjust to our readers, and false to our own belief, if we commented upon them in any terms but those of the most unmitigated reprobation.

Review VIII.

1. *De la Prostitution dans la Ville d'Alger depuis la Conquête.* Par E. A. Duchesne, Chevalier de la Légion d'Honneur, Docteur en Médecine, &c.—*Paris*, 1853. pp. 231.

Prostitution in the City of Algiers since the Conquest. By E. A. Duchesne.

2. *Die Berliner Syphilisfrage.* Von Dr. S. Neumann, Vorsitzendem des ärztlichen Comités des Berliner Gesundheitspflegevereins.—*Berlin*, 1852. pp. 74.

The Berlin Syphilis Question. By Dr. S. Neumann.

3. *Die Prostitution in Berlin, u. s. w.* Von Dr. Fr. J. Behrend.—*Erlangen*, 1850. pp. 294.

Prostitution in Berlin, &c. By Dr. Fr. J. Behrend.

4. *Ueber die in Kopenhagen zur Ueberwachung der Prostitution und zur Abwendung ihrer übelen Folgen eingeführten Maassregeln; mit einigen auf Berlin bezüglichen Bemerkungen.* Von Dr. Fr. J. Behrend. pp. 68.

On the Regulations carried out in Copenhagen for controlling Prostitution and preventing its ill results; with a few observations concerning Berlin. By Dr. Fr. J. Behrend.

5. *Note sur la Prostitution dans la Ville de Copenhague, et sur les Mesures prises en Denemark pour empêcher la propagation de la Syphilis, d'après un document manuscrit, communiqué au Congrès Général d'Hygiène.* By M. Braestrup, Directeur de la Police à Copenhague.

Note on Prostitution in the City of Copenhagen, and on the Measures used in Denmark to prevent the propagation of Syphilis; from a Manuscript communicated to the Congrès Général d'Hygiène. By M. Braestrup. In the 'Appendix to the Congrès Général d'Hygiène de Bruxelles.—*Bruxelles*, 1852. p. 416 to 429.

6. *Note pour servir à l'Histoire de la Prostitution en Espagne.* Par M. Ramon de la Sagra.

Note to furnish the History of Prostitution in Spain. By M. Ramon de la Sagra. In the 'Appendix to the Congrès Général d'Hygiène de Bruxelles.—*Bruxelles*, 1852. p. 402 to 415.

7. *Quelles sont les Mesures à prendre pour arrêter le Progrès et diminuer les Inconvènients et les Dangers de la Prostitution et de la Débauche?*

What are the Measures to be taken for arresting the Progress and diminishing the Inconveniences and Dangers of Prostitution and Debauchery? Being the conclusions arrived at, respecting this question, by the 'Congrès Général d'Hygiène.' p. 260 to 262.

8. *Review of the Works of Parent-Duchatelet, Talbot, Ryan, and Mayhew's Letters in the 'Morning Chronicle,' on Prostitution.* ('Westminster Review,' vol. liii. 1850, p. 448 *et seq.*)

(*Concluded from No. 25, p. 126.*)

Having so far described the means whereby prostitution is controlled in Berlin, we are naturally led to inquire into the origin of the system. Dr. Behrend's work, written in 1850, in reply to certain questions proposed by the Minister von Ladenberg, appeared originally in 'Henke's Zeitschrift für die Staatsarzneikunde,' has been reprinted under the above title, and furnishes us with the desired information.

The public documents which refer to this subject being few and imperfect, the historical details have been compiled from Fidicin's 'Geschichte der Stadt Berlin.' Interesting as such details are to those living in the Prussian monarchy, we shall only remark, that the early part of the fifteenth century was the date of opening the first tolerated tax-paying brothel, and that in 1486 the women were obliged to wear a mantle of a particular form. The oldest regulations, dated 1700, remained in force until 1792, and consisted of fourteen clauses, in some respects similar to those now in operation, the first of which reads thus:—"This occupation is by no means legally permitted; it is only endured as a necessary evil." (p. 20, rule 1.)

We in this country tacitly admit the statement contained in this clause, that prostitution is a necessary evil—otherwise, why permit it to exist?—and by so doing, tolerate it, if not in word, at least in fact. The following table, compiled from several parts of the first division of this work, presents, in some degree at least, a statistic of the amount of this class of crime in Berlin, at different periods.

Year.	Number of brothels.	Number of women living in brothels.	Number of prostitutes not residing in brothels.	Total number of prostitutes.
1780	100	800	—	800?
1796	54	190	67	257
1808	50	230	203	433
1844	26	287	18?	305?

Necessarily inaccurate as these numbers are, they most probably give us a glance at the relative, though not at the actual, frequence of prostitution; and allowing for the many changes which the social system of Prussia has passed through from 1780 to 1844, our author is led to the conclusion, "That every such attempt (to prevent public prostitution) has had as its result an increase of clandestine prostitution, with all its sad results." (p. 155.)

From 1700 to 1845, one or other form of regulations and control existed until the latter year, when, against the so-called legalization of crime, an outcry was raised by those who looked upon life from a pulpit only, and believed that the actions of men could be regulated conformably with their own dogmas and theories, nor ceased to complain until the last day of December, when all the brothels were closed, and the occupants, with those who lived by clandestine crime, were sent to their native towns, or conveyed out of the Prussian states. This system was continued till 1850, when the plan described in our last Number succeeded it. The result of this attempt, in 1845, to abolish prostitution forms the subject of the second part of the work under review; and, in reply to the first question, "Has prostitution in Berlin diminished or increased since 1846," it is stated, that while in 1839 between 600 and 700 clandestine prostitutes were known to the police, in 1840, consequent on the number of brothels having been restricted, they increased to about 900; while in 1847, or a year after these houses were closed, 1250 of these unfortunates were known to the authorities; and in 1850, the date of publication of this memoir, the average estimated number was as high as 8000. How great a difference between these thousands and the statistics in hundreds that we published for 1852! Our first proposition, then, to those who, on principles or presumed facts, oppose the establishment of regulations for, and a control over, prostitutes, is—Given about 8000 unfortunate fellow-beings under the abolition system of 1850, and less than 800 while the toleration of 1852 is in operation, to find on which side lie truth, humanity, and christianity?

"Has syphilis diminished since the abolition of brothels?" Such is our author's next inquiry, and to it the following statistics of "der Charite" return a direct reply (p. 181):

Year.		Number of cases of syphilis in Women.		Number of cases of syphilis in Men.		Total.
1845	......	514	......	711	......	1225
1846	......	627	......	813	......	1440
1847	......	761	......	894	......	1655
1848	......	835	......	979	......	1814

As respects the intensity of the disease, it can only be expressed in numbers by representing the average number of days occupied in treating the cases that presented themselves at the same hospital during the above years; and such a table we extract from p. 182:

Year.		In Men. Days.		In Women. Days.		In both sexes. Days.
1845	......	$26\frac{6}{7}$	......	$42\frac{8}{9}$	......	$34\frac{2}{3}$
1846	......	$30\frac{1}{2}$	......	$51\frac{1}{2}$	......	$40\frac{7}{8}$
1847	......	$34\frac{1}{6}$	......	$43\frac{2}{3}$	......	$38\frac{2}{3}$
1848	......	$33\frac{1}{3}$	......	$53\frac{1}{6}$	......	$43\frac{1}{2}$

We have given these tabular details, in the hope of carrying to the reader's mind the conviction of the truth of the conclusions which Dr. Behrend has drawn from these and other statistics—that after the closing of the brothels in 1845, syphilis increased, became more severe in type, and (nor do we see any reason for doubting that our author is equally justified in making this statement) extended widely into the best families, while unnatural crimes became very frequent.

Turning at this point, for confirmation of the baneful effects of closing the Berlinian brothels, to statistics derived from other than the public hospitals, we find in Dr. Neumann's pamphlet much valuable information, based upon the statistics of "der Berliner Gesundheitspflegeverein," a society composed of the workmen of all trades, and similar in many of its objects to our Trade Unions, which, though now abolished by order of, and for reasons best known to, the government, was instrumental in working out many social as well as medical problems. Among the latter is found the question involved in the title of the second work before us; and as neither the limits nor objects of this review allow us to follow the author through his interesting details, which, coming from one already known in reference to the medical statistics of Prussia,* will be read with interest and advantage by those engaged in such investigations, we shall make but one extract from this *brochure*, the value of which is increased by the details having been derived from those of the labouring classes who were treated by the medical officers of the society, and are reported by one who holds opinions in very many respects opposed to those of Dr. Behrend.

* Sur medicinischen Statistik des preussischen Staates, von Neumann, Berlin, 1849.

	Average monthly number of members.	Of every 1000 members there were syphilitic.	Of every 100 sick there were syphilitic.
2nd half of 1849	4901	5·32	13·51
1st half of 1850*	6177	4·59	11·90
2nd half of 1850	8112	3·55	7·78
1st half of 1851	9483	3·20	7·49
2nd half of 1851	10,525	2·83	6·98

The value of this statistic, as proof of the beneficial effects of a control, appears to us to be as evident as is the difference between 5 and 2 per cent.

It is not unimportant to trace, with Behrend, the influence which the stringent regulations, and final closing of the brothels in 1846, had on the number of illegitimate births. The results of his extensive tables are:

"That in the first period (1838 to 1841 inclusive), for about every 60 legitimate children, there were 10 born out of wedlock.

"That in the second period (1842 to 1846 inclusive), for every 54 legitimate, 10 illegitimate children were born.

"That in the third period (1847 to March, 1849, inclusive), for every 52 legitimate, 10 were illegitimate.

"Hence, in the first period every seventh child was born out of the marriage state, while in the third period nearly every sixth child was illegitimate." (p. 202.)

As the number of illegitimate births forms an important element in all inquiries into the morality of a country, a glance at the Registrar-General's Annual Reports may prove not uninstructive or out of place; as those who return from a continental tour, shocked with the immorality in which France is believed to be double-dipped, appear to be unaware that in England "the proportion of children born out of wedlock was (in 1846) 6·7 per cent. It was 7 in 1845, and 6·7 in 1842."†

According to the same authority, at p. 10 of the fifth Report, the proportion of illegitimate births in France is as 71 to 1000; and it appears from the above that in England, in 1845, they reached 64 per 1000. If we then take into account that a far greater number of illegitimate births would be reported legitimate in the latter than in the former country, it will be evident that the number of births out of wedlock is at least as high in moral England as in presumed immoral France!‡

The effects of closing the houses of prostitution were:

"1. A very considerable increase of clandestine crime.

"2. Great increase in the frequence and intensity of syphilis.

"3. A depression of morality, showing itself in the very frequent seduction of immature girls, in the increased profligacy of the married, and in the greater number of illegitimate births.

"4. That the public safety and tranquillity were much endangered." (p. 206.)

The third part of Behrend's book contains, together with their opinions, the cases given by Parent-Duchatelet of Paris, Ponton of Lyons, Tait of Edinburgh, and others, relative to the impossibility of abolishing prostitution by any forms of law; and as it is the duty of all governments to protect society, our author, in general terms, considers by what means a

* Here began the control.

† Ninth Annual Report of the Registrar-General, 1848, pp. 19, 20.

‡ "Of 248,554 children registered, 15,839 were illegitimate; so 1 in 16 children born in England is not born in wedlock. I can discover no grounds for supposing that less than 64 in 1000 English children are illegitimate. The proportion in France is 71 to 1000."—(Fifth Report of the Registrar General. Second Edition, p. 10.)

control can be best established. The recommendation of the formation of a "permanent commission," a Magdalene asylum, an hospital and clinic for syphilis, closes a work which we cannot lay aside without recording our admiration of the untiring, scientific, and, what is far more honourable, humane exertions of Dr. Behrend and Herrn von Hinkeldey, the present President of Police, to the latter of whom Prussia is chiefly indebted for her existing sanitary system; and though the names of those who endeavour to relieve a class scarcely sinning oftener than sinned against, may never be heard among the fashionable loungers of our drawing-rooms, yet such philanthropists may fearlessly appeal to time and eternity for a judgment.

Having completed our account of the Berlinian system, and closed two of the books under review, we resume our sketch of this class of crime, as it at present appears in Europe; and before leaving German territory, add a word or two respecting

Prostitution in Austria, Hanover, and Bavaria.

There exists, as far as we could ascertain, and Professor Sigmund most kindly assisted us, neither printed nor documentary evidence relative to prostitution in Vienna, Prague, &c.; neither in these cities, nor in Grätz, Innsbruck, Pest, Lembergh, or Cracow, is there any control over prostitutes, further than, that if complaint be made against them, they are punished, perhaps, somewhat more severely than if they had not led such a life. Where brothels exist, the police are particularly watchful to prevent a breach of the common law; but as to medical regulations, there are none; and for statistics relative to the extent of syphilis, we refer to the reports of the hospitals, and the elaborate articles by Professors Sigmund* of Vienna, and Wallert† of Prague.

In Hanover and Bavaria, also, we failed to find any form of sanitary regulations relative to this subject; but in the latter country, those who are found affected with syphilis, can, according to an old statute, be punished with imprisonment; if declaration be made, that any woman has propagated the disease, she must submit to be examined by Professor Escherich, judicial physician, and if found syphilitic, she is placed under treatment, and can be punished for having concealed the disease. It may not be uninteresting to observe, that in some of the small university towns, public prostitutes are only known as rare visitors; as by some statute, whether of the kingdom in general, or university in particular, we cannot state, these women can be driven from the town. Yet the result of inquiries that we have made, in Göttingen, for example, to which town these remarks apply, rendered it but too evident that the students were more frequently diseased after each of these visitations, while the spread of the disease continued long after the *public* prostitutes had disappeared. The lesson this teaches is self-evident.

Prostitution as it is controlled in Bruxelles.

Bruxelles, so far-famed for her statistics and sanitary measures, and so

* Aerztlicher Bericht über das k. k. allgemeine Krankenhaus zu Wien im Solar-Jahre, 1850, § 23; 1851, § 43; see also Deutsche Klinik, Nos. 21 to 29, 1851; and for last report, Zeitschrift der k. k. Gesellschaft der Aerzte zu Wien, 1853, Mai Heft.

† Vierteljahrschrift für die praktische Heilkunde, Prag.

justly proud of her "Congrès Général d'Hygiène," cannot, so far as our short stay enabled us to judge, boast of having carried out her sanitary regulations relative to prostitution as completely as Berlin has done. How surprised were we to find, that some physicians would not be seen visiting a brothel of the third class; we had, indeed, and shall long have, to learn, that there are prostitutes too low, *alias* too poor, to be visited by the medical officers of a sanitary system.

Having visited the houses of prostitutes in Bruxelles, in company with the chief medical officer, we presume these houses were fair specimens of others in the same city; and, as regards cleanliness, general regularity, and respect to the officers of the commission, there is room for many improvements. We say this reluctantly; for we brought from Bruxelles very many pleasing remembrances. Having already completed our account of the control used in Berlin, we shall proceed to give an outline of the regulations in force at Bruxelles, so as to enable our readers to judge between them.

The brothels of Bruxelles are of two kinds; first, "*les maison de débauche*," in which live "*les filles des maisons de débauche;*" second, "*les maisons de passe*," which are visited by "*les filles éparses*," and in which they keep their appointments. These houses are distinguished, one from the other, by different coloured lanterns hung over the doors; and if the women who walk our streets were permitted to enter only such houses of reception in which no prostitute lived, they would then be placed under circumstances in one respect similar to these of "les filles éparses" of Bruxelles.

All classes of prostitutes are ordered to be examined twice a week; those who live in brothels of the first and second class are visited by the physicians; while the very poor—we mean the women of the third class, and all those who do not reside in brothels—are obliged to attend at the dispensary. If the latter class attend regularly for four weeks, they are exempt from all tax; if, on the contrary, their attendance be irregular, they can be imprisoned from one to five days. Any one of the women who do not live in brothels, can be examined at her own dwelling, provided that she pays, at the dispensary, three shillings and fourpence, for which she receives four visits, which will be continued as long as the payment be made in advance. Thus the prostitutes of the first and second class brothels are saved the inconvenience of attending at the dispensary, as are also those who live in private lodgings, and who would much rather pay the prescribed amount than be seen going to the office as common prostitutes; while the half-starved, ill-dressed, pauper of the third-class brothel must wait at the dispensary until examined, and then return to, shall we say, her home, where none but her companions, the poorest of both sexes, and an occasional police officer, are ever seen. The prostitute who comes to the dispensary may have been once resident in a first or second class brothel, and was then visited by the physician; it is she, as being poorer, that must now visit him.

It must be evident that the regulations of a sanitary commission should apply equally to all who come under the denomination "prostitutes;" the physician should see no difference between the silk-dressed and slip-shod harlot. The regulations in force at Bruxelles most unwisely make such a difference; and even in the practice of British hospitals, we have seen such distinctions but too commonly made. In truth, a *poor* prostitute pays a bitter retribution for her faults.

The medical staff of the dispensary in Bruxelles is composed of, first, a "superintending inspector," whose duty it is to be present in the dispensary when the examinations are being made, and to visit the houses once a fortnight at least; second, two "medical inspectors," who, during alternate months, examine, one the women in the brothels; the other, those who attend at the dispensary. The date and result of each examination are marked on a card belonging to each woman, in the registries kept in each brothel, and at the dispensary. If a woman be found affected with syphilis, or any other contagious disease, the owner of the brothel must immediately send her in a car to hospital, and on the cure being completed, she is returned her card, and may resume her former life.

Such of our readers as are anxious to learn more of this system, are referred to an extract of the regulations of 1st July, 1844;* but as we are returning homewards, in our search among the sanitary systems of the Continent,

Prostitution as it is at present controlled in Paris,

next claims our attention.

There is no city in which the history of this subject has been so fully considered as in that where the second edition of Parent-Duchatelet's great work† appeared, in 1837. Since that period, many important alterations have been made; and thanks to M. Trebuchet, chief of the medical police, and M. Duval, chief of the "Dispensaire de Salubrité," we possess some information, together with the printed forms relative thereto, which, like those of Berlin, not having, as far as we can learn, heretofore appeared in any other form than as instructions for the use of the staff of this fifth office of the first division of the prefecture of police, require special consideration.

The act of "registration" consists in an entry being made of the name, age, birth-place, residence, previous occupation, and motives which induced to prostitution. This is, in almost all cases, made at the request of the women, as the office does not oblige any one to acknowledge herself as belonging to this unfortunate class, unless she has been frequently arrested for open debauchery, or when, on being attacked with contagious disease, she refuses to submit to the measures which it is the duty of the authorities to enforce, in order to preserve public order and health.

As it conduces to the interest of every member of society to protect public health and morals, and as the government consider it immoral and degrading to receive the earnings of crime, the entire expense of the control over, and treatment of, these women is defrayed out of the public treasury; prostitutes in Paris are therefore exempt from tax, are punished by imprisonment, and never by fines.

The women choose, at the time of registration, to which of the two classes of prostitutes they will belong; they are then registered as "*les filles isolées,*" who have separate dwellings, or as "*les filles de maison,*" who

* Congrès Général d'Hygiène de Bruxelles, 1852, p. 430.
† For review of first edition, see British and Foreign Medical Review, vol. v. 1836, p. 333; and vol. vi. 1837, p. 49.

live in the so-called houses of tolerance; and they can pass from one class to the other after making a declaration expressive of their desire.

A woman on being registered as one of the former class receives a card of this form:

185	{ *Here are entered her name, age, general appearance, residence, &c.*			
Month.	1st Date.	Signature.	2nd Date.	Signature.
January				
February				
&c. &c.				

on the reverse of which are printed the following

"*Duties and Prohibitions imposed upon Public Prostitutes.*"

"The public prostitutes, '*en carte*,' are bound to present themselves to be examined, at least once a fortnight, at the *Dispensaire de Salubrité*.

"They are ordered to show this card (the one we are now describing) whenever required by the officers or agents of the police.

"They are forbidden to induce to debauchery during the day; they cannot appear in the streets until a half hour after the time fixed for commencing to light the lamps, nor at any season before seven o'clock P.M., nor remain after eleven P.M.

"They should wear simple and decent apparel, such as cannot attract attention either by its richness, colour, or extravagant form.

"They are forbidden to appear in their hair.

"They are expressly prohibited from speaking to men who are accompanied by women, or children; and from addressing any one in a loud voice, or with importunity.

"They shall not, at any hour, or under any pretext whatsoever, show themselves at their windows, which ought to be constantly closed, and furnished with curtains.

"They are forbidden to stop in the public thoroughfares, form groups, walk in companies, go and come within too short limits, or be followed or accompanied by men.

"The neighbourhood of churches and temples to within at least sixty-five feet, arcades, public gardens, and all deserted or dark streets and places, are closed against them.

"They are expressly forbidden to frequent public or private establishments where clandestine prostitution is encouraged, as also the *tables d'hôtes*, or to take lodgings where there are boarders, or day-pupils.

"They are equally prohibited to share their lodgings with a mistress, or other prostitute, or to live in furnished lodgings without a special permission.

"When in their lodgings they shall avoid everything that may give cause of complaint from those living near, or passing by.

"Those who act in opposition to these directions, who resist the officers, who give a false name or address, subject themselves to penalties proportionate to the gravity of the offence."

With this card she enters the dispensary, is then examined, and the following form, printed on *white* paper, is filled up:

PREFECTURE OF POLICE. 1st Division. 5th Office. Medical Office. Filles Isolées.	FORM No. 33. Date, ..., 185 .
	.... (*Here her name is entered; and in order to prevent confusion resulting from their frequently calling themselves after some favoured lover, the sur-name of the woman's father is that which, even though she had been married, is always written.*)
	.... living in ...... Street, No. .... Has been visited and found ....
	.... Signature of the Physician on duty.

If she is found in health, her card is marked; with it she departs, leaving the last described form at the office. If, on the contrary, she is found diseased, she is then conveyed to hospital.

The women who are registered as *filles de maison* are examined once a week in the brothels in which they reside, and their state of health is marked in the last pages of a book kept by the mistress of the house, on the first leaf of which is written the name, &c. of the keeper of the house, number, name of the street, and the folio of the registration book, kept at the dispensary, in which are entered the names &c. of the mistresses of brothels. Certain general instructions are printed on the third and fourth pages, to which follow thirty-eight leaves of the following form:

Names of the women living with		
Names and Age.	Date of reception.	Date of leaving.
.... Aged ...... years.		

The last ten pages are divided into two columns, headed "visits," in which the physician writes his report.

All prostitutes are in addition to the regular visits, subjected to special examination, whenever they pass from one class to the other, change brothels, apply for passports, are arrested, or on leaving prison, or hospital. A woman, resident in a brothel, when reported diseased, is sent to the dispensary, where she undergoes a second examination, and is, if the report be confirmed, conveyed the same day to hospital. When a woman is examined previous to passing from one brothel to another, the physician fills up a form similar to that above, but printed on *red* paper.

As regards the opening of a brothel, the office requires a written declaration from the landlord, to the effect that he consents to his house being let for such purpose; and it must be situated at as great a distance as possible from churches, public buildings or offices, schools, &c.

As respects the mistress of a brothel, she is bound to register at the dispensary the names &c. of the women admitted into the house; and when one of them is about to leave it, the mistress must, within twenty-four hours, inform the office of the change. The windows must be furnished with double curtains and the glass ground, or the outside shutters kept constantly closed and locked; at present, cast fluted glass is used in all these houses, or at least in the windows on the ground floor. As some of the women may become diseased between the periodic medical visits, the mistress of the house is directed to bring immediately to the dispensary any woman whom she suspects, or knows to be diseased; she is bound to inform the police of any irregularities that may occur within or without her house; and forbidden to receive minors, pupils of the colleges, or those, in uniform, belonging to the civil or military national schools. The women are not allowed to be absent from the house without sufficient cause; the doors must be constantly closed; and neither bottles nor objects indicating that any kind of drink can be obtained within, are allowed to appear in the windows; neither are they at present permitted to have any name or sign over the door, excepting a number, and that is, we believe, not such a one as should occur in the regular succession of houses in the same street, but is the figure which in the police registration book represents that brothel. Men and women have, by these precautions, been prevented from entering the brothels, presuming them to be hotels, private lodging-houses, &c., a mistake that not unfrequently occurred under the old regulations. The mistresses of the houses in the environs are obliged to send the women in covered cars to the dispensary; and for any violation of the regulations the house can be closed for a longer or shorter period, according to the nature of the offence.

The medical staff is composed of twelve physicians, of whom one has the title of chief physician; and three of these officers are always present at the examinations at the dispensary. There are also a number of agents of the police employed in ascertaining whether all the women attend the examinations, and conveying the diseased to hospital; in addition to which, it is their duty to visit the brothels, in order to discover whether any irregularities are committed, and to prevent clandestine prostitution as much as possible.

If a woman desires to discontinue her dissolute course of life, and shows that she can, by some legitimate occupation, obtain a livelihood, she is kept under observation for a time varying according to the circumstances of her case, and if found to be leading a regular life she is removed from control. If, on the other hand, she wishes to have her name taken off the police books in order to become the mistress of any one, the office generally requires a guarantee from the person with whom she is about to live, that she shall be maintained and protected for a certain period; as it has been found, that without some such security they before long are deserted, return to their former mode of life, and, by practising clandestine prostitution, avoid the inconvenience of being under control.

As regards the internal management of these houses, the office makes no rules, nor is there any printed tariff hung on the walls; but the cost of drink and apartments is so far fixed, that there is no fear of even a stranger being made to pay very exorbitantly, as a complaint to the first police officer that passed would ensure an immediate inquiry, and a report to the office, which might be followed by such summary punishment as closing the house for a time. In the third-class brothels are allowed to be sold all kinds of drinks; and, though at first sight this to us appeared to be a most unwise permission, we were informed that it was tolerated in order to prevent the incessant egress for liquor that occurred when it was forbidden to make sale of drink on the premises; and wines, &c. can be had in all classes of brothels at little more than double their usual price.

Whether it is advisable to have in such houses one large room wherein all the women and visitors may assemble, or a number of smaller rooms, to prevent many persons meeting in one apartment, has been the subject of much consideration; from what we, while visiting all classes of the Parisian brothels, have had opportunities of ascertaining, it appears to us, that as a general rule, the richer the brothel, the greater the amount of immorality. In a brothel of the third class, where all meet in one room, and leave it to go to sleeping-rooms, the great number present prevents any gross sensuality; in the crowd there is protection; while in a first-class house, where each party is shown into a separate apartment, in which are only two or three women, this small company soon lay aside all restraint, and the women violate the commonest forms of decency, even while in the presence of their own sex. We were unable to obtain any statistics relative to prostitution in Paris, and considered ourselves particularly fortunate in being permitted to learn and see so much of the working of this very secret department of medical-police.

Here closes our description of prostitution as it exists, and is controlled, in countries, the medico-educational and sanitary systems of which we have studied during more than two years; had our attention been exclusively confined to the subject of prostitution, the present outline of this important branch of a sanitary system would have been filled in; but when it is remembered that this description forms but one chapter in the history of the sanitary regulations of Hanover, Saxony, Prussia, Austria, Bavaria, Belgium, and France, the impossibility of entering more fully into details will, we believe, be evident; and the first work that heads this article presents us, as we pass from what we have seen to what we have heard and read, with the past and present history of

Prostitution in the City of Algiers.

Charged with a scientific mission to Algiers during the summer of 1851, M. Duchesne undertook the investigation of this subject, in the hope of assisting in improving this branch of hygiene. With this end in view, he proceeds to consider the necessity, origin, and general history of prostitution, passes to prostitution in Algiers, notices briefly its history before 1830, the date of its conquest by the French, and since that period.

In 1850 the gross population of Algiers numbered 54041, the prostitutes 493; of the latter 113 were French, while Spaniards, Mahomedans, Italians, Germans, Anglo-Spaniards, Jewesses, Mulattos, Negresses, Arabs, and Moors, the last two numbering 248, formed this mixed unhappy company.

As regards their ages, it must be remembered that in Africa a female is marriageable between the ages of twelve and fifteen, and it is not uncommon to see a young Arab at fourteen or fifteen years married to a girl of from eight to eleven, or possessing at least a slave; hence, at Cairo, a young Genoese will sell for six thousand piasters, equivalent to 62*l.* 8*s.* Further, it is almost impossible to ascertain at what age the Moors begin to prostitute themselves, for if asked how old they are, the invariable reply is, "I was, or was not, born when the French came." How trumpet-tongued this tells of the conquered yielding to the conquerors! Each of these women pays a tax of ten francs per month, and is provided with a card, on which her name, age, residence, and the date of visit are inscribed. There does not appear to be any distinction between those living in brothels and those who occupy private apartments, further than, that if a woman prefers being visited at home to being examined once a fortnight at the dispensary, she is required to pay the physician three francs for each visit. Respecting this tax, M. Duchesne observes:—"We are convinced that if the system of personal and immoral taxation were abolished, a much larger number of inscriptions would be obtained, and that the proportion of clandestine prostitutes would diminish." (p. 140.)

For a short account of the control enforced in the provinces of Algiers, Oran, and Constantina, we refer to the 183rd and following pages of the original; but as respects the frequence of syphilis, its statistics have not been accurately kept for a time sufficient to render them valuable; and we refrain from making any extracts from the section on male prostitution; suffice it, that in 1850, specific ulceration of the oral mucous membrane occurred fifteen times, of the vaginal fourteen, and an equal number of times on the anal; in the first six months of 1851 it appeared, if we follow the same order of enumeration, eight, five, and eight times; and the author has been assured by a physician who resided many years in the East and elsewhere, that at Smyrna, Alexandria, Malta, and Rome, public prostitutes propose such a choice to their visitors, as may render the women liable to ulcerations of any of these mucous membranes. Lastly, with a scheme of regulations for public prostitutes in Africa, our author closes a work which will be referred to by all who shall hereafter treat of this subject.

Prostitution as it is controlled in Copenhagen.

The law has not made any provision for these women, further than by passing against them most stringent regulations, some of which we have mentioned at page 114; hence prostitutes are completely in the power of the police, who direct that certain acts shall not be committed, and recommend rules for their conduct. The state in this manner avoids even tolerating prostitution; the government only permits the police to do with it as it may please them. The police have formed a series of regulations, which are in many important respects so similar to those already described as enforced in Berlin, that we shall allude to only the leading differences between them.

It will, perhaps, be remembered that in Berlin the women live in brothels, the keeper of which is as responsible as the women themselves for their conduct; whereas, in Copenhagen, all may live separately in private lodgings, and when even four live together, none, save each for herself, is in any degree answerable for their behaviour. The owner, though resident in the house, is no more responsible for their conduct than the proprietor of any house usually is for the prevention of noise, &c. Hence, though there may be several women living in one house, it is not considered to be a brothel; and this enables M. Braestrup, the chief of police, to observe in his report, that "Formal concessions are not granted either to public prostitutes, or to those with whom they lodge; neither are there in Denmark brothels in the ordinary sense of the term, and as they are found in other countries." (p. 417.)

In this statement there appears to us to be a palpable evasion; for, although formal concessions are avoided, full toleration is given, with, however, less printing than in Berlin; we can see no difference between four or more women living, in Denmark, in what they and the chief of police call a private lodging, and the same number resident in Berlin, in what they and the state call a brothel, but that in the latter there are two persons responsible for the conduct of each prostitute—that is to say, herself and the keeper of the house—and that the house can at all times be examined by the police, whereas the sanitary condition of, and crime committed in, the so-called private lodgings in Copenhagen must, we presume, remain unknown to the police until it be complained of, as it is nowhere stated that they possess the power of entry into these, any more than into other private lodgings; and "the agreement between the landlord and the prostitutes is made without the interference of the police, unless there occurs at a later period some difference between the contracting parties." (p. 422.)

From Dr. Behrend's pamphlet, it appears that, in 1850, the number of prostitutes in Copenhagen was 201; and in the report of the chief of police, it is stated that "there are at this moment 68 persons authorized to lodge each from 1 to four prostitutes; in all, they lodge 139; 59 prostitutes have their own dwellings." (p. 423.)

There were, therefore, 198 prostitutes registered in Copenhagen in 1852. The examinations of these women, together with the treatment of the sick in the prisons, are conducted by a physician, who must also attend to all cases to which he may be called by the police.

"Care is taken that these women are all treated in the general hospital (*Almindelige*), and that they do not allow themselves to be treated elsewhere, unless they offer a sufficient guarantee not to propagate the disease, or unless their personal position requires certain considerations—a thing which can seldom apply to the generality of prostitutes." (p. 423.)

We cannot allow this clause to pass without observing, that this regulation can only exist in a country in which the police *feel it their duty* to accommodate certain individuals. Such a regulation is, in fact, very similar to those in force at Bruxelles and Algiers, only that in the latter cases, the women pay for each visit a stated fee, whereas, in Copenhagen, no payment is *required;* but we know the working of such regulation sufficiently well to venture, even at the risk of being accused of conjecturing relative to that which we have not seen, to assert, that as both the woman and her lovers are in the power of the police, these are rarely forgotten by those for whose "personal position," they make "certain considerations." Further, it appears to us most difficult to conceive what such a woman can give as guarantee that she will not propagate the disease; and would it not be far better to allow men to seek women in brothels, the localities of which are soon well known, than permit all these women to walk the streets in search of lovers?

We gladly pass from regulations, which to us appear highly objectionable, to the consideration of the very efficient means used to detect syphilis. Such, for example, as interrogating all persons placed under arrest, who are required to declare if they are then, or have been lately, diseased; which, if they conceal, they render themselves liable to punishment.

"A visit ought to be made each time a ship is about to put to sea. All non-commissioned officers, musicians, and soldiers are examined on entering and leaving the service, and also regularly every month.

"In order to prevent as much as possible the propagation of syphilis, the soldiers who are attacked are obliged to state by whom they were probably infected, in order that immediate information may be given to the police, and that the woman be prevented from communicating the disease to others. To attain the same end, the authorities have the right to use a writ and rescript of the 14th March, 1788, and of the 2nd July, 1790, by which the presidents of the districts are authorized to punish those who will not give early intimation of their disease, by imprisonment on bread and water after their cure." (pp. 426, 427.)

And in 1797, all the inhabitants of many districts were, by order of the chancellor, obliged to submit to examination.

The source from which Dr. Behrend derived the particulars respecting prostitution in Copenhagen is not mentioned, but it is marked as quotation, and is evidently a translation of the MS. which furnished the report of the Congrès Général d'Hygiène; and the following table appears to us deserving of notice, on account of its details being interesting, and as it has not appeared in the French report.

In Year.	Registered in the beginning of the year.	Registered in the course of the year.	Went to service or work.	Given over to the Commission for the Poor.	Sent to prison.	Married.	Left the country.	Died.	Committed suicide.	Total number removed.
1844	297	34	20	13	1	16	2	7	4	63
1845	284	43	14	27	0	24	4	3	0	77
1846	256	18	15	16	2	13	1	2	0	49
1847	241	22	20	17	1	17	2	4	1	62
1848	116	27	15	16	1	16	2	7	0	51
1849	208	19	17	10	1	9	0	6	1	44
1850	196	23	18	7	1	1	0	0	0	27

The numbers for 1850 are the returns for the first half-year only. (From Dr. Behrend's pamphlet, p. 45.)

Prostitution as it existed and exists in Spain.

M. Ramon de la Segra's note informs us that the ancient Spanish statutes made frequent mention of public prostitutes, as also of *barraganas* or kept mistresses; and the laws of Castile directed that a particular form of dress should be worn by them. In the chief cities of Andalusia, under the government of the Arabs, it was tolerated and limited under certain regulations; and in 1486, the *mancebias* or public brothels, in Seville and other cities, were, by the king, given to Alonso Fajardo, chief of the royal table, to whom the women paid rent and other duties. In 1559, at Granada, was fixed the amount to be paid by the women for rooms which contained certain furniture, as also for board, consisting of a certain description of food. It was forbidden to lend them bed-linen, or to receive them into the town before they had been examined by the physician charged with that duty; and he had to make oath whether the woman was then, or had been, diseased.

In 1570, by order of Philip II., the regulations in force in the principal towns of Andalusia were extended to those of Castile, by which it was enacted, that it was of her own free will a woman became a prostitute, and that, without its being in the power of any one to prevent her, she could cease to be such, although she had incurred debts. A surgeon was directed to pay a weekly visit to their houses, and report to the deputies of the consistory those who were diseased, in order that they might be removed to hospital. The keeper of a brothel could not receive into his house any who had not been previously examined, nor allow any one who was diseased to remain there, under a fine of a thousand maravedas, or eleven shillings and sixpence, with thirty days' imprisonment. Each room was to contain certain furniture, and the house to be closed on holidays, during lent, ember week, and on all fast days, under a punishment of a hundred stripes to each woman who admitted men, as well as to the keeper of the house. These and other orders were to be hung up in different parts of the house, under a fine of twenty-three shillings and eight days' imprisonment.

In 1552, in Madrid, a special hospital for venereal patients was formed by Antoine Martin, of the order of St. Jean de Dieu. Matters continued in this state until the beginning of the seventeenth century, when the regulations began to be less and less attended to, and,

"Since that period, under pretence of a hypocritical disdain, the regulations of cities ceased to make mention of brothels, and confined themselves to reviving the severe catholic legislation against women of ill-fame. This unwise and culpable negligence, as respects brothels in the chief cities of Spain, has continued, and still continues, while they increase in a manner dangerous to public morality." (p. 414.)

We have, however, been informed, that most of the first class brothels in Seville pay a private physician, who makes examinations regularly.

To Spain, then, belongs the merit of having been the first to endeavour to control prostitution, and prevent, by medico-sanitary regulations, the spread of venereal disease. True it is, that by order of the French parliament, in 1512, a house was rented in Paris, and in it were lodged all those affected with syphilis. Again, in 1536, l'Hôpital de la Trinité, in the Rue

St. Denis, was appropriated to such patients;* but even as late as 1762, the French police and administration considered any proposition for controlling prostitution, or limiting syphilis, as impracticable and utopian.†

As respects

Prostitution in Portugal,

we have been informed, by a Portugese physician, that neither in Lisbon, nor Madeira, are there any regulations for, or control over, prostitution, though, in the former city, a lock hospital exists; in Madeira, a very bad form of disease is rife.

Prostitution as it exists in Rome.

In Rome, prostitution is permitted, though not legally tolerated. There exists no statistics from which the number of prostitutes can be calculated; and probably the only legal reference to this class of crime consists in an enactment, that both parties shall be punished with three year's imprisonment; but this law most probably refers to cases of adultery rather than of fornication. Public prostitutes are not to be seen in the streets. In the city, there are scarcely any public brothels; "the result of this" (writes a physician long resident in Rome, who kindly furnished us with these particulars) "is not that the Eternal city is more pure in its morals, but that prostitution is clandestine," which, though a crime punishable with imprisonment, prevails to an immense extent; and the law is, in such cases, seldom applied, unless to gratify enmity or revenge. Respecting seduction, the law is more stringent; but, with the concurrence of the authorities, these cases are almost always compromised, and the punishment avoided by marrying, or bestowing a dowry on, the woman. The amount of syphilis in Rome is always very great, and the sixty beds in San Jacomo, for this class of disease, are always filled with women, who, though criminals in the eyes of the law, are never punished by the police; indeed, after the late siege of the city, syphilis prevailed to such an extent, as to deserve the name of a pestilence; yet no accurate information can be had on this subject, as the government endeavours in every way to prevent the actual amount of crime being known; and as a part of this system, the hospital reports are not published.

Before closing this part of the subject, a word respecting

Prostitution in Dublin.

From the statistical returns of the Dublin Metropolitan Police for 1851, p. 48, it appears that in 1848 there were 385 houses occupied, or frequented, by 1343 prostitutes; in 1849 the number of such houses was 330, of the women, 1344; in 1850 there were 272 houses, with 1215 prostitutes; in 1851, 297 houses and 1170 prostitutes;—hence, for every 101 males and 119 females, or out of every 220 persons, one is a prostitute.

* Paris Medical, par M. le Dr. Meding, 1853, t. ii. p. 84.

† Parent-Duchatelet, t. ii. p. 49.

Prostitution as it exists in Cork, Ireland.

From an inquiry made by a gentleman who possessed the assistance necessary for making such an investigation, and has kindly furnished us with the following particulars, it appears that, in 1847, there were in this our native city about 250 prostitutes, living in 80 brothels, besides 100 clandestine prostitutes. Their ages were between 16 and 30, though one of these women has been twenty-five years leading that course of life which she began at eleven years of age. Here are to be found daughters living on prostitution in the house with, and thereby supporting, their father and mother, while the causes and consequences of prostitution are the same in this as in other cities.

We may perhaps assume, that those who have accompanied us so far are in some degree interested in such investigations, and will therefore continue their attention a few moments longer; but as regards those who, from dislike of such subjects, or because their sympathies are not readily awakened on behalf of these hapless fellow-mortals, have perhaps scarcely glanced at the preceding pages, yet who would be not unwilling to disabuse themselves of prejudices unworthy of woman and discreditable to man, of these we have a request to make, which we feel assured they will never regret having granted; and, as reviewers seldom plead so urgently, there must be strong reasons for this request: that the article on prostitution in the 53rd volume of the 'Westminster Review' be read with attention by all who may light upon this page; and if their best sympathies for suffering humanity be not extended to the class for whom we plead, then indeed the facts, statistics, and arguments we have already given, as well as the calculations we are about to enter on, will, we fear, fail to work conviction. Should this request, however, be not acceded to, the courtesy and justice of our readers will, we trust, induce them to follow us to the end, although we are about to conduct them through a calculation which falls as far short of representing the enormity of the evils that result from prostitution, or of expressing the force with which this gigantic ill calls thunder-toned for a remedy, as the horrors of the life that many prostitutes lead are beyond our imaginings.

The number of public prostitutes in some of the principal towns of England and Scotland has been estimated by Talbot and Mayne (the latter one of the Commissioners of Police); to test the accuracy of whose reports we have calculated the proportion of these numbers to those of the population of each town as given in the census of 1851, and formed the following table, which represents the number of males, females, and population of both sexes to each prostitute: for example, opposite London, 10,000 represents the estimated number of these women, and the numbers in the succeeding columns are to be understood to mean, one prostitute to every 104 males, to every 120 females, or to 225 persons of both sexes.

	Number of Prostitutes.	One Prostitute to Males.	Females.	Population.
London	10,000	104	121	225
Liverpool	2,900	43	45	88
Manchester	700	156	169	325
Leeds	700	70	75	145
Edinburgh	800	106	130	236
Glasgow	1,800	87	97	184
Dublin	1,170	101	119	220
Cork	350	113	134	247
England, Wales, and Scotland	50,000	202	213	415

If we take into account the geographical position of these cities, as well as the proportion of the population to the number of prostitutes in each, it will, we believe, be evident, to all at least acquainted with the statistics of this subject, that these figures fall far short of representing the actual number of these women; and if allowance be made for the paucity of prostitutes in country towns, there is every reason to believe, *that to represent the public prostitutes in England, Wales, and Scotland,* 50,000 *is an estimate too low.* Further, we presume there will be no objection made to the assumption, that unless each of these 50,000 prostitutes submitted to at least one act of intercourse during every twenty-four hours, she could not obtain means sufficient to support life.

Though the result of the evidence contained in the first Report of the Commissioners on the constabulary force of England and Wales was, that at that time about two per cent. of the prostitutes of London were suffering under some form of venereal disease, yet we will descend even lower, and presume, that of 100 healthy prostitutes, taken promiscuously from England and Scotland, if each submits to one indiscriminate sexual act in twenty-four hours, not more than *one* would become infected with syphilis: an estimate which is without doubt far too low; yet if admitted to be correct, the necessary consequence will be, *that of the* 50,000 *prostitutes,* 500 *are diseased within the aforesaid twenty-four hours.*

If we next admit that a fifth of these 500 diseased women are admitted to hospital on the day on which the disease appears, it follows, *that there are every day on the streets* 400 *diseased women.* Let it be supposed that the power of these 400 to infect be limited to 12 days, and that, of every 6 persons who, at the rate of one each night, have connexion with these women, 5 become infected, it will follow, *that there will be* 4000 *men infected every night, and consequently* 1,460,000 *in the year.* Further, as there are every night 400 women diseased by these men, 182,500 *public prostitutes will be syphilized during the year; hence* 1,652,500 *cases of syphilis in both sexes occur every twelve months.*

If, then, the entire population had intercourse with prostitutes in an equal ratio, *the gross population of Great Britain, of all ages and sexes, would during* 18 *years have been affected with primary syphilis.* Be it remembered, we do not assert that more than a million and a half of *persons* are attacked every year, but that that number of *cases* occurs annually in England, Wales, and Scotland, though the same individual may be attacked more than once. Although it is evident that all the estimates used for these calculations are (we know no other word that expresses it) ridiculously low, yet we find that more than a million and a half cases of syphilis occur every year, an amount which is probably not half the actual number. How enormous, then, must be the number of children born with secondary disease! how immense the mortality among them! how vast an amount of public and private money expended on the cure of this disease!

As to the means whereby this fruitful source of disease and crime can be confined to certain limits, we hope we have proved that it can be accomplished by means of a *registration, examination, and control of prostitutes;* this, at least, has been the object for which these particulars have been thus prominently brought before the profession, and we trust the authorities may see the paramount necessity for speedy legislation on this subject.

We might dwell upon the fact, that prostitutes are often thieves or their abettors, the receivers of stolen goods, &c.; crimes of which only a part, probably a very small part, are discovered. One of the most important services rendered to society by a control of prostitutes is, that while it enables the authorities to watch over the outgoings and incomings of this class, to learn their histories, and thereby discover how so much crime escaped detection, it at the same time renders apparent the weak points in our criminal code.

We cannot avoid expressing also our strong conviction, that apart altogether from the prevention of syphilis, or of crime, a proper control over prostitutes would be attended with an incalculable benefit; it would assist in the reformation of those women. How many unhappy creatures go down to their graves uncared-for and unpitied, we tremble to think; how many drag on a base and hopeless life, may be guessed at by any one who will penetrate into their fœtid dens. A police control would at once ensure the lowest prostitutes, what we give to our felons, cleanliness, pure air, and some sort of order and discipline. It would give them also, at all times, an outlet from their wretched life, if they wished to avail themselves of it; and it would afford benevolent individuals incalculable aid in their attempts, now, alas! most feeble and ineffectual, to withdraw these women from the inevitable consequences of moral and physical decay.

We have avoided entering into any details as to the kind of control which should be put in force in this country; the time for such a consideration will not arrive until it has been enacted, *that the reception of money for sexual intercourse is a criminal act which places the woman under control;* then, with the aid of the experience derived from the working of the different systems in other countries, and assisted by the conclusions arrived at by the "Congrès Général d'Hygiène," we may be enabled to answer in detail this most important question—"What are the measures to be taken for arresting the progress and diminishing the dangers of prostitution, and for reclaiming, as far as may be, the unhappy women whom circumstances have forced into a life of debauchery?"

T. S. Holland.

NOTE.—We omitted to mention, that all the practitioners in Berlin were by circular requested to assist the Commission; whenever, then, any one comes to be treated for syphilis, his medical attendant inquires "By whom have you been infected?" The physician immediately reports to the Commission that a person, whose name is never given, is now under treatment, and measures are forthwith taken to examine the woman from whom the disease was supposed to have been propagated; or, if she has not been registered, her mode of life is inquired into, and if found faulty, she is examined.

REVIEW IX.

Ueber den gegenwärtigen Zustand der Nerven-physiologie und Nerven-pathologie. Von C. ECKHARD, in Giessen. (Archiv. des Vereins, Band i. Heft 3.) p. 478.

On the present Condition of the Physiology and Pathology of the Nervous System. By C. ECKHARD. (In the Archiv. des Vereins.)

DR. ECKHARD is well known as one of the most able among the physiologists who have followed, with so much ardour, the path opened in the physiology of the nervous system by the wonderful observations of Du Bois-Reymond. He has himself discovered many important facts; his experiments are ingenious; his reasoning powers are great; and he has, therefore, become an authority of weight in this department of physiology.

At the request of Professor Vogel, who was anxious to give to the practitioners of Germany some simple account of the great advances lately made in neurology, and especially in nervo-electricity, Eckhard has written the paper above cited. After apologizing to the Coryphœi of the subject for so popular an account, he has detailed, in a short and lucid form, the chief advances which have been made in the physiology of the nervous system since the publication of Volkmann's renowned article in Wagner's "Handwörterbuch der Physiologie."

It has appeared to us that, though there is little in Dr. Eckhard's sketch which will be novel to those who have studied this subject in the profound pages of Du Bois-Reymond, or of Ludwig, yet that a condensation of it will be interesting to those who have not had time to peruse the number of articles lately written, and that it will appropriately form an appendix to the admirable paper of Professor Tyndall, which we gave in our last number, and which conducted us step by step, without labour or difficulty on our part, to a point of view from which was spread before us the marvellous riches of this promised land. As far as possible, we shall give Eckhard's own words, condensing merely the less important or less certain points.

The advances lately made in our knowledge of this department of physiology concern chiefly the actions of the nervous system, taken as a whole; those actions—i.e., which are common to all parts of this system. To this general nerve-physiology Eckhard first alludes.

GENERAL STATEMENTS.

1. In every living nerve there are electrical streams. It must not be thought that such streams pass, in the motor nerves, from the brain or cord to the muscles, and in the sensory nerves, from the extremities to the centres. Such is not the case. Every nerve is composed of a quantity of isolated molecules, endowed with electrical properties. In every nerve an endless number of little, or so to speak, local electrical streams are present; and when, during experiments, an electrical effect is produced on the multiplicator, this is owing to these elementary currents, each of which produces a certain effect, according to its strength.

2. Every irritation, be it electrical, chemical, thermal, or mechanical,

produces, if the nerve be in connexion with a muscle, or with some sensitive point in the brain (so as to make the muscle contract, the brain feel), an alteration in the condition of the molecules, and in the molecular stream; which alteration is shown by the multiplicator, and becomes then visible to the eye.

3. In every *electrical* irritation of a muscle-nerve, the muscle contracts only during variations in the strength of the current, and remains tranquil when the stream is equable. Nevertheless, although not denoted by the muscle, *all parts of the nerve, and not simply those included in the current*, suffer electrical changes, to which Du Bois has given the name of "electrotonic." We shall have to return to this, and in the meantime beg our readers to refer to our last number, page 141, for a further description of this state.

4. In respect of electrical properties no difference has been detected between motor and sensory nerves.

TRANSMISSION OF THE NERVOUS IRRITATION.

The important researches of Helmholtz* have shown, that in the frog the nervous irritation passes at a rate of 85 to 88 feet in a second of time, and in man at a rate of about 200 feet in a second. The temperature of the nerve exerts a great influence on the transmission, which is more rapid when the temperature is higher.

As now the rapidity of this transmission is infinitely below the speed with which electricity passes along a wire (viz., 15,980 miles in a second), it is evident that the transmission through the nerves is not identical, is not even analogous, with the passage of common electricity along a wire. It is therefore supposed, that in the case of the nerves, the irritation passes by communication from one nervous molecule to another, in the same way as sound passes by the movement of one particle of air against its neighbour. It is supposed that this transmission takes place, less by any material shock of one nervous molecule on another, than as the result of opposite electricity acting from a distance (*die Folge gegenseitiger electrischer Fernwirkung*). Du Bois-Reymond explains it thus:—Suppose a number of magnetic needles, whose ends possess opposite electrical conditions, placed in a row in such a way that everywhere opposite poles are placed together. Such an arrangement represents the nerves and their electrical molecules. Apply now a magnet to one of the end needles; the result is that the first needle moves; its movement acts then on the second needle; this acts on the third, &c.; and so, by this "electrical far-working," the most remote needle is at last affected.

The transmission of the nervous activity is, then, of electrical nature, but *is not in the form of a galvanic stream; there is no nervous principle, but there are nervous molecules, and transmission along them is accomplished by an electrical agent, influencing molecule after molecule in succession.*

NERVE AND MUSCLE.

The contraction of a muscle is in proportion, not to the *strength*, but to the *variations* in the strength, of an electrical current passed along a

* Müller's Archiv., 1850, p. 276; und 1852, p. 199. See our last number, pp. 141, 142.

nerve; its degree is greater when the variation is greater. Other circumstances, also, affect the contraction, such as the direction of the current and the position of the electrode.

There are three important experiments which have each a peculiar interest.

(*a*) The so-called "paradoxical contraction" is produced by passing an electrical current through a nerve which lies along a second nerve connected with a muscle. The muscle of the second nerve contracts, because in this nerve an "electro-tonic" condition is produced. This will be best explained in Eckhard's own words:

"The essence of electro-tonus consists in this, that at the moment when a current is passed through a portion of a nerve, this (the nerve) along its whole length, and not simply in that part of it included in the circle, is equally electrified; so that, if any two special points of the nerve are connected by a conducting arc, either metallic or fluid, a current passes through the arc, and through the piece of nerve between the chosen points, in the same way as the stream passes in the other piece of nerve, which is inclosed in the galvanic current. Thus, if a nerve, *a*, is placed close to a nerve, *b*, which is connected with a muscle, and if a portion of the nerve *a* is included in a galvanic current, then the *whole length* of the nerve *a* is thrown into a state of electro-tonus. As the nerve *a* lies close to the nerve *b*, two points of *a* may be chosen at will, and may be connected by *b*; the stream must pass also always through *b*. The nerve *b* is therefore *suddenly* traversed by a great number of electrical streams, and its muscle contracts.

"We can, therefore, very easily avoid the 'paradoxical contraction,' if between the nerves, a very good conductor (platinum) or a very bad one (glass) is placed. In the first case the conductor platinum, and not the nerve *b*, forms the connecting arc for the electro-tonic current passing through *a*. In the second case, no current can pass between any two points of the nerve *a*, because there is no conducting arc. I have intentionally dwelt so long over this experiment and its explanation, because its value for the proper understanding of the physiology of the nervous system is very great. It leads us to the important doctrine of the new nerve-physiology, *that the law of isolated conduction is not universally valid, since it does not exist in the case of electrical irritation.* This experiment does, in fact, not show more than that if any nerve is irritated by electricity, not only do those muscles contract which are supplied by this nerve, but also those which are supplied by nerves connected with the electrified nerve; as in a plexus, for example. Some readers may, perhaps, wish in this way to explain the entire vast crowd of sympathies. But we earnestly warn against such precipitation, for, we repeat it, the 'paradoxical contraction' is peculiar to the *electrical* irritation."—pp. 486-7.

(*b*) A second important experiment is the so-named "unipolar contraction," which we may thus shortly describe. It is well known that if a coil of insulated wire, *A*, is placed near a second coil, *B*, and if then the ends of this second coil are brought into contact with the poles of an electric battery, a current passes not only along it, but along the first wire, *A*, also. It was believed, till lately, that this current in *A* (induction current) could take place only when its wire formed a closed circle. Such, however, is not the case; an electric change, not as an electric *current*, but as an electric *tension*, takes place in the wire *A*, even when its ends are *not* joined. Let, for example, one of the ends touch the nerve of a muscle, then as often as the current in the wire *B*, which is connected with the battery, is closed or opened, the muscle which is connected with one free end of the wire *A* contracts, provided the other free end be at the same time touched with the finger. This kind of contraction is called the "unipolar inductive contraction," because, when the circle B is closed, opposite electricity is induced in the two ends of the wire A, which affects the nerve and muscle brought into connexion with it.

(*c*) The third experiment is as follows. The tetanus of a muscle, however produced, disappears when a strong constant stream is passed through it. The explanation is, that the constant stream throws the electrical nerve-molecules into such a condition, that no other irritation leads to movement. This condition is perhaps identical with the electrotonus. It might appear that a remedy will thus be found for tetanus in man, but there are difficulties in passing the currents along the affected nerves.

With respect to *calorific* (*thermische*) irritation of a muscle-nerve, it is found that to cause contraction the temperature must be so high as momentarily to destroy the structure of the nerve. Temperature acts, then, like a mechanical irritant.

The *chemical* irritation of nerves (as those of the frog's foot by alkalies, acids, creosote, &c.) is a subject of great obscurity. It appears certain, however, that the chemical agents which cause the greatest contraction of the muscle, withdraw some element from the nervous substance, and momentarily kill it. The condition of the albumen, and of the water of the nerve, appears to be of special importance. It is proved, for instance, that the loss of water (by drying the nerve over sulphuric acid, or by laying it in finely powdered sugar,) produces contraction of the muscle into which the nerve passes; whether this contraction is owing to withdrawal of the water simply, or to the destruction of some ingredient to whose composition water is essential, is not known. Perhaps, as Eckhard remarks, the feeling of thirst is owing to some such partial deprivation of water.

SENSORY NERVES AND BRAIN.

The effect of irritation of a sensory nerve is essentially dependent on the spot in the nerve where the irritant acts. For example, if the irritant affect the *ultimate distributions* of the nerve, the sensation is within certain limits dependent on the *quality* of the irritant. Thus, we feel temperature, or pressure, according as the irritant is calorific or mechanical.

If, on the other hand, the irritant affects the *stem* of the nerve, and not its ultimate distribution, all kinds of irritants give rise to one common sensation: viz., pain. The sensation has no relation, as before, to the *quality* of the irritant. This law was laid down by E. H. Weber, and may be thus illustrated. Dip the elbow into snow and water, and the nerves of the skin feel at once the special sensation of cold; let the cold, however, penetrate to the trunk of the ulnar nerve, and then, instead of the sensation of cold being more clearly perceived, pain is felt, similar to that produced by pressure on the nerve. Again, it is well known that reflex actions can be called forth by irritating the extremities of a nerve, but not by irritating its trunk.

These facts have led to the supposition, that at the ends of the sensory nerves there must be special organs which perceive the *quality* of the irritant. But as such special structures (corpuscula tactûs) have been found at present only on the hands and feet and in the tongue; and as in ethernarcosis, the sensation of *pain* is blunted, while that of *touch* can remain, we are at present unable to decide in favour of this hypothesis, and we must still assume, that for each of these sensations a special process in the nerve and a corresponding condition of the brain are necessary.

SPINAL CORD.

Eckhard alludes first to the hypothesis of the *cerebral* origin of the spinal nerves, a creed which he evidently seems indisposed to receive. We pass over, however, his arguments for and against this opinion, as a matter which may be more conveniently considered hereafter. Eckhard then states, that in the "commissura alba anterior," a decussation of the nerve fibres of opposite sides takes place. It was known by the dissections of Edward Weber, that the anterior roots of the nerves passed into the anterior white commissure of the cord. More lately, it has been shown that this is the case only with one part of the nerve-fibres, and that besides this, there is an actual crossing from one side to the other. This has not yet been discovered in the case of the posterior commissure. This crossing of the anterior fibres may explain the experiments of Van Deen, Valentin, Stilling, and Eigenbrod, who found, on cutting one half of the cord, that there was imperfect paralysis of the parts below the cut part *on both sides.* Eckhard then announces that, *in respect of electrical irritation, the law of Bell* (that the anterior roots are motor, and the posterior sensory) *suffers exception.* If the posterior (sensory) roots of the nerves are cut, and the *peripheral face of the section* be electrified, movements occur. This result is simply a form of the "paradoxical contraction"—electrotonus is produced in the anterior roots. It is, of course, understood that in the *living body* such a form of irritation does not occur, and for all usual conditions the law of Bell holds good.

BRAIN AND CEREBRAL NERVES.

No advance has been lately made in cerebral physiology, and Eckhard, therefore, at once proceeds to some recent observations on the nerves.

1. *The motor oculi* (third nerve).—Many experiments on frogs and men have shown the influence which this nerve has in causing contraction of the pupil.

2. *Trigeminus* (fifth nerve).—Section of this nerve is known to cause contraction of the pupil. Formerly this was referred to reflex action through the third nerve; but Budge has asserted that, even after section of the third nerve, contraction of the pupil is still produced by section of the trigeminus. At present this assertion remains unconfirmed.

Whether the lingual branch of the fifth nerve is connected with taste, is yet undetermined, though an observation of Lisfranc, who, in operating on the jaw, cut the lingual nerve, and found that taste was lost, confirms previous analogous observations of Hyrtl, Parry, and Romberg.

More important than these observations are the experiments on the flow of saliva. Vella discovered that section of the trigeminus abolished the secretion of saliva; and Ludwig has shown that irritation of the branch going to the submaxillary gland produces increased flow of saliva, and that this flow is owing neither to contraction of the muscular elements in the glands, nor to change in the amount of the pressure of the blood, but apparently to a changed physical condition of the secreting membrane, produced by the irritation of the nerve. In other glandular organs besides the submaxillary, the same result occurs, when their nerves are operated upon.

3. *Facialis* (seventh nerve).—Nuhn has confirmed the statement, that irritation of the facial can cause movements of the palate. It is also shown, that in dogs and rabbits, the facial nerve regulates the secretion of saliva in the parotid. The fibres having this action run in the *chorda tympani*, which nerve, in these animals, sends a branch to the parotid gland.

4. *Glosso-pharyngeus* (part of eighth nerve).—Ludwig and Rahn have shown that irritation of the glosso-pharyngeus produces an increased flow of saliva by reflex action.

5. *Vagus* (part of eighth nerve).—(*a*) *Effect on the heart.*—E. Weber discovered, that after section of the vagi, the heart beats much more quickly; and that by irritation of the vagi through an induction-stream, the heart's action is first lessened in frequency, and is finally brought to a stand still. Some new facts have been now discovered. It has been shown that irritation of *one* vagus suffices to produce cessation in the action of the heart; also not merely *electrical*, but *chemical* irritation, as with chloride of sodium, has the same effect.

(*b*) *Effect on the respiratory organs.*—After section of one vagus, and still more after section of both vagi, the respiratory movements become remarkably slower. It has, therefore, been long supposed that the healthy vagi conduct to the brain some impression from the lungs, which gives rise to respiratory movements. This is rendered probable by the fact that, if the *central* end of the cut vagus be irritated, the respiratory movements increase in frequency, unless the irritation be too great, in which case they cease altogether, in the same way as the heart's action can be arrested by irritating the vagus. After section of the vagi, many animals die from contraction of the glottis; if not, they die in 6-10 days with pathological conditions of the lungs—viz., œdema of the air-cells. Whether this effusion is caused simply by food passing through the paralyzed glottis into the lungs, or through alterations in the activity of the pulmonary capillaries, or by the increased action of the heart, or by paralysis of the nerves of the vessels, is not yet known. Not less uncertain is the often-repeated statement, that the vagus acts on the muscular fibres distributed in the pulmonary tissue.

(*c*) *Effect on the stomach and digestion.*—Bischoff's statement that the vagus exerts motor influences on the stomach, has been abundantly confirmed. After section of the vagi, the gastric juice undergoes qualitative and quantitative alterations (diminution of acidity and of quantity), but these are dependent, not on any specific nervous influence, but, as ascertained by Bidder and Schmidt, from circumstances produced by the section of the vagi, such as the diminution of the supply of water on account of the paralyzed œsophagus.

GANGLIA AND SYMPATHETIC NERVES.

On this difficult branch of neurology Eckhard has little to say, and contents himself with the following general propositions, which we have somewhat abridged:

1. The parts provided with the fibres of the sympathetic are not subject to the will, yet in certain conditions, during emotions, and in some diseases of the brain and cord, they are more under the influence of the cerebro-spinal system.

2. In the system of the sympathetic are contained parts capable of exciting movements independent altogether of the brain or cord, or of external irritation. The ganglia are generally considered to be the seat of these movements, without, however, the many other conceivable possible circumstances being properly discussed.

3. In the sympathetic are sensitive fibres, which have, however, the characteristic of giving no sensations of pressure, temperature, &c., but which nevertheless can, under certain circumstances, have their sensation heightened; as occurs in the pain of peritonitis, for example.

4. All facts hitherto known which have been taken to prove that reflex movements can occur between parts of the sympathetic without the participation of the cerebro-spinal centres, are not free from contradiction. Even in many cases facts which seem to show reflex action between parts of the sympathetic and of the cerebro-spinal system, may be otherwise explained.

The pathology of the nervous system is not discussed in this paper, as its title led us to expect.

E. A. Parkes.

Review X.

Mémoires de la Société de Chirurgie de Paris. Tome Troisième. Fascicules 2 & 3. 4to, pp. 121 ad 376.—*Paris,* 1852–3.

We noticed the first part of the third volume of the Transactions of the Paris Surgical Society in our number for October, 1852; and in the present article we purpose directing our readers' attention to the more interesting papers of the two succeeding parts.

I. *On the Communication of certain Ovarian Cysts with the Fallopian Tube.* (*Tubo-Ovarian Cysts.*) By M. A. Richard.

M. Richard here presents us with an account of autopsies he has made of four bodies of women brought for dissection, in all of whom a cyst, which evidently, in the origin, was simply ovarian, had involved a considerable portion of the Fallopian tube, through which its contents could by pressure be forced into the uterus. The portion of the tube implicated had become much increased in length and thickness, and the folds of its mucous membrane, which are so numerous and resistant, were partly effaced. A distinctly formed aperture was the means of communication between the ovarian cyst and the tube, through which the contents of the former could be forced. Although, however, the portion of the tube which remained in its normal state offered no physical obstacle to the further passage of the fluid, this only passed out, even in small quantities, when a probe was introduced and pressure was applied, the latter alone not sufficing. M. Richard believes that some of the cases described as tubar dropsies have been in reality examples of this occurrence, and that in this way may be explained the course and disappearance of some encysted abdominal tumours.

II. *Observations upon the Employment of Tracheotomy in Croup, with a case in which it was resorted to twice with success.* By M. P. Guersant.

As we have repeatedly noticed in this journal the favourable opinions entertained by M. Guersant and other French surgeons upon the performance of tracheotomy in croup, the merest outline of the present paper will suffice. M. Guersant states that he has now performed the operation 150 times since 1834, and that although his earlier operations were very unsuccessful, those of a later date (whether from their earlier performance, the better mode of operating, or the more judicious subsequent treatment) have furnished far more satisfactory results. Of 40 children operated upon in private practice during 1850, 11 recovered; and of 20 operated upon at the hospital, 7 recovered. During 1851, of 31 operations, 13 were successful at the hospital.

He draws especial attention to one of the immediate ill consequences of the operation—viz., the escape of food by the opening in the trachea, during deglutition. In some cases, only a part of the food escapes, and enough may enter the œsophagus to support the child during the days or even weeks required for the re-establishment of normal deglutition. But where all or nearly all passes out, the child will perish of inanition if it be not fed by the œsophageal tube, passed through the mouth or nares. This complication has occurred 3 times among the 31 last cases; and is further dangerous by morsels of food gaining access to the air-passages, and exciting rapidly fatal inflammation.

M. Guersant relates an interesting case, in which two attacks of true croup occurred at an interval of nearly two years, tracheotomy being resorted to on both occasions with success. In a note, a second case is appended, in which the second attack occurred nine months after the first.

III. *Observations upon Lymphorrhagia and Dilatation of the Lymphatics.* By M. Demarquay.

In this paper M. Demarquay relates two cases of this occurrence, one which came under his own care, and another which was treated by M. Fitzer.

The subject of his own case was a youth of 17, who had always enjoyed good health. While playing with his schoolfellows, he perceived that his right thigh was wet, and on examination he found a fluid, at first clear but soon becoming milky, issued from a particular point. The discharge was arrested by a wet compress. M. Demarquay, seeing him the next day, observed, at the inner and lower part of the thigh, several small, depressible elevations, which he at first mistook for hypertrophied follicles. A recurrence of the discharge taking place in a few days, it was found always to issue from the same point, and with sufficient force to rise a little above the epidermis—resembling the bleeding of a very small arteriole at the bottom of a wound. The fluid, when caught in a glass, rapidly coagulated.

A careful examination was made in the presence of MM. Denonvilliers, Flourens, and Ricord; and, besides the small granular projections mentioned, an elevation three or four centimetres long was observed gently curving from the front to the inner part of the thigh, being also surrounded by these little projections. It was depressible, increased in size during walking, and diminished on repose; so that M. Demarquay had no doubt that it was produced by the varicose dilatation of a lymphatic vessel. Two or three recurrences of the discharge took place, when one that continued nine hours occurred, and would not yield to compression by a handkerchief, as had always hitherto been the case, but required strapping and bandages to arrest it. Several ounces of the fluid were collected, and on examination it exhibited all the characters of lymph.

The termination of the case is not given, but as far as the report goes, it states that the varicose dilatation increased, and that the little granular bodies became transparent and vesicular, discharging lymph on rupture. A number of these were also observed at another part of the thigh, and M. Demarquay regards them as being formed through a dilatation of the superficial network of the lymphatics. The discharge kept increasing in quantity and frequency, so that, at last, walking was always attended with its production.

The case which came under the care of Dr. Fitzer, occurred in a girl aged 16, who had never menstruated. When first seen, she had discovered a number of small granular bodies, which gave her no pain; and Dr. Fitzer found these seated in the midst of a brownish discoloration of the skin, three fingers in breadth, which extended from just below the umbilicus to the dorsal vertebræ. They were small papillary elevations, which disappeared on pressure, and were not at all tender. Called again to her six months afterwards, Dr. Fitzer found that fluid had been discharged from two of these projections that were more prominent than the others. If one of these were compressed, a larger quantity would flow from the other. The fluid was of a milky appearance, and alkaline taste, and gelatinized when collected. On snipping off one of the elevations, he found he could pass a probe for an inch to either the right or the left. Compression and alum proving of no avail, Dr. Fitzer resorted, with success, to the application of the nitrate of silver, the arrest of the discharge, owing to its quantity, having become urgent, as the girl's powers were rapidly failing her. The granular elevations, however, continued persistent, and new ones even appeared. The fluid, when examined, was found to be true lymph.

M. Demarquay is not aware that there are any other cases on record of discharge of lymph from spontaneous rupture of vessels. Nuck and Van Swieten relate cases of lymphorrhagia from wounds of the lymphatics. Assalini states that five pounds of lymph were discharged in three days from a little wound at the inside of the thigh; and the lymph upon which Müller made his microscopical examinations, was obtained from a wound of the foot. Various observations also exist of a dilated condition of different parts of the absorbent system. Baillie met with the thoracic duct increased to the size of the subclavian vein. Mascagni observed dilatation of the chyliferous vessels, and of the lymphatics of the lung. Amussat and Breschet have recorded the enlargement of the crural, iliac, and pre-vertebral lymphatics. Sömmering found those of the thigh varicose, as did Sir A. Cooper those of the cord. Quite recently, a similar condition of the lymphatics of the prepuce, has been noted by MM. Beau and Ricord.

IV. *On some of the Affections which fall under the province of Surgery, as met with in the Insane.* By M. Deguise, Senior Surgeon of Charenton.

The affections here noted by M. Deguise, are almost exclusively those observed during *general paralysis of the insane.* We need not follow him in his description of that now well-known malady, but may proceed to observe that he has found that in patients who are the subjects of it,

1. That fractures unite very slowly, and frequently do not unite at all.

2. Means for retaining the fragments in contact must be employed with great caution, any undue constriction being carefully avoided. The apparatus should also be frequently renewed, not only because such patients are very indocile, but also because in them all sensibility seems annihilated, and eschars form with such promptitude, that if all compression be not avoided, deep and large ulcers are rapidly produced.

3. Surgical operations should only be performed upon such patients when indispensably necessary, as in strangulated hernia, wounds of blood-vessels, &c.

4. Wounds, whether accidental, or resulting from surgical operations, take on at once a bad appearance, and follow an irregular course, furnishing large quantities of ill-conditioned pus, the patient's powers becoming rapidly exhausted. There is but one case in which excellent results attend a resort to the knife,—viz., the occurrence of *anthrax,* frequent enough, indeed, in these patients. Free crucial incisions, made at an early period, limit the extension of the disease, and hasten its termination. This operation, usually so painful, induces, in the majority of these persons, no expression of suffering.

5. Most of these patients pass urine involuntarily. Sometimes this results from the paralysis affecting both the body and neck of the bladder; but there are other cases in which the reservoir is paralysed, but not its neck. Catheterism is then required, and should be repeated whenever the bladder fills. It must be practised with the greatest precaution, owing to the slight resistance the canal offers to the instrument. False passages are made with the greatest ease; and as the slightest wound gives rise to the most serious accidents, many patients die from this cause. A gum-elastic catheter, without a stilette, can alone be safely employed.

6. These patients eat voraciously; and asphyxia sometimes results from accumulation of alimentary substances in the mouth, pharynx, or œsophagus. Usually, the patient can be freed from danger at once by the finger, forceps, or probang; but at other times the asphyxia seems so imminent, that tracheotomy is imperative.

7. The development of gangrenous eschars is the most commonly observed accident, and it takes place with extreme rapidity, in consequence of the patient being always in the lying or sitting posture, and the parts possessing so little vitality. In spite of all treatment that may be adopted, the eschar usually so increases in size and depth as to prove fatal.

8. Senile gangrene is of more frequent occurrence in these patients than in other individuals. Sometimes affections of the valves of the heart, or of the arteries, have been found after death; but in most cases no disease of either heart or vessels is discoverable—the disease being due to the

defective energy of the circulation, and the diminution of the afflux of arterial blood to the extremities.

9. A remarkable circumstance is that wounds of the head, which are of frequent occurrence in persons who fall about so much, heal very rapidly, contrary to what is observed with respect to those of other parts of the body. This may be explained by the excess of energy of the circulation towards the head, out of all harmony, as it is, when compared with that of the rest of the body. This excess is exhibited at the autopsies, when not only the membranes and surface of the brain, but the bones and scalp, are gorged with blood.

10. There is an affection which M. Deguise has never met with, but in these persons,—viz., abscess of the external ear. It is developed with great rapidity, involving the entire cartilage; and if it is not promptly opened, in a very short time the whole of the cellular tissue is destroyed. When called to the case later, several openings are required, the contents of the abscess being then well cleaned out by injections, and a compressive bandage applied. When adhesion cannot thus be obtained, it may be sometimes accomplished by passing in several small setons in succession.

In the course of his paper, M. Deguise refers to several cases exemplifying perversion of sensibility in the insane, especially the subjects of hallucinations and propensity to suicide. He observes also, that he has known patients sometimes cured of their insanity after the occurrence of extensive burns or wounds; but in a far greater number of cases he has found that a large wound and abundant suppuration have led to no advantageous modification of the existing condition.

V. *On Concussion of the Brain.* By M. Fano. *With a Report.* By M. Chassaignac.

In this paper M. Fano endeavours to show from the results of some experiments he has performed, that concussion of the brain, as usually understood, has no existence; and that when death occurs speedily after its production, it is due to extravasation at its base, or to a laceration of its substance. We will, however, give the conclusions in his own words:—

"1. External violence exercised on the skull produces effects varying in degree proportionate to the amount of force employed. 2. When death occurs immediately, or some minutes after the action of the vulnerant agent, a lesion of the brain, which our experiments have made us aware of, is constantly found,—viz., effusion of blood around the bulb: i.e., around the central portion of the nervous system. 3. When death takes place at a somewhat more remote epoch, the same lesion may still be found, or we may have minute sanguineous extravasations in the substance of the brain, which results from a contusion (laceration?) of the cerebral substance. 4. When the violence undergone by the cranium only produces a temporary loss of sensorial and locomotive functions, there is found in the brain only a kind of congestion: i.e., an engorged state of the vessels of the brain, characterized by the presence of a considerable number of bloody points. 5. As a consequence of these conclusions it results: that the morbid condition designated by the name of concussion of the brain, is a pure creation of the mind; and that we must admit, in pathological physiology, that the symptoms of concussion are those of sanguineous extravasation, or of contusion of the brain."—p. 199.

Such a statement as this could not pass unchallenged; but M. Chassaignac's able report on the paper saves us the necessity of criticising it. He observes that, after all that has been written upon the subject, every surgeon must feel there still exists a lacuna—viz., the absence of a constant and regular relation between the symptomatic phenomena of concussion, and a determined anatomical condition characteristic of it: and the question is now whether M. Fano has supplied, in any degree, this desideratum. The desire of discovering an anatomical relationship for a group of symptoms so strongly marked, and of so serious a character as those of concussion, long since led to the admission, by Littré and others, of a special physical lesion—a condensation of the cerebral substance; but M. Chassaignac agrees with M. Fano, in regarding this explanation as quite baseless. M. Fano's own experiments were eight in number, and were performed by striking animals on the head by hammers or pole-axes; but it is sufficient to state that, in the six of the number in which effusion at the base of the brain was found, fracture was also produced; while in the two in which no fracture occurred, there was no extravasation. It is quite evident that no conclusion as to the effects of simple concussion can be drawn from these—two of the number only offering the necessary conditions, and these exhibiting signs, not of extravasation, but of mere cerebral congestion. With respect to the evidence sought by M. Fano among cotemporary observers, that does not help his views much more. The first case referred to was observed by M. Chassaignac himself in 1829, and was an example of pure concussion from a fall on the head. There was found, diffused over many parts of the cerebral substance, what M. Chassaignac calls contusions, consisting in very minute, miliary, sanguineous extravasations. In the original report of this case, he had suggested that such miliary effusions, insufficient to induce symptoms of compression, might give rise to those of concussion; and that such appearances were very likely to be overlooked in autopsies, performed with little or no expectation of finding appreciable lesions. Some of these effusions, somewhat larger than others, might give rise to the persistence of certain partial paralyses, or loss of certain faculties, often attributed to mere concussion. Moreover, such miliary effusions might undergo complete resorption, without leaving any ill effects behind; while in other cases, imperfectly absorbed, they might induce various persistent symptoms, or give rise to a general encephalitis, almost necessarily fatal. All these suggestions were, and are, however, only offered by M. Chassaignac as probabilities, to be confirmed or rejected, by additional experience. Blandin is reported, on the strength of a somewhat similar case, to have not hesitated to declare, that this is the essential change which takes place in concussion, this being, in fact, but the first stage of contusion of the brain. To this M. Chassaignac cannot give his assent, believing that there is, up to the present time, not a sufficiently solid scientific proof of the identity of concussion and cerebral contusion. At all events, these facts lend no support to M. Fano's view, that fatal concussion is dependent upon extravasation around the bulb; but it is desirable that both this point, and the existence of "diffused contusion," should be investigated in all future autopsies.

VI. *On Cysts of the Maxillary Bones.* By M. Forget.

We do not think we can do better with this paper than transcribe the author's conclusions.

"1. The clinical study of cancer and of cysts of the maxillary bones demonstrates that the diagnosis of these affections may be established by differential signs, which generally will prevent their being confounded. 2. There are three varieties of osseous cysts, liquid and solid cysts, and those containing mixed products. 3. Of the liquid cysts, some, as the alveolo-dental, are simple; others, the multilocular or hydatiform, are compound. 4. Although the discrimination of the various kinds of cysts may be difficult in their early stages, their nature may almost certainly be recognised during their ulterior development. 5. The alveolo-dental cyst, whatever its size may be, always occupies a portion of the jaw of limited extent, and the encysted fibrous body observes the same disposition. The multilocular cyst is more diffused, and may involve the entire half of a maxillary bone. 6. The multilocular or hydatiform cyst takes its origin in the cells of the areolar tissue, and is due to a vital lesion and morbid secretion of the medullary membrane, which, changed or modified in its structure, constitutes the lining of the cyst. 7. The encysted productions of the lower jaw never originate in the dental canal, which long continues to efficiently protect the nerve and vessels it contains. This anatomical disposition of osseous cysts constitutes an important differential character between them and cancer. 8. In the course of the evolution of the cyst, the dental canal may become widened, displaced, worn away, or in part destroyed. 9. The general indications for the treatment of these tumours are, to open the cyst, to empty it of its contents, and then to inflame the cavity. The resection of the jaw becomes also sometimes necessary." (p. 247.)

VII. *A Memoir on Neuroma, with a case of Multiple Neuroma.* By M. Houel. *With a Report* by M. Lebert.

As is so frequently the case in the French Societies, the report being here far more valuable than the essay it reviews, we shall devote our attention mostly to it. M. Houel's paper consists, indeed, chiefly in the relation of a very interesting case of multiple neuroma, the autopsy of which M. Lebert believes to be more complete than that of any other on record. To this he appends a general history of the affection, which, to our readers acquainted with Smith's valuable treatise on the subject (to which both M. Houel and M. Lebert do ample justice), would present no novelty.

As was to be expected, M. Lebert dwells much upon the *structure* of neuroma, and the more readily so, as he regards Smith's work defective on this point. It consists in a hypertrophied condition of the neurilema. The hypertrophy is usually compact, but in exceptional cases assumes the form of an ovoid cyst, having dense fibrous walls, and containing a gelatiniform fluid. The compact hypertrophy of the neurilema may itself assume various forms, illustrating anew how arbitrarily the term tumour is employed. The neurilema of a more or less long portion of a nerve may become entirely thickened, furnishing the *cylindrical hypertrophy,* in which microscopic observation exhibits a few nervous fibres equally spread throughout a disproportionately abundant fibrous tissue—a condition, however, almost normal for many of the filaments of the great sympathetic. The more circumscribed hypertrophy is generally interstitial, rarely peripheric. The former constitutes the *interfibrillary neuroma.* The neurilema, in its normal condition, is composed of two parts that are anatomically distinct.

One of these constitutes the envelopes or true aponeuroses of the trunks, branches, and filaments, and divides in a manner analogous to the arborization of bloodvessels. The other, histologically the same, is interposed between the primary fibres, and does not follow the fibres in their numerous dichotomous divisions. In dissecting large neuromas, secondary ones are found, constituted by the simultaneous hypertrophy of a certain number of the sheaths of the branches, or of the nervous filaments themselves.

M. Lebert describes three forms of interfibrillary neuroma. (1.) The *central*, in which there is simultaneous hypertrophy of the sheaths, and the interfibrillary tissue of an entire nervous branch, giving rise to compact, fusiform swellings, which, when multiplied along the course of the nerve, give it a varicose appearance. (2.) The *lateral* form, in which the nerve exhibits a lateral swelling, a common envelope surrounding the nerve and the tumour. On removing this, we find that a certain number of the nervous cords pass the tumour without penetrating it, the neuroma itself being formed by one or more that enter into its composition. (3.) The *diagonal* form is distinguished by the upper end of the nerve expanding over one of the sides of the tumour, the reunion with the opposite end being only found at the diagonal point on the opposite side. *True peripheric neuroma*, consisting in a hypertrophy of the neurilema, without the nerves expanding on its surface or passing into its interior, is very rare. It is very different in cancer of the neurilema; for this is almost constantly peripheric, so that the nerve may be dissected out almost intact, passing below the cancerous deposit.

The fibrous tissue of a neuroma is usually homogeneous, shining, and of a yellowish-white, or giving a bluish reflection. It yields a small quantity of transparent juice on compression. Although containing an enormous multitude of fibres, its fibrous aspect is hardly appreciable by the naked eye, owing to the amorphous and finely granular matter interposed between the fibrillæ of the connecting tissue. The fibres closely cross each other at acute angles, following the axis of the nerve, although in small neuromas, transverse and arciform fibres are met with. On several occasions, nuclei and fusiform bodies of a fibro-plastic nature have been met with.

M. Lebert having occasion to examine a large neuroma, occupying the superior cervical ganglion of the sympathetic, found that all traces of ganglionary cells had disappeared, the tumour consisting only of fibrous tissue, with fatty elements and crystals of cholesterine. This is an additional proof of the error of those pathologists who regard neuroma as a ganglionary transformation of the nerves of animal life: for not only do not the specific elements of nervous ganglions appear in these tumours, but they actually disappear when the ganglions themselves are the subjects of neuroma.

Examining the neuroma which results from the changes the ends of the nerves undergo after *amputation*, in a patient whose thigh had been amputated twenty years before, M. Lebert found the crural and sciatic nerves terminating in olivary swellings, exactly resembling fusiform neuroma. The scattered nervous fibres were found amidst a very abundant, dense fibrous tissue. A certain number of the nervous filaments were rolled into a spiral form at their free extremities, thus imparting a peculiar character to this description of neuroma.

M. Lebert does not agree with those who regard the *painful subcutaneous tubercle* as a neuroma. He has found in this merely fibrous elements, mingled with a few nuclei and fusiform bodies of a fibro-plastic nature; and has never met with the highly-characteristic nervous fibre either at their surface, or in their substance. Moreover, these tubercles usually remain very small, even after a prolonged continuance, while neuroma, whether single or multiple, observes no such limits.

M. Lebert furnishes a brief analysis of the seventeen examples of *multiple neuroma* on record; but as most of these are narrated at length by Smith, we need not farther allude to them. Comparing the cases with each other, however, several interesting observations are deducible. The first is the greater liability of the male sex, and the middle period of life. Of the thirteen cases in which both these points are indicated, eleven were met with in men. In seven, the subjects varied from 30 to 40 years of age, and in five from 20 to 30. In considering the symptoms, we are struck with the much greater gravity of the change which takes place in the general health, than in the nervous system properly so called. With respect to the latter, nothing is constant. Several of the patients have had convulsions; but in some of these they were explicable by other causes, and in most cases they were absent. Numbness and paralysis of the limbs have been observed by no means in proportion to the size and number of the tumours. Again, how is the insignificant amount of pain to be explained in cases in which the entire nervous system is so seriously attacked, and in which mixed and sensory nerves are covered with tumours? Although, however, these patients do not succumb through any direct effect exerted upon the nerves themselves, we derive much information from these cases in respect to the mediate influence of the nervous system on nutrition. The autopsies usually exhibit the integrity of almost all the organs; and yet nutrition has been interfered with to such a point that almost all the patients have died in a condition of exhaustion and marasmus. Pallor, emaciation, anorexia, often with vomiting, diarrhœa, prostration of strength, restlessness, and insomnia, constitute the train of symptoms that successively usher in the fatal termination. Although, too, the period at which the neuroma first became developed, cannot in most cases be ascertained, the second stage of the affection does not vary much in its duration—seldom exceeding five or six months.

VIII. *On Localized Traumatic Paralysis.* By M. Debout.

The French medical periodicals have been much employed of late in detailing the results of M. Duchenne's electro-pathological researches; and if their reports prove correct, a new era in the diagnosis and treatment of paralytic affections would seem to have arrived. The object of the present paper is to exhibit their application to the case of traumatic paralysis, whether produced by injury of the nervous centres, the nervous trunks, or the muscular substance. M. Debout employs the term *localized* for the following reasons. When injury is done, say to the trunk of a nerve, the failure of muscular power is at first only a symptom; but if this injury has been very severe, involving the entire substance of the nerve, not only is there loss of power, but atrophy; and although the integrity of the nervous communication may have become restored, the muscles supplied by this nerve remain paralyzed, the paralysis having become localized. The paralysis has ceased to be a mere symptom, and is now a substantive disease, calling for its special curative treatment. It is for want of duly appreciating this difference, and acting upon it, that so many cases of traumatic paralysis have been pronounced incurable, which persevering treatment would have remedied.

M. Duchenne's researches are of importance, in the first place, in pronouncing how far a given case of paralysis is or is not due to cerebral lesion; for in all cases due to this, he finds the muscles retain their electrical contractibility intact, while in extensive lesion of a mixed nerve, this irritability is lost, and they do not contract under the influence of the electrical current. So, too, as the gravity of the paralysis, and the time necessary for its cure, is always proportionate to the degree in which this electrical contractibility is abolished, the prognosis becomes much more defined. Again, as by M. Duchenne's management of his media, a great perfection has been attained in the direction of the electric current towards a given muscle, this agent is susceptible of much more exact application.

We have not space to do justice to the practical merits of M. Debout's paper; but, as before long it will be necessary to enter into an examination of M. Duchenne's statements in detail, the consideration of the important points they involve is only postponed. We may observe, however, that he adduces, at considerable length, several cases in which the value of the laws of diagnosis laid down by Duchenne seems confirmed; and others in which examples of traumatic paralysis, usually considered hopeless, were advantageously treated by electrical currents.

Besides the papers we have now noticed, there are others, which require merely enumeration. These are, 1. Four cases of operation for vesico-vaginal fistula, in an aggravated form, for which modifications of Jobert's operations were had recourse to by M. Maisonneuve. 2. A case of gangrenous ulceration of the bladder, and consequent infiltration of urine, complicating and rendering difficult the diagnosis of a large scrotal hernia, related by M. Pytha. 3. Some additional cases, related by M. Bonnet, showing the safety and advantage with which angular anchylosis of the knee, consequent on acute arthritis, may be ruptured, after preliminary division of the tendons of the triceps and hamstring muscles. 4. The dissection of a case of chronic hydrarthrosis, by M. Verneuil, in which were found a number of sub-synovial abscesses, simulating fibrous bodies of the knee-joint.

John Chatto.

Review XI.

The Science and Art of Surgery. Being a Treatise on Surgical Injuries, Diseases, and Operations. By John Erichsen, Professor of Surgery in University College, and Surgeon to University College Hospital. —*London*, 1853. 8vo, pp. 951.

We cannot but feel some embarrassment in dealing with this very excellent volume. On the one hand, it would manifestly be impossible to critically consider, within reasonable limits, a work which presents a compendious summary of the present state of knowledge respecting the principles and practice of surgery; and, on the other hand, to dismiss it with a slight and superficial examination, might possibly seem to imply that we had formed no very high estimate of its value. We are, however, compelled to limit ourselves to little more than a meagre bibliographical notice; but our readers may be assured that a review, commensurate in length with the value of Mr. Erichsen's work, would engross a very large portion of our present number—brevity, in fact, is enforced by the nature and necessity of the case; and we can only attempt to exemplify Mr. Erichsen's precision, clearness, and power of condensation, by submitting to our readers a few extracts from his pages.

So far as we can just now remember, Mr. Erichsen is the only late systematic writer who correctly states Hunter's classification of the various modes of union of recently divided parts. The phrase, "union by the first intention," as applied by Hunter, has been so often misstated, that we quote the following from Mr. Erichsen:

"The *direct growing together of opposite surfaces* was termed by Hunter 'union by the first intention,' though this term is not employed in this acceptation by all modern surgeons, some of whom extend it to the next process. When wounds unite in this way, it is by the simple and direct coalescence of the opposed surfaces; and not, as Hunter had supposed, by the interposition of a layer of effused blood becoming the bond of union; or, as others have imagined, by lymph being poured out. Dr. Macartney, who pointed out the error of these doctrines, has shown that in this kind of union there is no intervention of blood or lymph, the process consisting essentially in clean cut parts, laid in opposition, uniting and growing together in the course of a few hours, without inflammation, or any of its products being required to effect the union." (pp. 84, 85.)

Much as has been written respecting aneurism, and its treatment by ligature, there is one curious and interesting fact in the history of that proceeding—viz., that Hunter tied the femoral vein along with the femoral artery, in his first operation, which we do not remember to have seen noticed by any one but Mr. Erichsen. Even he falls into some slight inaccuracies in the following passage:

"Between twenty and thirty years after Sharpe wrote, we find that Hunter introduced that great improvement in the surgical treatment of aneurism—the deligation of the artery at a distance from the sac, and in a healthy part of its course; but this great accession to the treatment of a most formidable disease was but coldly received, and ran some risk of being lost to the world, in consequence of the ill success of earlier operations. In Mr. Hunter's first operation, four ligatures were used, some tight and others slack; the artery was denuded, so that a spatula could be passed under it; and although, in his subsequent operations, Mr. Hunter contented himself with employing but one ligature, yet the vein was included in this." (pp. 131, 32.)

Now, in Hunter's first operation, the four ligatures were not "some tight, and others slack;" but, as may be seen by reference to Sir E. Home's Paper, they were *all* similarly applied—i.e., "So slightly, only as to compress the sides (of the vessel) together." And we are told that—"The reason for having *four* ligatures was to *compress* such a length of the artery, as might make up for the want of tightness; it being wished *to avoid* too great pressure on the vessel *at any one point*." It is thus plain that the ligatures were all drawn equally tight; and we do not know whence has originated the mistaken impression, that Hunter tied his inferior ligature sufficiently tight to interrupt the passage of the blood, and that the three others were applied each more loosely in succession, with the view of moderating the impulse of the blood upon the lower one.

In the next place, it can scarcely be said that "ill success" attended Hunter's "earlier operations." The first patient survived fifteen months; the second died, it is true, but the third, fourth, and fifth recovered; so that four of the first five patients operated on were cured. The cold reception of Hunter's innovation was referrible partly to Pott's unlucky operation—in which, however, he adopted not Hunter's but Anel's method—but chiefly to the repugnance of many surgeons of that day to deviate from the routine they were accustomed to; just as has been, and, perhaps, still is, the case with respect to the treatment of aneurism by compression, though certainly in a very minor degree.

Again, it is inaccurate to say that, "In his subsequent *operations*, Mr. Hunter contented himself with applying but one ligature, yet the vein was included in *this*." This clearly implies that, in the last four of Hunter's "earlier operations," the vein was tied along with the artery, but in point of fact the vein was included in the ligature in the second (and possibly the third) operation only. In Sir E. Home's Report of the *second* case we read: "The artery and vein exposed . . . . *they* were included in the ligature;" and soon after it is said, "After the operation, the superficial veins of the leg became exceedingly turgid and swollen:" in this case, then (the only fatal one of the five), the vein was certainly tied; nothing is said about the vein in the account given of the *third* operation, but if a guess may be ventured, it probably was tied in this case also, though the patient recovered; because it is expressly or impliedly stated, in the accounts given of the *fourth* and *fifth* operations, that the vein was *not* tied. Thus, in the Report of the *fourth* case, it is said—"In performing the operation, the *vein* was *not* included in the ligature, but, *in other respects*, it was similar to the former;" and as concerns the *fifth* case, all that is mentioned respecting the operation is, that "The artery *alone* was included in a single ligature." It appears, then, that Hunter tied the femoral vein, probably in only one, and certainly in not more than two, of his operations.

And here, perhaps, may be most appropriately extracted Mr. Erichsen's observations on the comparative merits of ligature and compression in those cases of aneurism in which the latter mode of treatment is applicable. But previously, we must notice a slight oversight. Mr. Erichsen says: "Pelletan and Dubois appear to have been the first who employed the pressure on the artery above the sac, instead of on the aneurism itself; this was in 1810." But Hunter had previously treated aneurism by compression on the artery above the sac, and in one instance he actually effected a cure; chronologically, therefore, the priority of the cure of aneurism by compression belongs to John Hunter, though practically and scientifically, it does not. The causes of Hunter's accidental success in his first trial of compression, of his failure in the second, and of his final abandonment of the practice, obviously appear on comparing his cases with those reported within the last few years. Mr. Erichsen, as has been already observed, does not mention Hunter's attempts to treat aneurism by compression; and he very explicitly assigns the credit of this great improvement in surgery to the proper quarter, in the following passage:

"The merit of having introduced the practice of compression in the treatment of aneurism into modern surgery, of having given it a definite place in our art, and of having established the true principles on which it acts, incontestibly belongs to the Dublin surgeons; amongst whom the names of Hutton, Bellingham, Tuffnell, and La Carte, deserve especial mention." (p. 494.)

Mr. Erichsen then discusses, with great clearness and impartiality, the value of the practice in question; and after summing-up the arguments, *pro* and *con*, thus proceeds:

"After all, surgeons will eventually be guided in their estimate of the value of the two plans of treatment, not so much by the question of submitting their patients to a slightly more tedious or painful treatment, as to the comparative risk of life attendant upon one or the other method. Upon this point the statistics have yet to be made; partly because the cases of the treatment of aneurism by compression have not as yet been very numerous, and partly because the unsuccessful cases of ligature have not been so commonly published as the successful ones. If, however, we compare the 32 cases of femoral and popliteal aneurism treated in Dublin up to February, 1851, as given by Dr. Bellingham, with the details of the 188 cases of femoral and popliteal aneurism recorded by Norris, in which the artery was ligatured, we shall find, that of the 32 compression cases 26 were cured;—in 1, ligature was applied after pressure failed; in 2, amputation was performed; in 1, death occurred from erysipelas; in 1, from chest disease; and in 1 case the pressure was discontinued. Thus it would appear that 6 out of the 32 failed, being in the proportion of 1 to 5·3 cases; and 2 died, being in the ratio of 1 to 16. Of the 188 cases in which the artery was ligatured, 142 were cured, 46 died, 6 were amputated, in 10 the sac suppurated, and in 2 gangrene of the foot occurred. Thus the deaths after ligature were in the proportion of 1 to 4, and the failures or serious accidents as 1 to 3; showing clearly a very considerable preponderance in favour of the treatment by compression. Besides which, in many patients who recovered after the ligature, various accidents, such as gangrene, erysipelas, secondary hæmorrhage, &c., resulted as the direct consequenees of the treatment, and these do not happen when pressure is employed.

"It should also not be forgotten, that in some cases, such as when aneurism is complicated with heart disease, or occurs in a very broken and unhealthy constitution, in which the operation necessary for the application of the ligature would scarcely, or not at all, be admissible, compression may be safely employed." (pp. 498, 499.)

With reference to the fact, that in ordinary depressed fractures of the skull the internal table is splintered to a greater extent than the external one, Mr. Erichsen says:

"This splintering of the inner lamina of the skull to a greater extent than the outer one has attracted much attention, being of considerable practical moment, and is usually said to be owing to its being more brittle than the external table; this, however, I do not consider to be the only cause. I should rather attribute it to the direction of the fracturing force from without inwards, causing a certain loss of momentum in passing through the outer table; and that thus the inner table is splintered more widely than the outer one, for the same reason that the aperture of exit made by a bullet is larger than that of entry. If this be the true explanation, the reverse ought to hold good if the force be applied in the opposite direction. It is very seldom that we have an opportunity of examining such a case; but a few years ago a man was brought to the hospital who had committed suicide by discharging a pistol into his mouth, and upwards through the brain. The bullet had perforated the palate, and passed out at the upper part of the cranium, near the vertex. On examining the state of the bones, it was found that the outer table of the skull was splintered to a considerably greater extent than the inner one. I have since found, by experiment on the dead body, that this is most generally the case when the blow is struck from the inside of the skull outwards." (pp. 263, 264.)

We happen to have seen one case which goes to confirm the ingenious explanation advanced in the preceding extract. A journeyman butcher was admitted some years since to Stevens's Hospital, under the care of the late Mr. Colles: he had slipped from a step-ladder when close to a hook on which he was about to suspend some meat, and the hook passing between the upper eyelid and the ball of the eye, penetrated the cranium, as subsequently appeared, without producing the slightest external mark of injury. The man died, and on examination it was found that the hook had not only penetrated the roof of the orbit, but had also traversed a portion of the brain, fractured the frontal bone from within outwards at a second point somewhat more than an inch above the centre of the eyebrow; and at that point the external table was broken to more than four times a greater extent than the internal table.

As fracture of the internal table of the skull has been noticed here, we may observe that Mr. Erichsen says (p. 263), that fracture of a portion of the internal table, unaccompanied with fracture of the external table, cannot occur. The accident, no doubt, is a very rare one, but it has happened. S. Cooper, for example, mentions at least one case of the kind; and Berard and Desnouvilliers relate another and a very remarkable one.

In considering fractures of the spine, Mr. Erichsen mentions the following case, which we believe is the only one of the kind on record:

"A woman was admitted into University College Hospital with an injury of the neck, the nature of which could not be accurately ascertained. She was in no way paralyzed, but kept her head in a fixed position. A few days after admission, whilst making a movement in her bed by which she turned her head, she fell back dead. On examination it was found that the spinous process of the fifth cervical vertebra had been broken off short; and was impacted in such a way between the arches of this and the fourth, as to compress the cord. This impaction and consequent compression probably occurred at the time of the incautious movement, thus producing immediate death." (p. 276.)

We must conclude this short notice by recommending Mr. Erichsen's work to the student as a valuable text-book, and to the practitioner as an excellent work of reference to inform him of the state of surgical science and practice up to the date of its publication.

R. C. Williams.

PART SECOND.

Bibliographical Record.

Art. I.—*The Modern Treatment of Syphilitic Diseases, &c.* By Langston Parker, Surgeon to the Queen's Hospital, Birmingham. The Third Edition, entirely re-written.—*London,* 1854. pp. 345.

Mr. Parker's enlarged third edition is much superior to the two former, and contains a great number of practical observations. The general details of his methods of treatment have been so long before the profession, that discussion on them is hardly needed. The second chapter, on the mercurial treatment, is a very judicious one. Mr. Parker has strongly recommended, for some years, the employment of mercurial fumigations, introduced into a vapour-bath; in this way, salivation is seldom induced, and the cure is generally rapid. He has treated 58 cases of indurated chancre by this method, combined with the internal administration of biniodide or bichloride of mercury, with the fortunate result that only one patient subsequently suffered from secondary symptoms. It is incidentally mentioned that, if patients use the ordinary vapour-bath while taking mercury, "salivation very rarely takes place." This observation is of great importance, if it be correct; and must indicate, we should think, that the mercury escapes by the skin as well as, in the ordinary case, by the urine.

We are rather surprised that Mr. Parker, in using mercury, should prefer the biniodide to the protiodide. We have found the latter, by far, more manageable and efficacious than the former. The biniodide is directed to be combined with the iodide of potassium; we think that we have found benefit from giving iodide of potassium a few hours after the mercurial. The mercury soon enters the system, forms its combinations, and accomplishes what it can; it is then ready to be removed, and this process, effected usually by the salts of the blood, especially the chloride of sodium, is hastened, as shown by Melsens, by the iodide of potassium. In this way, salivation is prevented or hindered, and the action of the mercury is accelerated.

The iodide of sodium has been used largely by Mr. Parker, in doses of fifteen grains, three times daily, with the same good results as are derived from the iodide of potassium. He confirms the statement of Gamberini that iodism is infrequent; and therefore iodide of sodium may, in some cases, be advantageously substituted for the iodide of potassium. It does not appear that the bichromate of potash has been employed.

Without committing ourselves to all Mr. Parker's modes of practice, we may say that his treatment appears to us highly judicious. It is certainly surprising how many excellent therapeutical measures are now employed against this disease; so that, perhaps, in the whole history of therapeutics, there is no malady whose every feature has been so minutely investigated, with a view to treatment, as syphilis.

Those who have to contend against this most frequent disease cannot do better than consult Mr. Parker's very useful work.

ART. II.—*Comparative Anatomy.* By C. TH. VON SIEBOLD and H. STANNIUS. Translated from the German, and edited, with notes and additions, by WALDO J. BURNETT, M.D. Vol. i.—*London* and *Boston*, 1854.

EITHER from the immense immigration of Germans into America, or from the natural tendency of the Anglo-Saxon mind towards its parent stock, there can be no doubt that of late years medical literature in America has received a decided bias from the Germans. The influence of the French school, which seemed formerly to have the greatest attractions for Americans, has now waned before the more congenial mode of thought, and methods of investigation, pursued at Berlin, Giessen, and Wurzburg.

The translations from the German published in America certainly surpass in number those published in this country; and works are selected which would not pay in England the expenses of translation and printing. It is a reproach that this should be the case, since it indicates, what is unfortunately but too well known, that the number of readers of first-class books in England is much smaller than might have been expected from the number, wealth, and position of the profession.

The great work on comparative anatomy, by Siebold and Stannius, has not only been well translated by Dr. Burnett, but a number of valuable notes are added, so as to bring up the subject to the present day, and to embody various original observations. The first volume only, the anatomy of the invertebratæ, by Siebold, has reached us, and forms a bulky tome of 466 pages. We shall be anxious to receive the second volume (the anatomy of the vertebratæ), and shall then take an early opportunity of noticing the work at greater length.

ART. III.—*Traité Elémentaire des Maladies de la Peau.* Par MAURICE CHAUSIT, M.D.—*Paris*, 1853. pp. 448.

THE French school of Dermatologists, founded by Alibert, and cultivated afterwards by Biett, is at present specially represented by Cazenave, a pupil of Biett in the early part of his career. Cazenave has succeeded to, and in part eclipsed, the fame of his master. His early works were reflections of the doctrines of Biett; his later treatises have exhibited original powers of no common order. He is understood to be now engaged on the composition of a great work, in which the labours of an active life are to be expressed. In the meantime, in the same manner as he was the mouth-piece of Biett, some of his pupils are commencing to be the channel through which his doctrines reach the medical world.

M. Chausit informs us that he has been, for many years, a pupil of Cazenave, and that he has been co-editor with him of the 'Annales des Maladies de la Peau.' He assures us that he has reproduced, in the work before us, the doctrines of Cazenave with scrupulous fidelity, and according to their latest developments. We believe him to be correct in this statement; and we can recommend the work to those who desire to know the present opinions of this learned dermatologist.

We find by this work that M. Cazenave still adheres to many of those opinions which have, with many dermatologists, shaken the prestige of his name. He still continues to underrate the important discoveries connected with the parasitic plants: thus the plant of pityriasis is not even mentioned; that of tinea tondens (herpes tonsurans of Cazenave) is considered to be an accident; and the doctrine which refers the true favus to a special parasite, is said to be in direct opposition with anatomical and clinical observations.

On these points, and in others which we need not stop to name, Cazenave has long ago formed his opinions, and he does not appear inclined to retract them. He is at present, however, decidedly in the minority, nor do the arguments brought forward by M. Chausit on these points appear likely to alter his position in this respect. Even those, however, who happen to differ on these topics, will welcome a book containing, in short space, the results of a life of careful observation. We most cordially recommend the work, and are sure that every one who reads it will derive great pleasure and instruction from it.

ART. IV.—*The Question Considered, Is it justifiable to administer Chloroform in Surgical Operations, after its having already proved fatal in upwards of fifty cases, when pain can be safely prevented, without loss of consciousness, by momentary benumbing cold?* By JAMES ARNOTT, M.D.—*London*, 1854, pp. 31.

DR. ARNOTT has attempted, with great perseverance, to draw the attention of the profession to the therapeutical applications of refrigerating mixtures applied to the skin. He has shown that the temperature of a part may be reduced to many degrees below zero; that sensation may be completely numbed, and circulation partially arrested, for a considerable time, without subsequent ill effects. He has proved, we think, satisfactorily, that in certain local inflammations of the skin, as in furunculi, erythema, &c., the application of an extreme degree of cold is a very valuable remedial measure. It is also clear that painless operations may be performed during the anæsthesia caused by cold.

Dr. Arnott, however, is not satisfied with these results, but entertains so high an opinion of the anæsthetic powers of his frigorific mixture, that he endeavours to drive chloroform altogether out of use, as being both dangerous and unnecessary. In adopting this argument, he is undoubtedly driving his hobby much too far. If chloroform has in some few instances been fatal, even when proper care has been taken, surely these cases have been abundantly counterbalanced by the improved results of operations performed under its use, and by the removal of the amount of pain which would have occurred without it. Many more lives, we will venture to say, have been preserved by it than destroyed. The application of extreme cold to destroy sensibility, useful as it may be in slight cases, as in onychia, or in puncturing an abscess, can only be of partial application in surgery; and Dr. Arnott's real position should be, not whether cold can be substituted for chloroform—for this is impossible in the majority of cases—but whether chloroform should be disused altogether.

We will venture to say that the disuse of anæsthetic agents is impossible, and if possible, would be most cruel. A few men here and there may perceive too forcibly their disadvantages, and may shut their eyes to their benefits; but the profession and the public have made their decision.

We must, however, admit that in some minor operations, such as those to which we have above referred, the use of cold may probably be advantageously substituted for chloroform: and in pointing this out, Dr. Arnott has conferred a great benefit on operative surgery.

ART. V.—*On Fatty Degeneration.* By the late W. F. BARLOW, F.R.C.S.—*London,* 1853, pp. 92.

THIS little work is a collection of the papers on fatty degeneration, published by the late Mr. Barlow in the 'Medical Times and Gazette,' and which attracted much attention, from the importance of the subject, and the able and enthusiastic way in which it was treated by Mr. Barlow; he had just completed them, when a short illness robbed the profession of one who appeared destined to confer honour upon it.

We are happy to see these papers preserved in a more permanent form. The style is very agreeable, and the subject is well discussed. So rapidly has our knowledge advanced, however, that some statements already require alteration, some general views demand expansion, and various details have to be supplied. We receive the little work, however, as a good epitome of the subject at the time of publication, and we also welcome it as the memorial of a mind, which unfortunately, to use Mr. Barlow's quotation from Dryden, was but too active, and

> "O'er informed the tenement of clay,
> Fretting too soon the body to decay."

ART. VI.—*Reminiscences of a Medical Life, with Cases and Practical Illustrations.* By JONATHAN TOOGOOD, F.R.C.S., Founder of, and late Surgeon to, the Bridgwater Infirmary.—*Taunton* and *London,* 1854, pp. 177.

MR. TOOGOOD is one of the most eminent among our provincial surgeons. He has passed a long life in active practice: and now towards the close of it, he has felt anxious that his great experience should be made available. He has, therefore, thrown together what he considers to be the most important practical points he has learnt during his useful career. The book is composed of notes and cases on a number of subjects, medical, surgical, and obstetrical; and in almost every case some interesting point or practical hint presents itself. The pathology, however, and even the exact diagnosis of the various diseases, are rather superficially dealt with. Perhaps the nature of the book will be best understood, if we say that it is the production of a very able man, who has evidently been immersed in practice, has not had much time for original observations and reflection, and whose mind has naturally been especially bent towards the curative part of his profession. An extract or two from it will best convey an idea of its scope and nature.

In speaking of dropsies, after detailing two or three cases, he thus refers to an heroic practitioner:

"The practice of my old master, the late Mr. Dawe, of Bridgwater, who acquired considerable reputation from his treatment of dropsy and diseases resulting from slow inflammatory action, was very different. During my pupilage I have often seen patients who consulted him, with bloated and livid countenances, oppressed breathing, œdematous extremities, and other symptoms of great obstructions, with greatly diminished secretion from the kidneys, so much relieved that they have continued well for many months together, and by the occasional repetition of the same treatment, have lived for years in comparative health. His plan was to give a dose containing eight grains of calomel, with the same quantity of jalap, and one grain of emetic tartar in the morning. This acted violently on the stomach and bowels, and used, according to his own expression, 'to ruffle his patient a good deal;' during and after its operation he directed a little wine or other cordial; afterwards the patient generally fell asleep, and awoke relieved. He then prescribed three grains of powdered squills, with two of digitalis, every night for six successive nights, and a mixture with bitter infusion, and small doses of tincture of squills and spirit of nitre, twice a day. The digitalis was then omitted, and the other medicines continued for six nights more; and if the symptoms were not much relieved, the digitalis was resumed again. If the case was unusually obstinate, the drastic dose was repeated, and sometimes the dose of the digitalis was increased from two to three grains. But this was rarely done; very little attention was paid to the origin or cause of the disease; the great object was to 'unload' the patient." (pp. 24, 25.)

Another example will suffice:

"Diseases of the skin are generally difficult of treatment, and obstinate of cure. I have been in the habit of employing a remedy which is not in general use, for many years with much success, in one particular affection. It was first suggested to me by the late Dr. Willan, whom I met in consultation on the case of a gentleman who had been tormented for many years with a disease which had baffled every practitioner whom he had consulted (and they were not a few), and resisted all the known methods of treatment. The following case, which I give in the words of a surgeon with whom I saw the patient, will illustrate the particular affection and mode of cure:—

"'E. H., a girl about 16 years of age, who had been liable to slight attacks of psoriasis about three years since, after having undergone great bodily fatigue and mental anxiety, became the subject of a much more aggravated form of this disease than I had ever before witnessed. The skin of the arms, legs, and face, was first affected, and it very rapidly spread over the whole body. The fissures in the bendings of the joints were so extensive that she could scarcely move, and on getting out of bed in the morning the scales fell from her in such quantities, that I could easily trace where she had been; and the eyelids were so retracted, that she could not close them when asleep. I tried Plummer's pill, hydrargyrum cum creta, with various tonics, in conjunction with liquor potassæ. I then had recourse to liquor arsenicalis, and baths of sulphuret of potash, none of which had the slightest effect; and the poor girl's strength failed her so fast that I began to think the disease would destroy her, when fortunately I was induced to consult you, who advised my trying pitch, in the form of pills. I did so, giving her at the commencement ten moderate-sized pills three times a day, and gradually increasing the dose until she took the enormous quantity of ninety pills every day, thirty at a dose. She had not taken the pills more than a week before there was a decided improvement apparent; and in six weeks or two months, she became quite free from every symptom of the disease, and has up to this time continued quite well; and, what is most extraordinary, there is not the slightest mark left on the skin.

"North Petherton, Aug. 6, 1840.

"'My dear Sir,—I regret very much that I did not make notes of the case which I send you, at the time you attended this patient with me, as I am sure the beneficial effects of so simple a remedy cannot be too widely circulated.

"'I remain, my dear Sir, yours truly,

"'R. Strong.

"To Jonathan Toogood, Esq., Bridgewater."

"This is not a solitary case. I have witnessed the efficacy of this remedy frequently. I do not subscribe to the doctrine—that specifics alone cure diseases of the skin—having seen numerous cases of failure, in which constitutional treatment has subsequently succeeded. The best mode of administering the medicine is in the form of pills, composed of three parts of pitch, to one of powdered resin." (pp. 29, 30.)

Art. VII.—1. *The Asylum Journal.* Published by authority of the Association of Medical Officers of Asylums for the Insane. Nos. 1, 2, and 3.

2. *The Dublin Hospital Gazette.* Vol. I., Nos. 1 and 2.

3. *The American Medical Monthly.* Vol. I. January and February, 1854. *New York.*

4. *The Indian Annals of Medical Science, or Half-yearly Journal of Practical Medicine and Surgery.* No. 1. October, 1853.—*Calcutta* and *London.*

During the last three months, four new journals have reached us. The 'Asylum Journal,' edited by an esteemed contributor to this journal, Dr. Bucknill, is intended to be the organ of the officers of asylums. A well-written address from the editor opens the first number; and in the contents of the second number we observe an useful abstract of the late enactments. It will evidently be a very useful periodical for the class for whom it is intended.

'The Dublin Hospital Gazette' is intended as a record of the most interesting cases in the Irish hospitals. It is issued twice a month, at the very moderate price of ten shillings per annum, and to all appearance will furnish a valuable series of cases.

The new American Journal which has reached us, is a good specimen of transatlantic literature. It contains, as usual, original communications, reviews, extracts from journals, and news. Some of the original communications are of considerable value. A long paper by Dr. Bowditch, on Paracentesis in Pleuritic Effusion, contains a great number of cases. It is not yet completed, but when it is so, we shall take care to bring its main results before the English profession.

The 'Indian Annals' is a half-yearly journal, chiefly occupied with original communications, and is intended as the organ of the medical profession in Bengal. It contains many very valuable papers, as will appear on reference to the Chronicle in our present number.

Although the 'Edinburgh Medical and Surgical Journal' is one of the oldest, and has long been one of the most esteemed of the British journals, we might now, in consideration of its new aspect, almost class it among the new journals. It has passed into fresh hands, has been diminished in size, but has gained in vigour. We have little doubt that it will soon appear that age has not shorn it of its power, and that its fresh management will not diminish its prestige.

Art. VIII.—*Summary of New Publications.*

In addition to the works already noticed, and to the 'Transactions of the Medico-Chirurgical Society,' and the 'Guy's Hospital Reports,' which will be hereafter reviewed, we have received works for which an enumeration merely, must at present suffice.

In *Medicine*, the concluding part of the first volume of Virchow's 'Manual of Pathology'* (a manual of a gigantic kind, since it will extend to six great volumes) has reached us. The first volume, now completed, contains 551 enormous pages. The portion lately received contains the remainder of Virchow's contribution, and includes—1. The local alterations in the circulation, such as congestion, fluxion, anæmia, obstructions, dropsy, hæmorrhages, &c.; 2. The general impairments of nutrition, such as gangrene, atrophy, hypertrophy, &c.; 3. A section on parasitic animals concludes Virchow's labours for the present. Professor Vogel almost fills up the remainder of the volume with diseases of the blood; under the headings—abnormities of the red corpuscles, of the white (leukæmia), of the fibrine, albumen, salts, extractives, fat, and alkalinity. Then follow abnormities in the quantity of blood (plethora, anæmia); abnormal presence of hurtful matters in the blood, as sugar, uric acid, oxalic acid, and ammonia, sulphuretted hydrogen, bile, urine, &c. A section on pyæmia, and septicohæmia, and scorbutus, is followed by one on gout and rheumatism. About thirty pages on rachitis and osteomalacia, by Dr. Stiebel, carry us to the end of the volume.

This work is of so extensive a nature, and is so valuable as representing the present aspect of medicine in Germany, that we should think a translation would be eagerly welcomed in this country. The Sydenham Society would, perhaps, find this to be a work suited for their purposes; and, with a little management, they might issue an English edition almost simultaneously with the German.

The 'Reports of the College of Physicians on Cholera,' written by Drs. Baly and Gull, reached us at too late a period for review. Dr. Baly's report is on the cause and transmission of cholera, and is a work of singular power. A body of novel facts are arrayed with great skill, and the inferences from them are clearly brought out. Dr. Baly, while he is no contagionist, in the strict sense of the word, attributes the spread of

* Handbuch der speciellen Pathologie und Therapie, redigirt von R. Virchow (Ersten Bandes Zweite Hälfte). Erlangen, 1854.

cholera, in great measure, to human intercourse; in other words, he advocates the theory of the portability of the disease, as distinguished from its contagion. Dr. Gull treats of the pathology and treatment of the disease, and has contributed, on these heads, a most ample and admirable report.

'The Nature of Cholera investigated' is the title of a work by Dr. French, which has reached a second edition, and which, in former years, attracted a good deal of attention. Dr. French attempts to prove that the disease consists in "paralysis, or paralyzed action of the heart."

The contents of a little book, called the 'Common Sense of Cholera, by a Practical Practitioner,' do not satisfy us of the propriety of its title. It shows, moreover, that a practical practitioner can be as hypothetical as other men.

Mr. Grove has written an interesting little pamphlet on 'Contagion and Infection in relation to Epidemic Diseases.' The common sense we could not find in the pamphlet we have just laid down, is abundant here, although little of novelty can be expected.

'The Laws of Epidemic and Contagious Diseases,' by Dr. James Bird, deals with a subject too vast to be compressed into a little pamphlet of thirty-two pages. The laws are announced as follows:

"1. Certain intrinsic and extrinsic epidemic agents, which suspend normal elimination of excrete materials from the blood, are also disassimilating organic poisons, which give rise to certain excretive febrile phenomena.

"2. Certain excretive febrile phenomena, particularly those of an exanthematous character, produce secondary poisons, capable of multiplying to a great extent like diseases; and the quantity of poison sufficient for producing such diseases is determined by the predisposition, temperament, or constitution of individuals at the time.

"3. The definite secondary poisons, by exhausting the pabulous fermentative matter necessary for their production, generally extinguish the susceptibility of the constitution for a fresh attack.

"4. The morbid poisons are more or less active in proportion to the constitutional susceptibility of individuals, while climate and season modify their activity."

There are several points here which appear to us more than doubtful, and which would have been better modified. We refer especially to the first and second laws. But we shall not attempt, at present, to justify our doubt, but remark merely, that whether Dr. Bird be right or not, he has evidently thought deeply on the subject.

Mr. Dendy has published a little descriptive work on the varieties of Pock, intended to assist the diagnosis between small-pox, varicella, and true and spurious vaccinia. A very well executed chart is appended.

Dr. Child has issued a second edition of his extremely useful work on Indigestion. We can recommend its perusal.

A second edition of Dr. Hughes's treatise on Auscultation has appeared. We have compared it with the former edition, to a considerable extent, and find the alterations not to be numerous—but when they do occur, they are judicious. It is an useful elementary work.

Mr. Spencer Wells has published a work on Gout and its complications; which we shall take an early opportunity of reviewing. Mr. Wells very properly devotes a good deal of space to the "hygienics" of gout, and gives us many useful hints. He speaks very highly of the iodide of potassium, with and without colchicum, in the treatment of the disease.

In *Surgery*, the most important work which has appeared is 'Velpeau's Treatise on Diseases of the Breast.'* At the present moment this treatise is of great interest, in connexion with the much disputed question of the value of the microscopical characters of tumours. Apart from this, it is a valuable work, and it is founded on an immense experience. We shall give as full an abstract of it as possible, in our next number.

The name of Professor Quain, of University College, is sufficient warrant that his work on 'Diseases of the Rectum' is one of no little value; it is written with great clearness and judgment, and will be an extremely good guide for all who have to deal with these troublesome complaints.

Mr. Ashton has also published a sensible work on the same subject. We shall notice both treatises in an early number.

Mr. Adams has published a second edition of his work on the 'Prostate Gland.' The book is so well-known as to require no praise from us.

We omitted to notice in our last number, Mr. Thompson's very complete Jacksonian prize essay, on 'Stricture of the Urethra,' as we intended to review it fully in this number. The review has been unavoidably postponed, but we hope to insert it in our next issue, and can in the meantime anticipate its judgment, by stating that the College of Surgeons have seldom selected a work more worthy of their prize.

Three parts have been issued of the second edition of Mr. Maclise's 'Surgical Atlas.' The sale of this work must have been almost unprecedented, and affords ample evidence both of the utility of the design, and of the success of the execution.

In *Physiology* there has been a deficiency of independent works, though none of journal articles. The physiological school of Oxford has given signs of life, in the issue of an admirable 'Synopsis of the Physiological Series in the Christ Church Museum,' by Dr. Acland. It has been arranged especially for students; and, in addition to elementary instructions, which are intended to point out to beginners the meaning of the preparations, reference is made to the most important works which should be consulted on each topic. We have seldom seen a purpose better and more perfectly carried out, than has been done in the present work.

In *Therapeutics*, a work on the 'Action of Medicines' has been published by Professor Albers, of Bonn.† What is meant by "therapeutical agents" is first discussed, and then the history of therapeutics, the modes of discoveries of remedial agents, their applications, &c., are fully related. The subject is treated with great precision. Dr. George Johnson has re-issued a reprint of his very interesting lectures on the 'Relation between Therapetics and Pathology,' delivered at the College of Physicians. Special attention is directed to the subjects of mental diseases, and the reciprocal influences of mind and body.

In *Chemistry*, the most important work is one just commenced by

* Traité des Maladies du Sein, par A. Velpeau, Membre de l'Institut. Paris, 1854.

† Handbuch der allgemeinen Arzneimittellehre, von Dr. I. F. H. Albers. Bonn, 1854, pp. 300.

Schlossberger,* on 'Animal Chemistry.' The first section only has been published, and contains the chemistry of the cartilagés and bones, and, in part, of the nervous tissue. The part on the cartilages and bones is most elaborately done, and besides being a summary of all observations hitherto made, contains a great number of original observations.

Wöhler's 'Handbook of Inorganic Analysis' has been translated and edited by Dr. Hoffmann. The value of this little work in the laboratory is well known.

In *Medical Jurisprudence* we have only received a treatise by Mr. Knaggs, on 'Unsoundness of Mind considered in relation to the Question of Responsibility for Criminal Acts.' The subject is so important, that we shall reserve Mr. Knaggs's book for the present; it is well written, and does him great credit.

In two readable volumes, Dr. Foissac† has given a general summary of the commonly-known facts of *meteorology*. The application of these facts to the production and course of diseases is not very well done; and the author has not, we think, supplied the blank which exists in this department of medicine. A much profounder study is necessary for this than has been bestowed on it by M. Foissac. Nevertheless, the book contains much interesting matter, and is well worth perusal.

Mr. Massy has written a treatise on the examination of recruits, which we shall notice at greater length hereafter. It arrives opportunely when "war's alarms" are likely to give the profession something to do in such matters.

Under the head of *Miscellaneous Subjects*, we may enumerate the following works:—the fourth edition of the 'Elements of Natural Philosophy,' by Dr. Golding Bird and Mr. Brooke. 'A Contribution to Scottish Ethnology,' by Dr. Beddoe, in which we find the results of an elaborate inquiry into the colour of the eyes and hair in various parts of Scotland. The following are the complexional characters assigned by Dr. Beddoe to the two great ethnological sections of Britons.

"1. CELTIC RACE.—Eyes grey or blue, passing through dark grey and dark green into brown and black. Eyelashes dark.

"Hair bright red or yellow, passing through various shades of brown, generally bright and tinged with red or yellow, into dark brown and coal black.

"2. TEUTONIC RACE.—Eyes blue or grey, passing through greenish grey, yellow, and hazel, into brown. Eyelashes light.

"Hair light red, flaxen or flaxen yellow, passing through various shades of generally dull brown into a very dark hue, but not into coal black." (p. 29.)

A popular work, entitled 'Orr's Circle of the Sciences,' which is written by various well-known contributors, and is evidently edited with great ability, is likely to have an extensive circulation. The monthly part for January contains treatises, or rather the commencement of treatises, on mathematics, natural history, chemistry, &c. The only possible fault which can be found with the work is, that it may be a little too deep for the class of general readers for whom it is intended. It is well illustrated with woodcuts, and is issued at an extremely cheap rate.

* Erster Versuch einer allgemeinen und Vergleichender Thier-Chemie, von Julius Eugen Schlossberger, Prof. der Chemie an der Universitat Tübingen. Erste Lieferung. Stuttgart, 1854.

† De la Météorologie dans ses Rapports avec la Science de l'Homme, et principalement avec la Médecine et Hygiène publique, par P. Foissac, M.D. Paris, 1854, tomes i. & ii.

PART THIRD.

Original Communications.

Art. I.

On the Peculiarities in Figure, the Disfigurations, and the Customs of the New Zealanders; with Remarks on their Diseases, and on their Modes of Treatment. By Arthur S. Thomson, M.D., Surgeon of the 58th Regiment of Foot.

I HAVE endeavoured in the following paper to give a brief account of the bodily conformation of the New Zealanders, and of some of their customs. The facts connected with their diseases are derived from personal observation during four years and a half; from inquiries made of the New Zealanders themselves, and of those most conversant with them; and from the answers to certain queries addressed by Governor Sir G. Grey to the surgeons of the native hospitals.

General Description of the New Zealanders.—The average stature of the New Zealand men is 5 ft. $6\frac{3}{4}$ in.; the average bodily weight, without clothes, is 10 stone, or 140 lbs. avoirdupois. The tallest man I saw was 6 ft. $5\frac{1}{2}$ in.; he was a chief; but I have seen chiefs of small stature. The average girth of chest is $35\frac{1}{4}$ in. There is great variety in their strength: one man raised half an inch from the ground 410 lbs. avoirdupois; but the average weight which thirty-one men raised was 367 lbs. Their bodily shape is peculiar: their arms are longer than Europeans, their legs are shorter, and the trunks of their bodies are longer. The lengthening of the upper extremities takes place in the forearm; the shortening of the legs, in the tibia and fibula. Many of them are flat-footed, and their feet are broad and short. They turn in their toes in walking. The hair of the head is generally black, straight, and wavy: in perhaps one instance out of five hundred it is frizzly, but the relations of this man may not have frizzly hair. Some of them have hair with a rusty, reddish tinge, under a cross light. This is not caused by alkaline washes, as some have supposed,* but is natural. The hair is of coarser texture than that of Europeans, but when softened with oil, kept clean, and carefully brushed, it assumes a raven darkness, and becomes soft and glossy. The beard and whiskers are scanty, are pulled out in youth, and are allowed to grow after forty. The hair on the trunk of the body is scanty, and nothing surprises a New Zealander more than to see the

* Latham on the Varieties of Man, see p. 262, Note 1. I knew this native from the island of Lafu. The reddish tinge of the hair he had, I have seen in New Zealanders who used no washes.

hairy chest of an European. The hair on the head occasionally becomes soon gray, even as early as twenty-five years. The teeth are good, and are placed more obliquely in the jaws than those of Europeans. I have seen several with decayed teeth, and they suffer from toothache. The eyebrows are arched, the hair of them abundant, and is placed obliquely; eyelashes long, but not abundant. The nose is shorter, broader, and less prominent than in Europeans; but I have seen many good noses, of a Roman and Grecian type. Sometimes there is no bridge to the nose, and this defect is occasionally increased by art. The colour of the skin is olive brown, something like a seasoned filbert nut. There are, however, many shades, and I have seen New Zealanders so fair that I could observe a blush distinctly on their faces; and I have seen others so dark that the tattoo marks were indistinct,—but these are exceptions. The eyes are placed obliquely; the cornea has not that clear appearance which that of Europeans has; the pupil is dark and large; the iris is brown, and, as far as I have seen, never blue. The mouth is coarse and prominent, and the lips are generally thick: the upper lip is long, and is often turned up. The forehead is high, narrow, retreating, and pyramidal; the features are not prominent; the face is broad; the expression in youth is open and happy, in middle age it is sleepy, morose, and thoughtful. Clothed in his native dress, the New Zealander looks like the lion of the forest; in European clothes he is squat and vulgar.

The women, on the whole, are not so good-looking as the men; the young are generally pleasing, but they soon become, in appearance, old, and an old New Zealand woman is far from a pleasant object—they are generally wrinkled and emaciated.

No person can look at a group of New Zealanders and Europeans without at once observing a difference in their figures. This may be produced by some slight alteration or peculiarity in the shape of a bone, or the development of a muscle; but to ascertain clearly what is the cause of this difference in figure between the two races, I carefully measured twenty-three men of each race of similar stature, besides several other men partially, with the following results:

	New Zealanders. Inches.		Europeans. Inches.
1. Average stature observed	$67\frac{1}{2}$	......	$67\frac{1}{2}$
2. Average distance from the tip of the middle finger of the extended right arm and hand, to the same finger of the extended left arm and hand	$69\frac{3}{4}$	......	$69\frac{1}{4}$
3. Average distance from the shoulder (acromion process) to the elbow (olecranon)	$14\frac{3}{10}$	......	$14\frac{4}{10}$
4. Average distance from the elbow (olecranon) to the tip of the extended middle finger	$18\frac{1}{2}$	......	18
5. Average distance from the shoulder (acromion) to the head of the thigh bone, hip-joint, trochanter major	$22\frac{2}{10}$	......	22
6. Average distance from the head of the thigh bone (trochanter major) to the middle of the knee joint (patella)	16	......	16
7. Average distance from the knee joint (patella) to the ground	18	......	$19\frac{3}{10}$
8. Average distance from the shoulder (acromion process) to a line drawn horizontally from the top of the head . . .	$11\frac{4}{10}$	......	$10\frac{3}{10}$
9. Man standing attention—average distance from the tip of the finger to the ground	$24\frac{4}{10}$	......	25

From this examination it appears that the arms of New Zealanders are a little longer than those of a European, and that this difference is caused by an increased length in forearm and hands.

That the legs of New Zealanders are an inch and a half shorter than those of Europeans, and that the greater part of this shortening is found to be in the bones below the knee joint.

That the inch and a half which a New Zealander loses in stature from the shortness of his legs, is made up in his having a longer body.

That the shoulders of New Zealanders are almost an inch farther from the crown of the head than those of Europeans.

I have given these measurements for the purpose of clearly indicating the general figure of New Zealanders. The increased length of the neck is produced by a drooping of the shoulders, which low position of the shoulders diminishes the distance from the acromion to the hip. The anatomical description of the difference is, that the tibia and fibula of a New Zealander are an inch and a half shorter than those of Europeans of similar stature; and that this difference is made up by an increased length in the lumbar and dorsal vertebræ and intervertebral substances between these bones.

The limbs of a New Zealander are stout and well shaped, and the calves of the legs are well down, although, from the shortening in the tibiæ and fibulæ, they look higher up than those of Europeans. Their feet are an inch shorter than those of Europeans, but more broad. The arch of the foot is often badly developed. There is little difference between the size of a New Zealander's two arms: this arises from the muscles of both being often more equally used than the arms of Europeans. A New Zealander takes a shorter step than a European, turns in his toes, and walks in a narrower path. His head is also more pyramidal.

The Disfigurations of the New Zealanders.—Almost every race of men have endeavoured to improve the human body by disfiguring it, and the New Zealand race have undoubtedly outstripped many other tribes in the progress which they have made in this art.

Tattooing.—The term tattoo is a Polynesian word, signifying a repetition of tapping, but it is a word unknown in the Maori language; the general term for tattooing among the New Zealanders being Moko on the face, and Whakairo on any other part of the body.

The invention of tattooing among the New Zealanders is ascribed to men called Mataora and Wharawhare; their names are celebrated in many of their songs. Other persons are immortalized for having invented certain lines. An artist, by name Rauru, has the merit of inventing the lines on the face. No bodily disfiguration has such a remote antiquity as tattooing. The mark on the forehead of Cain and his followers confirms this. Among the Hebrews in the time of Moses, marking the body was almost universal; and in other parts of the Bible a reference is made to it.* In more modern times it was common with many races. Some of the aboriginal races in India tattoo some part of the skin.† The male Burmese do so, as also the Indians in America. Cæsar and Pliny tell us

* Leviticus xix. 28; Ezekiel ix. 6; Jeremiah xlviii. 37; Isaiah xlvi. 16.
† Heber's Indian Journal.

that the Britons adorned their persons with figures made by puncturing the skin, and introducing certain juices. In 1642 the Dutch navigator, Jansen Tasman, visited New Zealand; but in the account of his voyage I have seen he does not mention that the New Zealanders were tattooed, and the pictures of the men in their canoes, as given by him, are totally destitute of tattoo marks on the face. Tradition says that when the Maories first arrived in New Zealand, the only part of the body tattooed was the legs.

Figures tattooed.—The men are tattooed on the face, on the hips and thighs; the women on the lips, the chin, under the eyelids, and a few straight lines are drawn on the trunk of the body. It is on the male sex that we find the marks most perfect. In no instance are the tattoo marks done in the shape of an animal or a plant. Every particular line on the face has a distinct name, and every figure on the face appears to be in its proper place. I have carefully examined the tattoo marks on the faces of natives in different parts of the country, and among all there is a general likeness; but in almost every face there is some slight difference in the number of convolutions or lines in the figures. European artists given to speculation have been able to detect in the spiral figures the general character of the people, and have traced a similarity between them and the figures on the tombs of the ancient Egyptians. I have looked at every plant or object in the country, or in the heavens, which could have suggested the figures, and must give the New Zealanders the credit of originality in this matter. The only thing in nature which could have suggested the figures on the men, is the direction of the fibres of the muscles underneath the skin over which the tattoo marks are made, or the supposed action of these muscles. Thus, over the glutei muscles there is a large circular figure; on the thighs the marks are various in shape, but generally long; and on the face this peculiarity may, with some exceptions, be detected. The present fashion is evidently modern; as the figures have undergone an alteration within the last century. Cook, in his first voyage, states that almost every tribe had a different figure. I showed the tattooed face of a New Zealander, as given in Captain Cook's 'Voyages,' to several New Zealanders. They were all struck with it, and I was for some time constantly visited by natives to look at this picture. They said it was a very old kind of tattoo, and could not have been much in use during his time. It was probably drawn by Cook's artist from a wooden image. None of the other Polynesian races have the tattoo marks resembling the New Zealanders.

At page 87 of the book entitled "The New Zealanders," there is the portrait of an original drawing of John Rutherford. On his chest and abdomen there are several strange tattooed figures. I showed this picture to several New Zealanders, and asked them if ever they tattooed their bodies like this. They laughed, and said it was "maming-a," or humbug. John Rutherford gives a history of himself, but I have heard it said in this country that he was a runaway convict.

In tattooing, the figure to be done is first drawn on the skin with the pigment, after which the operation commences. The instruments required for this purpose are, a slight stick about eighteen inches long, as a mallet, and an instrument something like a horse-lancet, which is made entirely or partly of bone, hard wood, stone, shells, or iron. The instrument differs in shape according to the places to be tattooed. The point of the instrument is first dipped into the pigment, and driven freely through the skin with the mallet; it is then withdrawn, wiped clean, and fresh pigment put on the instrument for another insertion. There are several pigments used, all of which are charcoal derived from different substances; that obtained from burning Kauri gum (*Damaria Australis*) is reckoned the best by the Ngapuhi tribe. It is a very painful operation on the eyelids, lips, and some other parts of the face. Considerable inflammation follows; hence a small part is only done at one time. In youth, about the period of puberty, the process commences; but manhood or old age is the lot of many before it is completed over the whole body. The marks remain perfect until the body decays, and the preserved heads show the tattoo marks entire. I have examined the tattoo done on a man's face fifty years ago, and, with the exception of its being a more subdued colour, could detect no difference from the marks done only seven years. The pigment which remains in the skin must be contained in the under layers of the dermis, or in the cellular tissue below it. The tattoo marks are rough to the touch, and deep canals are seen. The tattoo marks of all the other Polynesians I have seen, were smooth to the touch. Since the introduction of iron instruments or needles, the tattoo marks are not so rough, and are less durable. The black pigment under the skin looks blue. Much blood flows during the operation. Every person is not capable of tattooing; the priests were anciently the operators; and it requires considerable practice to become proficients; and hence good artists have a fame similar to that which Sir Thomas Lawrence had in England. During the operation, songs are sung, to encourage, divert the attention, and to increase the patience of the sufferers.

Tattooing was done, first, as an ornament; second, to strike terror; third, to obscure the expression of the features; and lastly, as a personal distinction.

It is for the first object alone that the custom is now kept up. The missionaries have endeavoured to eradicate it, and they have been tolerably successful in a few instances; but so long as a well tattooed face is esteemed and is fashionable with the women, it will be difficult to extirpate the habit entirely. Had the New Zealanders been possessed of either gold, silver, fine clothes, or any other mode of adorning their bodies, I do not think tattooing would have been continued. European clothing and the use of trousers are doing away with the tattooing on the hips.

In former times a tattooed face was the sign of manhood; it obscures the advance of years, and this appears to have been one of the objects of the art. To strike terror into the hearts of their enemies, and to look terrible in battle, were motives for tattooing; but now that combats are rare, and are carried on at a distance with fire-arms, this object for marking the body has ceased: hence fierce kinds of tattoo, called Moko kurimoko papa, are almost entirely discontinued.

It was the glory of a New Zealander, as it was that of an American Indian, to suffer the greatest agony without the least expression of pain being seen on the face; a well tattooed face did this, for the play of the muscles which gives the human face that expression which no other animals have, is almost completely destroyed by tattooing. Joy and sorrow, revenge and despair, may agitate the mind without a trace of their workings being seen on a face covered with tattoo marks.

Although a slave might have himself tattooed like a chief, if he had the payment to give the artist, yet slaves rarely did so, because they dreaded being laughed at for assuming a mark of dignity they were not entitled to. There can be no doubt that certain kinds of tattooing were marks of personal distinction. Chiefs, during their early intercourse with Europeans, attached a drawing of their tattooed faces as a mark of their names; and ethnologists have assumed this as an approach to a form of writing among the Polynesians. The tattoo on the hips called Rapa, was a mark of distinction, and is celebrated as such in some songs I have got.

Does tattooing serve any useful purpose? In a tropical country, where the face and body are much exposed to moisture and the burning rays of the sun, tattooing would prevent blistering, as it tends to harden the skin; but the New Zealanders, in their mild climate, cannot adduce this as an excuse for the custom. There is, however, one word to be said in its favour. Tattooing, as practised by the New Zealanders, does not interfere with the performance of any of the vital functions of the body; and it indicates a certain amount of artistic talent. So much cannot be said of the bodily disfigurations among other races, or to the female disfiguration in England of constricting the lungs to produce a small waist.

Holes in the Ears.—This is the second bodily disfiguration among the New Zealanders. Men and women have the pendulous part of the external ears pierced. In some persons the holes are small, in others they are large, and the lobe of the ear is torn in two parts. Pieces of green stone, shark's teeth, the tooth of a deceased husband, the purple flower of the clematis (*Metrox deros*) are kept in, or suspended from these holes. The tobacco-pipe is often placed for safety in these holes. This piercing of the ear is a very ancient custom; in Europe a simple ring is put into the pendulous part of the ear, but in New Zealand the hole is often large and disfiguring.

Genital Organs.—All the male New Zealanders, after puberty, have the prepuce tied over the glans penis with a string. This produces an exactly opposite effect to circumcision, and it is a custom they are most particular about. Young boys in a pa are seen running about naked, with a contrivance to prevent the retraction of the prepuce. Should a New Zealander be discovered having his glans penis uncovered with the prepuce, he is an object of much ridicule, and is frequently looked upon with contempt. The glans appears to be the only part of their private parts about which they have any delicacy. I do not know any race of men where this peculiar custom is recorded.

Depilation.—The custom of eradicating the hair from different parts of the body is very common, and a very ancient habit among many races of men. It evidently has its origin in personal cleanliness, and the modern art of shaving is clearly an alteration in the custom. Among the New Zealanders the eradication of the hair from the face is common.

A tattooed face and a beard look badly, and are incompatible. In the pictures given by Tasman of the New Zealanders, their faces are without tattoo, and they have beards; and in most of Captain Cook's sketches of New Zealanders beards are given. A New Zealander after forty allows his beard to grow, which will explain this peculiarity of Cook's pictures. Nature has given most new Zealanders a scanty beard, so that the operation of depilation is not a difficult one, it is done with two shells, which act as tweezers. The New Zealand women have a great aversion to hair on the face. "No wife for the hairy man," says a native proverb, a proverb which caused the young men to devote particular attention to the careful extraction of all hair from their faces.

Cuttings in the Flesh.—This disfiguration was common among the Jews* in the time of Moses. Among the New Zealanders, when a person dies, a beloved person goes away, or when any event occurs which produces grief, the men, but more particularly the women, were and are in the habit of cutting themselves on their faces, arms, and legs. The instrument used is a shell; the marks often remain during life; the incisions are deep, and much blood flows. Slight motives often made women cut their flesh. I have known one to do so on the departure of a person she had only known for a few weeks.

Muscular Contortions.—During the war-dance, and during some of their songs, the muscles of the face, eyes, tongue, eyelids, and indeed the muscles of the whole body, are thrown into the most horrid contortions. I have seen men writhing, in India, in the agony of spasmodic cholera, but the spasms are nothing contrasted with the disfiguration with which the New Zealanders convulse themselves. The powers they have over the muscles of their eyes, require to be seen to be believed. All expert dancers are expected to be able to conceal the pupil entirely, and show nothing but the white part of the cornea, and to elevate or depress the eyelids, so that the dark pupil may be seen surrounded with white. They are also able to convey, by a twist of their eyes, ideas which other people would require a sentence to express; indeed, without any metaphor, there is to be found among them a language of the eyes. The protrusion of the tongue is a sign of defiance. All their images stuck on war pas, have this disfiguration; and the New Zealanders, apparently from practice, can protrude their tongues further out of their mouths than any other people. In one of Captain Cook's pictures, now in the British Museum, this disfiguration is well seen.

Allowing the Nails of the Thumb to Grow Long.—The women about the East Cape allow the nails of their fingers to grow long, but more particularly the nail of the right thumb. They refuse to cut it, and assign no good reason for this desire to preserve it. This appears to me to be a remnant of some old Malay custom, for among the Malays in Borneo, and in some parts of the Philippine Islands, the right thumb nail is allowed to grow to a great length.† The hair of the head, and nails of the fingers and toes, are sometimes tapered.

Flattening the Nose.—Among some tribes, mothers paid particular attention to this mark of Maori beauty. In early infancy it was done,

* Leviticus xix 28; Kings xviii. 28.

† Latham on the Varieties of Man.

by frequent pressure of the fingers. This custom has fallen into disuse, and some tribes never had it.

Painting the Body.—In wars, at feasts, at funerals, the face and body were sometimes painted red or blue. It was done for ornament, and occasionally to prevent mosquitoes and sand-flies from biting the skin. There was no figure adopted. The petals of flowers were also stuck on the face for ornament.

Perforation of the Cartilage of the Nose.—The septum nasi is often perforated; a feather is put through the hole, and it extends across the face. Captain Cook states that he only saw one person with this disfiguration, but many New Zealanders have the cartilage pierced, women oftener than men.

AN ACCOUNT OF THE DISEASE CALLED "NGERENGERE" BY THE NEW ZEALANDERS (LEPRA GANGRÆNOSA).

THERE is a strange disease among the New Zealanders of which I have seen six cases. Dr. Johnson, the late colonial surgeon at Auckland, when in the interior, saw several cases, and he thought the malady was a kind of "dry gangrene," similar to that seen in Germany.* Mr. Edward Shortland, formerly protector of the aborigines, is of opinion that the disease is a species of mortification of the extremities, frequently seen at certain seasons on the Continent, and attributable to the use of unsound wheat or rye.† A medical man living in the interior, who treated a fatal case in 1851, told a missionary clergyman that it was an aggravated example of scrofulous disease. My own opinion is, that it is a variety of such maladies as the elephantiasis Grecorum, the lepra anæsthesiaca of India, the lepra of the Cossacks, &c.; but as it differs somewhat from all these affections, I have, with the view of endeavouring to convey a clear account of the malady, called it the Lepra Gangrænosa of the New Zealanders.

The Ngerengere, or the Lepra Gangrænosa of the New Zealanders, commences with a cutaneous eruption on the extremities, which extends over the trunk of the body. The eruption presents, in some parts, the oval patches and the copious exfoliation of a brown, scaly, morbid cuticle, observed in lepra vulgaris; the irregular patches of psoriasis; and, occasionally, the innumerable fissures, the elongated and extensive cracks intersecting each other, of ichthyosis.

This is accompanied with a pricking and itching state of the skin, of which the sufferers complain much. The eruption goes on for months or years, increasing, and decreasing, and disappearing, partially or entirely. Imperceptibly, and without the knowledge of the sufferer, the hair on the eyebrows, eyelashes, whiskers, and beard, fall out; not the hair on the head, the axillæ, or the pubes. The tattoo marks are not affected. The skin over the whole body, but more particularly on the face, assumes a livid pale colour, the eyeballs become prominent, and a copious discharge of tears flows from them. The voice changes its tone; the face, nose, lips, the forehead, and eyebrows, become swollen and shining, but there are no tubercular deposits in them.

* Notes from a Journal kept during an excursion to Roturoa, &c. in 1846 and 1847.

† The Southern Districts in New Zealand, a journal with passing notices and customs of the aborigines. By Edward Shortland, M.A., Cantab., Extra Licentiate of the Royal College of Physicians. London, 1851.

The skin is dry and harsh to the touch, and at no time, when free from dead cuticle, is it destitute of sensation. The least pinch or puncture on the skin is immediately felt. Although the sufferer eats well and sleeps well, yet the New Zealanders, sharp observers, soon perceive the horrid expression which their brother's face is assuming, and they at once detect the nature of the awful malady which afflicts him. In about a year, it may be more or less, from the appearance of the eruption, a small boil, blister, or dry crack, appears in the direction of the flexure, on the last joint of some of the fingers or toes. The soft parts ulcerate by a dry process, the phalanx falls away, and the part heals. Each revolving year carries off, by a similar process, one or more of the joints of the fingers or toes. There is sometimes pain or an uneasy feeling in the absorbents, which extends along the limb from the dying member, but generally there is little or no uneasiness. Nature in this ease carries on her amputations without pain or loss of blood, as if she were anxious to avoid aggravating the mental torture which such a malady must produce. The healthy toes and fingers are dry, shining, and scabby-like; they are as warm as the other parts of the body, but, from being kept bent, the skin and tendons appear to contract, and the fingers are stiff; dislocation at some of the joints takes place occasionally before the ulcer appears externally. On separation, the bone is found dry and dead. The pulse beats at the wrist; and although the acute sense of touch in the fingers is injured, still feeling is not gone, unless in the fingers about to drop off. Sometimes a paronychia of the hand occurs, and the limb swells greatly, but generally. There is no swelling of the legs or arms during these amputations. Three, four, or more years may elapse before the whole toes or the fingers are amputated. Nature appears to be satisfied with the phalanges, tarsal, metatarsal, the carpal and metacarpal bones; but this is, perhaps, because death, the poor man's friend, comes to the relief of the sufferer before the disease can extend near to the heart. The fatal event is generally hastened by an attack, or by several attacks, of bronchial inflammation or diarrhœa. In two cases, bleeding from the nose occurred, although the bones of the parts were sound. The blood flows slowly through the system when the disease is progressing. I have counted the pulse between 50 and 60. The tongue is moist and clean; digestion goes on regularly; but the action of the skin is generally stopped. The health does not appear to be impaired, and the body keeps up its usual weight. The patient sleeps well, perhaps too well. In one fatal case, before the gangrenous stage had commenced, the discharge of urine was very copious, albuminous, and had a specific gravity of 1006, water being 1000. It might be expected that men beholding their bodies, not figuratively but literally, "dying by inches," and not knowing whether they might not live to contemplate themselves a living trunk, should be miserable; but all the sufferers I have seen were cheerful and happy. A fig of tobacco or a pipe given to them diffused a horrid expression of joy and thankfulness over their satyr-like faces, and they appeared apathetic and indifferent to their sad condition. New Zealanders commit suicide to escape from disease; but I never heard of them doing it to escape from this disease. One person near Tera Wera, without seven fingers, had a foolish expression of face; I thought he was idiotic, because he voraciously devoured a quantity of dirty food which was thrown to some dogs; but we afterwards found this facial idiotcy did not extend to the mind, although, in such a diseased body, a sound mind can scarcely be expected.*

The loss of the hair on the face, the swelling of the forehead and the other features, make it difficult to determine the sex. A New Zealander told me, that men suffering under Ngerengere have little sexual desire.

To this description of Ngerengere I append the picture of a man affected with the disease. He has been ill two years, and has been, on different occasions, about two and a half years in the Colonial Hospital at Auckland. The separation of the joints commenced nearly a year ago. When admitted, he was covered with the eruption I have described: it has now almost gone, with the exception of a little on the hands and the right forearm; the eyebrows, eyelashes, whiskers, and beard, are all completely gone—not any of the hair on the head, the axillæ, or the pubes; his pulse in the horizontal posture is between 50 and 70; tongue clean and moist, appetite good. He had a severe attack of paronychia of the right hand, which produced much suffering and swelling; a copious discharge of purulent matter came out of the spot in the palm of the right hand, which is a scab over the opening. The spots on the left hand are something like the eruption which covered the body, but more circumscribed. The swollen state of the lips, nose, and forehead are well seen. The prominent state of the eyes and the expression is well shown in the picture. He has a scrofulous enlargement of the glands in the neck, and has frequently had discharges from them. The fingers of the right hand are a little swollen—the middle one, at the point, unusually so. The first joint of the little finger of the right hand has dropped off, and the point of the dead bone of the second phalanx is protruded and perfectly white—the red flesh is seen through the thin cicatrix. The first joint of the little finger of the left hand is dislocated and bent; it will soon drop off. The fingers are all bent, but dry and warm. He feels the least pinch or pressure. The first joint of the big toe of the right foot was swollen a day or two ago; a thick piece of cuticle was taken away from the part where the toe rests on the ground, and disclosed a round, deep opening, extending into the joint. There is no pain. He has been treated with nutritious diet, warm baths, cod-liver oil, iodine tonics, &c. &c., all without effect, although I am inclined to think the remedies have retarded the progress of the disease. He has suffered several attacks of bronchitis, and has had lately a little discharge of blood from the nose. The lungs are sound, and I can detect no hepatic enlargement. His urine is clear, specific gravity 1020, and contains no albumen.

The picture is a very good likeness, and several New Zealanders immediately told me the name of the man on looking at the picture.

Lepra gangrænosa rarely or never attacks infants, the youngest person I have heard suffering from it, was a boy aged 12 or 14 years. Most of the cases occur after puberty, and under thirty. Five of the six sufferers

* I must not be blamed for not furnishing the post-mortem appearances in a fatal case. I applied for permission to a high authority; but although anxious to have it done, he would not allow me to do so. It might be supposed that men who formerly eat each other can have little respect for a dead body; but this is not the case. One of the greatest misfortunes which can befall the New Zealanders, is to have their dead insulted; an insult to one is an insult to the whole tribe.

C. J. Hogan del. Ford & West Chromo lith.

New Zealander affected with Ngerengere (Lepra Gangrænosa.)

I saw were males. The fingers, I think, are more frequently affected than the toes—at least, the fingers are first affected. Several members of one family have died from it. It is generally fatal, but not always. Its duration varies from one to five or eight years. I have never seen ulceration of the nose.

Frequency of the Disease.—Lepra gangrænosa appears, by all accounts, to have been more common twenty years ago than at present. Travellers rarely see the sufferers, unless they ask about them. An intelligent New Zealander told me he has known ten persons ill with it in one village. At present, if you ask a native if he knows any one ill with Ngerengere, he will generally recollect one or two cases. Four patients have been seen at the Colonial Hospital, at Auckland, during the last four years, two of whom have died.

The Nature of the Disease.—I have already stated that medical men who have seen the disease in New Zealand, hold different opinions as to its nature. The foregoing account of the disease I have submitted to the Colonial Surgeon, at Auckland, Dr. Davies, who says it is correct according to his observation. The Elephantiasis Grecorum, as described by Bateman, is characterized by "shining tubercles of different sizes, of a dusky red or livid colour, on the face, ears, and extremities; the falling off of all the hair, excepting the scalp; and the loss of sensibility of the skin." Now there were none of these symptoms in the cases I have seen. With these exceptions, the lepra gangrænosa and the elephantiasis Grecorum are the same. In the lepra anæsthesica of India,* the skin is described to be "so entirely insensible, that you may with hot irons burn to the muscle before the patient feels the pain, the whole body becomes alike devoid of sense. The integuments of the fingers and toes slough off in successive layers of half-an-inch, the legs and fore arms swell." There were none of these symptoms in the New Zealand lepra gangrænosa which came under my observation. I have perused all the accounts of dry gangrene and mortification of the extremities, which I can find in the medical books I have had an opportunity of consulting in this distant colony, and I can see no resemblance between them and lepra gangrænosa, beyond the dropping off of the fingers and toes. There appears to be considerable similarity between the disease of the Crimea, or the lepra of the Cossacks, the lepra turica of Martins, and the lepra of the New Zealanders. All the patients I have seen with the disease were highly scrofulous, but I have never read or seen strumous caries produce such symptoms as the lepra gangrænosa. I do not think it is a syphilitic disease, as the malady appears to have been known in New Zealand previous to the introduction of the venereal disease; and I have heard of fewer cases of the lepra gangrænosa at the Bay of Islands than in any other district: and the venereal disease is more frequent among the tribes in the neighbourhood of the Bay of Islands than in any other part of the country.

Dr. John Hunter† has described a disease which occurs among the negroes in Jamaica, which in some respects resembles Ngerengere. They call it cacubay. It begins in whitish spots upon the skin, near the ends of the

* Transactions of the Medical and Chirurgical Society of London, vol. x.
† Hunter on the Diseases of the Army in Jamaica, 1788.

extremities; these spots turn to ulcers, commonly on the fingers and toes, there is much swelling, with pain, and the joint affected drops off without any mortification. The sore afterwards heals up, and remains well for months; but returns again, affects the next joint, which after a time drops off; and the disease attacking one joint after another, in the end reduces the miserable sufferer to a mere trunk. It continues often several years before it proves fatal.

Is the Lepra Gangrænosa a variety of Elephantiasis Arabica?—In most of the tropical Polynesian Islands in the Southern Ocean, the swollen legs of the Arabian elephantiasis are often met with; but in all my trips in New Zealand, and I have been where the natives are most numerous, I have never seen—with the exception of one slight case—a New Zealander afflicted with elephantiasis Arabica; and no New Zealander has presented himself with the disease at any of the hospitals. I could trace no similarity between the two diseases above mentioned, unless in the patient now in hospital. In the skin of the right arm below the elbow, there are several flat thickened cords in the skin, as if the lymphatics were enlarged. The cutaneous eruption covers, in some places, these swellings, which to the hand and eye are perceived and felt to be elevated above the skin.

Localities where the Disease is found.—Lepra gangrænosa is not confined to any particular part of the country, as has been generally supposed. I have heard of cases in the middle island, in the northern parts, in the southern parts, in the interior, and at the sea coasts of the northern islands; but most of the cases I have seen, or have heard about, occurred among the tribes living in the interior, near the lakes of Taupo and Roturua.

Probable Causes of the Disease.—Dr. Davies, the Colonial Surgeon at Auckland, is of opinion that there is some connexion between the hot sulphur and silicious springs and the lepra gangrænosa, in consequence of the frequency of the disease among the tribes living at Taupo and Roturua, where these springs abound; but I have heard of men suffering from the disease who had never been within fifty miles of the above-mentioned localities. Another opinion is, that it is produced by eating the karaka berry, a fruit about twice the size of a large acorn, of an orange colour, having somewhat the flavour of an apricot, the produce of the tree called Corynocarpus laerrigator, natural order Lawrineæ. Against this opinion, and it is a strong argument, I may mention that the patient now in the Colonial Hospital with the lepra gangrænosa, states that he never eat the karaka berry. In the fresh state the New Zealanders say that the karaka berry is poisonous, but after it has been steeped in a running stream for a fortnight, and cooked, it is an esteemed article of food by some tribes.

My own opinion is, that the lepra gangrænosa is produced by the use of poor food; food, perhaps, containing some decayed matter, but deficient in agotized nutriment; want of personal cleanliness; and indolence of body and mind. It is a disease indicative of a low state of civilization. I have not heard of an European, a half-caste, or a native woman living with an European, having had the disease. The malady is now most frequent in the interior, because there civilization is lowest, and because there European food (I do not include potatoes), and European clothing, are little used. The former tends to nourish the body, the latter to produce personal cleanliness. Women appear to be less subject to the disease than men, probably because they are obliged to do more work. No animal on the face of the earth is dirtier than a human animal in its natural state. In a tropical climate men go into the water not to clean their bodies, but to cool them. Many New Zealanders, during the six cold months, sleep, eat, and walk about in dirty, stinking, coarse mats, having the pores of their skin glued up with dirt. The consequence is, that cutaneous diseases are very numerous. In the village of Ohinimutu, where most of the inhabitants eat their food and spend their days in baths at nearly at 100° Fahr., I never saw a case of lepra gangrænosa, and few cutaneous diseases; and it is well known by the New Zealanders that these baths are highly curative in cutaneous diseases, probably on acccount of personal cleanliness.

Captain Cook has recorded that the New Zealanders eat food which natives of Van Diemen's Land rejected; indeed, they will eat almost anything. There is a custom among the New Zealanders of putting maize and potatoes into water, where they are allowed to remain until they become putrid; the smell which issues from the places where this process is carried on is worse than from any dunghill. In this state the potatoes or maize are boiled and eaten, and they are highly relished. The smell of the food when cooked is like human excrement, but its taste is not bad, being somewhat like cheese. This mode of preparing food, although not described by Captain Cook, is not new, for a similar plan is adopted in preparing other kinds of food in New Zealand, and among the Polynesian Islands in the Southern Ocean.

Every patient I have seen with Ngerengere was very partial to the above food, and where the disease is most common the people are in the habit of using much of it. I do not mean to say that the food alone would produce the disease, but when conjoined with filth and indolence it may do so. In what manner bad food, dirt, and indolence, act in producing the disease, I cannot pretend to say; but that they must have some specific action is, I think, indicated; because men in tolerably good condition as regards fat, are affected with lepra gangrænosa. May not the disease be produced by food deficient in agotized matter, or that kind of nutriment which goes to form the muscles and bones of the body? Ngerengere is supposed to be hereditary; but the circumstance of two members of one family having it does not prove this. The habits of the father are transmitted to the son, and what causes the disease in the one may do so in the other.

The New Zealanders' ideas about the malady.—Persons afflicted with leprosy among the Jews were supposed to have incurred the Divine wrath. Ngerengere among the New Zealanders was supposed to be a disease most frequently inflicted by the gods, through priests and witches, for a violation of the laws of the Tapu, and for other transgressions. A chief explained to me that the disease had become very rare to what it was formerly, because most of the people had become Christians, and since that event the gods had lost the power of inflicting that disease.

Formerly, and even now, sufferers are tapued; a house is built for them, and they are fed apart from healthy people, and it is still believed that the disease may be communicated by the touch. Sufferers are held in disgust. They endeavour to cure the disease by keeping the sufferer, from sunrise to sunset, in a vapour bath; and there are a variety of prayers applicable for this disease, which the priest repeats during the time the patient is steaming. The food given to them during the treatment was entirely vegetable; no pork or fish were allowed. They have got two names for the disease: Ngerengere in the south, Tuwhenua in the north part of the island.*

Treatment.—All the cases yet treated have run their awful course unchecked, although I do not think uninfluenced, by medicine. If the disease arise from poor diet, filth, and indolence, animal food, cleanliness, and activity may, with the aid of tonic medicines, &c., assist to cure the disease; but I fear prevention, not cure, is alone powerful in our hands. It is a pleasing task for me to record that commerce has introduced considerable activity of mind and body among the New Zealanders living near the English settlements, and among those who can reach them in canoes and small vessels. Shirts, and other European clothing purchased by this trade, have tended to promote cleanliness, &c. What has been the consequence? The disease I have now endeavoured to describe is becoming rare; probably in twenty years more, civilization and her handmaidens, industry and cleanliness, will have extended themselves to the tribes in the interior of the country, and Ngerengere, or the Lepra Gangrænosa, may become extinct. New Zealanders may then tell their children of the dreadful disease which their old gods inflicted on their fathers, and how Christianity deprived them of this power. Perhaps the above imperfect history may be the only record left of this dreadful malady.

(To be continued.)

Art. II.

Pathological Observations on the Bodies of known Drunkards. Part I. By Francis Ogston, M.D. Aberdeen.

The subjoined observations have, for the sake of perspicuity, and to admit of ready comparison with others to be subsequently adduced, been numerically arranged. They are deduced from a careful study of the appearances, *of a chronic kind*, met with in the bodies of individuals known to have lived intemperate lives, or who, in Scottish phraseology, were "habit and repute" whisky-drinkers, and who *had perished suddenly* from the effects of accident, suicide, or homicide, and *while apparently in ordinary health and activity*. In this way, it is believed, they may be fairly taken to represent, as nearly as is practicable, the effects of alcoholic stimuli, in excess, on the organs and tissues of the body; a result scarcely attainable in any other way. It is not meant to be inferred from this that the

* As almost all words in the New Zealand language have a meaning, I endeavoured to ascertain what these names meant. "Tu" means "struck;" and "whenua," "the earth." Literally, "Struck to the earth." Ngere is a man's name, or it is lazy, stupid, inactive. Ngerengere has several meanings; among them are to be found fearful, dreadful, terrific.

abnormal appearances tabulated below had all been the direct and necessary consequences of previous intemperance; or that they are such as could only have arisen from this cause. Nevertheless, it must be pretty obvious that, in the special circumstances of the parties, a leading part in their production must have been the intemperance of their habits; as otherwise it would not be easy to account for the numerous points of resemblance amongst a series of cases having only one feature in common: a conclusion by no means weakened by the apparently exceptional fact, that only one of the bodies examined was found entirely free from any trace of diseased structure.*

Forty-eight of the cases were instances of death by direct asphyxia; 42 by drowning, 5 by hanging, and one by suffocation; 20 of them were instances of violent death, either by syncope, or by direct coma, speedily fatal without vital reaction. The remaining 5 were cases of rapidly fatal coma, from narcotic poisons.

Of the whole, 25 were known to be cases of suicide, 13 of homicide, and 18 of accidental death, leaving 17 who must have died either from accident or suicide.

The bodies were all recent. In 51 of the cases, where the time of decease was ascertained, the inspections were commenced, on an average, within 23 (22·76) hours after death.

Of the bodies inspected, 49 were those of males, and 24 of females. Their ages, on *an average*, nearly corresponded; that of the males (of whom 4 were under 30) being 42·43; and that of the females (5 of whom were under 30), 44·08 years.

The adipose tissue was abundantly developed on the trunk and limbs in 21 of the bodies, but deficient in 24 of them. Of those in the former state, were 7 females averaging 38·7, and 14 males averaging 40·9 years.

The voluntary muscles were highly developed in 13 of the males and 2 of the females, averaging respectively 36 and 42·5 years.

The sole peculiarities on the surfaces of the bodies were chronic ulcers on the legs in 7, cicatrices on the groins in 1, a large scar across the front of the neck in 1, and in 1 steatomatous tumours on the scalp.

I. Abnormal Appearances in the Head.

1. *Cranium*, unusually thin in 7 (4 suicide, 3 homicide), 5 males, 2 females; average ages 39·3 years.
   unusually thick in 11 (5 suicide, 1 homicide), 7 males, 4 females; average ages 38·9 years.
2. *Dura mater*, adherent to the calvarium in 11 cases.
   highly injected in 4.
   much thickened (leathery) in 1.
   serum betwixt the, and skull-cap, in 1.
3. *Arachnoid*,† thickened, in 31 cases.
   serum under the, over the cerebral hemisphere in 40 (in 30 coincident with arachnoid thickening).

* The idea of selecting this precise class of cases, as affording a means of determining the chronic effects of alcoholic stimuli in excess, has been anticipated by Dr. Roesch, in a paper on 'alcoholic poisoning' in Henke's Zeitschrift f. d. Staatsarzneikunde, 4 V. Y., 1850, reviewed by the writer in the London Medical Gazette for March, 1851, p. 558.

† One or more of the appearances included in this section were encountered in all in fifty cases.

serum under, at base of skull, in 17.
betwixt the, and dura mater, in 2.
in the cerebral ventricles in 24 (in quantity in 14; limited to these in 4).

4. *Pia mater*, injection of the, in 20 (in 19 very minute).
limited to base of brain in 2.
figured surface in 1.
coincident with ventricular effusion in 14.
effusion at base of the brain in 9.
serum, in quantity, under the, in 1.

5. *Brain*, hypertrophied, in 2 cases.
indurated, in 26 (highly in 10;) coincidently with subarachnoid serum in 21; with abundant ventricular serum in 5; with lymph on portions of the cerebral surfaces in 3; with hypertrophy of the conar in 1; and with softened fornix in 1.
softened, in 4 (average ages 43 2 years).
œdema of the (at its base), in 5 (coincident with injected pia mater).

6. *Cerebellum*, softened, in 6 cases (coincidently with softened, in 3; and with indurated cerebrum, in 1.)
indurated, in 8 cases (coincidently with indurated cerebrum).

7. *Medulla oblongata, and spinal cord*, indurated in 1 (coincident with indurated brain and cerebellum).

8. *Choroid plexuses*, vesicles in the, in 14 cases (average ages 38·2 years).

9. *Cerebral arteries*, fatty degeneration of the, in 1 case.

Abnormal appearances within the cranium, in all, in 65 cases, or 89 per cent. of the whole.

II. Abnormal Appearances within the Chest.

1. *Thymus gland*, persistent,* in 4 cases.
2. *Pleural cavities*, serous effusions into the, in 3 cases.
3. *Lungs*, adhesions of the, to the chest, in 25 cases (both lungs adherent in 9, the right lung in 9, the left lung in 7; adhesions close in 6, in only 1 recent).
emphysematous, in 21 cases (emphysema limited to their margins in 3, to their upper lobes in 3).
œdema of the, in 1 case (right mostly affected).
tubercles (latent) of the, in 1 case.
blood clot (large) in one of the, in 1 case.
unusual friability of one of the, in 1 case (coincident with close adhesions).
bronchitic in one case.

Abnormal appearances in the respiratory organs in all in 41 cases, or 56·16 per cent. of the whole.

Simultaneous abnormal appearances in the encephalon and respiratory organs in 34 cases.

4. *Mediastinum* loaded with fat in 2 cases.
5. *Pericardium* adherent to the heart in 3 cases (in 1 closely).
osseous plate in the, in 1 (the same) case.
serum very abundant in the, in 1 case.
highly injected (with effused serum) in 1 case.
6. *Heart*, investing membrane of the, thickened and opaque, in 1 case.
walls of the, loaded with fat, in 11 cases (in 3 coincidently with abundant subcutaneous fat).

*** It may be doubtful how far the fully-developed state of the thymus gland in the adult may claim to be classed as a *morbid appearance*. As an isolated circumstance, it may be placed here, however, without blame.**

general enlargement (hypertrophy) of the, in 11 cases (in 5 coincident with abundant fat on its walls).
hypertrophy of left ventricle of the, in 5 cases (in 2 hypertrophy concentric).
dilatation and attenuation of the right, in 9 cases.
tricuspid valve of the, affected, in 4 cases (thickened and cartilaginous in 2, covered with warty vegetations in 1, adherent to the ventricular wall in 1).
mitral valve of the, affected in 8 cases (warty vegetations at their margins in 2, cartilaginous thickening in 3, adhesions to the ventricular wall in 1, cretaceous in 1, atheromatous—coincidently with atheroma of aortal arch—in 1).
aortic valves of the, thickened in 2 cases.
warty vegetations of the, in 1 case.
ossification of the, in 1 case.

7. *Aorta*, dilatation of the ascending, in 4 cases.
bony plates in the ascending (and its arch in 1) in 3 cases.
atheroma of the ascending, and its arch, in 1 case.

Abnormal appearances in the pericardium, heart, or aorta, in all, in 30 cases, or 41 per cent. of the whole.

Abnormal appearances in the chest, in all, in 48 cases, or in 65·7 per cent. of the whole.

III. Abnormal Appearances within the Abdomen.

1. *Omentum* loaded with fat in 4 cases (in 1 reaching to the pubis, in all coincidently with abundant subcutaneous fat, in 2 with fat around the heart, in 1 with conversion of the vermiform appendages into large fatty masses).
displacement of the (crural omental hernia) in 1 case.

2. *Stomach* unusually small (atrophied in 9 cases).
highly congested in 10 cases (interior reddening uniformly on exposure to the air).
false melanosis of the interior of the, in 2 cases.
lining membrane of the, softened, in 2 cases (in 1 at the cardia, in 1 at the great cul-de-sac).
hour-glass contraction of the, in 5 cases.
unusual thickening (hypertrophy) of the walls of the, in 1 case.
copious muco-purulent fluid in the, in 1 case.

Abnormal appearances in the stomach, in all, in 20 cases, or 27·3 per cent. of the whole.

3. *Intestines*, unusual contraction of the, in 6 cases (contraction of the smaller in 1, of the larger in 1, of the lower half of the ileum in 1, of the colon and rectum in 1, of the sigmoid flexure of the colon in 1, and of the sigmoid flexure of the colon and the rectum in 1.
displacements of the, in 2 cases (scrotal herniæ).
softening of the mucous coat of the, in 2 cases (the ileum in 1, the larger intestine in 1).
enormous distension of the, in 2 cases.
atrophy of the, in 1 case (descending colon attenuated and translucent).
congestion of the duodenal portion of the, in 1 case (coincident with congested stomach).

Abnormal appearances in the intestines in all, in 10 cases, or 13·6 per cent. of the whole.

4. *Liver*, general enlargement (hypertrophy) of the, in 9 cases (1 lobular).
partial enlargement of the, in 2 cases (left lobes equalling the right).

cirrhosed, in 4 cases (in 2 partly fatty, in 1 adherent to the diaphragm).
nutmeg, in 4 cases (in 2 granular, in 1 unusually firm).
granular in 4 cases (in 3 with considerable enlargement, in 2 with additional lobes on the anterior margin, in 2 with fatty patches).
fatty degeneration of the, in 4 cases (partial in all).
softening of the left lobe of the, in 1 case.
intense congestion of the, in 1 case.
anæmia of the, in 1 case.

Abnormal appearances in the liver in all, in 30 cases, or 41 per cent. of the whole.

5. *Spleen*, indurated (hepatized) in 10 cases.
hypertrophied, in 2 cases.
atrophied, in 1 case.
softened, in 1 case.

Abnormal appearances in the spleen, in all, in 14 cases, or 19·1 per cent. of the whole.

6. *Pancreas*, hypertrophied, in 1 case.
7. *Kidneys*, general fatty degeneration of the, in 1 case.
hyperæmia of the, in 4 cases (in 1 with nutmeg liver and albuminous urine).
hypertrophy (enlargement) of the, in 13 cases (in 5 lobulated, in 2 the cortices pale and attenuated, in 2 with yellow patches of fatty degeneration over the cortices, in 1 with partial obliteration of the tubuli, in 2 with granular surfaces, in 1 with (twelve) pus depôts in one kidney, coincidently in 2 with fatty liver, in 4 with enlarged liver, in 2 with nutmeg liver, in 1 with albuminous urine).
atrophy of the, in 1 case (kidneys attenuated, pale, with yellow fatty deposits in their cortices, serous cysts on their surfaces, and albuminous urine).
buff-coloured, and cortices attenuated, in 4 cases (in 2 with albuminous urine, in 1 coincidently with fatty liver, in 1 with enlarged and fatty liver).

Abnormal appearances in the kidneys, in all, in 23 cases, or in 31·5 per cent. of the whole.

8. *Bladder*, hypertrophied, in 1 case.
9. *Uterus and its appendages:*
Reddish fibrous mass in the uterus, and os tincæ closed, in 2 cases.
Pus in the uterine wall at its fundus, in 1 case.
Bony concretion in the left broad ligament in 1 case.
Serous cysts in the ovaries in 4 cases (in 3, both ovaries).
Cartilaginous degeneration of the ovaries in 2 cases.
Blood-clot occupying the whole of the ovaries in 1 case.
Hydatids in both ovaries in 1 case.
one, and pus in the other, Fallopian tube, in 1 case.
in both Fallopian tubes in 1 case.

Abnormal appearances in the female generative organs, in all, in 8 cases, or in 33·3 per cent. of the sex.

Abnormal appearances within the abdomen, in all, in 54 cases, or in 73·9 per cent. of the whole.

Simultaneous morbid appearances in the head, chest, and abdomen, in 32 cases, or 43·8 per cent.

Simultaneous morbid appearances in the head and chest in 40 cases, or 54·7 per cent.

Simultaneous morbid appearances in the chest and abdomen in 38 cases, or 52 per cent.

Simultaneous morbid appearances in the heart, liver, and kidneys, in 9 cases, or 12·3 per cent.
Simultaneous morbid appearances in the heart, lungs, liver, and kidneys, in 6 cases, or 8·2 per cent.
Simultaneous morbid appearances in the liver and kidneys, in 13 cases, or 17·8 per cent.
Simultaneous morbid appearances within the head, and in pericardium, heart, and aorta, in 26 cases, or 35·6 per cent.
Entire absence of morbid appearance in the body in 1 case.

ART. III.

Some Remarks upon the Nomenclature of Auscultation. By W. O. MARKHAM, M.D., Assistant-Physician to St. Mary's Hospital.

THE precision and directness in diagnosis and treatment of diseases of the thoracic organs which have been gained to the art of medicine by the discoveries of Percussion and Auscultation, present, in our view, the most striking existing proofs of its progressive strides; so absurd indeed and impossible, now-a-days, would an attempt to treat those diseases without such diagnostic aids appear, that it requires some effort of the imagination to realize to ourselves an idea of the utter uncertainty which must have hung around the hesitating practice of physicians of the past generation.* Voltaire's acute wit was guilty of no injustice, in this particular at least, when he likened the physician of his day to a giant dealing around him random blows at some enemy in the dark: his simile was an exact representative of the truth. Cullen tells us that his diagnostics do not serve him to ascertain the exact seat of inflammations affecting the viscera of the thorax, and that he includes the whole of them under the general head of pneumonia; and even Corvisart speaks of the exceeding obscurity of pericarditis, of the "scrupulous attention and subtile sagacity" requisite for the diagnosis of that disease.

The vast amount of information which now illustrates the study of thoracic diseases has been spread among us, as it were, within the memory of the present generation; the first edition of Laennec's famous treatise appeared in 1819, and with his name Auscultation must ever stand associated; whatever isolated and unfruitful facts, bearing on this subject, lie scattered through the page of ancient history, cannot be regarded as other than mere matters of literary curiosity. An entirely new field of research in the domain of medicine has thus been opened, whose successful cultivation alone requires the almost undeviating attention of the physician.

I say that auscultation dates from Laennec, but it is impossible to believe that he who gave to medicine 'A New Method of discovering, by Percussion of the Human Chest, the hidden Diseases of its internal Organs,' and whose work was introduced into France and minutely commented upon by Corvisart,† could have failed by his writings to have exercised great influence over the mind of Laennec, in directing his

* We cannot agree with Mr. Guthrie, that, "whilst auscultation has thrown a clear and steady light on the nature of the mischief which is going on, it has added little or nothing dissimilar to the practice pursued some forty years ago."

† "Clariss. præceptor meus," Laennec says of Corvisart.

attention to the physical diagnosis of diseases of the thorax; it is not unreasonable, and in no way detracting from Laennec's merit, to question, whether, if Auenbrugger's labours had been earlier and better known, auscultation would have been so late a discovery; the alliance between these methods of diagnosis is close; percussion, as Dr. Latham has well remarked, may be properly called auscultation by percussion, and though they are often spoken of as being different things, they are after all only different modes of appealing to the same sense. This much is certain, that the date of the introduction of Auenbrugger's treatise into France marks the period of the rise of auscultation.

Laennec lived long enough to see the results of his labours firmly established in the practice of medicine, by a wide-spread recognition of their value—far more fortunate he than his not less laborious, and not less highly gifted predecessor, Auenbrugger: *his* labours were crowned by ridicule (which he anticipated*) and the neglect of his cotemporaries, and though after two generations have passed away, and time, which rarely fails to vindicate the rights of genius, has in some sense made atonement for their injustice and ignorance, yet can it not be said, that his true position as a discoverer has ever been assigned him, or his worth fully recognised.

I have here introduced the subject of auscultation to the attention of the reader, for the purpose of inquiring whether the method of analysis of the auscultatory signs, which has been generally followed in France, and hitherto almost universally adopted in this country, is correct in its general principles, and best adapted for the advancement of the particular objects of the study. And, in the first place, it seems impossible not to regret, that an indiscriminate subserviency to the views and doctrines of Laennec should have prevailed, and still generally prevails amongst auscultators, even to this day. French physicians are the unswerving disciples of Laennec, and we have hitherto been the docile followers of French physicians; in our investigations into the subject of auscultation, we have exhibited a lack of that insular spirit of independence which is ofttimes our boast; whatever France has transmitted to us has been willingly received, but whatever has attempted to take root here, opposed to the opinions of that school, has been stigmatized as an intrusion, and kept at arm's length as heretical. A gleam of better things, however, appears to be at length spreading its influence around us, and I hesitate not to assert, that from the thoughtful mind of Germany that gleam proceeds.

The grand structure reared by Laennec stands, and must ever stand, in all its broad outlines, a monument of its author's searching genius; but in its details, time has shown—and how, from the nature of the subject, could it be otherwise?—that there is much to correct in the study of auscultation as fashioned by his hands. Laennec's authority would naturally exert a powerful influence over the minds of his successors; the splendid nature of his discovery, and the degree of excellence which it reached in his hands, could hardly fail to prepare the minds of men for a ready acceptance even of error, when it came before them stamped with

* Enimvero invidiæ, livoris, odii, obtrectationis, et ipsarum calumniarum socii, nunquam defuerunt viris illis, qui scientias et artes suis inventis aut illustrarunt, aut perfecerunt.

the authority of such a name; they would distrust the correctness of their own observations, should they find them differ from his, and would more readily believe their own judgment erring, than that his could be fallacious; but surely no one, now-a-days, will pretend that auscultation has yet reached that height of perfection from which nothing can be taken, and to which nothing can be added, or that all the details of the study are so patent and manifest, that neither doubts nor difficulties still cling around it. "Les hommes les plus satisfaits de la vieille" will hardly assert this much. An enthusiastic student may find the matter not hard to understand, but the experimented observer well knows how often his best powers of research in this field of diagnosis fail him, how often his judgment deceives him, and how often impossible it is, from the complicated problem before him to extract the true solution. He discovers a want of reconcilement between the ordinary accepted theories of the study and their application in practice, between the clearly and well defined signs as recorded in his books and as they present themselves at the bedside of the patient. He may be told, for example, that the most perfect bronchophony is at times coincident with a highly emphysematous condition of the lungs, and is he then to remain incurious, and satisfied with the explanation of the mode of production of that sound as given by Laennec, and as still generally taught amongst ourselves—viz., that it is the voice conducted to the ear by condensed pulmonary tissue? and this, too, when his experiments demonstrate that the voice passes more readily through healthy than through condensed pulmonary tissue, or through liver, when auscultation is practised on these organs taken out of the body?* Then, again, is œgophony to remain in his mind the unquestioned representative of pleuritic effusion, when he knows that it may be present though not one drop of fluid be contained in the pleura—that it may exist in health, be an accident attendant on simple pneumonia—and that in all probability it is more frequently not heard than heard when fluid does exist in that serous cavity?

Author after author has, by isolated proofs, demonstrated the fallacy of regarding pectoriloquy†—the cavernous voice—as the certain representative of a pulmonary cavity, but still the term holds an undiminished value in currency amongst us. And what are we to say, again, when we find, contrary to all tradition, and almost universally received opinions, that a *partially* condensed lung may give a clearer percussion sound than a healthily-inflated lung?‡ When a study presents itself to us, surrounded

* We do not quote these experiments of the German auscultator as *conclusive proofs* of the incorrectness of Laennec's theory, but to illustrate the unexpected difficulties which surround and obscure what is at present a pretty widely admitted fact.

† "The pectoriloquism of phthisis, and the egophonia of pleurisy, are the least valuable of the physical signs of these diseases," says Dr. Stokes. Beyond all kind of doubt, the very same resonance of the voice may be produced by consolidation of the pulmonary tissue, and in pulmonary cavities.

‡ There can be no shadow of doubt, however little the fact be generally known and appreciated in this country, of the correctness of Skoda's assertion, that the percussion sound of a lung which is partially deprived of its natural amount of air, either by compression from without, or by deposition of fluid or solid materials from within, *may* become clearer than that of a healthy lung. (On this head I beg to refer the reader to a paper in the last June number of the Edinburgh Monthly Journal.) What becomes, then, of the value of the statements of those many observers, who tell of their "cures" of phthisis? These cases, of course, pre-suppose a correct diagnosis, the establishment of the fact of the existence of tubercular disease; and

by such anomalies as these, it surely is not unreasonable to believe that there must be something erroneous in the method of observation which physicians have hitherto adopted in pursuing the objects of that study, and which has been productive of results so unsatisfactory.

My object here is to endeavour to point out some, at least, of the causes which, as I think, have brought about such anomalous effects, and to advise, as the only sure escape therefrom, the adoption of the modern German method of analyzing the phenomena of auscultation and percussion.

Among the many causes which have retarded the progress of this branch of diagnosis, no one has, in my opinion, been more fatally injurious than what may be called the empirical method of dealing with its signs, which was, in a limited sense, adopted by Laennec, and has since been carried to the most extravagant lengths by some of his successors. To this method must be attributed the main share of the misconceptions which surround the subject—the method, I mean, by which auscultatory signs are forced into becoming representatives, are made pathognomonic, in fact, of special diseased conditions of the thoracic organs. The plain recognition of a fact does not satisfy the teachers of this method; it is not enough for them that a râle should be large or small, moist or dry, high or low-pitched, &c.; it must be mucous, cavernous, pneumonic, bronchitic, &c. But the error of all this is manifest upon the slightest consideration; for what is that character of a râle which absolutely proves the presence of mucus, what that which demonstrates *per se* the existence of pneumonia or bronchitis, what that of a cavern? May not blood, pus, or serum produce just the very râles produced by mucus? What râles do observers mean to indicate when they speak of crepitations of pneumonia, and capillary bronchitic râles? Are they the same, or do they differ? There are, in fact, no râles pathognomonic of pneumonia, and none pathognomonic of capillary bronchitis; and those who, following this loose method of observation, are contented, upon the recognition of certain râles, to admit the existence of such diseases, will very surely, and

in the majority of instances, the diagnosis of the disease, at all events in its first stage, is based upon the presence of a comparatively dull sub-clavicular percussion sound. Now I believe that further investigation into this subject will show, that in the majority of cases, at least, when the tubercular matter is *first* deposited, and in such quantity as to cause an alteration in the percussion sound, so far from the sound being duller, it is often actually clearer, and more tympanitic, over the affected than over the healthy lung, and this in accordance with the fact stated by Skoda. Previously to my knowledge of that fact, I had frequently been surprised at the apparently opposing results offered by the percussion and auscultation signs in cases of suspected deposition of tubercle; these, where percussion elicited the *clearer* sub-clavicular sound, alterations in the respiratory murmur, and other abnormal signs were perceptible, but where the percussion sound was *comparatively* duller, the auscultatory signs remained unchanged from the natural, and were difficult to describe as other than healthy.

I cannot refrain, whilst on this subject, from giving an instance of the boldness of auscultators, engendered by a false exaltation of our diagnostic powers. A late writer on consumption speaks of the curability of this disease previously to the deposition of tubercle in the lungs; but how does he prove his negative, the non-deposition? It can only be by the absence of those particular auscultation and percussion signs, which indicate its presence. But our best observers, from Louis downwards, have admitted that a certain amount of deposition of tubercular matter may take place within the tissue of the lungs, without producing any appreciable change whatever in either their natural auscultation or percussion sounds. What legitimate facts, then, justify the writer in speaking of phthisis apart from the presence of tubercle? Where is the pathologist who will venture to speak of death depending upon consumption, if he find an absence of tubercular matter in the body?

not unfrequently, determine their patients to be the victims of maladies of which they are perfectly innocent.

The method followed by the German school, on the other hand, is manifestly philosophic, and must be positive in its results; and by contrast with the French, may fairly be called the rational. Here the explanation of the observed phenomenon is sought in its necessary coincidence with certain anatomical conditions, and by the application of physical laws. Viewed in this manner, auscultatory signs become in the physician's mind simply the resultants of certain physical causes, of reciprocal actions between fluids and solids, and as such to be calculated and measured according to the ordinary laws of sound; not converted into sure and positive exponents of particular morbid states: thus the crepitations of pneumonia, as they are called, judged of by this method, become sounds which indicate the presence of some fluid in the finer bronchial tubes and air-cells, and the entrance of air into the latter. As a corollary, they, of course, preclude the idea of the existence of those pathological conditions which prevent its entrance into the air-vesicles; but these crepitations may accompany several *different* abnormal states of the lungs. They, therefore, as such, can be rightly made to indicate no one *special* disease; their value is relative, not absolute, and can only be determined when judged of in comparison with other signs present. Therefore, to call them crepitations of pneumonia, is manifestly to miscall them. Thus the German system abjures the theoretical, and confines itself to a simple exposition of facts; ignoring pathognomonic signs, except in so far as they are the certain and invariable representatives of special pathological conditions,* it takes us as far as physical facts warrant us in advancing. The French leads, and has led us, to a pretence of knowledge far beyond what those facts justify us in assuming. The one keeps auscultation in its place, using it as a handmaid to diagnosis; the other makes it a master-key, of itself sufficient to unravel the nature of the hidden disease.

The German, again, is plain and practical, and tends to simplify the details of the study; and by following its precepts, the observer will at all events scarcely be *misdirected* in forming his conclusions; he will be contented to call a râle a râle, and to assign to it just those physical characters which his ear tells him are incidental to it, without qualifying it by the hypothetical, and, of necessity, very frequently erroneous, terms, mucous, sub-mucous, cavernous, &c.

Auscultation thus practised will occupy its due position as a diagnostic aid, and not be made, as it now-a-days too often is, to usurp an undue influence over our judgment; its signs, measured by such rules, will of necessity require confirmation: and thus, as it were, despite of ourselves, we shall be forced to seek out their actual meaning, in a general review of the whole of the morbid phenomena which the ailing body presents: not resting on a single and isolated phenomenon, precarious and uncertain in value.

* If there be any auscultatory signs to which the term pathognomonic (i e., which are the unerring indicators of certain fixed pathological conditions) is applicable, it would seem to be those which have an amphoric or metallic echoing character; these sounds certainly seem never to be present except when cavities exist in the lungs. The brûit de pôt fêlé has been said to have a like meaning; but this is undoubtedly erroneous: I have heard it produced most distinctly over a lung, which, after death, was found *partially* consolidated from pneumonia.

Moreover, another unfortunate tendency which, in my opinion, has been imparted to the study by this pathognomonic measuring of signs will likewise find its corrective, and that is an indulgence in such minutiæ of detail, as to render auscultation in some sense impossible to the great body of practical men in our profession; and most difficult, even to the skilled observer. Can any one doubt, that if our powers of observation were not so unduly magnified—if they were, in fact, more restricted, and less pretentious—our knowledge would become more sure. There cannot be a doubt that the pretence of correctness in diagnosis, continually surpasses the capability of the auscultator; a high degree of perfection in the art can never fall to the lot of many; few indeed possess those physical qualities, that nicety of tact, that fine sense of hearing, which are requisite to serve the observer in such minute differences as the subject offers.

"The extreme delicacy of ear," says M. Fournet, "necessary for the appreciation of symptoms on their very first appearance; the danger there is of confounding them with passing modifications of normal actions; the difficulty of recognising, among the slight shades of differences which distinguish them, those which have a real fixed value as signs; the practical ability requisite to discover the actual connexion between the first symptomatic manifestations, and the first changes which the tissues of the organ undergo; all assist in retarding the progress of this branch of medicine." I would rather say, that attempts to teach men the observation of such inappreciable shades of differences in signs, can scarcely fail in retarding its progress.

Thus, the study has been forced beyond its legitimate limits; beyond, I mean, the limits of its present capacity for producing truthful fruits. Men boast of greater perfection in their powers of diagnosis, than the nature of the subject, as it now stands—what minute degree of certainty it may some day reach, we stop not here to enquire—justifies them in assuming.

I may, perhaps, here illustrate with advantage the position I am maintaining, by reference to some particular case; and will take, for example, the râles, "the cracklings of pneumonia," "le râle crepitant de la pneumonia," as they have been called, and ascertain what value they really do possess as signs, as compared with what is generally attributed to them, and which is assigned them by such titles.

Le râle crepitant was considered by Laennec pathognomonic of pneumonia, and in the judgment of his immediate successors, and of some of our latest and most esteemed writers, appears to be held in no less estimation. Now it is generally admitted that these fine râles take their rise for the most part in the smallest of the bronchial tubes, and in the air vesicles, and that they are caused by the passage of air during respiration through fluid contained within these; if this be true, and there can be little doubt that it is, it follows of necessity, that fine râles will be produced, whatever be the nature of the fluid matters contained within the tubes and air vesicles, whether such fluids are composed of blood, of pus, of serum, or any other liquid.* That the râles in question are more

* It is not necessary here to take account of what difference in the sound may be produced by the *quality* of the fluid, as to tenacity, &c.

frequently heard in pneumonia than in other affections of the lungs is true, for the simple reason that in this disease, more frequently than in other diseases, fluids are present in the finest ramifications of the bronchial tubes.

Now, Laennec himself was forced to admit, that a râle in character closely resembling this, was occasionally to be heard in cases of œdema of the lungs, and that it was often hard to distinguish between this, and what he called sub-crepitant râle. Other writers since Laennec have insisted upon this point, and have also shown that a similar râle may be caused by pulmonary hæmorrhage, and in asphyxia produced by large secretions of mucus into the bronchial tubes. "On entend souvent," says Andral, "un râle tout-à-fait semblable au râle crépitant chez des individus qui ne sont atteints que d'une bronchite intense, and chez lesquels on ne trouve, après la mort, ni pneumonie, ni œdème pulmonaire;" and again, referring also to Laennec's mucous râle, "ces deux râles presentent une foule de degrés, and de nuances où ils se confondent." And there is little doubt that there are other sources whence sounds most difficult to distinguish from the râles attendant upon pneumonia, may arise; I might mention certain fine pleural friction sounds, and perhaps also the passage of fine bubbles, under certain circumstances, through fluids contained in pulmonary cavities.

These fine râles, then, have a wide signification, and can rightly be used in no restricted sense: and when writers speak of them as "cracklings of pneumonia," they cannot fail to leave upon the minds of their readers an erroneous impression as to the real import of the sign. Surely, when several distinct pathological conditions produce a like sign, nothing can be more illogical than to affix to that sign a term which is distinctive of only one of such conditions: or, in other words, to designate the species by a name common to the genus. And what is the result, in practice, of this way of dealing with this particular sound? What, but that when heard, it is apt immediately to impress on the auscultator's mind a conviction of the existence of pneumonia—an idea, indeed, often so irresistible as to distract him from the necessity of establishing the fact by collateral evidence. Can it be denied that there are at this day many observers for whom this râle stands, *per se*, the positive indicator of inflammation of the lungs; and who, having heard it, unhesitatingly found their practice upon it? As regards Laennec and his immediate disciples, the remark is undoubtedly true; and as for these latter days, I can only say, "scripsi ut vidi." But what practice can be more irrational than that which confounds together, on account of identity of auscultatory signs, pneumonia, "pure et simple," and the inflammatory condition of the lungs produced by miliary tubercles? Fine cracklings may attend idiopathic (as it is termed) inflammation of the lungs; and like fine cracklings may attend the inflammation excited by tubercular deposits—the local expression of a specific disease—but how distinct the therapeutical indications in the two cases, how different the prognosis, when judged by a rational pathology! And yet how fatally similar they often are in practice, by reason of the judgment being warped by these verbal misconceptions!

Another source of error might be referred to, as apt to spring from the unjust value which the student is taught to attach to this sign. So all-important has it become for him in connexion with pneumonia, that he can with difficulty bring his mind to believe in the existence of that disease in a person who may be suffering from thoracic ailment, if it be absent, however strongly the collateral evidence may be in favour of the presumption; forgetful that these fine râles are often but fleet and passing signs, that they often do not present themselves throughout the whole course of the disease, and that they may have passed away before the patient comes under the observer's notice. And thus it is that in a positive and in a negative sense, by its presence and by its absence, this physical sign, unduly magnified in value, may become, and I say from actual experience, often does become, a direct source of erroneous judgment.

How much more true to fact, and how free from all hypothesis and prejudging of the question, it would be to speak of these râles simply as fine moist crepitations; and to say of them, that they are met with in different diseases; that they depend for the most part on the presence of fluids in the air-vesicles and finest bronchial ramifications, and upon the passage of air through them, but that they may arise from other causes; that they are much more frequently met with in pneumonia than in other diseases, for the reason that, in pneumonia the conditions under which they are produced, more frequently obtain than in other diseases; *and to judge of their semeiotic value, whether as indicative of pneumonia, or of other diseased conditions, only in connexion and by comparison with all the other signs and symptoms which those diseases may present.*

One of the most natural consequences of this affixing to the sign presented by auscultation, a certain pathognomonic value, has been the introduction into its study of a most vicious system of nomenclature; and perhaps no single cause has more fatally obstructed, and continues to obstruct, its progress than this. A certain degree of faultiness and want of definitiveness must, of necessity, be attached to terms used to express ideas of whose exact value we have not positive evidence; and Laennec, in the infancy of auscultation, could hardly have done otherwise than fall into the error of occasionally affixing to sounds terms which give an incorrect notion of their real nature—this is comprehensible enough; but that his successors at the present day, with the advance of the study, and with a nearer and correcter knowledge of its details, should not only copy and adopt such error, but should follow further on in the same faulty direction, thereby enlarging and spreading wider its area, is much to be regretted. It would seem, indeed, as though each successive writer had vied with his predecessor in complicating the phraseology of auscultation, as if for the express purpose of bewildering and perplexing his reader's understanding.

The confusion which has been thus introduced into the study of auscultation, has sprung up in different ways; and, in the first place, I may refer to it as arising from the circumstance of words being used as the exponents of things, with whose real import they have no connexion, or to which their sense may indeed be actually opposed. I will illustrate this point by reference to the term "mucous râle"—*le râle mucueux* of Laennec: now, is this term used to signify, as its sense naturally tends to show, that the sound heard is produced by the presence of *mucus* in the air-passages, &c., of the lungs? nothing of the kind, for as we know, the râle thereby intended may be produced equally well by the passage of air through blood, or pus, or serum, or any other liquid (indeed, in its most common use it implies its passage through pus in pulmonary cavities): but what does this râle *in reality* indicate? surely nothing more than this —that large air-bubbles traverse fluid matters which are present in the larger bronchial tubes, or in pulmonary cavities; "The sound," as Dr. Latham has said, "will indicate the *situation* and quantity of the fluid and *no more;* it is beyond the truth to say that the *quality* of the fluid through which the air passes *can* be distinguished by the quality of the sound that results." Now, would it not be much more simple, and afford less chance of creating and leaving erroneous impressions upon the mind, if auscultators, instead of using a term thus manifestly false in itself, were to substitute for it that of "large bubblings?" such an expression tells the truth, and no more; it involves no hypothesis, and, above all, cannot mislead the judgment of the practitioner, or engage his attention in a wrong direction.

The same remarks will apply, and with still greater force, to those consecrated terms, "cavernous râle," "cavernous voice," "cavernous respiration," "sounds resounding in a cavity in a lung;" for, if there is one fact more certainly established than another, since Laennec's day, in the matter of auscultation, it is this—that the most perfect pectoriloquy, "cavernous voice," may be heard over a lung in which no cavity exists; that it may, in fact, be the product of the voice operating in the bronchial tube of a consolidated lung; that "cavernous râle" may be the râle of a bronchial tube; and "cavernous respiration" have a like origin; and thus, in fact, that all these falsely-called cavernous sounds may be the productions of no caverns at all. Every one's experience must afford him ample proofs of errors in diagnosis, which are the continual consequences of this abuse of the adjective "cavernous" in auscultation. The pertinacity with which we cling to the error, with such proofs before us, is very surprising. MM. Barthez and Rilliet tell us, in a late communication to the Archives de Médécine, that they have heard the most perfect pectoriloquy in certain cases of pleurisy—"Que la respiration caverneuse, la respiration amphorique, et la gargouillement peuvent être perçus dans la pleuresie and en l'absence de toute excavation pulmonaire;" but yet they do not seem to be aware that they are committing any solecism by writing of the cavernous voice when there is no cavern concerned in producing it.

Again, if we consider the expression "cracklings of pneumonia"—used by an author of justly-esteemed authority in this country, and for which, by the way, we are probably indebted to the profuseness of M. Fournet's vocabulary, to his "râle crepitant de la pneumonie"—and measure it by the above criticisms, we shall be forced to condemn it as most objectionable. Why call those the crepitations of pneumonia which *may be* the crepitations of pulmonary œdema, of pulmonary hæmorrhage, of acute bronchitis, and which at times so closely resemble, as to be undistinguishable, from certain râles called sub-crepitant by Laennec? If we admit that these "cracklings" may arise under other diseased conditions of the lungs besides those which constitute pneumonia, and we cannot deny the fact, our nomenclature stands manifestly condemned as erroneous.

The same arguments will apply, and with equal force, against the use of other terms which will readily suggest themselves, such as sub-mucous, capillary bronchitis, &c. It is not necessary, however, for me to speak of them further in this place.

Perhaps it may be suggested, that the objections I urge are frivolous, and that I am combating a shadow, inasmuch as that it matters little what the nomenclature of the study be, provided authors fully explain, and their readers be generally agreed as to the meaning of the things thereby implied; and that there is no actual necessity for the words being an echo of the sense. Such objections I dissent from, and would anticipate. It is perhaps true, that he who has arrived at a certain degree of mastery over the objects of auscultation, may have no great trouble in the matter; he may have fought his way to something like a safe position through the mists and errors which surround it, but I much doubt whether he has done so without having been again and again unwittingly misguided by the deceptive tenor of its language: despite of ourselves and of our better judgment, words, mere words, will hold a sway over our understandings; it is hard for the most instructed observer to hear of cavernous breathing without the image of a pulmonary cavity presenting itself to his mind, or of mucous râles and cracklings of pneumonia, without the idea of mucus in the one case, and of pneumonia in the other, being for him associated with those terms.

But how must it be with the mere student, when he, " mit allem guten Sinn," comes for the first time to approach the subject of auscultation? Is he to be told at the very outset, that he must accept the terms consecrated by use amongst us, but not as in themselves conveying any idea of the things thereby signified? and must his teacher, in justice, be compelled to commit the extravagance of cautioning him that those terms do not only not represent the idea which their natural sense would indicate, but that they are often employed to indicate the very reverse of what their ordinary sense implies? If, for instance, he meet with the word rhonchus, the idea of a snoring sound naturally suggests itself to his mind, but alas! " Es ist so schwer den falschen weg zu meiden," for once that his conjecture is correct, it will turn out many times fallacious; used as a generic term, and qualified by "crepitant," he will find this rhonchus actually made to represent the finest and gentlest of crackling sounds which arise within the thorax, and thus become a fine crackling snore; then, at another time, it will be a hissing snore, a sibilant rhonchus; and then indeed a snoring snore, a sonorous rhonchus. How is it possible that, amidst such confusion of language, any exact idea of the thing signified by the author can leave its due impress on the mind of the student, or that the reader can ever be sure that he is rightly interpreting the thought which the writer desires to express? Such falsifications of terms are very finger posts, erected as if for the express object of directing the observer into the path of error: you say to him, what you hear there is a mucous râle, but it is produced by pus; that is the cavernous voice, but there may be no cavity in the lung; that is the crepitation of pneumonia, but there is no pneumonia; that is cavernous respiration, but it may arise in a bronchial tube, and so on; and then, as if to crown all, and make confusion worse confused, you indifferently affix to these different sounds different appellations; thus one and the same râle is known as a fine crepitating râle, moist crepitous rhonchus, crepitation, crepitating rhonchus, crepitant râle, minute crepitations, small crepitations, vesicular râles, &c.; and, moreover, you use terms whose meaning we have no measure to value, and which undoubtedly hold a different signification in different observer's eyes. Surely we need express no astonishment that there is a want of fixed notions and of a clear understanding amongst observers, as to the details of a study which has such a language as this for its exponent!

But it is not alone from the improper use of words that misunderstanding has been introduced into auscultation; another source of error will be found to exist in the wrongful ideas which the student is taught to attach to words which in themselves convey no erroneous impression. Thus we find pectoriloquy described as the resonance of the voice in a cavity—i.e., it remains still written down amongst us as a sign bearing the same import as given to it by Laennec; but, as I have before observed, it has been proved to demonstration, over and over again, that the most perfect pectoriloquy may exist independently of the presence of any cavern in the lungs. "A *less degree* of this sound," says Dr. Watson, "is called bronchophony," that is, is the representative of consolidated or compressed lung-tissue; but who is to assign the limits where that degree of resonance which is to be called pectoriloquy ends, and that which is to be called bronchophony begins—i.e., where we are to cease to diagnose the existence of a cavity from the quality of the thoracic voice? There can, we should imagine, be little doubt, from the great frequency with which Laennec seems to have met with this sign, that what he called pectoriloquy was in fact very frequently produced by consolidation of the lung-tissue through tubercular and inflammatory infiltrations. Even Laennec was unable to define what that quality of the voice is which marks the distinction between pectoriloquy and bronchophony, and was consequently, in practice, forced to admit a perfect and an imperfect pectoriloquy, and to trust to collateral evidence for the certain establishment of the existence of a cavity in the latter case. It surely is no light matter, in a practical point of view, whether the thoracic voice is ascribed to the presence of a pulmonary cavity, or to consolidated lung-tissue,—the one indicating the existence of a fatal disease in a fatal stage, the other perhaps being the result of simple inflammation; unhesitating faith in the sign as the indicator of a cavity may lead to error: doubt will force the observer to correct his diagnosis by other evidence.

Such are some of the many reasons which, in my opinion, prove the assertion originally made, viz., that the method of dealing with the facts of auscultation, as generally adopted in this country, is unsatisfactory, and requires modification.

The remedy proposed for the evils complained of may be gathered from what has been already said: the first grand step in a better direction would be the admission into practice of some such system of auscultatory signs, indicated as emanating from the German School of Medicine; and a reform in this particular would be the infallible precursor of other changes, especially in our valuation and interpretation of those signs.

I readily admit that the inherent difficulties of this subject of auscultation, demonstrated by the acknowledged obscurity which involves so many of its phenomena, forbid the hope of our yet attaining any perfect method, and that the study must therefore be considered still in a transition state; but then some facts are surely gained, some firm ground reached which may serve as a positive basis for further advances; and it is on this basis of sure fact that we should take up our position; there can be no hopes for better things, so long as the hypothetical is permitted to usurp the place of the demonstrated; so long as observers—instead of establishing the broad and general facts laid down by the discoverer of auscultation, by separating in them what a more extended experience and the progress of scientific research have found to be fallacious, from what bears the touch of those tests—busy themselves with impossible refinements in research, impossible because founded on and proceeding from a faulty origin. Can any one believe that the perfection of diagnosis boasted of by observers, such as Piorry or Fournet, has in reality ever been attained in actual practice? that we can distinguish, for example, between *le râle humide à bulles continues de la congestion active*, and *le râle crépitant de la pneumonie?*

But innovations are to be shunned, it will be objected, by reason of the difficulties they create. I willingly admit their undesirable nature, but in this instance insist upon them, because I think the case made out justifies them; and as the Father of Medicine said, *ες τα εσχατα νουσηματα αι εσχαται θεραπευαι*, and, happily, what I propose would be no complicating, but rather a simplification of the subject.

I have shown that very many of the terms at present used to express the abnormal sounds heard over the human thorax during the movements of respiration, must, from their nature, infallibly convey erroneous impressions to the mind of the observer; as when, for instance, men speak of "cavernous resonance,"* without any proof of the existence of a cavity, or of mucous *râles* produced by the gurglings of blood or pus: that many of these terms are used in no definite sense whatever, and by different writers to convey different ideas—e.g., mucous râle, sub-mucous râle, capillary bronchitic râle, muco-crepitant râle, mucous rhonchus, and so on: that thus different authors have their own particular phraseology, and that, consequently, the ideas of one observer can scarcely be transmitted, with any certainty of their being correctly interpreted, to another; the natural results of all this being, mystifying of the subject of auscultation, and impediments to its progress.

The plain remedy for the correction of these evils is the use of a language which simply expresses facts, and conveys no hypothesis; interpreted by such language, the *moist* abnormal sounds heard over the thorax during the respiratory movements, become bubblings, large or small, fine or coarse, scant or multiform, high-pitched or low-pitched, gurgling, &c., and thus the sound heard is simply noted down with its physical qualities, and we cease to prejudge it as a sign by affixing to it a term of hypothetical value; cavernous crepitations become inadmissible so long as we are unable to define what that quality of the crepitations is which determines

* I may observe, that some auscultators use this expression to indicate a *percussion sound.*

its cavernous nature;* and so, also, mucous, sub-mucous, &c. crepitations, because the proof that they proceed from the accidental presence of mucus is wanting, and we know that they may be produced equally well by other fluids. Such a term, again, as fine bubblings, can deceive no one; it is the plain statement of the phenomenon observed, and, unlike such phrases as "crepitations of pneumonia, cracklings of pneumonia, capillary bronchitic râles," &c., involves no *à priori* conclusions. Fine bubblings are, as shown by experience, the possible products of different morbid conditions; their real meaning, therefore, will be sought for by the observer, if his judgment be not swayed by the falseness of language, elsewhere and from other sources than from the mere sounds themselves, and his diagnosis thus be rested upon a more extended and firmer foundation.

It will, perhaps, be objected, that the terms—viz., fine, small, large, gurgling bubblings, with their various accidental qualities of high-pitched and low-pitched, of soft and coarse, of scant and multiform, metallic, &c.,—which I propose to substitute for those generally used to indicate the moist sounds heard during respiration, are of vague and indefinite value; they doubtless are so, but they are only in conformity, in such particular, with the nature of the subject they illustrate, whose phenomena, in the present condition of our knowledge, admit of no positive demonstration. The distinction between what I call fine and small bubblings may be marked enough in their extremes, between those, viz., which arise in the air-vesicles and smallest branches of the bronchial tubes, and those which arise in tubes many degrees larger in size; but how is it possible to mark or define those minute gradations by which the one set of these sounds pass into the other? our powers of diagnosis are imperfect; and so, consequently, must our language be. The attempt, indeed, of defining and giving a fixity to the innumerable varieties of sounds produced within the thorax, has been the very cause of the confusion which has attached itself to the subject, and from which I am desirous of seeing it freed.

Large and gurgling are qualifying terms, to which objections may no doubt be taken; a gurgling bubbling is of necessity a large bubbling, but we do not believe that any inconvenience can arise in practice from their use: every one will readily understand that a gurgling indicates something beyond a large bubbling; and when we speak of large bubblings, again, we are clearly only referring to sounds which exceed in degree what we term small. The truth is, that these moist bubblings form a long chain, whose extremes are wide apart, but the links forming it are continuous; and it is not, therefore, possible to do more than mark the comparative distinctions which different portions of the chain offer.

Moreover, the observer will continually meet in practice with varieties of moist sounds, which he scarcely knows how to qualify or how to class, and it is not difficult to understand the source of such confusion; bub-

* Large and small crepitations may equally arise in a cavity, and the gurgling character is, I believe, that which is generally supposed to indicate their cavernous nature; but the truth is, that gurglings, large coarse bubblings, are rare in comparison with the frequency of pulmonary cavities, and rarer, indeed, the larger the cavity; and may exist of the loudest kind in bronchial tubes. Who has not witnessed the diagnosis of cavities, over the lower part of the back, in cases of pulmonary consolidation, wrongly made solely from the observation of such gurgling crepitations?

blings of different kinds, and other sounds, may arise at the same time, and so strike upon the ear as to prevent the recognition of any particular one. The fine bubblings—Andral's term of vesicular I object to, for it prejudges the seat of the sign—for example, may be present, and yet rendered inaudible by large and coarse bubblings arising in their neighbourhood, or by some loud and distant rhonchus; and these large bubblings themselves modified, and become of indefinite character, through the disturbance of other sounds, so that there results from these combinations of sounds, sounds hard to describe, and to which we must be contented to apply the term of indeterminate—a frank admission of incapacity is surely more to be desired than a blundering attempt at correctness of diagnosis, where correctness of observation is impossible.

The bubblings here spoken of are called *moist*, because they convey to the ear the idea of the presence of fluid as the chief agent of their production, and by this character are distinguished from another set of abnormal sounds accompanying the respiratory movements which present the opposite character, of dryness; or, as Dr. Latham has well put it, of less moist sounds. The characters of these sounds are well marked, and hardly to be mistaken when uncomplicated with other sounds; they are justly and truly described by the general terms (each term of course admitting the idea of a louder or weaker sound) rhonchus, sibilus, snorings, whistlings, and hissings: the acutest of these, the hissings, must be supposed, for the most part, to have their seat in the finest of the bronchial tubes, and the gravest in the larger tubes. The word rhonchus ought to be used solely as indicative of grave, dry, respiratory sounds; but yet of what a variety of ideas has it not been made the representative by different writers: one speaks of a sonorous rhonchus, which is indeed something like speaking of sugar as being sweet; another, of crepitant rhonchus, a fine crepitating snore, and so on: nothing, surely, can be more absurd and prejudicial to a clear comprehension of the subject of auscultation, than such perversion of language; at one time employing the word rhonchus to represent the dry snoring sound of a large bronchus, and then, at another, to use it as the generic indicator of the whole series of moist crepitations, even to the softest and gentlest of them. I will venture to recall to the memory of those who thus make the word represent, under different phases, such opposite ideas, the origin of the term,—*ρογχος, ρεγχω*, to snore; if this had been always kept in view, it is certain that such vagaries would never have been played with the word.

In addition to these, I may also enumerate as dry sounds, the following varieties—viz., crackling, clicking, and friction sounds.

The variations in the force and other qualities of the voice, as heard over the thorax, may be dealt with on principles similar to those here referred to. The word pectoriloquy, to which I have several times alluded, certainly conveys in itself no objectionable meaning, but the contrary; in use, however, it has unfortunately been made to stand as the representative of a pulmonary cavity; thus, when men call the voice pectoriloquous, they mean thereby to say, that they have ascertained the existence of a pulmonary cavity: but, as we have already shown, in this sense the term is most fallacious, and should be absolutely discarded; and in its place I would substitute that of loud thoracic voice; the word loud bronchophony is open to the objections we have often raised—viz., that it conveys a meaning prejudging the fact, for loud bronchophony may be produced in a cavity as well as in a bronchial tube. There can be no doubt that pectoriloquy, weak and loud, would be the correctest expressions by which to designate the varieties of the thoracic voice, if its adoption were not precluded by the idea of a cavity, so generally attached to that word.

The varieties of sounds produced by the voice, and heard over the thorax, may be sufficiently represented as loud thoracic voice, weak thoracic voice, indistinct humming, bleating or thoracic voice, amphoric buzzing, and metallic echo of the voice. Ægophony is an objectionable term, it in fact merely points out a particular quality of the thoracic voice, and may be associated with either loud or weak bronchophony; hence it has become known amongst us, according to the taste of the describer, as ægophonic bronchophony, or as bronchophonic ægophony. I would therefore venture to suggest as a substitute for these grandiloquent barbarisms, the expression bleating voice.

There is, I believe, no abnormal auscultatory sound occurring during respiration, which may not be included under some one of the above varieties of sounds, or some combination of them: as when a moist bubbling and a whistling sound are audible simultaneously. If the sounds heard be of such a nature as to defy classification, better far, surely, to write them down indeterminate, and draw no inference therefrom, than blunderingly to attempt their reduction under some recognised class of sounds.

It is manifest, upon consideration, that no nomenclature of auscultation in the present state of our knowledge can be perfect. We cannot describe a sign in an absolute manner, for what standard have we by which to measure and try its exact value? A natural respiratory murmur is a healthy respiratory murmur; but how define positively the characters of a healthy murmur? The educated ear detects some slight shadow of difference, whether in strength, pitch, duration, smoothness, &c., in the healthy respiration of every individual; however alike in its general characters in all, it in some particular varies in each.

> " Non omnibus una,
> Nec diversa tamen,"

may be truly said of it.

Dry and moist, small and large, strong and weak, are terms of comparative meaning; the term dry merely indicates (in my sense) a less moist sound; and it is impossible to define with precision where the dryness of a sound ceases, and it begins to take a moist character; equally true is it that no exact line of demarcation can be drawn between the other opposite accidental characteristics of abnormal sounds. We must be contented with a language which gives us an *approximative* knowledge of the truth, and which thus bears a relation to, or is on a par with, the nature of the information obtainable by auscultation: which carries us along securely, though humbly, in the path of diagnosis, and is founded on facts, not on those shifting beds of sand, the opinions and fancies of individuals.

I am far from assuming, that the nomenclature suggested in the foregoing view is not open to many objections; but of this I feel well assured, that the adoption of some similar method, founded on the principles indicated in the foregoing remarks, will at least enable us to find a way to escape from those faults which are fatally entwined around our present method, and must be sooner or later adopted, unless all hopeful progress of the study of auscultation is to be abandoned, and its attainable facts still to be perverted and obscured, or buried beneath the weight of mere verbiage.

In a word, I ask for auscultation a nomenclature which shall record, in the plainest and simplest terms, the facts appreciated by the ear of the observer; which shall give admission to a class of doubtful signs; which shall not pervert or anticipate the judgment of the observer, by being possessed of a theoretical and pathognomonic value; and which shall enable him who reads to comprehend the thought of him who describes.

I here present in a tabular form the different divisions of auscultatory signs above related:—

1. *Respiratory murmur.*
   - *a.* Vesicular, { weak, smooth, distinct, interrupted. / loud, coarse, indistinct, &c. &c.
   - *b.* Bronchial, ditto, ditto.
   - *c.* Respiratory murmur, accompanied by metallic echo or amphoric resonance.
   - *d.* Indeterminate respiratory murmur.
2. *Vocal Resonance*
   - *a.* Indistinct humming.
   - *b.* Thoracic voice, { weak. / loud.
   - *c.* Bleating voice.
   - *d.* Voice accompanied by metallic echo or amphoric resonance.
3. *Dry and moist sounds heard during the movements of the respiratory organs.*
   - *a.* Dry sounds, { hissing. / whistling. / snoring. / crackling. / clicking. / friction.
   - *b.* Moist sounds (bubblings,) { fine, / small, / large, / gurgling, } scant, abundant &c., accompanied by metallic echo and amphoric resonance.
   - *c.* Indeterminate dry and moist sounds.

I have already, I believe, sufficiently extended these remarks, to give a clear idea of the objects present to my mind whilst putting them upon record: and I trust that the conclusions which a consideration of the subject has forced upon me, will find some echo in the minds of others. Assuredly I have entered upon this discussion of certain of the facts of auscultation from a strong conviction that the introduction of the busy agency of some reforming hand into the details of the study is imperatively required. I have endeavoured to point out the unsafe helps in diagnosis which auscultatory signs, measured by our present standard of their worth, frequently become: whether from the untrue ideas of which they are ofttimes made the representatives, or from the overstrained conclusions at which observers are apt to arrive through undue confidence in their powers of diagnosis, engendered by an ill-founded faith in the value of those signs. I have, moreover, ventured to suggest that there is in another direction a path of safety, along which we may travel with less chance of blundering, but one less seductive than the other, for it leaves little room for the exercise of the fancy; and less flattering to the vanity, inasmuch as it offers fewer occasions for exhibitions of that minuteness and correctness of observation upon which observers are at times too apt to pride themselves, even when the nature of the subject investigated renders these impossible. And following this path, we shall avoid another error, into which I believe I am correct in stating that auscultators, as such, have a tendency to fall, and that is, into an improper exaltation of the stethoscopic signs over the general symptoms, so that the disease is read and treated by the guidance of the former signs, to the prejudice of, and without due consideration being given to, the general symptoms.

I need scarcely say, in concluding, to those who have paid any attention to the subject, that the ideas of reform herein referred to, are none of my own, but proceed from that German school of which Skoda is the acknowledged head; I have introduced them here in a crude and germinal condition, with the hopes of bringing the subject more positively before the attention of auscultators. Criticism and discussion I am convinced are all they need to demonstrate their value, and it is this criticism and this discussion of the subject which I desire thereby to evoke.

It is surely not as a mere matter of curiosity, or as an interesting episode in the history of auscultation, that I have here introduced the subject, but from a firm conviction that the German method is correct, and the French wrong, in principle; and that by adopting the one, auscultation will be placed upon a higher and firmer position than it can yet boast to have attained; and that by continuing to follow the other, we must be contented still to find its signs very frequently the faithless and erring representatives of diseased conditions of the thoracic organs.

Art. IV.

On some points in the Abnormal Anatomy of the Arm. By John Struthers, F.R.C.S., Lecturer on Anatomy, Edinburgh.

A considerable number of varieties in the anatomy of the brachial region have fallen under my notice in the dissecting-room, and some of these are so rare and interesting, that I have thought it worth while to draw up a systematic account of them, and especially to call the attention of anatomists to the occurrence of the supra-condyloid process on the humerus of man.

I shall arrange my cases and remarks under three heads: Varieties of the Muscles—The Supra-condyloid Process—and Varieties of the Arteries.

I. Varieties of the Muscles in relation to the Axillary and Brachial Arteries.

1. *The Axillary Artery crossed by a Muscular Band.*

It is noticed by several anatomists that, occasionally, a slip of muscle is prolonged from the latissimus dorsi, across the axilla, to join the tendon of the pectoralis major, the coraco-brachialis, or the fascia over the biceps. This variety has occurred in 8 out of 105 subjects dissected in my rooms since the first instance was noted. Of these 8, at ages between 3 and 70, 3 were males, and 5 females. In 7, it was present on both sides, of equal size in 5 of these 7, and in 2 of them, broader on the left than on the right side. In 1 it existed on the right side only, and a few scattered fibres were prolonged from the pectoralis major to join it. These fibres were present on the left side also, but not the slip.

This abnormal muscular band usually arises from the upper edge of the latissimus dorsi, about the middle of the posterior fold of the axilla. It is usually, I find, not a direct prolongation from the fibres of the latissimus, but, while the higher fibres may be so, the greater part or whole arises by a tendinous intersection between it and the fleshy fibres of the latissimus, or by a short intervening tendon, which, in one instance, was an inch in length. The muscular band varied from a quarter to three-quarters of an inch in breadth; in one case it was as thick as the pectoralis minor, where this muscle crosses the axillary vessels; and its length varied from three to four inches. It disappears under the tendon of the pectoralis major, and in four of the cases, where it was afterwards traced, it formed a tendinous expansion, and became blended with the tendon of the pectoralis major.*

The point of greatest interest in connexion with this variety is the position of the muscle in relation to the axillary artery, as it might mislead the surgeon in the operation of tying this vessel. It crossed always in front of the axillary vessels and nerves, and usually opposite the upper part of the conjoined tendon of the latissimus dorsi and teres major. It occupied this situation in all of the eight instances, except the one in which it was present on one side only, and, in this instance, it crossed as high up as opposite the tendon of the subscapularis muscle, and an inch higher than the edge of the conjoined tendon. It is usually fleshy where it crosses the vessels; but in one instance was tendinous on its posterior surface at this point.

As the artery in the upper part of the third stage of its course, opposite to, or at the lower edge of, the subscapularis muscle, gives off large branches, the circumflex and subscapular, the part to be chosen for operation is where it lies upon the conjoined tendon;† and, therefore, a little

* This corresponds to the normal arrangement in some animals. It is well seen in the cat. As the latissimus dorsi approaches the axillary vessels it divides, the anterior part, being about a fourth of the entire muscle, passing across or in front of the axillary vessels and nerves, and ends in an expanded tendon, which above joins with the deep part of the pectoral muscle, and below expands as an aponeurosis, binding down the flexor radii muscle, which corresponds to the biceps of man. The great part of the latissimus crosses behind the vessels and nerves with the teres major, and rather higher up than the anterior portion. Both are fleshy where the vessels and nerves lie between them. The whole mouth of the axilla is closed over by a thin muscular layer, continued upwards to the edge of the pectoral muscle from the edge of the anterior part of the latissimus, and also partly from behind from the panniculus muscle. The fleshy fibres forming the anterior part of the latissimus are continued onwards without any intervening tendinous intersection. In the cat, this anterior part of the muscle, as it crosses over the vessels and nerves, is one-third of an inch broad. In the lion, it forms a tendon broader than the conjoined tendon of man, though not so thick; the lower edge continued into the aponeurotic expansion over the brachial region, the upper edge rounded and free, and the tendon, having crossed over the vessels and nerves, runs in underneath the pectoral muscle, to be inserted into the same ridge on the humerus, having formed, at the same time, a large expansion over the flexor radii muscle, which it thus binds down.

† I use the term "conjoined" tendon, as it is a convenient one, in describing the relative anatomy of the parts. Strictly speaking, there is but a very partial insertion of the fibres of the teres major into the tendon of the latissimus; and a little further on, they are altogether separated by a bursa, the teres ending in the internal bicipital ridge, while the latissimus glides over the ridge and bursa, and is inserted in the floor of the bicipital groove.

below where this muscular slip crosses over the vessels. It will, however, be exposed in the operation. In one of the cases, the operation was being practised by one of the pupils, and the ligature had been applied below the muscle on one side, and above it on the other. In another of the cases, I was showing the operation, and the muscle was exposed in the wound, and recognised at a short distance above the point where the artery was tied. The fibres of this unusual muscle are transverse, whilst those of the coraco-brachialis, the only normal muscle which should be seen in the operation, are vertical.

2. *Muscle concealing the Brachial Artery in the Upper Third of the Arm.*

This variety occurred in the right arm of a male subject in 1851. The coraco-brachialis muscle is twice the usual size, and sends a muscular layer over the vessels and median nerve. This muscular covering begins at the lower edge of the conjoined tendon, and reaches down for three inches, near to the insertion of the coraco-brachialis. It is of considerable thickness, passes quite round to the inner side of the vessels, and joins the internal head of the triceps, or a tendinous septum between them. There is thus formed a kind of tunnel, which admits the little finger, and ends below by a distinct tendinous arch, the outer side of which is the ordinary tendon of insertion of the coraco-brachialis. This tunnel encloses the undivided brachial artery, its venæ comites, and the median nerve, in their usual relative position. There is no other variety, except that the posterior circumflex artery arises with the superior profunda, and, as usual when it so arises, courses up behind the conjoined tendon to gain its usual position at the neck of the humerus.

During the present winter, I met with an example of an approach to this variety. The coraco-brachialis sent a thin muscular prolongation inwards over the vessels and nerve, as far as to conceal them partially; but on division of the aponeurosis continued from it, it passed outwards, and left the sheath exposed.

3. *Brachial Artery concealed in the Lower Half of the Arm by a Broad Third Head to the Biceps Muscle.*

This variety occurred in 1848, in the right arm of a male subject. The brachial artery is concealed in the lower half of its course by a broad, thick, muscular layer, extending from the intermuscular septum to the biceps muscle. The artery, its venæ comites, and the median nerve, disappear through a tendinous arch, situated a little above midway between the internal condyle and the lower edge of the tendon of the teres major. This fibrous arch is about an inch in length, extending obliquely downwards and inwards, and is continuous above with the insertion of the coraco-brachialis, and below with the intermuscular septum.

The unusual muscle arises from this arch, and from the intermuscular septum, as far down as to within an inch of the internal condyle, being a length of four inches and a half, leaving only a narrow cellular space between it and the pronator teres. The fibres pass downwards and outwards, approaching and accompanying the inner part of the biceps. About one half of the muscle ends in the tendon of the biceps, just where that tendon receives the belly of its muscle, and the other half ends in the semi-lunar process from the biceps to the fascia of the forearm. It forms an uninterrupted muscular layer of a thickness between an ordinary sartorius or gracilis muscle, concealing the brachial vessels and median nerve in more than the lower half of their course. As it approaches the biceps, it becomes narrower, from its obliquity, so that where it joins the biceps, it is only two inches in breadth. When the upper part of it is held inwards from the biceps, the upper part of the artery is exposed; but beneath its lower half, the artery could not be exposed without division of the muscle. The radial artery appears as usual between the tendon and semi-lunar process of the biceps.

On raising the biceps and looking under it from the outside, this large unusual third head is seen to be double, or split by the artery, a portion lying underneath the vessel besides that already described. This deeper portion corresponds to the upper half of the superficial portion. At its origin from the intermuscular septum and internal margin of the humerus, it is two inches in breadth, but contracts below to three fourths of an inch, and joins partly the tendon and partly the semi-lunar process of the biceps. The lower edge of this deep portion seemed to twist round or cross over the artery on its outside, to reach the semi-lunar process in front of it. This deep portion was thicker, though narrower, than the superficial. The fibrous arch protected the vessels and nerve from pressure as they entered on the deep part of their course; but it is not easy to see how below this the artery could escape compression, as it lay close between the two layers of muscle.

We have here then an instance of a large third head to the biceps muscle, consisting of a superficial portion, forming a broad continuous muscle concealing the lower half of the brachial artery, and of a deeper portion, lying at first beneath the artery, and about half the breadth of the other.

On the left side of this subject, the biceps had a third head, corresponding to the deep portion of the third head on the right side, and, like it, lying behind the artery. There was no other variety on the left side.

4. *Muscular or Tendinous Slip passing inwards across Brachial Artery in Lower Third of Arm.*

The three following instances have occurred during the present winter session.

(1) *Slip from biceps across brachial artery.*—This fleshy slip is three inches in length, one quarter of an inch in breadth, and one eighth of an inch in thickness, and ends in a broad aponeurotic tendon two inches in length. The muscle separates from the inner edge of the biceps two inches below the lower margin of the teres major. It crosses obliquely over the brachial artery and conceals it for an inch and a half. The belly having just crossed to the inside of the artery, the tendon is placed to the inside, as it passes down to end in an aponeurosis over the pronator teres, close above and to the outer side of the internal condyle. The inner edge of the tendon is joined to the intermuscular septum. Between its outer

margin and the tendon and semi-lunar process of the biceps, there is now a triangular space three quarters of an inch in breadth below, but they appear to have been joined across in front of the artery by a thin portion, although this cannot be determined now. This variety occurred on the left arm of an adult subject, and appears, from the remains of the tendon, to have been present on the other side also.

(2) *Muscular and tendinous slip crossing over brachial artery.*—This variety occurred in the left arm of an adult female subject. The muscle arises from the external bicipital ridge by a long tendon, and again ends in a tendon, which is inserted into the aponeurosis over the pronator teres. The upper tendon arises from the upper part of the bicipital ridge of the humerus close to the great tuberosity, and crosses obliquely behind the long tendon of the biceps in the groove. After a course of two inches and a half it ends in a fleshy belly, which appears on the inside of the arm between the biceps and coraco-brachialis muscles; passes down along the inner edge of the former, and parallel to the outer side of the artery, which it now crosses obliquely, and, after a course of three inches, ends in a narrow flattened tendon, which is three inches in length before it spreads out over the pronator three quarters of an inch below the condyle. The fleshy portion is one sixth of an inch and the lower tendon one twelfth of an inch in breadth as they lie in front of the artery. They cross the artery very obliquely so as to lie in front of it for three inches, one inch of the fleshy and two inches of the tendinous portion. The artery lies in a groove in the brachialis anticus muscle, from the raised portion on the outer side of which is sent off, over the artery, an aponeurosis to join the tendon of this abnormal muscle, which is quite distinct from the intermuscular septum.

(3) *Tendinous slip from pectoralis major to internal condyle, crossing over brachial artery.*—This variety occurred on the right arm only, in an adult female subject. The pectoralis major gave off its usual expansion to the aponeurosis of the arm. The tendinous slip comes from below this, from the deeper part of the tendon of the pectoral, as a cord or tendon about the size of a common probe. It crosses over the brachial artery obliquely, at and below the insertion of the coraco-brachialis. It now lies behind the basilic vein and internal cutaneous nerve, passes to join the true intermuscular septum, and is continued with it to the condyle, an inch above which it also joins with the ligamentous cord behind the septum, from which, above, it is quite distinct. As this long ligamentous arch passes down the arm, its outer or anterior edge is free and rounded, and the posterior edge is continued backwards into the deep fascia of the arm. It lies obliquely over the artery for about an inch, just at, and a little below, the tendon of the coraco-brachialis. In an operation, this slip might mislead from its resemblance to the intermuscular septum.

Remarks.—The two cases first related exceed any that I have read of in the extent to which the artery was concealed by a muscular covering. Mr. Quain* has met with a case in which the artery was covered, for an inch and a half, by a thick muscular slip from the biceps to the inter-

* The Anatomy of the Arteries of the Human Body. London, 1844.

muscular septum (plate 37, fig. 5). He also mentions another in which a third head to the biceps crossed over, instead of under, the artery; and refers to a preparation in Mr. Allan Burns's museum, in which a slip of muscle passed across the brachial artery, and is said to have impaired the activity of the limb during life.

The surgeon should be prepared to meet with these varieties of the muscles. Certainly, the operation of tying the brachial artery in the two cases first related would have been a puzzling one, from the resemblance of the unusual muscle to the natural coraco-brachialis and brachialis anticus. But, seeing that the incisions were well-placed, the nature of the difficulty might be recognised, and the pulsation of the covered artery, and the effect of pressure on it, would probably indicate the position in which it would be found on division of the muscle. If there happened to be a high division, and one of the arteries placed under such a muscle, it could scarcely but be overlooked in the operation.

5. *Brachial or Third Head to Biceps Muscle.*

The occasional occurrence of a third head to the biceps has long been well known to anatomists. Theile* states that it occurs as often as once in eight or nine subjects. In one case there were two additional heads from the humerus. I have noted this variety in four instances during the present session.

(1) Third head arises as upper and inner part of brachialis anticus, with the fibres of which it is here quite continuous. It is half an inch broad and six inches long, and ends in the upper and inner part of a broader than usual semilunar process, and in the contiguous part of the tendon of the biceps. Left arm: no third head on right.

(2) Right arm, none on left side. Arises as highest and most internal part of the brachialis anticus, immediately outside insertion of coraco-brachialis. Is half an inch in breadth and five inches in length. Inserted, fleshy, into deep surface and inner edge of tendon of biceps at its commencement, and a few fibres run into the semi-lunar process.

(3) Both arms. Is small and narrow like a flattened lumbricalis muscle. Arises by a short tendon, as in the preceding cases, and inserted also as in them.

(4) Right arm. Left unascertained. Arises separate from the fibres of the brachialis anticus, for two inches in depth, immediately external to insertion of coraco-brachialis, but reaching an inch above this, and also higher up than the brachialis anticus. Is one inch in breadth and six inches in length. Insertion exactly as in preceding cases. As the third head in all of these cases passed, as usual, behind the brachial artery, this occurrence does not affect the surgical anatomy of that vessel.

6. *Brachial Artery overlapped or covered by a portion of the Brachialis Anticus Muscle.*

Mr. Quain relates two cases in which he saw the principal of two arterial trunks covered by a thin layer of the brachialis anticus muscle

* Traité de Myologie et d'Angéiologie. F. G. Theile. Encyclopédie Anatomique. Paris, 1843.

(plate 37, figs. 3 and 4), and mentions in connexion with this, that the fibres of the brachialis not unfrequently project at the outer side of the artery, in some cases even overlapping it. This condition I have repeatedly noticed, so often indeed that I gave up noting the individual cases. It occurs more frequently perhaps in the case of two arteries, when the deeper one has this position, but often enough it is seen with the single and otherwise normal artery. It is seen in various degrees. The brachialis presents a raised portion on the outside of the artery, and an aponeurosis, deeper than the common aponeurosis of the arm, is sent inwards from it, over the artery, which is thus closely bound down upon the brachialis, and is as if sunk in a groove in the substance of the muscle. In a further stage, the fleshy fibres overlap the artery from the outside, although not so far but that, on division of the aponeurosis, these overlapping fibres slip outwards off the artery. In one of the cases noted this winter, the artery in both arms of an adult female subject was thus covered for three inches. But what is most commonly seen is a raised portion of the brachialis muscle projecting on the outside of the artery, sending a deep aponeurosis inwards over it, and binding the artery down in a kind of groove in the muscle.

7. *High Origin to Pronator Teres Muscle.*

We not unfrequently meet with the pronator teres muscle arising a little higher than usual above the internal condyle, but without much altering the relative anatomy of the bend of the arm. In the two following instances, however, the muscle arose unusually high, and changed the relative position of the arteries at the bend.

(1) On *left* side pronator teres arises one inch and a half above the condyle. An aponeurosis reaches from it to join brachialis anticus, external to position of artery. There is thus formed a kind of arch or tunnel, under which the principal artery and the median nerve pass, so as to become concealed half an inch above the transverse level of the condyle. Radial artery arises above middle of arm, and passes over this aponeurosis, but lies under the aponeurosis of the arm and semi-lunar process of the biceps.

Right side. No high origin to pronator, but a high division of artery, and the two arteries are separated by a deep layer of aponeurosis at the bend of the arm.

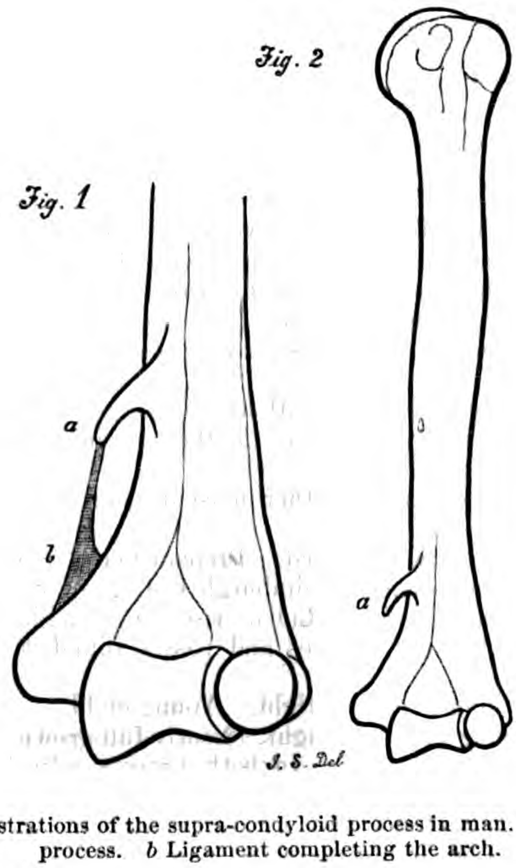
(2) Right side. All the anatomy on left side normal. Pronator teres arises two inches above condyle, from intermuscular septum. The high portion is almost a separate muscle, being at first separated from the usual pronator by a narrow cellular space. High pronator is soon joined, by aponeurotic fibres, to a raised portion of the brachialis anticus external to the position of the artery. Radial artery arises in axilla, and is covered by the usual aponeurosis of the arm and the semi-lunar process of the biceps, but the principal artery passes, along with the median nerve, underneath the above-mentioned deep aponeurosis and high pronator, by which they are concealed for an inch and a half above the transverse level of the condyle.

II. On the Occurrence of a Supra-Condyloid Process in Man.

The part so named is a more or less hook-shaped process, which is occasionally developed on the inner surface of the humerus, two inches above the internal condyle. A ligament is continued from it to near the condyle, completing an arch, through which the median nerve and brachial artery pass, after deviating from their usual course; the whole forming an arrangement analogous to that which obtains in many animals, in the passage of the nerve and artery through an opening, the supra-condyloid foramen, in the humerus, in the same situation.

A few instances of this variety had been recorded previous to 1848, when I gave a short account* of several which I had met with in the dissecting-room, and I have now met with the variety so frequently, that I am desirous of noticing it more fully, and of calling the attention of anatomists to it, as a variety which, I believe, will be found to occur not unfrequently, to a more or less marked degree, if this part of the arm be carefully examined.

The accompanying sketches are taken from two of the numerous specimens of this variety now in my collection. In fig. 2, the bone and process



Figs. 1 and 2. Illustrations of the supra-condyloid process in man. *a a* Supra-condyloid process. *b* Ligament completing the arch.

are represented one-fourth the natural size. In fig. 1, the lower end of the humerus is shown half the natural size. The supra-condyloid process is broader and stronger in this specimen. The bone is seen a little on its

* Edinburgh Monthly Journal, Oct. 1848.

external and inferior aspect, to show the size of the oval space, completed by the ligament, through which the nerve and vessels passed. Fig. 2 is from case No. 4, and Fig. 1 from case No. 8, in the following list.

List of Cases of Supra-condyloid Process, with notes of the peculiarities of each.

1. Left side. Process one-third of an inch in length. Adult female. Nothing known of right side.

2. Both sides. Process broad and long; nerve and artery passed round it on both sides. Adult female.

3. Both sides. Process of moderate length. Nerve and artery passed round on both sides. Adult male.

4. Both sides. Process three-quarters of an inch long on left side, and very short on right. Nerye and artery passed round on left side; on right side, nerve only, accompanied by small muscular arterial twig.

5. On right side only. Process of moderate length, and well-marked groove behind it. Nerve and artery pass round process.—*Left side.* Groove well marked, though process entirely wanting. High division at axilla; both arteries run down inner edge of biceps, radial most external. A small third head to biceps, behind arteries. Female æt. 37.

6. Right side only. Process was of good length, but now partly broken off. Median nerve passed round process, but artery did not. Uncertain whether a small branch did not accompany nerve.—*Left side.* All anatomy normal. Female æt. 4.

7. Right side only. Process moderate. Median nerve passed round process, accompanied by small muscular artery from inferior profunda. Artery did not deviate—was crossed behind by median nerve—divides an inch above elbow, and ulnar passes superficially, close under aponeurosis of forearm.—*Left side.* No process, but a well-marked groove, and slight roughness just where process arises on other side. High division of artery at lower edge of conjoined tendon. Arteries usual in position, separated only by median nerve. Female æt. 50.

8. Left side. Nothing known of right. Process three-quarters of an inch in length. Nerve and artery pass round it. This specimen is from the case related by Dr. Knox,* and came into my possession with his Museum of Human Anatomy. Middle-aged male.

9. Left humerus, in my collection, without any history. Rough ridge a quarter of an inch in length, where process arises in other specimens, and a distinct groove behind it.

The following are specimens in other museums, or from cases described by others.

10. Both sides. Process well formed. Preparation of child, showing arteries, in Barclay collection in museum of Edinburgh College of Surgeons.—*Right side.* Artery passes round process.—*Left.* Radial arises above middle of arm, and keeps by edge of biceps; chief trunk deviates and passes round process. Nerves not preserved.

11. Left side. Nothing known of right. Young child.

12. Left side. Nothing known of right. Nearly full-grown arm. This, and the last, also in Dr. Barclay's collection. In both, process is well developed, and in both there is the same arrangement of arteries as on left side of No. 10.

13. Right side only. Skeleton, also in College museum. Female, and probably æt. about 30. Process moderate on right humerus, with well-marked groove behind it. Left has no process, and only a shallow groove.

14. Case by Tiedemann.† Right side. Nothing said regarding left. Process distinct. Brachial artery usual in position. High interosseous passes round pro-

* Edinburgh Med. and Surg. Journal, 1841.

† Tabulæ Arteriarum, 1822. Plate xv., fig. 3.

cess; nerve not represented. Process designated "an unusual excrescence from the humerus."

15. Case by Mr. Quain.* Right side. No information regarding left. Adult. Nerve and artery pass round process.

The following generalizations may be made with regard to this process, and its relation to the nerve and artery.

As regards Situation, Length, and Form.—The situation is remarkably constant. In the adult bones it is two inches above the internal condyle, measuring from the upper edge of that prominence to the middle of the base of the process. In one it was one-sixth of an inch nearer, and in one a quarter of an inch farther from, the condyle. In case No. 2 only did it lie considerably nearer the condyle, being an inch and a quarter from it, and here the process was long and of unusual breadth.

The length varies from one-tenth of an inch to three-quarters of an inch. It is seen in all stages—as a short rough line, a pointed tubercle rising from a base elongated upwards and downwards, and a hook or spur-like process of greater or less length. I have never seen it longer than three-quarters of an inch. In the specimens figured it is of this length. It begins by a vertical ridge half an inch to an inch in length, gradually rising to the commencement of the process proper. The process projects away from the bone, forwards, downwards, and inwards, is flattened from before backwards, tapers to a blunt point; and, if prolonged for an inch, would form an arch of bone joining the ridge half an inch above the condyle. This is represented by the ligament; and the process and ligament together, inclose between them and the bone an elliptical space, an inch in length and one-third of an inch in breadth, through which the median nerve and the brachial artery, with its venæ comites, passed.

The orgin of the process with regard to the borders of the humerus is also constant. It arises from the internal surface of the bone, midway between the internal and anterior borders, or a little nearer to the latter; and behind it there is usually a well-marked groove, to which I shall again allude as existing on many arm bones on which there is no process.

As regards Symmetry.—In 7 of the cases, nothing was known of the other arm. Of these 7, 6 were on the left arm and 1 on the right.

In 4 of the cases it was present on both sides. In 3 of these, it was of equal length on the two sides; in one pair long, in one pair short, and in the third of medium length; and in the fourth they were unequal, the left very long, the right very short.

In 4 cases in which both bones were examined, but in which the process was present on one side only, this side was the right in all; but in the case where it was present on both sides, and unequal, the left was much the longest.

As regards Sex and Age.—The sex was known in 10 of the cases; 3 of these were in males, and 7 in females. In 2 of the 3 males it was present on both sides, and was so in only 1 of the females. The specimens are from subjects of all ages. Three of them were young children, and in all of the three the process is well formed.

As regards the Arteries.—The state of the arteries was known in 12 of

* Op. cit., p. 223. Plate xxxvi, fig. 3.

the subjects, and 4 of these presenting the process on both sides, we have 16 instances in which to notice the relation of the arteries to the process.

Where the artery is affected by the existence of the process, it leaves the biceps near the insertion of the coraco-brachialis, and passes down, with the median nerve, along the internal intermuscular septum, to reach the concavity of the process, underneath which it then passes.

Out of the 16 instances, in 9, the undivided brachial artery deviated and passed round the process; in 4, there was a high division, one of the arteries keeping normally along the edge of the biceps, the other deviating to pass round the process. The vessel which deviated in 3 of these 4 was the principal trunk, the high radial not deviating; in the remaining instance—that by Tiedemann—the deviating vessel was a high interosseous. In the remaining 3, there was no deviation of the artery, although the process was present. In 2 of these, the other arm was in all respects normal; in the third, there was a long process, and the artery deviated.

Relation to Median Nerve.—In all the instances in which the artery deviated, and in which the position of the nerve was known, it also deviated and passed around the process, lying internal to the artery as they entered the arch. In 3 of my cases the nerve deviated without the artery. In at least 2 of these it was accompanied by a small artery derived from the inferior profunda, which ran down with it in the same way as the inferior profunda itself runs down with the ulnar nerve. This, however, was not a leading vessel, but only a muscular branch, not extending beyond the elbow, nor again joining with the brachial or either of its divisions. In every case where there was a process, whether short or long, and the nerve examined, it deviated to pass under the process, whilst in 3 of these instances the artery did not deviate with it. This points to the inference that the supra-condyloid foramen is provided not so much for the artery, as is commonly supposed, but principally for the nerve.

(*To be continued.*)

ART. V.

On the Existence of Sugar in the Liver and other parts of Hybernating Animals. By Dr. G. VALENTIN, Professor of Physiology in the University of Bern; communicated by WILLIAM BRINTON, M.D., (Joint) Lecturer on Physiology at St. Thomas's Hospital, &c.

THE following is an extract from a letter recently addressed to Dr. Brinton by Professor Valentin:

"As you are desirous of knowing something about my researches on the winter-sleep, I venture to select a point which has lately excited much attention,—viz., the sugary content of the organs.

"You are aware that Bernard discovered the existence of sugar in the liver. His numerous experiments upon this subject were published last year in France and Germany, simultaneously.

"It has hitherto been believed that, in continuous fasting, the hepatic sugar constantly decreases, and at last altogether disappears. Hence it became interesting to determine how it was affected by hybernation.

"There are four chief methods of recognising the sugar:—

"1. That of Peligot and Heller. When a solution of sugar is boiled with a solution of caustic potash, it becomes brown or brownish yellow, and developes a treacly odour. This method certainly recognises very small quantities of sugar. But many deceptions may occur; and it is useless for a quantitative determination of the saccharine contents.

"2. The fermentation-test consists in the mixture of the suspected saccharine solution with yeast, and in the decomposition of the sugar, under warmth, into carbonic acid and water. But even with large quantities of sugar, the exact amount of this substance can only be approximatively deduced from the carbonic acid developed; since considerable quantities of this gas are absorbed by the moisture present. And an attempt to expel this absorbed gas by heat introduces grave sources of error into the experiment.

"3. Trommer's test is very serviceable, when people understand how to go about it: but when this is not the case, errors are very easily made. The best means of applying it is by Fehling's solution.* And, in order to be accurate, we must proceed in the following manner:—We must mix the copper solution with the fluid to be tested, and cautiously warm the whole over the spirit lamp, *without* boiling it. Supposing grape, urinary, or hepatic sugar to be present, we then see a few bubbles of gas, the yellow colour of reduction breaks out in the warmest place, and rapidly spreads over the remainder of the fluid. Even when none but minute traces of sugar are present, the fluid becomes yellow throughout; though it remains transparent, and only deposits its precipitate of oxide of copper on long standing. And where the whole contains a moderate quantity of these kinds of sugar, the above procedure renders it intensely yellow and opaque; so that we get very voluminous precipitates from but small amounts of saccharine matter. If we *boil* the mixture, we now and then get a reduction, even in the absence of grape sugar, or any substance akin to it,—or in the mere presence of other organic substances, whose nature I should not like to specify. Hence I should hesitate to refer to sugar any of those reactions which only appear *after* boiling. This procedure is also well adapted to determine the quantity of the sugar. For this purpose I make use of one part of Fehling's solution, and nineteen parts of distilled water. One cubic inch of this corresponds to about ·0734 grains of sugar, and with tolerable care no errors of importance can be committed.

"4. The polarizing apparatus shows the presence of grape sugar very well, by its rotating the plane of polarization towards the right. For mere ordinary recognition in this way, the small French apparatus used to determine the sugary content of milk (and consisting of two Nicol's prisms) is quite sufficient. But it is not so well adapted for quantitative researches; since one degree of the scale corresponds to 1·1 per cent. of sugar, and it is easy to make a mistake of one degree in adjusting the analyzing prism. In order to obtain the quantities of sugar by this physical method, we must have either a larger scale, or a suitable prism of calcareous spar, instead of an analyzing Nicol.

* Text Book of Physiology, p. 292.

"In examining any organ for sugar, it must be cut into shreds, or rubbed down, and then boiled continuously with water. The filtered solution is then to be examined with the copper test. Should this give a positive result, the observations must be repeated with the solution of caustic potash and the polarizing apparatus.

"In a marmot which had begun to hybernate only four days before, but had eaten nothing for a week, the stomach was found quite void of food, and containing nothing but the grayish-white neutral substance usually present during the winter-sleep. The solution of its liver afforded, however, a large quantity of sugar. This observation rather surprised me, and led me to examine more carefully into the matter.

"A male marmot, which at the beginning of the winter-sleep had weighed $2\frac{1}{3}$ lbs. avoirdupois, and had eaten nothing for many days before falling asleep, lay twenty-five days fast asleep in a glass receiver. It then woke up; but fell asleep on the following day, and again remained eleven days in a state of complete hybernation. It now woke up a second time, passed urine and fæces for the first time since beginning to hybernate, and next day fell asleep again. After lying three days in this state, it was killed by being kept for some hours in an air-tight receiver. The dark-blue colour of the tongue showed that it had died of asphyxia. The body had lost nearly three ounces (about one-twelfth) of its weight, since the beginning of hybernation.

"The yellow and perfectly clear solution of the liver had a neutral reaction, was unchanged by nitric or muriatic acid, but was rendered opalescent by sulphuric acid. It caused an energetic reduction of the oxide of copper, acquired a deep yellow colour by boiling with solution of potash, underwent an active fermentation when mixed with yeast, and rotated the plane of polarization towards the right. We had thus a distinct saccharine content in the liver of a hybernating animal which had not taken food of any kind for more than six weeks.

"Assaying the fresh liver by means of the prepared solution of copper,* showed that it contained 2·87 per cent. (by weight) of sugar. Its finely divided substance had to be boiled with thrice its quantity of water, renewed no less than six different times, before the last solution failed to give a saccharine reaction.

"Unexpected as were these results, they were equalled by those obtained from the examination of other parts. Of these, the solid organs were treated like the liver; while the blood, which remained fluid many days, was merely boiled and filtered. On account of its want of coagulability, it did not require the addition of any sulphate of soda.

"We may group the results of these examinations as follows:—

[* Should the reader desire to prepare such a solution for quantitative estimates, his simplest plan will be that of dissolving, in a pint of distilled water, 351 grains of sulphate of copper, 1403 grains of tartrate of potash, and 4910 grains of a solution of soda, having the specific gravity of 1·12. A fluid ounce of this coppery liquid corresponds to about two and a half (2·53) grains of grape sugar in the urine; which latter should be diluted and dropped into it until a precipitate ceases to be formed.—W.B.]

Reduction of Oxide of Copper—

Strong.	Weak.	Doubtful.	No trace.
Decoction of the substance of liver. Boiled and filtered blood flowing from an incision in the liver. Bile. Greyish-white gastric fluid.	Boiled and filtered diaphragm. Very weak. Filtered decoction of right supra-renal capsule, and of the muscular substance of the heart.	Filtered decoction of the tongue, deltoid, and pectoralis major.	Filtered decoction of neutral duodenal mucus, contents of cæcum, spleen, left supra-renal, left lachrymal and submaxillary glands, hybernating (thymus ?) gland, kidney, bladder, brain, and blood of inferior cava and left ventricle.

"About $\frac{2}{5}$ths of a cubic inch of the blood which had flowed from an incision in the left lobe of the liver was boiled, filtered, and assayed with the prepared solution of copper. It yielded ·82 per cent. (by weight) of sugar.

"The bile and the watery contents of the stomach were also shown to contain sugar, by Fehling's solution. But their small quantity did not suffice for the test of fermentation, and still less for that of polarization.

"The diaphragm, which gave a marked saccharine reaction, was carefully washed with water before being boiled, to remove every trace of the hepatic blood which might have come into contact with it. The same statement holds good of the right supra-renal capsule also.

"This diffusion of the sugar in a hybernating marmot is remarkably contrasted with some observations of mine on a young hedgehog. The animal was *not* in a state of complete hybernation. It breathed deeply when gently touched and shaken; woke up about every week (once completely); and finally died after two months' abstinence from food. Not a trace of sugar could be found either in the liver, the bile, the diaphragm or other muscles, the kidney, or the bladder. But one must remember that, in this instance, the winter sleep was imperfect, respiration frequent, and finally, death was probably due to starvation."

PART FOURTH.

Chronicle of Medical Science.*

ANNALS OF MICROLOGY.

By Robert D. Lyons, M.B., T.C.D., M.R.I.A.
Honorary Professor of Anatomy to the Royal Dublin Society, &c. &c.

[Second Year.]

Part I.—Anatomical and Physiological Micrology.†

HISTOGENESIS.

We have but few researches to notice specially relating to this subject. The Annalist has communicated some results of investigations on the primary stages of histogenesis, which have been already noticed.‡ In a critical review, which contains also much highly valuable original matter, Huxley has entered on the consideration of the cell-theory in a large and comprehensive manner. §

* The Annals of Micrology give, in two sections (Physiological and Pathological), the advances made in minute anatomy during the year. The Quarterly Reports on Medicine, Surgery, Midwifery, and Medical Jurisprudence, embody the most important facts which appear during the last month of one quarter, and the two first months of the succeeding one. In the present No., the Surgical Report has been unavoidably omitted for want of room.

During the months of December, 1853, January and February, 1854, the following foreign journals were received:

GERMAN.

1. Archiv für phys. Heilkunde, von Vierordt. 1853, Heft 4; 1854, Heft 1.
2. Archiv des Vereins für gemeinschaftliche Arbeiten. Band i. Heft 3.
3. Archiv für pathol. Anat., von Virchow. Band vi. Heft 2.
4. Archiv für Anat., von J. Müller. 1853, Heft 5.
5. Archiv für phys. und pathol. Chemie, von Heller. Heft 8, 9.
6. Zeitschrift für rat. Med. von Henle und Pfeüffer. Band iv. (N. F.) Heft 1.
7. Zeitschrift (Henke's) für die Staattarzneikunde, von Behrend. 1854. Heft 1.
8. Zeitschrift der K. K. Gesell. der Aertze zu Wien, von Hebra. 1853, Oct. Nov. Dec.; 1854, Jan.
9. Prag. Vierteljahrschrift für die Prak. Heilk. 1853, Band xxiv. (Partly reviewed in our last number.)
10. Schmidt's Jahrbucher. 1853, No. 12; 1854, Nos. 1, 2.

FRENCH.

11. Archives Générales de Médecine. 1853, Dec.; 1854, Jan. Fév.
12. Revue Méd. Chir. de Paris. 1854, Jan. Fév.
13. Bull. Gén. de Thérapeutique. 1854, Nos. 1—3.
14. L'Union Médicale. Dec. Jan. Fév.

ITALIAN.

15. Bulletino delle Scienze Mediche. Bologna. 1853, Aug. Sept. Oct.

AMERICAN.

16. The American Journal of Science and Arts. 1853, Nov.
17. The American Medical Monthly Journal. 1854, Jan. Feb.
18. The Philadelphia Medical Examiner. 1853, Oct. Nov. Dec.; 1854, Jan.
19. The American Journal of Insanity. 1853, Nov. Dec.
20. The Southern Journal of the Medical and Physical Sciences. 1853.
21. The American Journal of Medical Science, Jan. 1854 (received too late for analysis).

EAST INDIAN.

22. The Indian Annals of Medical Science. No. I. Calcutta. 1853, Oct.

† It would greatly abridge the labours of literary research on the part of the "Annalists," if they were favoured with early copies of essays and papers. Such as may be intended for this department—the Annals of Micrology—will readily reach us, if enclosed with exchange parcels of Reports and Transactions, forwarded to the Royal Irish Academy, Dublin.

‡ British and Foreign Medico-Chirurgical Review, Oct. 1853. § Ibid.

SPECIAL ORGANIC CONSTITUENTS.

Albuminoid Compounds.—Leconte and Goumaeus* state, that by submitting protein bodies to the action of crystallizable acetic acid during a month, they become separated into two substances; the one soluble in acids, which they call *Oxolyin;* the other, insoluble in acids, is called *Anoxolyin.* They find (with all observers) that fibrine, even when rendered white, presents two microscopic elements, fibres of a white or pale-yellow colour, without further structure, and numerous corpuscles. The fibres resist the action of the crystallizable acetic acid for some time; but the corpuscles disappear after a few hours. After being subjected to the action of the acid for a month, the fibres are recognisable after the addition of an alkali. The two new substances are characterized by the following reactions, independent of acetic acid. Anoxolyin is soluble in dilute sulphuric acid, exhibiting a red colour; oxolyin is only partly soluble, and shows a yellow colour. The former is coloured carmine, or cinnabar red, by Millon's re-agent; the latter assumes a rosy red, or is not coloured at all. Several other reactions are enumerated.

Animal Cellulose and Starch.†—A substance similar in chemical reaction to the cellulose of plants was first discovered in the ascidia, by Carl Schmidt, whose observations have been confirmed by Kölliker, Löwig, Schacht, and Huxley. VIRCHOW subsequently called attention to the great similarity in structure between the umbilical cord in the human subject and the cellulose tissue of the ascidia. In the course of researches on the so-called *corpora amylacea* of the brain, he found that these bodies, on the addition of iodine, first acquired a pale blue, and on adding sulphuric acid, the beautiful violet reaction of cellulose became developed, being the more remarkable by contrast with the surrounding yellow or brown coloured nitrogenous substances. He uses an aqueous solution of iodine and dilute sulphuric acid; and in every case adopts the most careful precautions to exclude all possible sources of error. From his researches he draws the following conclusions:

1. The corpora amylacea (Purkinje) are chemically different from the concentric spherical corpuscles of the brain-sand, with which they have been hitherto confounded. The organic constituent of the brain-sand corpuscles is manifestly nitrogenous, being coloured intensely yellow by iodine and sulphuric acid; this reaction being found not only in the sand of the pineal gland and the choroid plexuses, but in that of the Pacchionian granulations, the dura mater, and also in the angular plates of the spinal arachnoid.

2. The cellulose corpuscles are found, so far as his investigations yet reach, only in the substance of the *ependyma ventriculorum*, and its prolongations, especially the lining of the cerebral ventricles and the *substantia grisea centralis* of the spinal marrow, the transparent central mass so described by Kölliker.‡

The lining membrane of the ventricles consists, according to Virchow, of a basic tissue analogous to the connective tissue, on which the epithelium rests; it is in the deepest layer of this membrane, and close to the nerve-fibres, that the cellular corpuscles are found in greatest quantity, and chiefly where the ependyma is thickest. In the septum, fornix, stria cornea, in the fourth ventricle, they are very rich.

He describes the ependyma as sending prolongations in between the nervous elements, and as the result of some investigations into pathological conditions, not yet published, he states his belief that a soft basic mass, most closely related to the *connective substance*, penetrates everywhere the nervous elements, binding and holding them together; and that this substance is continued into the interior of the higher nerves of sense. The ependyma he regards as only the free superficial part of this tissue.

In the spinal marrow, the substance corresponding to the ependyma lies in the

* Schmidt's Jahrbücher, No. 10, 1853. † Virchow's Archiv, Band vi. Heft 1.

‡ Mikrosk. Anat., Band ii. Heft 1, p. 413.

middle of the grey matter, in the situation where, in the fœtus, the canal of the spinal marrow runs. This *ependyma spinale* forms a fine continuous gelatinous thread, as far down as the *filum terminale.* It presents the cellulose granules throughout, but in larger quantity above than below.

Virchow failed to find these bodies in the fourth ventricle of a rabbit. The cellulose corpuscles, he considers, therefore, to be in connexion with the substance of the *ependyma*, and it is probable they exist as one of its constituents.

In the child, he has as yet failed to find them; and he therefore considers it probable, that as they appear only later in life, like the brain-sand, they result from a pathological change. In further researches, he found them in the nerves of sense, in the soft grey intermediate substance of the olfactory, and in the auditory nerves: in this latter nerve they have been found by Meissner also. Rokitansky has observed them in the optic nerve, and in the bones in cases of osteomalacia. Kölliker has seen them in the retina, and they have been found in the Casserian ganglion by Luschka. Virchow* himself has since found bodies, with the same reaction as the corpora amylacea, in the Malpighian corpuscles in the "waxy spleen."

This pathological condition consists in a peculiar degeneration of the contents of the Malpighian follicles, by which they assume a gelatinous aspect; they have been compared by Christensen to the sago-grains swimming in soup. In the altered contents, Virchow found bodies resembling the corpora amylacea, but not concentrically striated; they are round or slightly angular, larger than the usual lymph-corpuscles of the follicles, and lie thickly packed together. On the addition of an iodine solution, they assume a strong yellowish-red colour; and if sulphuric acid be added, a well-marked violet is produced. This reaction takes place more quickly than in the ependyma corpuscles, but appears to be less permanent in the corpuscles of the spleen. This reaction was capable of being produced on a scale to render it visible to the naked eye. Some of the corpuscles dissolved out of the tissue, treated with water, and then acted on by iodine and sulphuric acid, exhibited the peculiar coloration very clearly. In a specimen of waxy spleen, preserved in spirit in the museum for some years, the reaction was quite manifest; the purity of the colour was not so great as in the first specimen. Virchow further noticed a remarkable exemption from putrefaction in these bodies; after three weeks' maceration, the corpuscles exhibited the reaction perfectly. Schrant has called this waxy condition of the organ the colloid spleen. Virchow thinks it more probably due to a cellulose metophosis of the cells.

Busk† has taken up this investigation, and has arrived at results differing somewhat from those of Virchow. He found the corpora amylacea in "enormous abundance" on the septum lucidum, and the choroid plexuses, in the olfactory bulbs, and in the superficial parts of the brain, both cortical and medullary, contiguous to the olfactory nerve. He also observed them in greater or less quantity in almost all parts of both cerebrum and cerebellum towards the surface, and in the very middle of the cerebellum; their distribution, however, appeared irregular. They were absent in the corpora striata, being replaced by brain-sand. This observer, moreover, regards the corpuscles as starch, and not as cellulose; and found them to possess all the structural, chemical, and optical properties of starch as it occurs in plants. They varied in size from less than a blood-disc to $\frac{1}{500}$th inch and more; many of them showed the same appearance of lamination as true starch corpuscles. By Schultz's solution of chloride of zinc and iodine, and the addition of sulphuric acid, the corpora amylacea unfolded themselves into empty, flaccid, thin-walled, blue sacculi. By simple watery solution of iodine they were coloured deep blue. Their action on polarized light was similar to that of starch corpuscles, some of the smaller grains exhibiting a sharply-defined black cross, the lines intersecting at an angle of 45 degrees in the middle of the grain; in the majority, however, only a single long dark line was observed.

* Archiv, Band vi. Heft 2, p. 268.

† Quarterly Journal of Microscopic Science, No. vi. p. 105.

Many particles of sabulous matter—the ordinary brain-sand corpuscles—appeared to be lodged in irregular masses of an immature connective tissue; and on the addition of iodine, each mass of crystals was seen to be surrounded with an irregular transparent border, which, on the addition of iodine, turned of a light purplish-pink, thus closely resembling the early condition of the cellulose wall.

In connexion with this subject, it may be interesting to refer to the researches of Schultze,* which go to establish an identity between a certain colouring matter present in several animals (Hydra, Turbellariæ, &c., also several Infusoria), and the chlorophyll of plants. It is probable, in a considerable degree, that further investigation will show still greater resemblance in the constituents of animal and vegetable organisms.

DEFINITE MORPHIC ELEMENTS.

Spermatic Particles.—BURNETT† has instituted a series of researches on the origin, mode of development, and nature of these particles, among the four classes of vertebrate animals. In reference to the question of the nature of the lining of the seminal tubes, he states that in the tubes of the testes of animals which have not arrived at the age of puberty, a simple epithelial lining is found, the cells of which do not differ from those of the pavement form covering mucous membranes. When, however, the generative function begins to be developed, the character of the cells seems to undergo modification; they appear to pass to a higher grade of function, but without, as he says, losing their primitive type as epithelial cells. He agrees in the opinion of Robin as to a segmentation of the vitellus. He follows and confirms the chief statements and views of Kölliker and Wagner. This paper contains much interesting matter, and is illustrated by an excellent lithographic plate.

Blood.—Several valuable papers on this subject have been published by WILLIAMS;‡ they embrace a very wide field of investigation, being devoted to the comparative micrology, chemistry, physiology, and pathology of this fluid.

New Formation of Blood and Bloodvessels.§—The free formation of blood in an organizable exudation, admitted by Rokitansky, Vogel, and Engel, appears to be confirmed by the researches of WEDL; he has not, however, been able to determine whether the blood-corpuscles are nucleated at their origin or not. He maintains the free formation of bloodvessels in an exudation, independently of any connexion with the vessels of the surrounding parts. He believes the fibre-cells to have a power of independent subdivision or partition; besides the two regular terminal prolongations of these cells, he has observed a third, which lies somewhat nearer to the middle of the cell-body, the cell-nucleus presenting at the same time a lateral extension, which proceeds to a separation as the process of partition in the cell advances from both prolongations towards the body. Gradually two cells are formed, which lie obliquely towards each other. When the fibre-cells are developed in great quantity, as in cancer, we then find an entire system of chains of fibre-cells, with dentritic branchings. The nuclei lie with their long axis in the direction of the branches, in oblique lines, which indicate a spiral arrangement. A layer of transversely disposed nuclei is added afterwards. Those vessels which result from the process of free formation, subsequently unite with those of the neighbouring tissue, and thus a collateral circulation is established.

Numerical Estimation of Blood-corpuscles.—Since our last notice of this mode of investigation of the blood, introduced by Vierordt, further researches have been made in the same direction by this observer himself, as well as by others. WELCKER‖ communicates the results of a tolerably extensive inquiry, with useful

* See Journal of Microscopic Science, No. 4, p. 278, 1853.

† Memoirs of American Academy of Arts and Sciences, vol. v. part 1, p. 29.

‡ Philosophical Transactions, part 2, 1853. See also late numbers of this journal.

§ Schmidt's Jahrbücher, No. 12, p. 295, 1853.

‖ Archiv des Vereins für Gemein. Arbeiten, Band i. Heft 2. Gottingen, 1853.

modifications in the modes of enumeration, by which the process can be performed much more expeditiously than by Vierordt's original method. He employs a special form of micrometer, divided into a known number of parallelograms, each of which is indicated by a particular number; and also a capillary tube of particular dimensions. He has estimated the quantity of blood-corpuscles in his own blood as 4,600,000 to the cubic millimetre. He states that, for practical purposes, enumerations to the extent of from 2000 to 3000 blood-corpuscles permit us to form an opinion as to the relative proportion of corpuscles to the volume of the blood.

In a short subsequent communication, he describes a method of approximately estimating the quantity of blood-corpuscles, by a process of quantitative dilution of the blood, and comparison of the colour of solutions thus prepared with a determined colour scale.

By both methods he has already arrived at some interesting comparative results. Thus, healthy blood (his own) contained 4,600,000 blood-corpuscles to the cubic millimetre; a case of hemiplegia after apoplexy, 3,900,000; a case of chronic disease of the brain, 3,800,000; a case of hysteria, 3,800,000; a drunkard, 3,500,000; a case of diseased uterus, with profuse leucorrhœa, 3,300,000; a case of tuberculosis in the last stage, 2,400,000.

Blood-Crystallization.—L. Teichmann* has succeeded in obtaining crystals from blood without any preparatory evaporation, by the addition of four, five, or more parts of water to one of blood, and allowing the fluid to stand sufficiently long. In this way, and by the insertion of a small piece of cork under one of the angles of the covering glass, he has produced crystallization in the blood of all the animals he examined, and in all the blood-vessels indiscriminately. His observations were conducted on the blood of man, oxen, swine, rabbits, pigeons, and fish; frogs' blood, for a time, formed the only exception. This observer believes that the crystallizable substance is contained in the blood-corpuscles; he has procured crystals from the filtered washings of the blood-cake, and states that he found them more perfect the more the corpuscles were freed from serum and fibrin. With regard to the influence of temperature, he has observed that the slower the evaporation takes place, the more complete will the crystals be; but if it be required to produce them quickly, the temperature may be slightly raised, but it must not be carried to the point at which the albumen coagulates. In subsequent experiments on frogs' blood, he procured crystals by the addition of a very considerable quantity of water, at a very low temperature; the quantity of the crystals was proportionately smaller than in other specimens of blood, and they are always colourless when thus obtained. From blood four months old, and also from dried blood, he has succeeded in forming crystals.

Hämin Crystals.—By this name Teichmann designates crystals obtained by treating the mass of dried blood-corpuscles in hydrochloric, acetic, oxalic, tartaric, citric, or lactic acids, under a glass cover in a temperature of 20° to 50° R. These crystals have a yellow, brick-red, brown, or black colour; they form rhombic prisms, or sometimes stellæ, needles, or granules, like those of black pigment. They are insensible to the action of air, and are insoluble in water, ether, alcohol, acetic, hydrochloric, and nitric acids; in boiling nitric acid, however, they are entirely dissolved; they are soluble in dilute liquor potassæ, but become blackened in this fluid when concentrated; concentrated sulphuric acid, and still more readily ammonia, dissolves them. To the substance which undergoes this form of crystallization, and which he supposes to be derived from the blood-corpuscles, he gives the name of Hämin; he considers it to be different from the hæmatin of fresh blood, which is soluble in water, and also from the hæmatoidin forming the crystals

* Ueber die Krystallization der organischen Berlandtheile des Bluts. Zeitschrift für rat. Med., N. F., Band iii. Heft 3. For an account of previous researches on this subject, see the number of this journal for Oct. 1853.

described by Zwicky and Virchow. He has obtained Hämin crystals in the blood of various animals. Finally, he considers the black pigments as forms of undeveloped crystals in different degrees of oxydation. As a practical result of the knowledge of the properties of Hämin, he suggests the employment of the above manipulations to obtain crystals in cases of suspicious spots and stains.

PERMANENT TISSUES.

Structure of the Cornea.—Two opinions seem chiefly to prevail with regard to the histological elements of the cornea: (*a*) that it is composed of intercrossing bundles of fibres; (*b*) that it is of lamellar structure. HIS,* who has made some researches on its structure under the direction of Virchow, considers that much of this difference of opinion has arisen from the difference of the procedures adopted in its dissection by different anatomists. This observer finds two distinct elements—corpuscles yielding protein compounds, and intercellular substance yielding gelatine. The corpuscles are those described by Virchow as cells. The corpuscles may be demonstrated by treating the cornea with strong hydrochloric acid, or by boiling it in distilled water for from thirty to forty hours. The insoluble flocculent remainder, when examined under the microscope, exhibits the corpuscles forming a continuous meshwork. They appear as very clear, lightly granular, flattened bodies, chiefly polyhedral, with numerous prolongations on all sides, some of which branch, anastomose, and so not unfrequently form a small network. The membrane of the corpuscles is marked by a dark border; the nucleus, which usually contains in its interior many shining nucleoli, is so pale that it is difficult to establish its presence; it is, however, proportionately large, and of round form. His has never seen simple round or spindle-shaped cells in the cornea; and, from the form of the elements just described, he considers it to present greatest similarity to osseous tissue. By nitric acid and potash the corpuscles are coloured yellow, whereby they exhibit a relation to the protein bodies, while an aqueous solution of the intercellular substance gives a chondrin reaction. In vertical sections, the corpuscles appear between the layers, which occasioned Henle to consider them, when first described by Virchow, as pale interspaces with dark bodies lying in them. In parallel sections, however, they are best seen, appearing to be arranged in a certain order, and often with great regularity; the prolongations of contiguous cells anastomose, and thus, as before observed, form a small network. The intercellular substance readily splits into fine lamellæ, and then into fibrils.

With a view to test the statements of Bowman, that the cornea contains a system of tubes subservient to its nourishment, HIS made a number of injections, partly with quicksilver, partly with other substances, and, amongst them, with the ocular pigment suspended in water. He concludes that the passage of the injections, often remarkably regular, is accomplished by a splitting of the corneal substance, first into lamellæ, and then into fibrils. Coccius, as the result of a series of injections from the bloodvessels, has advanced an opinion that the corpuscles (which he considers as nuclei) are in direct connexion with the central lumen of the vessels. HIS, from an examination of some of the preparations of Coccius, thinks this opinion unfounded, though he admits that such a direct connexion of cellular structures with bloodvessels is not without analogy, H. Müller having observed it in the Cephalopoda.

Articular Cartilage.—REDFERN† communicates the results of researches undertaken to determine the relative thickness of articular cartilages at different periods of life. A very general opinion has prevailed that they gradually undergo diminution in thickness; and statements have been made to this effect by the chief writers on the growth and structure of bone and cartilage. In opposition to these

* Verhandlungen der Phys. Med. Gesell. Würzburg, Band iv. Heft 1, p. 90.
† Edinburgh Monthly Journal, Jan. 1854.

views, Redfern finds, from a series of comparative measurements, that articular cartilages do not become gradually thinner as life advances, and that they are not uniformly thinner in aged persons than in early life. He has also obtained additional evidence in support of his opinion that, as life advances, these structures, and particularly in certain joints, change their elementary characters, and become fibro-cartilaginous, or altogether fibrous; he terms this change the senile degeneration of cartilage.

*Structure of Dentine.**—A very interesting paper on this subject, by SALTER, contains the results of original investigations, and also gives a valuable summary of the researches of Czermak. He finds that the curves of the so-called contour markings are in proportion to the primary curves of the dentinal tubes at any particular spot, and cross them at right angles. With a power of 200 linear, these patches are seen to consist of globular masses of dentine, more or less isolated; they are traversed by dentinal tubes; their diameter varies from $\frac{1}{250}$th of an inch to $\frac{1}{10000}$th. Sometimes a large globule shows as many as five or six tubes traversing it. These globules are regarded as the chief agents in the calcifying process: an arrest of calcification at the globular stage, explains the phenomena of contour markings, which is thus to a considerable extent to be regarded as an abnormal condition.

We shall here only refer to a valuable paper by Huxley, on the 'Development of Teeth.' One of his chief and most important conclusions is, that all the tissues of the tooth are formed *beneath* the basement membrane of the pulp, and that they are consequently all to be regarded as true dermic structures, none epidermic.†

CONTRACTILE TISSUES, MUSCLE.

Contractile Substance of Animals.‡—Our knowledge of this subject is still so obscure, that we gladly receive any, even small, contributions to it. ECKER describes the following forms under which it occurs:—

1. A transparent, homogeneous, structureless substance, reticulated, with clear spaces, contractile in all directions, and continuous throughout the whole body, or even constituting the greater mass of it; no nervous system being present; this he calls the "amorphous contractile substance." (Infusoria; Hydra; Hydroida.) This corresponds to Lebert's anhistic tissue of spontaneous movement.§

2. A transparent, homogeneous, structureless substance, non-fibrillated, but divided into isolated, muscle-like portions; an appearance of nerves. (Systolida; young insect larvæ.)

3. A substance composed of fibres, and contractile in the direction of these fibres. "Formed contractile substance, or muscular substance."

4. Contractile cells, leaving out of view the Gregarina and ciliated cells, occurring in only the embryonic condition according to Ecker, but according to Huxley they abound in the tissue of Medusæ, Hydra, and even in higher animals.

Inquiries are still to be prosecuted as to the *modus operandi* of contractile tissue, on which subject little or no knowledge exists.

Twin Spiral of Muscles.—Dr. Barry continues the publication of his singular views on the structure of muscle. They will be found in several numbers of the 'Philosophical Magazine.'

Muscular Tissue of Skin.—LISTER‖ has conducted some researches which verify the discoveries of Kölliker regarding the muscular apparatus of the skin and its

* Journal of Microscopic Science, No. 4, p. 252, 1853. † Ibid. No. 3, 1853.

‡ Siebold and Kölliker's Zeitschrift, vol. i. p. 218: we quote from Journal of Microscopic Science, No. 6, p. 111.

§ See Lyons' Retrospect of Microscopic Investigation: Dublin Quarterly Journal of Medicine, Aug. 1850.

‖ Journal of Microscopic Science, No. 4, p. 262, 1853.

appendages. He recommends sections to be made from portions of integument, as the scalp, after being allowed to dry to a horny state for twenty-four hours. His sections and drawings show the muscles in all cases to arise from the most superficial parts of the corium, and to pass down obliquely to their insertions into the hair follicles, immediately below the sebaceous glands; and they are placed on the side to which the hair slopes, which readily explains their action in producing erection of the hair in horripilation. In transverse sections, the muscles appeared often circular, polygonal, or elliptical, constituting more or less rounded bundles; he states their average diameter as $\frac{1}{2000}$th of an inch, less than half that stated by Kölliker. He has failed to confirm Henle's statement that muscular tissue exists in parts destitute of hairs.

Cilia—re-induction of Ciliary Motion.—VIRCHOW's observations on the reproduction of ciliary motion by the action of a solution of caustic potash deserve attention. They have been already noticed in the 'Annals of Physiology.'*

MUCOUS MEMBRANES. GLANDULAR APPARATUS, &c.

Gastric Mucous Membrane.—Opportunities but very rarely occur of examining the gastric mucous membrane in a fresh and normal state in the human subject. In a case of suicidal drowning of a female, whose body was obtained apparently a short time after death took place, Kölliker† made several investigations, and on the gastric mucous membrane, amongst other parts. Particular attention was directed to the glandular apparatus. The gastric glands were found to present great variety of form, but admitted of being reduced to three types. (*a*) Simple utricular glands, with flat scale-like cells. (*b*) Compound utricular glands, with similar cells. (*c*) Compound utricular glands, with cylinder epithelium. The first form is the most common, and corresponds with that already described in his work on Histology, and occurs chiefly in the middle zone. *The compound utricular glands with flat cells,* occur in the cardiac zone. They commence by a tube 0·04—0·08^{mm} long, 0·03—0·04^{mm} broad, lined with cylinders, which at a certain point divides, first into two or three, and then into four or seven long cylindrical utricles, lined with flat cells, and which run side by side to the base of the mucous membrane.

The terminal utricles have a peculiar varicose appearance, owing to numerous lateral dilatations. Kölliker could not observe the "glandes en grappe" in this situation, though he found them in the lower part of the œsophageal membrane. The third form, *compound utricular glands,* with cylinder epithelium, occurs in the pyloric zone, resembles that last described in basic form: the utricles are, however, larger. In the place of scale or flat cells, they are entirely filled with short cylinders, in which a fine molecular fat was observed; neither simple glands nor the "glandes en grappe" were observable here. Summing up his observations in comparison with those of other recent inquirers, he finds a general correspondence between his results and theirs: with, however, a few points of difference, principally regarding the distinction of the varieties of glands just described. Kölliker confirms the opinion of Donders as to the existence of two varieties of glands—proper gastric glands and mucous glands. He has also observed contractile fibre cells between the proper gastric glands, but denies the existence of the spiral fibre cells investing the glands, as described by Ecker; the nuclei remarked by the latter observer, he considers to belong to the gland-membrane. *In the mucous membrane of the intestine,* in the same case, the muscular fibres of the villi were very well shown. Besides these fibres, the villi exhibited very numerous small nuclei, distinguishable from those of the vessels by their smaller size, round form, and the absence of vesicular appearance and of nuclei. Virchow is disposed to

* See our last number, p. 262.
† Verhandlungen der Phys. Med. Gesellschaft zu Würzburg, Band iv. Heft 1, p. 52.

consider these nuclei of the parenchyma of the villi to lie in small round cells, which become transformed into pigment cells. Kölliker and H. Müller have actually found cells with similar nuclei in isolated villi in the duodenum and jejunum.

Cells of the Villi.—In his 'Microscopic Anatomy,' Kölliker describes openings in the cells of the villi in the rabbit. More recently, Brücke describes these openings as a normal condition in the epithelial cells of the villi. Kölliker, however, failed to detect them in the case in question, and seems to consider the appearance of openings to be due to the bursting of the cells on the addition of water.

Spleen.—In the case of suicide above alluded to, Kölliker was able to confirm his former observations with regard to the presence of blood-vessels within the Malpighian vesicles. Gerlach had also, by independent investigations, confirmed this view: he found within the vesicles very evident capillaries and even small veins, which branched off, not from the vessels, which bore the vesicles, but from those of the pulp. Kölliker failed to find lymphatics in the spleen, and still doubts the recently stated connexion between the Malpighian vesicles and the lymphatics. The pulp of the perfectly fresh spleen exhibited no trace of the spindle-shaped cells, compared by Kölliker, who first observed them, to muscular fibres; these cells became visible only after the spleen had lain for twenty-four hours, and then in greater quantity the more the organ appeared decomposed. He is, therefore, now of opinion that they belong to the epithelium of the vessels. Referring to his hypothesis that the blood corpuscles undergo disintegration in the spleen, Kölliker states that he inclines more and more to the opinion, that all changes of the blood corpuscles effected in the parenchyma of the organ, belong to the order of abnormal phenomena: and that in so far as the spleen is an organ in which normal blood-corpuscles are disintegrated, this change is accomplished only within the vessels.

Malpighian Bodies of Spleen.—HUXLEY* communicates the results of researches on these bodies in man, the sheep, pig, rat, and kitten. In all these animals he finds that one or more minute arterial twigs enter, and frequently subdivide, in the substance of the Malpighian body, making their exit on the opposite side, and finally breaking up into minute branches in the pulp. The mode of demonstration adopted, is to place a thin section of a fresh spleen under the simple microscope, weak syrup being added so as to retain the colouring matter in the blood-vessels, and thus to have the advantage of a natural injection. Slight manipulation with needles, or gentle pressure with a thin glass cover, renders the bodies transparent, when a quarter-inch lens will show the arrangement of the vessels above described. In man, the structure of the minute arterial twigs is the same within the bodies as elsewhere; both the transversely (smooth muscular) and longitudinally fibrous casts are well developed. The addition of acetic acid is stated to produce a clear line external to the former, representing the innermost portion of the *tunica adventitia*, which passes into, and is continuous with, the Malpighian pulp. This confirms the opinion of Müller. Besides the arterial ramifications, there exists also within the bodies a tolerably rich network of capillaries.

The author considers that the interior of the Malpighian bodies is filled with a substance as solid as any other indifferent tissue; he has not found any fluid, as admitted by Kölliker and others; he describes it as consisting of a homogeneous, transparent, structureless matrix, containing closely-set rounded or polygonal vesicular bodies from $\frac{1}{5000}$th of an inch to $\frac{1}{2500}$th in diameter, and usually containing from one to three, sometimes more, minute granules. Mr. Huxley does not regard them as true cells, but such are occasionally met with. He denies a true "wall" to the Malpighian body in the human subject, but agrees with Wharton Jones as to its presence in these bodies in the sheep; he has not found the

* Journal of Microscopic Science, No. 6, p. 74, Jan. 1854.

"granular membrane" of Sanders, but confirms the statements of this observer and Kölliker as to the presence of a meshwork of pale fibres on the exterior of the bodies. He concludes that the Malpighian bodies of the mammalian spleen are not closed follicles,and that they have no analogy with the acini of ordinary glands, but that they are portions of the spleen everywhere continuous with the rest of its substance, but distinguished from it (*a*), by immediately surrounding and, as it were, replacing the *tunica adventitia* of the arteries; (*b*) by containing no wide venous sinuses, but at most a network of delicate capillaries; and (*c*) by being composed of absolutely indifferent tissue. He seems further to concur in the views of Leydig, that there is no line of demarcation to be drawn between the spleen, the lymphatic glands, Payer's patches, the glandulæ solitariæ, the supra-renal capsules, the thymus, and the pituitary body: to which must be added, as Kölliker has shown, the follicular glands of the root of the tongue, and the tonsils. On the general structure of these latter glands, Huxley's researches agree with those of Kölliker, except that he cannot regard them as closed follicles, for he finds them permeated by a network of capillaries like those of the Malpighian bodies.

Uterine Glands of the Human Subject.—In the suicidal case already quoted, H. Müller* investigated the uterine mucous membrane. The cavity of the uterus contained some bloody mucus, and the mucous membrane itself was infiltrated with blood, and of a deep-red colour. By pressure with the blade or back of a scalpel, the uterine glands could be isolated and pressed out of their beds; they were both simple and compound, lined with a cylinder epithelium, and presented a clear lumen to their globular end throughout. *Cilia were nowhere observable.* Notwithstanding the general infiltration with blood (considered menstrual), the gland utricles were almost altogether free from it. This observer has, by former researches, established the existence of uterine glands at opposite periods of life, in a child of two years old, and in many women of seventy to eighty years.

Erectile Tissue in the Nasal Mucous Membrane.—Kohlrausch† describes a cavernous venous network on the turbinated bones in the nose, with which he states that he has been long acquainted, though, as far as he is aware, it is mentioned only in "Hyrtl's Anatomy," and there briefly. It may be seen very simply by injection of air; preparations so injected, if hardened in spirits of wine, may be cut into thin sections, which display the structure extremely well. The venous network lies between the periosteum and mucous membrane, and is from $1\frac{1}{2}$ to 2^{mm} thick; the vascular loops run perpendicularly to the bone. The mucous glands, which in other parts of the nasal mucous membrane lie superficially, and have short funnel-like openings, are here placed somewhat deeply, and have their walled ducts of $\frac{1}{3}^{mm}$ long. This vascular arrangement explains the sudden swelling of the nasal mucous membrane, also the immense excretion of fluid, so frequent in this situation, and likewise the free hæmorrhages from this organ.

NERVES.

Grey Fibres of the Olfactory Nerves.—Kölliker‡ considers that only a portion of the elements usually described as grey nerve fibres are really so, and such as are naturally so he compares with the embryonal fibres, and ascribes to them an investing sheath and homogeneous albuminous contents, the latter being analogous, but not quite similar, to the axis cylinder of the dark walled fibres. He conceives that all real grey nerve fibres are tubes; many of the grey elements, however, are so fine, that it is scarcely possible to form a definite opinion as to their tubular or

* Verhandlungen der Phys. Med. Gesell. Wurzburg, Band iv. Heft 1. p. 64.

† Müller's Archiv, No. 2, 1853, p. 149.

‡ Verhandlungen der Phys. Med. Gesell. Würzburg, Band iv. Heft I p. 60, (alludes to Remak in Monatsbericht d. K. A. in Berlin, v. 12, Mai, 1853.)

non-tubular structure. He is of opinion that, in many instances, the peculiar anastomosing and reteform areolar tissue which he has described has been taken for nerve-fibres. Further investigations are still required, he thinks, to determine how far real marrowless nerve-tubes are distributed in the system, and whether, on the other hand, the grey fibrous elements occur only in nerves of extraordinary nature. In doubtful cases, he believes that no way exists of determining the question accurately, except the following of a grey fibre into an evident dark-bordered tube, or to a true prolongation of a nerve-cell.

Structure of the Retina.—KÖLLIKER and H. MÜLLER* have made this a common ground of investigation, and have published, both separately and conjointly, the results of their inquiries. They consider the retina to be composed of different layers: 1. The layer of rods and cones; 2. The layer of nucleiform bodies; 3. The layer of grey substance; 4. The expansion of the optic nerve; and 5. The limiting membrane. Kölliker finds the expansion of the optic nerve to be interrupted at the punctum aureum, so that there is, in this situation, not the least trace of a layer of nervous fibres; in all other places, the nerve-fibres form a very thick and uninterrupted layer in the bottom of the eye, then become lost on the borders of the punctum in the layer of cells, which here form the most internal layer, and are only covered by the limiting membrane; the cells at this point form a very thick layer, showing, on vertical section, from nine to twelve rows of cells, placed one behind the other, and possessing the same characters as the other cells of the retina. Kölliker maintains a direct connexion between the nerve-fibres and the nerve-cells; these cells, which are entirely absent at the entrance of the optic nerve, are all provided with from one to six prolongations, in all respects similar to those found on the nerve-cells of the brain and nervous ganglia, and which, after repeated ramifications, become continuous with the true varicose nerve-fibres of the optic expansion, in such a manner, that these nerve-fibres may be regarded as taking their origin in the nerve-cells—an important histological fact, the discovery of which is due to Corti of Turin, who established it first in Ruminants, and afterwards in the elephant. Kölliker has verified these results on the human retina; he has not been able to observe any free terminations. Müller† finds that the rods pass through the entire thickness of the rod-layers without altering their diameter; on the exterior, they impinge on the pigment-cells, the inner sides of which are filled with molecules. Some of the rods are united with the molecules by a fine thread-like prolongation. The cones have in man the form of a narrow flask, and appear like a body with a conical point; the point usually reaches only half-way through the rod-layer. The granules are divisible into two layers, which vary in thickness not only in different animals, but in different parts of the same eye. In man, the exterior granular layer in the yellow spot is $0{\cdot}012^{mm}$, reaches then $0{\cdot}03^{mm}$, and afterwards diminishes in size again at the anterior border of the retina. The inner layer is thickest at the yellow spot, $0{\cdot}04^{mm}$ and more, and then diminishes to $0{\cdot}01^{mm}$. The ganglion cells, likewise, are disposed in many layers at the yellow spot, but farther forward scarcely form a single continuous layer. From several considerations, he believes that the inner parts of the radiating fibre-system are not to be looked on as continuations of the fibres of the optic nerve; these nerves appearing rather, as above stated, to be in connexion with the ganglion cells. The cones are conical or pyriform bodies, three or four times thicker than the rods situated on the internal aspect of the latter layer. At the punctum aureum, as observed by Henle, and confirmed by Kölliker and Müller, the rods are deficient, but the cones form a very thick and continuous layer. Müller has found that there proceeds from the internal part of each cone and each rod, a fibre which, after traversing all the layers of the retina, becomes lost on the

* For a summary of their discoveries, see Comptes Rendus de l'Académie des Sciences, t. xxxvii. No. 13, Sept. 1853; also papers in Verhand. d. Phys. Med. Gesellschaft zu Würzburg, Bands iii. & iv.

† Verhand. d. Phys. Med. Gesell. Würzburg, Band iv. Heft 1, p. 26.

internal surface of the limiting membrane. These fibres, which are all in relation with the nucleiform bodies, which in man form two layers, as demonstrated by Bowman, constitute a system altogether peculiar in the retina, and are called by Kölliker and Müller the radial fibres. Each cone is in relation by its internal part with a swelling which contains one of the nuclei of the external layer; from this swelling, which, they say, may be regarded as a cell, there passes a fibre which, after having reached the internal layer of nucleiform bodies, is placed in relation with one of these bodies, which are nothing else than little cells with a large nucleus; the fibre then traverses the cells and nervous fibres, and ends by becoming fixed to the limiting membrane by a swollen and sometimes ramified extremity. Similar but finer radiating fibres pass also from the internal part of the rods, and are placed in relation with such of the nucleiform bodies of the two layers as are not fixed to fibres coming from the cones, and terminate in the same manner by becoming attached to the limiting membrane. With regard to the nature of these fibres, they state that they are very delicate, like the retina-fibres, but never exhibit varicosities, and are thus distinguished from true nerve-fibres. Kölliker and Müller, from various considerations, are led to the conclusion, that it is the cones and rods which receive and transmit the luminous impressions; they admit that the layer of nerve-cells of the retina forms a true ganglion, and that it also possesses the power of perceiving light.

Remak has made a reclamation, in which he ascribes to himself the merit of the discovery of some of the most valuable points in the structure of the retina; we shall not however, in these "Annals," take up the controversies of science.*

Hannover,† whose labours may be said to have first cleared the way for the modern investigators of this delicate structure, differs in several most material points from the views of Kölliker and Müller; thus he opposes most strongly the opinion that the fine threads are directed inwards from the conically-pointed ends of the rods. Moreover, he does not recognise the subdivision of the retina proper into two parts—the bacillar, or rod layer, and the cone layer; as he considers that the inner ends of the cones and rods lie in the same plane. Further, he does not agree with these observers as to the absence of optic fibres at the yellow spot. Lastly, and what is of most vital moment, he seems to deny that the rods and cones can be regarded as of nervous nature, which of course, if established, overturns the theory of Kölliker and Müller as to their office of receiving luminous impressions.

Corpuscula Tactûs.—Much attention is still devoted to the investigation of these singular bodies. Meissner, who has, in our opinion, the merit of their discovery, considers the entire question as to their nature and structure in a separate work.‡ The chief point at issue is the nature of the striæ on the corpuscles. Kölliker maintains them to be elastic fibres; Meissner regards them as nerves, but denies absolutely that they have any connexion with the nervous loops described by Kölliker within the corpuscles. He believes that the corpuscles are invested with a special membrane, which separates them from the substance of the papillæ; that therefore they are like vesicles, filled with a finely granular substance, into which the nerves enter. He regards the cross-fibres as the terminal loops of dividing double-contoured primitive fibres. In two cases of paralysis of sensitive nerves, the cutaneous branches showed the characteristic changes described by Waller; they formed single outlined cords, here and there enlarged, the contents consisting of smaller and larger fat vesicles. The corpuscula tactûs were degenerated in a similar manner, and the transverse fibres were replaced by rows of fat-

* We think it proper, however, to give the reference to his memoir, which was published in the Comptes Rendus of the Academy of Berlin, for May 12, 1853.

† Zeitschrift f. Wiss. Zoologie, Band v. Heft 1, 1853. We quote from a special copy of the paper, for which we have to thank the author.

‡ Contributions to the Anatomy and Physiology of the Skin, by Dr. G. Meissner. Leipzig, 1853. We quote from Schmidt's Jahrb., No. 9, 342.

drops, which sometimes showed the original course of the fibres, sometimes filled the entire corpuscle.

Meissner denies expressly the presence of corpuscula tactûs in the borders of the lips and the papillæ of the tongue, where, however, they have been asserted to exist by Kölliker, Funke, and others; he confines them to the hand and foot, and amongst animals has found them only in apes—in the volar surface of the finger and hand, and the sole of the foot; they are smaller than in man. Notwithstanding numerous researches, he has failed to find them in other animals.

Huxley,* who has investigated these corpuscles, agrees with Kölliker in considering them as not histologically, in any respect, distinct or special structures, but merely rudimentary connective tissue. He admits the existence of vascular loops in corpusculated papillæ, but he has not observed any papillæ without corpuscles to present nerves; he has not been convinced of the existence of loops, but has frequently seen free terminations, the dark-contoured fibres passing sometimes only a little beyond the proximate extremity of the corpuscle; he agrees with Wagner as to the resemblance of the corpuscula to the Pacinian bodies. In these latter bodies, in the human hand, Huxley finds no interspaces filled with fluid, nor any central cavity; and the nerve lies not in a cavity, but in a solid homogeneous substance. He concurs with Leydig, that the Pacinian corpuscles, the Savian bodies (Torpedo), and the so-called muciparous canals of fishes, are homologous organs, which form a series whose lowest term may probably be the corpuscula tactûs. He further conceives that the *vibrissæ* are in fact the highest and most fully developed of this series of cutaneous organs, and of such the Pacinian corpuscles and the corpuscula tactûs may be only rudimentary representatives.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

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I. Inflammation and its Effects.

The Absorbtion of Firm Exudations. By Dr. Michaelis. (Prag. Vierteljahrsch. Band xxiv. p. 45.)

Dr. Michaelis, preparatory to a discussion on the mode in which exudations are absorbed, enters into a statement of the present opinions regarding fatty degeneration, and narrates some experiments in which pieces of flesh and of washed fibrine were introduced into the peritoneal cavities of hens, and were found, after some days, to have greatly lessened in volume, and to have undergone fatty degeneration, as in the experiments of Wagner. Michaelis discovered that ammonia was given off during this process: this led him to examine if urea were also formed during fatty metamorphosis, but the result was negative. He then debates at considerable length the possibility of a solution of a fibrinous exudation in the serum of blood, and also whether firmly coagulated fibrine can become organized. He sums up his conclusions as follows:

"1. It is improbable that firmly coagulated exudation can be dissolved in serum.

"2. Firmly coagulated fibrine is not fitted for organization.

"3. Coagulated exudation has, in all visible cases, only one metamorphose. It tubercularizes—i.e., it breaks down into a granular mass, which parts with its nitrogen (as in putridity) under the form of ammonia, while the elements of the ternary compound form fat. The ammonia serves for the saponification of a por-

* Journal of Microscopic Science, No. 5, p. 1, 1853.

tion of this fat, and the soap thus formed being soluble in blood-serum, is absorbed into the blood.

"4. Whether another process for the absorption of firm exudation occurs in the organism, is not yet known.

"5. With the release (freiwerden) of the ammonia is connected the so-called calcification. The phosphate of lime is precipitated by the ammoniacal solution, and cannot be absorbed. The abundant calcification, which occurs in atheroma, tubercle, &c., owes its origin to one and the same process—the fatty degeneration."

Dr. Michaelis then remarks, that whether after the entire process of fatty degeneration has been gone through, or during its later stages, some substance (such as lactic acid) may be separated from the blood, and dissolve the earthy phosphate, is not yet known.

II. The Acute Specific Diseases.

1. *Historico-pathological Researches on the Lung Typhus, and on Angina Maligna.* By Dr. Hirsch. (Prag. Vierteljahrschrift. 1853. Bd. 24.)
2. *On an Epidemic Fever with Scarlet Eruption, prevalent in Calcutta in* 1853. By Dr. Edward Goodeve. (Indian Annals of Medical Science. No. I. p. 248.)
3. *Statistical Notes on Small Pox.* By J. R. Bedford, Esq. (Indian Annals of Medical Science. No. I. p. 179.)
4. *Notes on Cholera.* By Dr. John Macpherson. (Indian Annals of Medical Science. No. I. p. 110.)
5. *On the Influence exerted on Small Pox by Vaccination during the Incubative Period.* By Dr. Barthez. (Revue Med.-Chir. Jan. 1854.)

1. The term "lung typhus" is applied by Dr. Hirsch to an epidemic fever, in which the organs of the chest suffer, as the patches of Peyer do in typhoid fever, or certain lymphatic glands in bubo-plague. According to the lung affection, the disease may receive the name of pneumonia, pleuritis, or bronchitis typhosa. In the first half of the sixteenth century, the author finds several descriptions of this disease. Montanus speaks of a malignant pleurisy which prevailed in Venice in 1521. Mundella speaks of a "pleuritis pestilens" which ravaged Brescia and the whole of Lombardy in 1537. In 1550, a similar disease prevailed in Switzerland. In the latter half of the same century, numerous notices of it occur. Coitez and Cardanus describe it as spreading over all Italy in 1563-4. In 1565, it also appeared in Germany, and was compared by Wier to the Black Death. Gemma saw it at the same time in Belgium, and Dodonæus in Antwerp. It had the following characters: it commenced with violent shivering, and pain in the head and chest; heavy cough, at first dry, then with sputa, followed. In three days there was a remarkable change for the worse; the sputa became bloody; then delirium and coma set in, and there were often bloody stools. If the patients lived to the seventh or ninth day, they generally recovered. The mortality was very great. Both Coitez and Dodonæus made post-mortem examinations, and found the lungs greatly congested, "indurated in a wonderful manner," or filled with abscesses.

Dr. Hirsch then gives references to numerous other authors, who described the disease at the end of the sixteenth, and in the seventeenth century. One of the most remarkable outbreaks was that mentioned by Chioccus, in Verona, in 1604. The disease had the characters above mentioned, but was also frequently combined with angina maligna, and with parotid swellings. Colle mentions a papular eruption; and Lælius states that the lungs were filled with abscesses. In 1633, the same disease spread over the whole of the Romagna; but according to Baronius, who termed it "pleuro-pneumonia," it was sometimes combined with buboes and carbuncles, and then exactly resembled the terrible Black Death of the fourteenth century. We pass over the other references, except to notice that the author quotes from Noah Webster (vol. i. p. 210), to prove that the disease was seen in Connecticut and New Hampshire (U.S.) in 1698.

At the end of the eighteenth, and the beginning of the nineteenth century, the "lung typhus" and the angina maligna began to disappear, and the petechial typhus, and at a later date the abdominal typhus (typhoid fever), gained ground. Still the lung-typhus is seen, and the author traces up its history from 1812. He describes especially its prevalence in America, and refers to no less than forty-five American works and papers. In the years 1811-17, this disease, "an exquisite lung-typhus," appears to have spread epidemically over the greater part of North America, while typhus and typhoid fevers ravaged the European continent, and the bubo-plague devastated the Levant and Egypt. The last and partial outbreaks in Europe appear to have been, in France, in 1827 and 1828;* in Dublin, in 1835;† and again in some parts of France in 1842 and in 1848.

Dr. Hirsch then enters upon the interesting but most difficult subject of angina maligna. He denies the correctness of the view which refers this disease to scarlatina; but, as may be gathered from the previous pages, he looks upon it as a true pestilence or epidemic disease, closely allied to the lung-typhus. He argues this question with wonderful industry and erudition; but to follow him would be useless without giving all the references, which would far exceed our space. Under the names, "Diphtheritis" (Bretonneau), "Stomatitis typhosa" (Kosciakiewicz), this disease has been encountered in the present century, altogether separate from scarlet fever. It has been described in America (Coates and Bolden), and in Algiers (Laverau and Haspel), under the name of typhoid fever. At the end of his paper, the author vindicates the use of the term, "lung-typhus." He believes that the following diseases—viz., bubo-plague, petechial typhus, typhoid fever, epidemic cerebro-spinal arachnitis, lung typhus, and angina maligna, have so close a relationship to one another, that they should all be called generically "typhus," and be distinguished by a specific prefix.

2. A peculiar fever with scarlet eruption prevailed in Calcutta and other parts of Bengal in 1824, and was described by Twining, Mouat, and others. In 1847, a similar fever was seen, and was recorded by Dr. H. GOODEVE. The fever now described by Dr. E. Goodeve appeared to be of a similar kind. The symptoms set in suddenly with shivering, and fever, which was generally paroxysmal; each paroxysm lasted from two to fourteen hours, being followed by a remission of variable duration. A bright scarlet eruption rapidly followed the initiatory symptoms, being in six cases visible within twenty-four hours, and in two cases appearing in the first febrile paroxysm. It was first seen on the upper part of the neck, the face up to the scalp, and the upper part of the thorax; it then extended to, but was always fainter in, the upper extremities; it seldom extended to the abdomen or the lower extremities. In colour, this eruption varied from bright red to a faint rose hue. It was occasionally slightly papular, and was obliterated on pressure. Its duration varied from forty-eight hours to six days; it was seldom followed by desquamation. The mucous membrane of the mouth and throat, and in some cases of the nose, was involved; there was redness, follicular enlargement, swelling of the tonsils, and sometimes, but rarely, ulceration. There was occasionally catarrh and congestion of the bronchial mucous membrane; sometimes there was catarrh of the alimentary mucous membrane; the urine was albuminous in one case.

This fever differed from those previously recorded in two important particulars; first, in the decided implication of the mucous membrane of the mouth and throat; and secondly, in the almost entire absence of articular symptoms, which in former

* Mergaut, Bulletin des Sc. Méd. de l'Académie, vol. xix. p. 284; and Pigeotte, Revue Méd., 1828, vol. ii. p. 157.

† Hudson, Dublin Journal, vol. vii. No. 21.

epidemics have been so marked, as to lead Dr. Copland to describe this disease in his Dictionary, under the term, "Scarlatina rheumatica." Only in one of twenty-eight cases did these joint symptoms occur.

With respect to the identity of this disease with the European scarlatina, Dr. Goodeve hesitates to draw a conclusion, although he recapitulates the striking symptoms, and observes that they are the same as those which go to make up scarlatina.

The mortality was not great; only one case in twenty-eight was fatal. The treatment was simple.

3. Mr. Bedford gives tables to show the comparative proportion of the population protected by vaccination or inoculation in different parts of Bengal, as drawn chiefly from the examination of the prisoners in the jails. The result is, that of the whole population, no less than 82·12 per cent. are protected by inoculation, and 5·6 per cent. by vaccination; the remainder are unprotected. It is a remarkable circumstance, that although inoculation is so commonly practised in Bengal, it is unknown, or almost so, in the North-west Provinces.

Small-pox in Bengal, as appears from the experience of eighteen years, is most prevalent in the months of March and April, the months intermediate between the cold and the hot season, and having respectively a thermometrical average of 82° and 86° Fah.

Vaccination is most successful in one of these two months—viz., in March, when 94·5 per cent. of the vaccinations take; in July, only 86·1 per cent. succeed; in January, 89·6; in October, 90·1. (The average of successful cases in England is 95·2 per cent.) While in Bengal small-pox exists, and vaccination is successful, in varying degrees throughout the year, in the Upper Provinces there is no small-pox between May and November; and at the same time vaccination becomes impossible.

After a general discussion on the relative amount of protection conferred by inoculation and vaccination in England, in which the papers of Drs. Gregory, Balfour, and Grainger are referred to, Mr. Bedford inquires into the number of deaths which occur from the process of inoculation; and after alluding to the extraordinary statements published by the Indian Small-pox Commission, brings evidence to show that the mortality from the practice is only 3·25 per 1000 of those inoculated. He also believes that the chances of diffusion of small-pox from the practice of inoculation are overrated, and advises that inquiry should be made before the practice be forbidden in Bengal, where it is so prevalent, in order to substitute vaccination, which is at present little known to, or practised by, the natives.

4. From an analysis of the cases of cholera admitted into the General Hospital at Calcutta, during the years 1842—53, Dr. Macpherson arrives at the following conclusions: (*a*) That the disease among the European residents is most common in the months from January to June—i.e., before the rains, and when malarious fevers are least common; (*b*) That the mean annual mortality has been 55·7 per cent., the extremes being 49·0 and 63·0 per cent.; (*c*) That the monthly mortality varied from 38 to 71 per cent.; (*d*) That cholera is rare under the age of 10 years —that the period of 20 to 30 years produces most cases, and after the age of 50 the fewest cases (may not the exemption under 10 years be attributable to the class of patients, adult sailors and soldiers, which furnish the cases examined by Dr. Macpherson? *Rev.*); (*e*) That no example of contagion has ever been noted; (*f*) That the old treatment of "opium, calomel and opium, fixed and diffusible stimulants and sinapisms," is the best; (*g*) That consecutive fever is common, and that $\frac{1}{4}$th of the fatal cases die in this stage.

5. After a brief recapitulation of opinions, M. Barthez relates two observations bearing on the effect of vaccination at the commencement of small-pox, and then concludes—

1. That if vaccination be practised during the incubation of variola, so that the vaccine eruption precedes the variolic, this latter will be almost always modified.
2. That in the great majority of cases, this modification will be favourable.
3. That if the variolic fever does become severe, this is to be attributed to the previous impaired state of health, and to the youth of the patient.
4. That in this last case, the vaccinia, the third malady added to the two former, does not exert a favourable influence on small-pox.

III. Diseases of the Nervous System.

1. *On the Secondary Affections of certain Fasciculi of the Spinal Marrow.* By Dr. L. Turck. (Zeitschrift der Gesell. der Aerzte zu Wien, Band ix. Heft 10.)
2. *Atrophic Muscular Paralysis.* By M. Aran. (L'Union Médicale, Jan. 7, 1854.)

1. Dr. Turck relates some interesting observations on the manner in which certain fasciculi of the spinal marrow, and their continuation into the brain, exhibit morbid alterations, in consequence of a pathological process going on in a more or less distant portion of the nervous centres. The inferences he had previously drawn, in 1851,* are borne out by numerous observations made since that time. Concerning the secondary affections of the cord, connected with disease in one of *the hemispheres of the brain,* the outlines of 21 cases are given, from which we abstract the following points as those of most importance. The *nature of the secondary affection* differs according to duration, extension, and localization of the primary disease. The shortest duration after which secondary affection was found is about five weeks. Granular corpuscles (Koernchen zellen) in moderate number, and characterized by the paleness and indistinctness of the single granules, was the principal morbid phenomenon observed in this early stage of secondary affection. Increase in the number of the granular corpuscles, darker and more distinct outlines of the single granules of which they are composed, a higher degree of refracting power, and lastly, the presence of isolated granules with the appearance of fat-granules, were the prominent features of the more advanced secondary affection.

The secondary affection gradually lessens in the lower part of the spinal marrow (the lumbar portion)—viz., the granular cells decrease in size and number until they disappear altogether. The intensity of the morbid phenomena is not always quite equal in the same case throughout the whole length of the fasciculus, from the brain to lower part of the spinal portion, nor does it always decrease in proportion to the distance of the part from the original seat, but sometimes the signs of disease were more considerable in a more distant spot than in another one situated nearer to the brain.

As regards the influence of the *localization*—i.e., of the part of the brain originally diseased, the following points may be learnt from the analysis of the 21 cases given by the author: 1. Considerable depositions (Herde) in the grey substance of the corpus striatum—i.e., in the nucleus candatus, without, however, touching the capsula interna, do not cause perceptible change in the spinal marrow; 2. The same is the case with depositions in the thalamus opticus, not exceeding the size of a pea or bean; 3. Deposition in the third portion of the nucleus lenticularis, and of the capsula externa, appear to have only a slight influence in producing secondary affection; 4. Depositions in the medullary substance of the hemisphere, not exceeding the size of a hazel-nut, produce no secondary affection; those of a square inch or more in circumference cause only a slight alteration in the fasciculus lateralis of the opposite side; 5. Depositions in the capsula interna, between the grey substance of the corpus striatum and third portion of the nucleus lenticularis, even those of small size, effect considerable secondary disease of the fasciculus

* Wiener Zeitschr., 1851. See this journal, No. 20, October, 1852, p. 529.

lateralis of the opposite side; 6. The grey substance of the corpus striatum, of the thalamus opticus, of the capsula interna between the grey substance of the corpus striatum and the third portion of the nucleus lenticularis, as also of that portion of the capsula interna on which the posterior part of the thalamus opticus is resting, may be much diseased without effecting any morbid alteration in the fasciculi anteriores of the same side; 7. Considerable depositions in the first and second portion of the nucleus lenticularis are connected with a high degree of secondary disease of the fasciculi anteriores of the same, and of the fasciculi lateralis of the opposite side; 8. A deposition of the size of a pea in the one side of the pons, which destroyed the centre of the posterior half of the middle layer of the pons, produced a considerable secondary affection in the opposite fasciculus lateralis, while all the other fasciculi were normal; 9. A deposition of the size of a lentil in one half of the pons, which destroyed the centre of the middle layer of the pons, caused only a slight alteration in the lateral fascicle of the opposite, and in the anterior of the same side; 10. Various deposits were found in the cerebellum, but none of them were connected with any perceptible secondary disease.

In 12 other cases observed by Dr. Turck, the seat of the original affection of the nervous centres (caries of the vertebræ, or circumscribed tumours compressing the spinal marrow) was in the spinal marrow. Their analysis leads to the inference, *that the posterior fasciculi of the spinal marrow are liable to secondary affection only in the centripetal direction, the anterior only in the centrifugal, the lateral in both directions.* These observations must induce us to conclude that, in consequence of disease in a certain place of the nervous centres, certain fasciculi cease to receive an impulse—they are therefore out of activity and degenerate, and that this degeneration takes place in the same direction in which the fasciculi convey impressions; thus, in the centripetal (posterior) fasciculi, the secondary affection occurs always in the centripetal direction—in the centrifugal (anterior), it shows itself in the centrifugal direction; in the mixed (lateral) fasciculi, in both.

2. M. Aran exhibited at the Academy a case of atrophic muscular paralysis, in which, after death, the anterior roots of the spinal nerves, especially in the cervical and dorsal regions, were remarkably atrophied, as in the case recorded by Cruveilhier.* The muscles were universally atrophied, but there was fatty degeneration only in those of the hands, of the legs, and in the intercostals.

IV. Diseases of the Thoracic Organs.

1. *Spontaneous Cure of Pulmonary Perforations, of Tuberculous Origin.* By Dr. Woillez. (Archives Générales, Dec. 1853, p. 674.)
2. *The Stethoscopic Signs of Contraction of the Left Auriculo-Ventricular Opening.* By Dr. Herard. (Archives Générales, Nov. 1853, Fev. 1854.)
3. *Constriction of the Left Auriculo-Ventricular Opening, without Incompetency, accompanied during Life by rough Systolic Murmur.* By Dr. Caron. (L'Union Méd., Jan. 5, 1854.)

1. The rarity of recovery, says M. Woillez, after perforation, in phthisis, and the establishment of hydro-pneumo thorax, is well known. M. Saussier, who collected 147 cases of pneumo-thorax, found that 81 were due to tubercle, and all of these were fatal. Examples of cure, however, have been noticed by Laennec, Beau, and others; and M. Woillez now adds another example to the number. In this case, there was lung-tuberculosis of four months' duration; followed by marked symptoms of perforation of the right pleura and hydro-pneumo thorax. In six months, all the symptoms had disappeared, except the succussion sign. Two months afterwards, death occurred from tuberculous meningitis. On examination, the right pleura was covered by a thin false membrane; it contained half a litre of

* See this journal, No. 23, p. 267.

purulent fluid, and a little air. The opening was easily recognisable; it was completely blocked up by false membrane, but had communicated with a cavity.

The author then relates briefly the other recorded cases of cure, which amount altogether to 7, in 5 of which autopsies were made. In all these cases, the perforation was closed by pseudo-membrane, thrown out on the inflamed pleura, and being adherent firmly or slightly to the opposed costal pleura, or being entirely non-adherent. In 3 cases, the right lung was affected; in 4, the left; three times the opening was near the apex; once in the centre; once at the base.

In 4 of the 7 cases, in spite of the closure of the opening, the air was not absorbed from the pleura, but persisted for many months.

2. Dr. Herard has published two excellent papers on the signs of mitral contraction. After alluding to the opinions of those who assert that contraction of the mitral orifice is attended by a diastolic murmur, with its greatest intensity at the apex; 2nd, of those who state that the murmur is systolic and not diastolic; 3rd, of those who state that the murmur is a little before the systole of the heart; and 4th, of those who declare that this lesion is unattended by murmur. Dr. Herard declares that all these opinions are more or less true, and then proceeds to the enunciation of the following propositions.

(a) *Contraction of the mitral orifice may be attended by a systolic murmur.*—This opinion has been strongly advocated by M. Beau, but is difficult of proof, as cases of contraction are usually coincident with insufficiency of the valves, and this might be held to be the cause of the murmurs. The author conceives, however, that there are recorded cases of pure contraction of the orifice (without incompetency of valve), and refers to Bouilland, Beau, Notta, Surmay, Vernois, and Grisolle. He then records a case of his own, in which there was a loud *systolic* murmur at the left apex, dependent only on *contraction*, for the mitral valve was perfectly competent. This observation appears so conclusive to the author, that he demands whether in all cases of combined contraction and insufficiency the systolic murmur is really owing to the latter lesion, as commonly supposed.

(b) *The contraction of the mitral orifice may produce a presystolic murmur.*—As no one contests this fact (demonstrated by Barth and Roger, Gendrin, and Fauvel), the author passes on to the next proposition.

(c) *Contraction of the mitral orifice may produce a diastolic murmur.*—This proposition was admitted in France for a long time as established, until M. Bean, struck with the frequence of mitral constriction, and the rarity of diastolic murmur at the apex, and actuated by physiological considerations, rejected altogether the opinion that a diastolic murmur may accompany mitral constriction. This extreme opinion has been followed by several French observers, and among others by Fauvel and Valleix. M. Herard, however, entirely dissents from this view, and cites seven cases to show that mitral constriction can produce a diastolic murmur at the apex of the heart, and at the left. This murmur is rare compared with the two former, but does occur. A long discussion on Beau's doctrines on the action of the heart is added, which it is unnecessary to extract.

3. A man was admitted into the hospital St. Marguerite, at Paris, under M. Legendre, with the following physical signs of heart disease:—cardiac bulging; strong impulse; marked systolic thrill at apex in the fifth intercostal space; increased transverse pericordial dulness; murmur at apex (maximum at sternum, and at the right), commencing with the systole, but prolonged beyond it, and masking the second sound, but not heard after the second sound ("commençant au premier temps, couvrant tout le petit silence, et allant jusqu'au deuxième temps"). At the autopsy there was found to be dilatation of the right auricle, insufficiency of the tricuspid valve; great hypertrophy and dilatation of the right ventricle; no affection of the pulmonary valves; hypertrophy with slight dilatation of the left auricle; adhesion of the mitral flaps, which formed, as it were, a diaphragm pierced by an opening between the auricle and ventricle, but which did not permit regurgitation; normal size

of the left ventricle; healthy aortic flaps. The author does not attribute the systolic murmur in part to the tricuspid insufficiency (as we should be inclined to do, *Rec.*) but entirely to the mitral constriction; he believes that the case supports the opinions of Beau, but does not negative those of Herard.

V. The Diseases of the Urinary Organs.

1. *Contributions to Urology.* By Dr. Beneke. (Archiv des Vereins. Band i. Heft 3.)
2. *On Diabetes.* By Dr. Dursch. (Zeitsch. für rat. Med., von Henle und Pfeuffer. Band iv. Heft 1.)
3. *On the Elimination of Fat with the Urine.* By Dr. C. Mettenheimer. (Archiv des Vereins. Band i. Heft 3.)

1. Dr. Beneke has written a most elaborate paper, more physiological than pathological, on the condition of the urine under various circumstances of diet, &c. The experiments were conducted on himself, and have reference especially to the quantity, acidity, the specific gravity, the amount of earthy phosphates, oxalate of lime, the presence of albumen and of sediments.

(a) *The quantity.*—The average was 1250 cubic centimetres (=40⅔℥) in 24 hours. The average amount of fluid drunk during this time was 1058 cc., but much water-rich vegetable food was taken at the time. Although, as a general rule, the amount of urine passed was proportionate to the fluid drunk, this was not the case always. Sometimes the urine was copious when the fluid ingesta were scanty, and the reverse. The condition of the skin as to dryness or moisture, or the state of the intestinal membrane, did not entirely explain this; and Dr. Beneke believes that the state of the nervous system exerts also a marked effect on the urinary excretion; whenever he felt well, with well-toned muscles and an active mind, the quantity of urine was great; whenever he was languid and valetudinarian, and particularly when under the influence of depressing circumstances, the urine was lessened in amount. Under these circumstances, Dr. Beneke thinks that the tissue-metamorphosis was impeded.

(b) *The specific gravity.*—The general rule was, the greater the quantity of urine, the lower the specific gravity, and the reverse. But when, instead of being satisfied with this general result, the author calculated what the specific gravity would have been, had the average quantity of 1250cc. been every day excreted, he found the rule exactly reversed; the smaller quantity of urine had the lowest specific gravity: that is to say, although the specific gravity of the urine, when in small quantity, was very high, if this urine were diluted to the standard of 1250cc., the specific gravity was found to be below the average; and on the other hand, although the specific gravity of the large quantity of urine was small, yet if the urine was concentrated to the standard of 1250cc., the specific gravity was found to be above the normal. This is, in fact, to state (as done years ago by Dr. Prout) that a small quantity of urine represented a small amount of solid (although its specific gravity was high), and the reverse. There were, however, exceptions to this general rule. The author then goes on to show that the amount of excreted solid as measured by the specific gravity, was affected by the condition of the nervous system, and, as already well known, by the taking of food.

(c) *The acidity.*—This was determined by neutralization with a solution of carbonate of soda. Especial attention was paid to the influence of food; and in reference to the statements of Dr. Bence Jones, that the acidity of the urine diminishes after food, when the acidity of the stomach is greatest, the author states that he has made more than one hundred observations, in various healthy and diseased individuals, without being able to confirm Dr. Bence Jones's statement. Sometimes he found diminution of acidity after meals, but this was inconstant; and in one single instance only did he find the urine alkaline. The acidity of the whole urine of twenty-four hours was found to vary without appreciable cause; it was not in relation to the quantity of urine passed, to exercise, or to quality of food. A long and important discussion on the difficulties of accurately determining this problem follows.

(d) *The earthy phosphate and the oxalate of lime.*—Dr. Beneke believes that the quantities of the earthy phosphate, and of oxalate of lime, bear a determined relation; and that the oxalic acid ("an invariable product of the metamorphosis of nitrogenized tissues") is the solvent of the earthy salt. He believes also (and has adduced his arguments in a former work) that a want of phosphate of lime hinders the formation of cells, and that in diseases attended with rapid emaciation, an excess of earthy phosphates is in the urine. This excess he attributes to an increased quantity of oxalic acid being present, from want of sufficient oxidation.

In the urine of perfectly healthy persons, he states that no oxalic acid is found; the acid formed in the system is entirely transformed into carbonic acid. The quantity of earthy phosphate is then at its normal amount—viz., 10 to 12 grains in twenty-four hours; if oxidation is arrested, as in lung disease for example, the oxalic acid is not transformed, but circulates in the system, and acts upon the earthy salts, the excretion of which increases regularly in proportion to the oxalic acid.

Sometimes, however, there is not an exact coincidence of time between the appearance in the urine of the oxalic acid, and the earthy salt; the latter may follow the former.

As the appearance of oxalate of lime denotes an impeded metamorphosis of tissue, so it of course generally coincides with a diminution of the solids and of the quantity of the urine. And as the nervous system has so great an effect on metamorphosis, it follows that a lowered tone of the nervous system is coincident with the excretion of oxalic acid.

If there be oxalate of lime in the urine, the author concludes that there must be increase of earthy phosphate; but it does not follow that there may not be increase of the earthy phosphate *without* the appearance of oxalate. In rapid metamorphosis of tissue the oxalic acid is entirely transformed into carbonic acid, and therefore does not appear in the urine; but this heightened metamorphosis may give rise, without the intervention of oxalic acid, to an increased excretion of earthy phosphate.

(e.) *Albumen and Sediments of Urates.*—Four times, albumen, as detected by heat and nitric acid, appeared in the urine. At four other times an albuminous substance was precipitated by heat, but not by nitric acid, in which reagent, however, it was not soluble when once precipitated. The cause of the appearance of the albumen was not determined. In reference to the sediments of urates, which appeared sixteen times, Dr. Beneke enters at great length into a discussion on the cause of their appearance. He believes that the opinion of Prout, "that a very large proportion of the lithate of ammonia found in the urine on common occasions, appears to be developed from the imperfect albuminous matters formed during the primary assimilating processes," is rendered extremely probable by the experiments of Bidder and Schmidt.

He then considers the immediate physical causes of the deposits in the urine; but to this point we shall probably return on another occasion.

2. Dr. Dursch communicates two cases of diabetes mellitus, in which the quantity of ingesta, of urine, sugar, &c., are accurately noted. The results do not, however, add anything to our present knowledge. That the quantity of sugar greatly varies, without corresponding change in the specific gravity of the urine; that food has a very rapid effect on the sugar in the urine; that of all food the starchy substances are the greatest sugar-furnishers; but that even on strict flesh diet sugar may be formed—are familiar facts.

After relating his cases, the author proceeds to a critique of the present opinions on diabetes, and sums up the conclusions to a certain point as follows:—

"1. The formation of grape-sugar from starch is not pathological, for in the healthy system all digested starch is changed into sugar.

"2. A part of this sugar passes into the blood; another undergoes further transformation in the intestinal canal into lactic and butyric acids.

"3. In the healthy system, sugar is formed in the liver from the azotized foods, and passes by the hepatic veins into the blood:

"4. The sugar thus furnished by a double source, disappears from the blood, as it seems, in respiration, and is found in no excretion.

"5. In diabetes, grape-sugar is in the blood and in all fluids, and is separated by the kidneys.

"6. The sugar thus appearing in the urine arises,

> "(*a*) From the sugar formed in the liver out of azotized food;
>
> "(*b*) From the sugar formed from starch in the alimentary canal, and absorbed into the blood;
>
> "(*c*) From the sugar formed from starch in the alimentary canal, but which is not, as usual, further transformed into lactic and butyric acids. [This statement is based on a calculation that the sugar in the urine, in these cases, must have required the whole quantity of starch to furnish it.]

"7. Therefore, through some unknown obstacle, the transformations of the starch-bodies are arrested at sugar; and

"8. Through an equally unknown obstacle, the sugar in the blood is not consumed (in respiration), but passes unchanged with the urine."

So far the author thinks may be considered tolerably well proved doctrine; and certainly the above cited propositions are no novelty in this country. But he proceeds to a further suggestion, distinctly characterizing it, however, merely as an hypothesis. In diabetes mellitus, he says, we have two deviations in functions—viz., in the digestive apparatus (the conversion of sugar into lactic and butyric acid being hindered), and in the blood (the transformation in the lungs being hindered). It is not probable that these are two independent and merely coincident conditions; they arise probably from a single cause. May it not be that some secretion of the intestinal canal contains a substance which hinders the transformation of the sugar into lactic and butyric acids, and that this same substance, passing into the blood, exerts a similar action there, and prevents the destruction of the sugar in the lungs? The sugar then emerges from the body as a perfectly useless ingredient. And as the functions of the liver are deranged in diabetes, it may be that the bile contains this substance, whatever may be its nature. The author gives no experiments in support of this hypothesis, but refers the point to others to examine, and to confirm or refute.

3. Dr. Mettenheimer relates two cases, in which a considerable quantity of fat was passed with the urine. The first case was that of a man who died with cancer of the lungs, and mediastinal tumour, &c., and who took a tablespoonful of cod-liver oil twice daily. The fat appeared in the urine after the oil had been commenced. In the second case, a woman was convalescent under an acute inflammatory affection attacking kidneys already apparently degenerated; there was much blood, and renal casts in the urine, without anasarca. An emulsion of henbane and hemp was ordered. Large quantities of fluid pale-yellow fat appeared in the urine, and disappeared again when the emulsion was discontinued.

The author narrates the cases at considerable length, in order to show that no other explanation appears probable, except the one suggested by the coincidence of the taking of fat as medicine, and the elimination of fat as excretion. He also tried some experiments on himself, and on a friend, with emulsions, but could not detect any fat in the urine; and therefore concludes, that if the urinary fat in these cases really was attributable to the remedies, there was some weakness of the digestive powers which had a share in the phenomenon.

VI. Diseases of the Digestive Organs.

1. *On the Waxy Degeneration of the Liver and Spleen.* By Drs. Gairdner and Sanders. (Monthly Journal, for February and March.)
2. *Note on the same subject.* By Professor Virchow. (Archiv fur Path. Anat., Band vi. p. 268.)

Drs. Gairdner and Sanders communicate some very important facts connected with the waxy degeneration of the spleen, a change little studied in this country, but which would appear from these observations to be very common, as in the Edinburgh Infirmary it is found in the spleen in 10 per cent. We merely refer, however, to these papers, which will have been already in the hands of most of our readers, in order to mention a most extraordinary observation of Virchow on this point. As we have mentioned in the "Report on Micrology," Virchow has discovered the existence of cellulose in the brain and spinal cord, and it has been now found in other parts. As already mentioned, he has also discovered that the altered Malpighian bodies of the waxy spleen, which form the semi-transparent hard granules seen on section, and which are easily picked out from the substance of the organ, *are composed of cellulose*, and give the characteristic violet colour with iodine and sulphuric acid. This is a most unexpected result, and will no doubt lead to some important modifications in our pathological, and perhaps in our physiological doctrines.

VII. The Diseases of the Cutaneous System.

1. *On Lupus.* By Dr. Pohl. (Virchow's Archiv, Band vi. Heft 2.)

1. Dr. Pohl's elaborate paper on Lupus contains a discussion on the terms used by authors, on the nosological position of the disease, on its earliest symptoms, its several forms, its morbid anatomy, and on its treatment.

The only new matter is contained in the section on the morbid anatomy. Dr. Pohl ascribes to Berger* the credit of having first properly investigated this subject. According to Berger, lupus is "An hypertrophic new formation of cells, which, under the microscope, are finely granular, and have a single nucleus. They have the same figure as the normal cells of the *Rete Malphigi*, only sometimes they are egg-shaped. Their diameter amounts to $\frac{1}{300}$th to $\frac{1}{200}$th of a line. They are surrounded by a soft amorphous mass, composed of fine molecules, and between them lies normal fibrous tissue."

Dr. Pohl's description adds some novel points. In most cases, this hypertrophy is developed under the influence of an hyperæmic condition of the skin at the lupose point, and around it. If the hyperæmia have lasted some time, and if a portion of the epidermis has exfoliated, the evidently widened capillaries are seen through the thinned cicatrix-like skin. The hyperæmia, according to the varying course of the affection, extends to the panniculus, to the muscles, to the cartilages, glands, &c. If ulceration has commenced, and if the epidermis is entirely detached from the papillæ, loops of wide capillaries spread themselves near the ulcer, taking their origin from larger and deeper straight vessels. The structure of the cutaneous papillæ is no longer discernible, and the greatly widened loops of vessels remain ramifying in the new-formed layer of cells or of nuclei. The swelling which is connected with this enormous hyperæmia consists only in an hypertrophy of the

* De Lupo. Diss. Inaug., Griphiæ, 1849.

layer of nuclei, and of the youngest epidermic layer, with young cells of uncertain characters here and there, and especially in cases of chronic course, with streaks of newly-formed uniting tissue. These new formations pass in different directions, and cause atrophy of the deeper-seated tissue, or of the epidermis.

The round or oval, roughly granular, thick-walled, yellow shining nuclei lie close together, with an intermediate fine granular substance, which is made turbid by water or dilute acetic acid, but is rendered transparent by strong acid, while the nuclei are unaltered. Nearer the surface are numerous nuclei with cell-walls, which grow and form epithelium cells, or disintegrate. Sometimes young cells of uniting-tissue, and sometimes very smooth fibre-cells, are seen. This layer of nuclei is pierced by the above-referred-to great capillaries; and like other new formations follows the course of the vessels, with or without continual connexion with the place of origin, between the previously formed normal tissues, which may either become atrophied, or for a greater or longer time may remain unaltered.

Exactly as in the skin, the lupose thickening of the mucous membrane consists in an hypertrophy of the epithelial layer.

By the growth of the layer of nuclei, the epidermis-layer is gradually thinned, till at last the moist highly-albuminous exudation surrounding the cells reaches the surface, dries and forms scales, in which epithelium-nuclei or cells are also seen. Many of these undergo the fatty metamorphosis. Between these elements, pus-cells are seen in all stages of development.

Berger and Virchow have described in lupus, white corpuscles arising from widened sebaceous follicles, or widened hair follicles, or from both. Dr. Pohl examined similar corpuscles, and found them composed only of epithelium-cells, arranged in very various modes, but chiefly concentrically and thickly pressed together; the centres of some corpuscles were softened, and showed, under the microscope, fat and cholesterine. Through the centre of most of these corpuscles passed a hair whose bulb was atrophied; and therefore Dr. Pohl believes that the origin of the corpuscles was in a disease of the root-sheath of the hair. He never could find sebaceous follicles.

In addition to these changes, Dr. Pohl describes another morbid change in some cases of lupus. Instead of the *Rete Malphigi* being thus hypertrophic, it is compressed by a new formation of areolar tissue which lies below it, and penetrates through the panniculus. Considered from an anatomical point of view, lupus may, then, be divided into two great classes—*Lupus cellulosus*, and *Lupus fibrosus*.

A few remarks are made on the causes of lupus, which contain nothing new; and Dr. Pohl then passes to the treatment. He speaks in high terms of cod-liver oil. In referring to the statement of Alibert, that, in some places, lotions of cow's urine are employed by the country people in lupus, he mentions, that Dr. Baur, of Tubingen, has employed urate of ammonia, externally and internally, in chronic cutaneous diseases, and in lung-tuberculosis.

He advocates the extirpation by the knife, of lupous growths, in preference to destroying them by corrosive preparations; and states, that after the extirpation, if needles are used, they must be withdrawn very soon, as, in ten or twelve hours, pus is formed in their channels.

QUARTERLY REPORT ON MIDWIFERY.

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I. Menstruation and Conception.

1. *The Physiology of Menstruation and Conception.* By Professor Bischoff. (Zeitschrift für rat. Med. Band iv. Heft 1.)
2. *On the Period of Menstruation among the Burmese.* By E. J. Waring, Esq. (Indian Annals. No. I.)

1. Professor Th. L. W. Bischoff, of Giessen, has published an interesting contribution to this subject, chiefly founded on the observations resulting from the examination of the organs of generation of thirteen women, who had died during or soon after menstruation. He begins by referring to the difficulties attending the investigation of the subject, and especially to the obstacles opposing a successful search after the ovum in its transit through the Fallopian tube and uterus. In the course of a minute detail of the best mode of conducting the examination into the state of the parts concerned, he particularly points out the objections against steeping them in alcohol; and states that he entertains more hope from an investigation of a specimen already far gone in decomposition, than of one that had been preserved in spirit. He also objects to the plan of examining the parts under water.

Professor Bischoff then relates the principal points of interest in the thirteen cases he has observed.

1. The body of a healthy, strong woman, of about 20 years of age, examined on the 5th of December, 1839. The signs of existing menstruation were manifest; there was general turgescence, and blood effused in the cavity of the uterus and in the vagina. The ovaries were fully developed, and contained numerous developed Graafian follicles, which could be easily turned out of the soft and vascular stroma of the ovary. Both ovaries exhibited several corpora lutea in various stages of degeneration: the right one presented one freshly ruptured and filled with blood. The search for spermatozoa or an ovum was unsuccessful. The inner surface of the uterus was developed into a velvety appearance, and seemed on a transverse section to be reticular, on a longitudinal section to be shaggy; the uterine glands were somewhat enlarged. There was no distinct division between the mucous membrane of the uterus and the other structures.

2. On the 26th of October, 1840, the body of a strong young woman who had drowned herself was examined. The organs of generation were well developed, and the right ovary contained an unusually large Graafian follicle. This was still closed, and contained an ovum. The cells of its disc were round, not spindle-shaped; and therefore the ovum and the follicle were not yet ripe. The zone was very pale, and without sharp outlines; the yolk did not completely fill it. Besides this follicle, the right ovarium contained one, and the left two corpora lutea in different stages of degeneration. The search for spermatozoa was vain. Although in this case there were no signs of present menstruation, it was plainly impending. The inner membrane of the uterus appeared to possess small villi, which were plainly nothing more than the already partially developed uterine glands.

3. On the 6th of June, 1843, a strong, healthy woman, who had never been pregnant, drowned herself in the Nekar. The circumstances attending the case were such as to render it possible that connexion had recently taken place. The ovaries were of considerable size, soft, and each contained a great number of very large Graafian follicles. Their surface showed numerous cicatrized spots, and the left ovary a very marked yellow body. In the right ovary there was an imperfectly cicatrized, bluish-red spot, in which a fresh blood-coagulum showed that a Graafian

follicle had recently burst; its walls were thickened, and a corpus luteum was in course of development. The os uteri was deeply injected, and, as well as the cervix, was closed up with a delicate opalescent mucus, proceeding from the numerous highly-developed ovula Nabothi. The inner surface of the uterus was vividly injected, but no blood was effused; it was, however, covered with a whitish mucus, which under the microscope was found to consist almost entirely of cell-nuclei. On a perpendicular section, the tissue of the mucous membrane was found to be well defined from the proper fibrous tissue of the uterus. There were no glands, and especially no developed cylindrical uterine glands, nor a villous condition of the inner surface. A transverse section showed an injected network of blood-capillaries encircling round openings, probably of small, undeveloped glandular follicles. The right Fallopian tube and the uterus were searched in vain for an ovulum. It was ascertained positively that menstruation had just taken place in this person.

4 & 5. In the spring of 1844, Professor Bischoff received from Dr. Varrentrapp, of Frankfort, the organs of generation of two young women who had died in the hospital of an acute angina whilst menstruating. The parts were not highly developed or injected. In both, the right ovarium exhibited a freshly-burst Graafian follicle, containing a blood-coagulum; the walls showed the commencing development of a corpus luteum. The inner surface of the uterus showed in neither case a very marked development. In neither could perfectly-formed uterine glands, or the beginning of a decidua, be discovered. In both cases an ovulum and spermatozoa were sought for in vain.

6. The sixth case of recent menstruation, in which pregnancy had probably followed, has been already published in 'Müller's Archiv.,' 1846. This case was remarkable by the considerably advanced formation of a decidua, the developed uterine glands, and the perfectly-formed corpus luteum. Although no ovum was found, it was concluded that conception had taken place.

7. On the 9th of August, 1851, a young woman of 19, glowing with health, drowned herself. She had been married three weeks before against her will. The organs of generation could only be examined on the 13th, when, through the warmth of the weather, decomposition had commenced. The parts were, however, much developed. The right ovary exhibited a follicle recently burst; the point of rupture was large, and surrounded with the remains of the broken follicle and of the tunica albugimea; the follicle retained a very slender connexion with the structure of the ovary, so that it could be easily taken out with the forceps: but this was perhaps owing to decomposition. In its cavity was found a considerable blood-coagulum. The inner surface of the uterus was highly developed, vascular, velvety, and when floated out in water showed a commencing decidua. No ovulum was found either in the Fallopian tube or in the uterus. The hymen was destroyed. It is certain that she had not borne a child.

8. On the 26th of June, 1852, the body of a young woman of 19, who had hanged herself two days before, was examined. The organs of generation were fully developed. At the uterine end of the right ovarium there was seen a very large corpus luteum projecting far from the surface, as it often does in the cow, but as Professor Bischoff had never before seen it in the human female. This corpus luteum was not quite round, but, like the ovary, flattened; its long diameter was 29·5 millimetres, its width 22·5, and its thickness 9. The ovary weighed 17 grammes. It was very vascular, and contained a black-coloured cavity. Its thick walls consisted of a yellow body, of irregular folds; it was easily turned out of the substance of the ovary. Both ovaries further showed numerous scars, and several blackish, ancient, spurious corpora lutea, of which one on each side was very large. The uterine mucous membrane was highly developed, and formed a stratum, the boundaries of which were very distinct from the fibrous tissue of the uterus. This development extended to the os uteri internum, and terminated abruptly at the insertions of the Fallopian tubes. The glandulæ utriculares were beautifully developed, thickly planted, and bound together by a soft intermediate

substance. The inner surface of the uterus was not very vividly injected; it was quite smooth, and covered with a very distinct and beautiful cylindrical epithelium. The cylinders were bigger than those of the Fallopian tube. The hymen was destroyed, but she had never borne a child. In this case, also, no ovum could be found; but just in the opening of the right tube into the uterus was found a peculiar spot in the tissue of the mucous membrane, which, under the microscope, exhibited a nest of cells, granules, and spindle-shaped cells. In this a proper ovum could not be identified; but if the fate of the human ovum resemble that of the porpoise, this point seemed especially fitted to serve for the attachment of the ovum during its earliest development in the uterus. This young woman had menstruated as usual during four days, eighteen days before her death. She had intercourse with a poor lad, who had promised her marriage. Professor Bischoff concludes, from the persistent development of the corpus luteum, and of the uterus, eighteen days after menstruation, that conception had taken place.

9. On the 15th of July, 1852, a young woman of 19, otherwise perfectly healthy, died of trismus, following a slight injury of the nose. She came into the hospital on the 10th; menstruation began there, and lasted till the 13th. The parts were examined on the 16th, but the warm weather had already induced considerable decomposition. The parts were perfectly formed, but bore no trace of unusual turgescence. The right ovarium showed a dark-blue follicle, projecting over the surface of the uterus. No opening on the surface of the ovary could, however, be discovered. The follicle was filled with a blood-coagulum. It may have been that an opening existed, but the point of rupture in mammalia is for the most part so minute, and closes so quickly, that it is often difficult to find. Both ovaries showed, besides, several scars, and smaller ancient corpora lutea; and the left ovary one of larger size, with a scar overhanging the surface of the ovary. No unusual development of the inner surface of the uterus was observed. The mucous membrane on a perpendicular section was 1 millimetre thick, and showed no glands. The inner layer was discoloured, and without marked epithelium. This person had not borne a child. Whether she was a virgin is not known. No ovum could be discovered.

10. On the 9th of August, 1852, a young woman of 20 drowned herself, after a quarrel with her lover. The body was examined early on the 11th, whilst quite fresh. The parts were strongly developed, and the uterus, from its unusual size, indicated pregnancy. In the right ovary there was observed a recently-burst Graafian follicle, not projecting over the ovary, forming a scarred dark-red spot on the surface. One out of five irregular fragments of a freshly-formed corpus luteum could be easily turned out; a dark-red coagulum filled the cavity. The inner surface of the uterus was soft, velvety, smooth, vividly red, and marked with single dark-red spots. The mucous membrane was thick, and allowed the very beautifully developed glandulæ utriculares to be distinctly seen. Between them ran numerous fine bloodvessels, which formed a capillary network on the surface around the mouths of the glands. The os uteri was large, scarred, and indicated a previous delivery. No ovum or spermatozoa could be found. No certain information could be obtained as to the antecedents of this person. The great development of the corpus luteum favoured the conclusion that conception had recently taken place.

11. On the 4th of December, 1852, the organs of generation of a strong young woman, who had drowned herself on the evening of the 30th of November, were examined. The parts were well developed, but showed that the person had not borne a child. Both ovaries showed on their surface numerous scars, and especially in the left one were four or five distinct remains of false corpora lutea, as well as numerous Graafian vesicles shining through. In the right ovary there appeared a still closed, but very large Graafian vesicle. No opening or scar could be found, and the spot fluctuated. Taken out, this follicle weighed 3·48 grammes. When opened there flowed out, not the usual limpid contents of an unopened follicle, but a thick, chocolate-brown mass, which formed no coagulum, and con-

sisted, under the microscope, of altered blood-corpuscles. The walls of the follicle were not thickened, and no commencing structure of a yellow body was observed. The cavity of the uterus contained no blood; it was not reddened at the fundus, but the cavity of the cervix was so. The mucous membrane was little developed, and was not distinguished by defined boundaries from the fibrous tissue. The glands were scarcely observable. No delivery had taken place. No ovum or spermatozoa could be found. It was ascertained by Dr. Lorenz, of Offenbach, that this person had menstruated ten days before her death, and that coitus had afterwards taken place.

12. On the 7th of December, 1852, the body of a woman of 34, who had died suddenly after an injury of the cervical vertebræ and spinal marrow, received during an epileptic fit, was examined. It was only some days later that, a fresh corpus luteum being observed in the right ovary, the organs of generation were investigated. The parts were fully developed. Both ovaries presented a surface much scarred, and the left one the remains of five or six corpora lutea. The right one showed a fresh, conspicuous scar, and beneath it a corpus luteum, consisting of a ragged yellow surrounding substance, which enclosed a greyish, seemingly-discoloured coagulum. The inner surface of the uterus was not bloody, and the mucous membrane was slightly developed. Uterine glands were not observed, either with the naked or assisted eye. No delivery had taken place. The search after an ovum must necessarily have been useless. It was ascertained that she had menstruated seven days before death; recent connexion was improbable.

13. On the 21st of May, 1853, the body of a woman, aged 27, who had hanged herself on the evening of the 19th, was examined. It was unusually fat. The organs of generation were strongly congested, and the uterus large, as in the case of a multipara in the non-pregnant condition. In the right ovary was a large ruptured follicle, which, in consequence of commencing decomposition, was pultaceous. There was no blood coagulum, and the walls were not thickened. The follicle retained but slender adhesion to the stroma. The inner surface of the uterus was much macerated; it was covered with a bloody mucus, but was clearly villous, owing to the formation of a decidua, and the enlargement of the uterine glands. The os uteri was large and widely open; the vagina was large and covered with reddish mucus. Ovum and spermatozoa were sought for in vain. Dr. Pfeffer of Butzback learned that this person had been married, and had borne a child fourteen months before, which she had suckled until within three weeks, when it was weaned for want of milk. During lactation she had been regular, and for the last time on the 21st of April, exactly four weeks before her death. She had had connexion for the last time on the 15th of May, four days before her death.

In his observations upon the foregoing thirteen cases, Prof. Bischoff remarks that they confirm the doctrine, that in woman at every menstruation a follicle ripens, swells, and bursts, that the ovum escapes, and that a corpus luteum is formed. He then points out how these cases illustrate other questions connected with this subject. *Does the bursting of a follicle and the escape of an ovum always take place without exception?* Coste has answered this in the affirmative, but has not advanced the necessary proofs. The eleventh case seems to prove that the full consequences of menstruation are not in every instance necessarily carried out, but that a follicle may swell, and the ovum ripen without the bursting of the follicle, or the escape of the ovum. Such a condition will cause sterility, notwithstanding menstruation. Perhaps, also, the pain of the menstrual period depends upon this incomplete development.

What are the relations of the secretion of the blood from the uterus to the changes in the ovary? This is but symptomatic. It is certain that it may be wanting, and yet that the changes in the ovary may be accomplished. A woman may not menstruate, and yet she may conceive; for the essential condition, the ripening and escape of an ovum may proceed, and only the usual outward symptoms of this event, the secretion of blood, fail. Prof. Bischoff remarks, that he could never succeed in finding the ovum, but observes that he does not believe that it is so soon destroyed. He says there are but two cases known to him where it was so found, and that only one of these presents the desired certainty. This is the observation of Prof. Hyrtl, of Vienna. It is considered important to relate this case at length, in the form in which Bischoff received it from Prof. Hyrtl.

Thérèse Michal, aged 17, died on the 10th of October, 1844, of peritonitis in the clinique of Prof. Oppolzer. She had only menstruated twice in her life, and the last time was two days before her admission into the hospital on the 8th October. The breasts were tolerably developed; the pubes slightly covered with hair. The hymen was uninjured: the uterus of moderate size, compact, its cavity filled with a considerable quantity of thick blood; the mucous membrane was so loosened as to resemble a half-liquid plastic exudation. The lining membrane of both Fallopian tubes was suffused, loose, and covered with mucus. Both ovaries were of fair size, but the left one showed a ruptured follicle of the size of a large hazel-nut, filled with semi-fluid blood. Prof. Hyrtl submitted the parts to careful inspection under the microscope on the same day. Neither in the vagina, uterus, or tubes, could he find a trace of spermatozoa. But by a careful examination of the left tube, in that part which traverses the substance of the uterus, he found an ovulum, with all its characteristic properties, somewhat dull and turbid indeed, but fully recognisable. Hyrtl believes he even detected the germinal vesicle. This is a remarkable anomaly, since the germinal vesicle of every ovum, when it has left the ovary, has disappeared. Prof. Hyrtl had the ovulum immediately drawn by his artist, and exhibited it on the same day in his lecture on physiology. This case is free from doubt, as there is no room to suspect coitus. Prof. Bischoff regrets that he cannot admit as much with reference to the cases of Dr. Letheby, published in the 'Philosophical Transactions' for 1852. He considers that in the first case it is not proved that an ovulum was found. The existence of menstruation is doubtful, since no blood was observed. In the left ovary there was indeed a yellow body, but only of the size of a large pea, which scarcely permits of the belief that it was quite fresh. Still less can it be admitted that the body found in the left tube was an ovum. After the preparation had lain several days in spirit, the tube was cut open under water, and a vesicular body was found, which bore no proper character of an ovum, and was evidently nothing but a heap of mucus and epithelium. The second case, says Bischoff, gave him still less confidence. The formation of the corpus luteum does not in the least prove that the person died during menstruation. It appears too small and too little developed. The ovum was of the size of a small pin's head, and surrounded by a strong layer of nucleated cells, which Letheby calls the membrana granulosa, but which is always very soon lost in the tube. The ovum is also said to possess the germinal vesicle, although it had first been treated with water, then with vinegar, and lastly with ether. Where in the tube the ovum was found is not mentioned.

We think it right to express our opinion that the objections taken to these cases by Prof. Bischoff are not altogether borne out. In the first case, it is expressly stated by Dr. Letheby that a *sero-sanguineous fluid* was found in the vagina. There was, therefore, that evidence of actual menstruation which Bischoff says was absent. Nor does the description of the corpus luteum fail in any essential point: indeed, the preparation which is preserved still bears the characteristic appearance of a corpus luteum, resulting from the recent escape of an ovum. The highly refractive oil-globules found in the body taken out of the Fallopian tube, afford strong evidence that it was in reality an ovum. The second case appears wanting in no essential particular. Menstruation was present, coitus improbable, the corpus luteum characteristic of the congested state of the Fallopian tubes obvious, and the body represented to be the ovum proved to be such by the detection of all the elements of an ovum. It is true that the body was treated with acetic acid and ether, *but not until after* the several parts had been clearly distinguished.

Prof. Bischoff next considers *the constant increased development of the inner* *layer of the mucous membrane of the uterus and the follicular glands, attending menstruation*, and adverts to the opinions of Pouchet, Coste, E. H. Weber, and Sharpey. He believes he has discovered the key to all the differences of opinion emitted upon this question. It appears to him beyond doubt that a *regular* menstruation in a *healthy* person is attended by a stronger development of the mucous membrane of the uterus as a *normal* appearance. But it appears to him equally certain that the menstrual development is not thoroughly carried out when disease intervenes, for then this stronger development of the mucous membrane and its glands does not take place. Hence this was wanting in the 4th, 5th, and 9th cases, in which disease attended the death of the subject. In the 11th case, in which the follicle had not opened, although it was ripe, and blood was effused, and in which menstruation was attended with pain, an increased development of the inner surface of the uterus could hardly be observed. In the 12th case this was also wanting in an epileptic woman. In these cases the full stage of menstrual development was not attained. He believes that the want or defective progress of this preparatory development of the uterus is in all probability a frequent cause of sterility, when the other appearances of menstruation and all the other conditions are present. The uterus, says Bischoff, exhausts itself earlier than the ovary, and only arrives at the development necessary to conception under the most favourable conditions; thus an ovum, although ripe and fully developed, and even impregnated, may perish. The present cases leave no doubt as to the period of menstruation when the development of the inner membrane of the uterus begins, and how long it lasts.

The Professor then takes occasion to discuss the question as to the differences between the corpus luteum of simple menstruation, and that of conception. In the cow and sow the corpora lutea of the non-pregnant are altogether similar to those of the pregnant, and he has made the same observation in the bitch, the cat, and the porpoise. They disappear more quickly when no conception follows. In the human female, at the earliest period of the bursting of the follicle, the filling of it with blood, and the beginning of the development of the membrane of the follicle, to the formation of the corpus luteum, there is no difference at all. But in the further development of the corpus luteum, the impregnation of the ovum and gestation bring about a material difference. The corpus luteum in simple menstruation never attains the full stage of formation up to the complete filling-up of the follicle and the metamorphosis of the blood-coagulum: but this substance only developes itself into a peripheral layer, and soon falls into retrogressive metamorphosis. It shrivels up, and only leaves at the next and the succeding menstruation a more and more indistinct spot, changing from yellow to brown and black, and soon there remains nothing but a scar; which lasts for a time and gives evidence of the past process which has at an earlier period taken place on the surface of the ovary. The corpus lutea of conception, on the other hand, goes on developing itself throughout the first months of pregnancy, and attains a fulness of size, colour, and texture, which the menstrual corpora lutea never reach. It lasts throughout the whole period of pregnancy, and disappears after delivery. It may, however, be questioned whether it has a great diagnostic value. In the first period, so long as there are no other signs of pregnancy, these differences either do not exist, or are very difficult to determine. After delivery, when the signs of pregnancy in the uterus have disappeared, it again becomes difficult to distinguish them from the corpora lutea of fourteen days or three weeks standing, resulting from menstruation. And where pregnancy is plain, from the state of the uterus and other parts, the corpus luteum is of no value. Prof. Bischoff, therefore, concludes that the corpus luteum cannot be used in the determination *of doubtful cases.*

The question of *the dependence of conception upon menstruation* is then considered. Since menstruation indicates the ripening and escape of an ovum, it follows that conception also, inasmuch as it requires a ripe ovum, is dependent upon this process. But it has been conjectured that conception may take place at other times and under other influences. To this it may be enough to reply, that for centuries, and even for thousands of years, it has been established that the first condition of conception in a woman is menstruation. If it be said that conception has been observed without preceding menstruation, in this case menstruation means only menstrual flow of blood; that conception without this is as possible as is the ripening of an ovum without it. Professor Bischoff holds it to be proved that conception and pregnancy depend absolutely upon menstruation as the period of the ripening and expulsion of an ovum. He remarks, however, that it can scarcely be doubted that the times occupied by the transit of the ovum from the ovary, by the arrest of the ovum, and the preparatory changes in uterine mucous membrane, suffer great differences in individuals, and hence entail a longer or shorter duration of gestation.

2. Mr. Edward John Waring, of the Madras Medical Service, has contributed some interesting observations upon the physiology of menstruation and child-bearing among the Burmese. He finds that the periods of the commencement and cessation of the catamenia amongst the Burmese women do not present any remarkable differences from what has been observed amongst the English. In only 1 case out of 150 did this phenomenon make its appearance so early as 13 years of age; in nearly one-third of the whole number it appeared at 15. Out of 60 women it ceased between 45 and 50 in 25 cases. Of 125 women one bore a child at 15; this was the earliest example of parturition. It is observed that the ages and the events recorded may be relied upon as correct, the periods of the accession and cessation of the menstrual discharge being carefully recorded in each family.

II. Pregnancy.

1. *On the Amount of Respiration during Pregnancy.* By Dr. Küchenmeister. (Archiv. des Vereins, Band i. Heft 3.)

Dr. Küchenmeister has examined the comparative breathing capacity of several women during and after pregnancy, by means of the spirometer. In five instances, he found that the quantity of air taken into the chest during pregnancy was *greater* than that taken in after delivery. It must, however, be remarked, that the examinations after delivery were made within thirty days of that event—that is, at a time when general debility and muscular relaxation might partly account for the diminished breathing-power. Dr. Küchenmeister examined the capacity of the lungs of these same women, after an interval of five years. The general result was still found to be, that the capacity of the lungs is less in the non-gravid than in the gravid state. He therefore declares, that he no longer believes in the doctrine of the older physiologists and obstetricians, that during pregnancy the cavity of the abdomen is increased at the expense of that of the chest. He admits, the long diameter of the chest is encroached upon; but denies that the gross capacity of the chest is diminished.

III. Labour.

1. *On a new Abortion Tenaculum.* By Dr. Fleetwood Churchill. (Dublin Quarterly Journal, Feb. 1854.)
2. *A new Method of inducing Premature Labour.* By Professor Scanzoni. (Verhandl. Phys.-Med. Gesellschaft in Würzburg, Band iv. Heft 1.)
3. *The Induction of Premature Labour.* By Dr. Sack. (Deutsche Klinik, 40—1853.)
4. *The Induction of Premature Labour by the Douche.* By Dr. Sinclair. (Dublin Quarterly Journal, Feb. 1854.)
5. *The Induction of Premature Labour by Galvanism.* By Dr. Barnes. (Lancet, vol. ii. 1853.)

1. Dr. Churchill has submitted to the Dublin Obstetrical Society a new abortion tenaculum. In principle, it resembles an instrument in common use for extracting corks which have slipped into the cavities of bottles.

Dr. Churchill also submitted a new pessary. It is made of rod gutta percha, and forms an arch at the upper and lower extremity. The lower extremity is bent forward, so that a lateral view somewhat resembles the letter L with the posterior angle rounded. The upper arch is to be passed behind the os uteri, and the lower is seated on the anterior edge of the perinæum.

2. The method of inducing labour proposed by Scanzoni has been cursorily alluded to in some of the British journals, but the cases upon which the author founds his proposition are deserving of record: we therefore present an analysis of this paper, although it dates back beyond the limit embraced in this report. The author refers to the several known examples of consent or sympathy (or reflex action) between the breasts and the uterus. These facts led him to inquire whether irritation of the breast might not be made available in the induction of premature labour. An opportunity occurring, he put his conjecture to the test.

1st case. Rosina Picket, æt. 24, a healthy servant-maid, was delivered by means of perforation and cephalotripsy, on account of contraction of the brim, in December, 1850. She suffered some time after delivery from endometritis, and afterwards from neuralgia of the right leg. Being warned in case of pregnancy to place herself early under observation, she came to the lying-in hospital on the 5th December, 1852. She had menstruated for the last time on the 27th June; and had felt the movements of the child at the end of November. It was judged from this, and the examination made, that she had reached the twenty-first week of gestation. It was determined to bring on labour in the last week in February, by means of irritation to the breast. On the 25th February, the first trial was made with an air-pump, which was worked for two hours: the patient suffered no kind of inconvenience. The manipulation was repeated on the evening of the same day, and also on the 26th and 27th; on the two latter days, however, the apparatus was employed three times, and each time for two hours. Altogether, suction was exerted during sixteen hours. After the third application, the vaginal portion of the uterus had become perceptibly shortened. Uterine contractions began on the 27th, after the sixth application of the air-pump, and increased in the night so that on the 28th the os uteri was fully open, and the membranes were ready to burst. The lower extremities and the cord presented. The child was extracted apparently still-born, but after some minutes, it recovered. The delivery of the placenta and the puerperal week went over without any disturbance, and the mother left the hospital on the 9th day.

2nd case. Barbara Schutz was seized on the 25th October, 1852, with a pleurisy of the right side, and was received into the hospital under the care of Dr. V. Marcus. The inflammation was soon followed by exudation. As the exudation decreased, symptoms of previously existing tuberculosis became more marked. The distress in respiration became great. The patient had menstruated for the last time on the 4th August, 1852; in the first half of January, she perceived the first movements of the child, so that the end of gestation might be expected about the 11th May. On the 1st March, excessive difficulty of respiration appeared. The patient complained of great oppression of the chest; the fits of coughing were violent. The pulse was 132 in the minute. Dr. Von Marcus requested Professor Scanzoni to make a minute examination, in order to determine whether the dangerous dyspnœa might not be aggravated by the compression of the lungs, caused by gestation, and whether relief might not be gained by the induction of premature labour. The result of this examination was, that it was determined to bring on labour. It is proper to state, that no symptom of impending labour existed. The suction-apparatus was applied at ten o'clock, on the 2nd March, to both breasts, and its action continued for three hours. At two o'clock, the proceeding was repeated, but, through the restlessness of the patient, for about an hour only. About three o'clock, the patient complained of pains, having all the characters of true, strong labour-pains. The os uteri was expanding; and the apparatus was again resorted to, in order to hasten the labour. In a short time, the os uteri was fully open—the membranes burst, and the head descended. The labour was soon terminated by the birth of an apparently still-born child, which quickly recovered.

The anticipation that the dyspnœa would be relieved after delivery was fully realized. The Professor appeals to the fact that, as there was no sign of labour at ten o'clock, whilst delivery was completed in seven hours from that time, he cannot be deceived in attributing a powerful influence to the irritation of the mammary nerves; but he refers to his own experience as proving that strong attacks of dyspnœa may bring on and accelerate labour.

The author enters upon a review of the relative merits of this new method and of those in use. After pointing out the advantages it appears to him to possess over puncturing the membranes, plugging with a sponge, Kiwisch's douche, and Cohen's method of injecting the cavity of the cervix, he admits that further observations are necessary in order to determine the question. In referring to the douche, he especially points out the numerous instances in which this method has failed. He does not, however, refer to the plan of inducing labour by *direct* excitation of the uterine muscular fibre by galvanism, of which there are now several successful cases on record. In considering the objections to his plan, Professor Scanzoni adverts to the danger of causing inflammation of the breast; observing, however, that the first patient did not experience the slightest inconvenience. But, as he observes, "one swallow makes no summer;" and when we consider the proneness of the breast to disease, and the long-continued action of the air-pump that may be necessary in order to excite uterine contraction, the danger indicated by Scanzoni will not appear unworthy of attention.

3. In the case related by Dr. Sack, the method of Scanzoni described above was first employed: the suction-apparatus was applied six times; during twenty hours in all, after which it could not be continued, on account of the painful condition of the breasts; still the uterus remained unaltered, beyond a state of tension during the application. Kiwisch's plan was then tried, with no better success, although twenty douches were administered, and followed by warm hip-baths, use of belladonna, and the plug. The injection of four ounces of warm water into the cavity of the uterus then acted like an electric shock. The whole nervous system was called into action, until the general disturbance was gradually concentrated in the uterus; delivery took place in fourteen hours after the injection. It is advised, in pursuing this plan, to employ a tube large enough so to fill the os uteri as not to permit of the reflux of the fluid injected, but to force it to make its way between the membranes and the uterus.

4. Dr. E. B. Sinclair has related to the Dublin Obstetrical Society another example of the successful induction of premature labour by means of the water douche, as proposed and practised by Kiwisch. Dr. Sinclair sought to explain the *modus operandi* of the douche. He submitted that the action of the douche differed only from the first stage of labour in that the mechanical power is applied from without, and that the waters impelled against the os have not a membrane covering them. He therefore insisted strongly upon the importance of preventing the escape of the injected water from the vagina, so as to obtain a sufficient distending power.

5. Dr. Barnes has related two cases in which premature labour was successfully brought on by the agency of galvanism. Dr. Barnes points out, that inasmuch as galvanism appears to act as a direct stimulus to muscular contractility, and not by primary action upon the nerves proceeding to supply the uterus, that it is not necessary to apply one pole of the battery to the os uteri, but that contraction is readily induced by placing the poles one on each side of the abdomen, over the uterus.

IV. Diseases of the Puerperal State.

1. *Puerperal Convulsions and Albuminuria.* By MM. Depaul and Mascaret. ('L'Union Méd., Jan. 1854.)

M. Depaul has read an able report upon a very instructive memoir by M. Mascaret, to the Académie de Médecine, upon this important subject. The author divides the causes of eclampsia into predisposing and occasional. He regards as special predisposing causes, first labours, the sanguineous and lymphatico-sanguineous temperaments, infiltration of the legs; but he does not consider albuminuria, and in this M. Depaul confirms the views of the author, as an essential cause of the disease. M. Depaul cited three cases of puerperal convulsion, in which no trace of albumen could be discovered in the urine: one which had occurred in his own practice, one recorded by Dr. Leuer, and a third by Professor Dubois. Two additional cases are recorded in M. Mascaret's memoir. M. Depaul again refers to the frequency of albumen in the urine of pregnant women, and the comparative and absolute rarity of convulsions. Out of 41 women in whom the urine was found albuminous, observed by Dr. Blot at the Maternity, only 7 were seized with eclampsia. In order to collect these 41 cases, he examined the urine of 205 women, taken indiscriminately from the wards of the hospital. Further, M. Depaul cites two cases, in which, having examined the urine before labour without finding any albumen present, convulsions broke out, and the urine was found to contain albumen after the second fit in the first case, and after the fourth fit in the other case. M. Depaul also observed that the albumen disappears with remarkable rapidity after delivery, whilst not seldom convulsions only appear some hours or even some days after parturition. The reporters, however, admitted that albuminuria was too frequently observed in the course of gestation, and coincided too frequently with puerperal convulsions, not to render the investigation of the relation of these conditions necessary. M. Depaul observed that the common explanation of albuminuria in pregnant women was not to be found in inflammation of the kidney. In the autopsies he had made, the kidneys were found either perfectly healthy, or simply congested. The true point of departure he believed to be, the modifications that gestation caused in the blood.

V. Diseases of the Pelvis.

1. *History and Description of the first known Case of Pelvis with so-called "Dislocation of the last Lumbar Vertebra forwards."* By Dr. Spaeth. (Zeitsch. der Gesellsch. der Aerzte zu Wien, Zehnter Jahrg. 1 Heft, 1853.)

Dr. Joseph Spaeth, assistant in the Obstetric Clinique at Vienna, refers to the example of this obstructive deformity of the pelvis recorded by Kiwisch in 1850, and to that of Kilian in 1853. The present case occurred in 1836. The patient was 29 years old, of middle stature, well nourished, with neck, bust, and limbs well developed. The body in walking was perceptibly bent backwards: the lumbar vertebral column unusually concave; the abdomen hanging forwards. On examination at the commencement of labour, the head of the child was found directed towards the left hip. The os uteri could only, with the greatest difficulty, be reached by the point of the finger. "*The last lumbar vertebra was felt projecting considerably forwards.*" The labour-pains became irregular and painful; after forty-eight hours the labour had scarcely advanced; the os uteri was swollen and not open, and the head could hardly be reached. Impending or actual rupture was dreaded, and the child appearing to be dead, perforation was resorted to. On the following day strong pains came on, and the delivery took place. The after-birth came away spontaneously. The uterus contracted. Not much blood was lost. The patient sank on the 3rd of March, of metro-peritonitis. The pelvis was preserved by Professor Rokitansky in the anatomical museum of the General Hospital of Vienna; it is marked 1715 and 5203. The greatest deformity in this pelvis consists in the remarkable relation of the last lumbar vertebra to the sacrum, projecting forwards, occupying the place of the promontory, and shortening the conjugate diameter of the brim. Through the projection of the last lumbar vertebra, there results a double twist of the vertebral canal, and a considerable narrowing at the points of bending. The conjugate diameter of the inlet in the dried pelvis is 9″ 5‴, that of the transverse 4″ 9‴. Dr. Spaeth agrees with the view taken by Kiwisch as to the origin of this condition, that it is congenital, since he could find no evidence of injury of the joints concerned. He refers also to the remark of Kiwisch, that "although no other case of this deformity be yet known, yet it cannot be doubted that analogous examples will soon be brought forward."

QUARTERLY REPORT ON FORENSIC MEDICINE, TOXICOLOGY, &c.*

By W. B. KESTEVEN, M.R.C.S.

I. INJURIES, WOUNDS, DROWNING, BLOOD-STAINS, ETC.

Laceration of the Liver—Recovery—Death from other causes—Autopsy.—The following case, which is related by Mr. HAMMOND, a dresser to Mr. Hilton, at Guy's Hospital, presents features of great practical interest to the medical jurist. The fact of the healing of so grave a lesion as accidental laceration of the liver leaves room for the inference that a wound of the same organ might likewise heal, and the patient do well, if he escape the immediate consequences of hæmorrhage.

A man, aged 33 years, fell from a height of thirty-five feet, to the ground, upon his right side. He was immediately put into a cab, and taken to Guy's Hospital. When admitted, he was in a state of collapse; complaining of great pain and tenderness in the centre and right side of the abdomen. A wound about an inch and a half was observed above the right eyebrow, laying bare the bone. The patient was kept for several days without any other food than a little simple fluid, now and then, to allay thirst. Opium and mercury were administered, and leeches were applied. By the fourteenth day all symptoms referrible to the abdomen had disappeared, but others showing cerebral mischief had supervened, and from which he died on the thirty-eighth day after the occurrence of the accident. On post-mortem examination, extravasation of blood and recent suppuration of the brain were found—besides other appearances denoting old-standing disease. These, and other important facts in this case, our space compels us to pass over, that we may confine our attention to the reparation of the injury done to the hepatic structure. The abdominal cavity did not present any traces of inflammation, but a slight degree of laceration had been suffered by the serous and muscular coats of the duodenum. The cæcum and colon exhibited the traces of ecchymosis. The liver had been torn or broken on its convex surface, from the margin near the fundus of the gall-bladder obliquely backwards towards the left side, to the extent of three inches, a part of which was adherent to the diaphragm by recent but firm adhesions. At that part of the laceration nearest to the acute margin of the organ, the edges of the wound were separated from each other to the extent of about three-quarters of an inch, and not upon the same level; this separation gradually diminished until it became a mere crack or fissure, losing itself in the substance of the liver. In the gap of the broadest fissure, an insulated portion of liver-structure seemed to have been detached from the surrounding parts at the

* Owing to the late period at which we received the German Medico-legal journals, we are compelled to defer the extracts from them until our next Number.

time of the injury, and to have remained, forming a rounded and abrupt elevation, detached from the neighbouring textures, except at its base, where it rested on the subjacent liver-structure. The lacerated surface was closed in and covered over by recent false membrane, but the rough edge of the laceration separating it from the smooth convex surface of the liver, was very strongly and satisfactorily marked.—*Lancet*, Dec. 10.

Detection of Blood-stains on a Knife covered with Rust.—M. DAUBRAWA was requested to ascertain the existence of blood-stains on a knife which was suspected to have been used in the commission of a murder. The knife having lain a long time in a damp place, was rusted; but certain bright rust-free spots could be distinguished amid the rust. On heating the point of the blade, these spots scaled off, while the rust remained adherent; on the other hand, on immersing the knife in dilute hydrochloric acid, the bright spots remained untouched, although the rust was readily dissolved. It was probable that these bright spots were blood-stains; but as some non-nitrogenous organic acids will produce similar marks, some of the detached scales were heated in a test-tube, and, by the disengagement of ammonia from the hæmatine of the blood, caused a blue colour on reddened litmus-paper. The whole blade was then macerated for a long time in distilled water, which acquired a reddish discoloration; and by the aid of a lens, fibrine could be seen adhering to the blade in the situation of the bright spots. Ammonia added to the solution caused no precipitate; nitric acid gave a white precipitate; it became turbid from heat; solution of chlorine at first produced a green tint, this colour then disappeared, and white flocculi were deposited. These different fluids having been evaporated to dryness and burnt, and the residue dissolved in hydrochloric acid, demonstrated the presence of iron by its appropriate reagents.—*Journal de Chimie Médicale*, December.

Detection of Blood-stains on Garments.—M. MORIN has related a series of experiments to illustrate the mode of detecting blood-stains from which the colouring matter has been removed by boiling water. Some human blood was received on a cloth as it escaped from an opened vein. The stains were after some hours washed in water, at a temperature above that required for the coagulation of albumen; they were then immersed in boiling soap-and-water, and afterwards in cold water, until the water presented no longer any opaline tint. When dried, the spots were firmer in consistence than the surrounding tissue. These portions having been cut out, and macerated in distilled water for a considerable time, the fluid acquired no discoloration, nor did the application of heat give any indication of a trace of dissolved albumen. Macerated in a solution of potash, the addition of nitric acid occasioned a white precipitate.

Blood-stains on garments that have been washed, do not, by this alkaline treatment, lose what colour they may have retained. The tests for iron will detect the presence of this metal as the cause of their colour.

The coexistence of iron and a protein element in suspected stains, furnishes an important element of the proof of culpability.—*Journal de Chimie Médicale*, December.

Glycerine in the Detection of Blood-stains.—Dr. ALFRED TAYLOR has informed the writer that he has employed pure glycerine in medico-legal inquiries, as a means of separating blood-corpuscles, and has found it serviceable in that respect, from its not drying; a property of that fluid of considerable practical importance in the examination of blood-globules under the microscope, or by the help of a lens, as the globules are in no wise altered in form or size thereby.

On Bloody Footprints of the Feet, and their Measurement.—Dr. CAUSSÉ having had occasion to examine the bloody footprints of a naked foot on the floor of an apartment, in order to ascertain its correspondence with, or difference from, the foot of a certain individual, has contrived a plan which he considers certain to arrive at the truth in all similar investigations. The main features in this plan consist in taking the measurement, by a line passing along the inner border of the foot, and joined at right angles by a series of lines traversing the sole of the foot. Dr. Caussé has devoted a vast deal of time to this subject, and has illustrated his observations by diagrams, showing the mode in which the measurement of the minutest points are accurately taken.—*Annales d'Hygiène, &c.*, January.

Fibrinous Concretions in the Heart, as the result of Suicide by Hanging.—At a meeting of the Medical Society of London (Dec. 10th), Mr. B. W. Richardson related the case of a man, who was cut down after a quarter of an hour's suspension. Intense vascular reaction set in a few hours afterwards. The man lay in a perfectly inactive state, breathing rapidly (sixty in the minute), pulse beating with great frequency, the temperature of the surface increased. Mr. Richardson was of opinion that, with these signs of increased combustion, the blood would become super-oxidized, and that the case would terminate rapidly by sinking from fibrinous concretions in the heart. This took place at about sixty hours after the suspension. On examination of the body, the brain was found highly congested, and serum effused under the arachnoid. The lungs were congested. The right auricle was distended with coagulated blood to nearly three times its natural size. In the right ventricle there was found a large, firm, fibrinous cylinder, which filled up more than one half of the ventricle. The base of the fibrinous cylinder was attached to the mitral valve, and it had not been detached. Long prolongations of a similar kind ran into the pulmonary artery.—*Medical Times and Gazette*, Dec. 17.

II. Infanticide.

Fractures of the Bones of the Cranium in a newly-born Infant.—An inquest was recently held in Islington, before Mr. Wakley, on the body of an infant, whose death, there was reason to believe, had been caused through violence wilfully inflicted by the mother. On examination of the body, it was found that unusually extensive and numerous fractures of the bones of the skull had given rise to extravasation of blood within and beneath the membranes covering the upper and lateral surfaces of the brain. There was ample evidence that respiration had been fully established; it was indeed admitted by the mother, that the child had been born alive; its death she alleged had been occasioned by its having fallen into the pan of the water-closet, where she asserted that she was seated at the moment of its birth. Although the cord was torn at the distance of four inches from the navel, this explanation was inadmissible, as the distance of the fall was too small to have occasioned such a result, since the force of the fall would obviously have been interfered with by the obliquity of the direction of the child's exit from the maternal outlet, whereby the child's head would have been brought in contact with the side of the pan, down which it would have glided safely to the lowest parts. There were other circumstances which threw a serious doubt upon the whole of the woman's statement. It was clear that such an accident as that mentioned could not have given rise to such extensive injuries as were discovered. These were as follow. Over each orbital ridge, the frontal bone was fractured horizontally to the length of about an inch. On the right frontal prominence, the bone had been driven in to the extent of three-quarters of an inch, in the form of an acute wedge-like fracture. The parietal prominences were both fractured vertically to the length of an inch and a quarter; on the left side, this vertical fracture was connected at its lower end with another of about the same length, joining it at a right angle, and passing horizontally forwards to the edge of the bone. There were several minor fractures of all the bones forming the superior and lateral portions of the skull. These were not connected with each other, nor did any of them present a stellate character. No fractures were detected at the base of the cranium.

It was a matter of surprise that such lesions as are here described should have existed without any trace of injury to the scalp; which presented only the ordinary *caput succedaneum* belonging to a hard labour. No evidence was obtainable as to how these fractures of the skull had been occasioned. The medical witness (the writer) could only assert that they were the cause of death, and that they could not have been produced in the way alleged by the mother. All beyond this was mere speculation; it is not conceivable how they could have been accidentally caused. On the other hand, any suggestions making them attributable to wilful violence involved a degree of atrocious cruelty, and of total absence of natural feeling in the mother, from which we willingly seek refuge, by giving the accused the benefit of the doubt arising out of the want of evidence. The verdict of the coroner's jury was in accordance with these views: that death had been occasioned by fractures of the skull; but that how these were caused, there was no evidence to show.

III. Medico-Legal Psychology.

Medical Testimony and Evidence in cases of Lunacy.—Dr. Thomas Mayo has made these the topics of his Croonian Lectures for 1853. He examines the meaning of the various terms used in law to express states of insanity,—e. g., "lunatic or insane," "idiotic," "of unsound mind." The word *lunatic* being used in a generic sense, containing, as species, other heads of the enumeration, as in either case the writ runs *de lunatico inquirendo*, and the commissioners who watch over the proper management are the "Commissioners of Lunacy."

Dr. Mayo enforces the caution upon the medical witness, that he has the simple fact of insanity to speak to, and not to take into consideration its consequences, which come within the functions of the judge and jury. Considering insanity in its medico-legal relations, where we are not called upon to propound the treatment, but merely to certify the reality, the author further observes, that the physical phenomena on which the deranged state of mind may depend, are not fairly within the cognizance of the audience addressed, and on whom our reasoning, as drawn from this question, will have little or uncertain effect.

Dr. Mayo, with Pinel and Cullen, adopts the term *delirium* as containing the essential mental elements of insanity, and then proceeds to examine the various characters of delirium and its delusions. The grounds of imputing insane delirium are the presence of inconsecutiveness of thought and of certain delusions, over which the patient has no control, or no such control as should prevent their influencing his conduct. The medical witness should make these two elements throw light on each other. Dr. Mayo follows the subject of notional delusions, by examination of some of the most celebrated cases which have lately been under public investigation.

Having discussed the subject of insanity in its relations to medical proof, under its essential element—delirium, Dr. Mayo, in his second lecture, proceeds to the consideration of a peculiar state under which the leading and important subject for judicial investigation, is an orgasm, or intense and sometimes sudden desire, leading the sufferer to perform some criminal act; this orgasm, not always susceptible of being construed into delirium, as not being obviously attended either by a morbid delusion, or by a state of inconsecutive thought. In many of these cases objective false perceptions exist, and lead to a criminal act; in these cases there is little difficulty in affirming insanity. With reference, however, to that large number of cases in which a sudden and motiveless criminal act is perpetrated, Dr. Mayo discusses at considerable length, and shows to be wanting of proof, the doctrine of Dr. Prichard, with regard to what he termed "instinctive" or "moral insanity." Dr. Mayo objects to these expressions, first, that it breaks down an accredited distinction, which assigns an intellectual lesion as characteristic of the disease termed insanity; secondly, that they practically introduce confusion on the point of responsibility or irresponsibility of the acts of a class of individuals merely eccentric or atrociously depraved.

Dr. Mayo, in his third lecture, discusses idiocy and mental unsoundness, distinguishing the latter from insanity, and pointing out the civil and criminal relations of the distinction. The subject is treated strictly in its legal relations. Dr. Mayo cites several instances in which vice has escaped with impunity, as lunacy, to the neglect of the principle that insanity or unsoundness of mind should confer impunity only so far as it implies ignorance of the illegal nature of the act in question.—*Medical Times and Gazette*, Dec.

IV. Toxicology, &c.

Coloration of Condiments and Fruits with Preparations of Copper.—M. Moride has met with numerous accidents originating in the employment of salts of copper for colouring fruits, pickles, &c.; and from neglect of cleanliness with copper culinary utensils. Some prunes, which had produced vomiting, &c. in seven individuals, being analyzed, were found to contain sixty-five centigrammes (=9·265 grs. English) of sulphate of copper, in from about twenty-seven to twenty-nine grammes (=about six pounds troy) of prunes. Gherkins, as sold in the shops, have also frequently been found, according to M. Moride, to have produced similar inconveniences. (*Journal de Chimie*, February.) It may, however, be doubted whether so small a proportion of salts of copper as is usually detected under the circumstances above referred to, would be likely to give rise to symptoms of an alarming character. We are familiar with the medicinal administration of sulphate of copper without injury, in much larger doses than usually indicated by analysis in alleged cases of poisoning. It has been suggested, with much reason, by Dr. Paasch (*Casper's Vierteljahrsschrift*, 1 *band*, 1 *heft*, *p.* 79) that the alleged poisonous effects must rather be attributed to animal poison, analogous to that known as sausage poisoning, arising from the decomposition of the alimentary matters, through neglect and want of cleanliness. For an abstract of the paper by Dr. Paasch, and remarks thereon (by the writer), we may refer to the *Legal Examiner and Medical Jurist*, Nov. 13, 1852.—*Journal de Chimie Médicale*, January.

Attempted Poisoning with Acetate of Copper.—M. Chevallier has written an elaborate report, occupying twenty-seven pages of the *Annales d'Hygiène*, upon the case of a woman whose husband had attempted to poison her by mixing verdigris with her food. The poison was administered with some haricots, but on account of the nauseous taste and acrid effects upon the mouth, they were only partly eaten; the greater portion were thrown away into the garden. Having been again collected, they were submitted to analysis by M. Chevallier. The beans and garden mould were together macerated in dilute sulphuric acid, and then dried by heat, until the organic matter was charred, and a portion of the acid volatilized. The black residue was then treated with nitric acid, and boiled in distilled water. This fluid was subjected to several tests, by which it was ascertained that neither arsenic nor antimony was present, but a salt of copper was detected in small quantities. It was alleged by the accused that he had employed the coloured material from the ends of lucifer matches, that if any copper were present it must have been derived from the garden mould. It is unnecessary to follow the details of the analyses—which destroyed every pretence of the kind advanced by the accused, who was proved to have administered verdigris, with intent to kill, and was accordingly condemned to perpetual hard labour.—*Annales d'Hygiène, &c.*, January.

Poisoning with Arsenic—Death—Exhumation.—If the promoters of a legislative enactment to secure the appointment of Public Prosecutors wanted an additional argument, which we do not admit to be the case, they might find it in the inefficient manner in which inquiries into the causes of death are often carried through in Coroners' Courts. To the numerous instances that have occurred before, we have now to add another, in which the deaths of no less than six individuals have been the subjects of investigation. To the energetic exertions of Mr. BOTTOMLEY, of Croydon, is attributable the discovery of the fact that six persons, whose deaths had previously been declared by the verdict of a coroner's jury to have been caused by "want of sufficient nourishment and typhus fever;" had, in truth, all fallen victims to deliberate and intentional poisoning, by the administration of arsenic, at the hands of one, herself among the victims, the mother of the family. The particulars are briefly as follow:

The family in question, residing at a village named Waddington, near Croydon, Surrey, consisted of a man named Atlee, his wife, and four children; the man being between forty and fifty years of age, his wife twenty-seven, and the children who died respectively of the ages of eight, six, and four years. The husband was an agricultural labourer. On the 14th of December last, the mother was absent from her home on business for about two hours. She left the children in perfectly good health, but on her return she found them sick and vomiting violently. Their illness continuing, Mr. Hubert, the district union surgeon, was called to attend them on the following Tuesday. Two days afterwards, on the 22nd, one of the children died, on the 23rd another, on the 24th the third, and on the 25th the mother. Four days afterwards—viz., on the 29th—an inquest was held on the bodies by Mr. Carter, coroner for East Surrey, when, after the examination of two or three witnesses, the medical gentleman gave it as his opinion that the deaths had been caused by "want of sufficient nourishment and typhoid fever." Mr. Hubert at the same time attributed to the consumption of pond water by the family, the exciting cause of the typhoid fever. The inquest was adjourned for the purpose of an examination of the body of one of the children. In the meantime the father died. At the adjourned inquest the medical witness, Mr. Hubert, who had examined the body, stated that he was convinced that death had been occasioned by typhus, and that there was no ground of suspicion of poisoning. Mr. Bottomley, of Croydon, having read the reports of the inquests in the newspapers, and feeling convinced that the deaths were not satisfactorily accounted for, either by the evidence, or by the verdict, addressed a memorial upon the subject to Lord Palmerston, requesting that the inquest should be reopened. The Secretary of State, upon Mr. Bottomley's representations, instituted a further investigation, the consequence of which has been the exhumation of one of the bodies, and an analysis of portions of the contents of the abdomen by Dr. Taylor, parts of whose report we subjoin.

"1. *Contents of the Stomach.*—The brown pasty matter was free from any undigested portions of food. When diluted with water, it deposited no mercurial sediment; the contents were submitted to the usual chemical tests and processes for the detection of such virulent poisons as would produce the inflammatory appearance found on the stomach, including arsenic, corrosive sublimate, the salts of mercury, lead, copper, and antimony; *but there was not a trace of any one of those poisons.*

"2. *Coats of the Stomach.*—The coats of the stomach were cut up and boiled in hydrochloric acid and water, and the decoctions thus obtained were submitted to the same tests and processes for the detection of irritant poisons, but no poison was present in them.

"3. *The Small Intestines.*—These, with their contents, were treated like the coats of the stomach; but the result was, that no poison was contained in them.

"4. *The Liver.*—About 4oz. of the liver were next subjected to analysis, and the result was, that this organ was found to contain arsenic in a comparatively large quantity. The arsenic was separated from the liver in the state of metal; this was converted to crystals of white arsenic, and tests then applied left it beyond the slightest doubt that *the liver of the deceased contained that poison.* The arsenic had been deposited in the liver as the result of the absorption of the poison into the blood while the deceased was living."

After this discovery, it was not considered necessary to protract the investigation by separate analysis of the pancreas and spleen.

"I entertain no doubt," says Dr. Taylor, "in this case, that death was caused by arsenic; at the same time, having carefully read the depositions, and considered the medical evidence given at the first inquiry, I beg to make the following remarks:—The evidence given by the medical gentleman (Mr. Hubert) is consistent with the facts as they appeared. It appears to have been clearly established that probably as a result of poor living and unwholesome food, deceased and her family suffered from worms to a remarkable degree. In the absence of any strong grounds for suspicion, the presence of these worms might account for the symptoms of irritation of the bowels and fever under which they laboured. The fact that deceased survived for a period of seven or eight days after the first attack of vomiting and purging, is a most unusual circumstance in poisoning with arsenic, and might fairly disarm suspicion. *Had an analysis of the stomach and intestines been made at the first inquest, no poison would have been found.* In the case of the deceased it has been now detected in the liver, but to detect it in this organ requires practice and experience in such matters, and the appurtenances of a large and well-furnished chemical laboratory. It appears to me probable, from the evidence and analysis, that the deceased took the poison in small doses at different times in some article of food, and that the last dose taken was probably some hours or even days before she died."

The verdict of the jury at this inquest was to the effect that "the deceased, Harriet Atlee, died from poison administered by her own hand, and that she had also given poison to her children." This verdict seems clearly to demand further examination of the bodies upon which verdicts of death from natural causes still stand recorded.—*Lancet of Feb.* 11, *and Local Papers.*

Poisoning by Arsenical Ointment. An instance of the injurious effects of the absorption of arsenic from the cutaneous surface, has come under the notice of Mr. Mitchell, of Liverpool. A man who had pediculi upon his scrotum and pubes, applied thereto a mixture of arsenic and soap, which he had prepared for killing bugs; the result was that the cuticle of the scrotum peeled off, leaving the *cutis vera* exposed; inflammation of the testicles followed. The patient complained of stiffness in the neck, and difficulty of swallowing, thirst, headache, an indescribable sensation, as if his hair was standing on end and being pulled out by the roots. He suffered also from vomiting and purging, with burning pain in the bowels, and tenderness of the epigastrium. These symptoms did not wholly subside for a fortnight.—*Medical Times and Gazette*, Dec. 9.

On the comparative Poisonous Properties of the Arsenious and Arsenic Acids.—In 1848, MM. Woehler and Frerichs express doubts of the accuracy of the generally received opinion that arsenic acid possesses more energetic properties than does the arsenious acid. Their doubts were founded upon the less considerable local lesions produced by the former than by the latter, in dogs and rabbits poisoned by both acids. M. Schroff, however, has pointed out that the intensity of the poisonous action of any substance is not necessarily measured by the degree of local injury it produces. With reference to this point, M. Schroff refers to cases where death has rapidly ensued on arsenical poisoning, and in which no lesion of the stomach or intestines has been detected. M. Schroff has performed experiments upon rabbits, in order to determine the comparative poisonous activity of these two arsenical compounds. The conclusions from these experiments are, that the arsenic is not a less energetic poison than the arsenious acid, although for the most part it produces less intense local action. With regard to arsenious acid itself, the severity of its action varies, but it may be stated to be greater in proportion, as it is better dissolved.—*Journal de Chimie Médicale*, December.

Mercurial Poisoning—Death.—An inquest was held at Lambeth, in December last, on the body of a lady, aged forty-six years, who having purchased some "worm lozenges" of a druggist, was attacked with profuse salivation and glossitis. She had taken one lozenge each night, for four nights in succession. She died at the end of eighteen days. The medical witness, Dr. HUMBLE, gave it as his opinion that the deceased had died from mercurial poison contained in the lozenges as calomel. Verdict, "accidental poisoning."—*Morning Herald.*

Poisoning by Tartar Emetic.—A girl, aged sixteen years, complained of feeling bilious, and was advised by a nurse to take a dose of tartar emetic; one penny-worth was procured on Nov. 21, and two-thirds of the quantity taken the same evening, at six o'clock. Within a quarter of an hour vomiting of dark matter came on, and a little while after, purging occurred. These symptoms continued for about three hours. A burning pain in the œsophagus was complained of. She then fell asleep. On the following morning she took some tea, and did not appear so ill as to require medical attention. About four o'clock in the afternoon, however, she "felt as if dying;" medical aid was then obtained, the pulse was thin and cord-like. Brandy, beef-tea enemata, and other means were employed, but failed to rally her. The head was continually thrown back, and the knees drawn up, the skin warm and moist, the pupils dilated; constantly screaming, she lingered in a delirious state until the morning of the third day.

On examination of the body, thirty-six hours after death, the throat appeared swollen, and both iliac regions discoloured green. The lungs were slightly congested. The heart healthy, and containing about six drachms of fluid blood; the left ventricle so firmly contracted as to close its cavity. The liver full; gall bladder half full of thick green bile. Kidneys congested. The stomach containing about sixteen ounces of thick grumous fluid, a large patch of greenish discoloration on the posterior part of the greater curvature, near the cardiac orifice, penetrating to the peritoneal covering; the coats of the stomach were at this part softened, and blood was effused beneath the mucous membrane; the same was observed in eight or ten places in the larger end of the stomach: only very slight traces of the poison were obtained by the appropriate tests. The intestines contained quantities of grumous fluid and mucus, but presented no appearance of inflammation. The ovaries contained cysts and coagula of blood. The hymen was perfect.

In this case, the quantity of tartar emetic taken was about one drachm. Ten grains is the smallest fatal dose on record. (Taylor on Poisons, p. 485.)—*Lancet,* Jan 21.

Poisonous Effects of Iodine injected into the Sac of a Chronic Abscess.—M. NELATON, on the 20th June, opened the sac of a lumbar abscess, and injected it with a solution of iodine of the following strength—viz.: One part of tincture of iodine, and two parts of iodine, with sufficient iodide of potassium to prevent the precipitation of the iodine. About half the quantity returned by the canula, which was then withdrawn, and the wound closed. This was done about half-past ten in the forenoon; at three o'clock of the afternoon, the patient experienced vertigo, with impairment of vision; soon afterwards, vomiting of serous matter mixed with chocolate occurred. At the same time, he felt general indisposition; the skin was moist, the extremities cold, the pulse was small and thread-like; the respiration hurried, and he was becoming prostrated. The patient continued in the same state during the evening, and succeeding night.

On the 21st, the vomiting continued. He lay faint and groaning; the palpebræ were greatly swollen, and had a violet tint. The patient complained of a distressing sensation in the throat. On the 22nd, although apparently in the same condition, he expressed himself as being better. On examining the throat, it appeared merely dry, but respiration was impeded, especially during inspiration. A rough croupy cough was present; and the voice was whispering. This aphonia and dyspnœa, arising from œdema of the glottis, are noticed by Orfila among the signs of poisoning from iodine. The tumefaction of the palpebræ, and the vomiting, left no doubt that the indisposition was the result of the absorption of iodine injected into the sac of the abscess. The indication of treatment, therefore, was the elimination of the iodine. Ice, and iced beverages, were given; slight vesication over the larynx was effected; sinapisms to the extremities, and subsequently doses of croton oil were administered. The result is not stated.—*Journal de Chimie Médicale*, January.

Poisonous Effects of Chromate of Potash.—(The following instructive case is related by Mr. G. Heathcote.) "August 2nd, 1853.—I was requested to visit William H——, a spare, anæmic-looking man, aged thirty, single, about five feet ten inches in height. He had been under medical treatment for ten weeks, during which time he gradually became much emaciated and exhausted. He had been suffering from ulcerated sore-throat for more than three months, which presented the following appearances: several ulcers on the tonsils and throat, the surface of which seemed covered with an ashy slough, and the surrounding mucous membrane was dark, livid, and swollen; pulse 120, small and sharp; great thirst; loss of sleep; tongue rather dry and red; difficulty in swallowing, in consequence of the state of the throat.

I considered him labouring under syphilitic sore-throat, although he stoutly denied it. I commenced with iodide of potash and mercury pill; but after four or five days, finding that the ulcers were spreading, I made further inquiry, and found he had been some time a crystallizer of bichromate of potash under Mr. Wm. Dentith, of Collyhurst. He told me it was a complaint to which the men were all more or less subject. His father, employed in the same occupation, died some months ago with ulcerated sore-throat, aged fifty-five, though before he commenced as a crystallizer he had never been ill.

8th.—Having failed to produce any effect by the above treatment, and wishing much to get the man under the influence of mercury, I began to give him one-sixteenth of a grain of bichloride of mercury every four hours, at the same time sponging the throat with a lotion consisting of two grains of nitrate of silver to one ounce of water. This treatment, I am happy to state, was remarkably successful. The patient was convalescent by the middle of September." Mr. Heathcote adds, that several milder cases of the same nature have recovered rapidly under similar treatment.—*Lancet*, Feb. 11.

The Poisonous Effects of Chromium.—Mr. Herapath was requested by a farmer to examine the water drunk by his cows, which had been attacked with great thirst, attempts to be sick (indicated by restless motions of the mouth and frothing of the lips), diarrhœa, scouring of black offensive motions, diminution of the secretion of milk, abortion, partial atrophy. The best marked symptoms of the action of chromium were, great lassitude and partial paralysis of the hinder extremities, and the eyes and mucous surfaces being covered with "matter." Upon examining the stream at which the cows watered, the water was found to contain 2·92 grains of chromic acid per gallon, derived from neighbouring soap-works.—*The Chemist*, Jan. 1854.

Poisoning attributed to Vapour of Cyanide of Potassium.—The bodies of four individuals were found dead in a cottage in the village of Elscar, in Yorkshire. On the inquest, it was stated that the cottage abutted on the foundation of a smelting-furnace, and it had been discovered that there were cracks in the wall of the furnace. Death was attributed to the vapour of cyanogen having escaped, and having been inspired by the inmates after they had retired to bed. The symptoms and circumstances of their death could not be known, as all four had evidently been several hours dead when the catastrophe was discovered. According to the report of the inquest in the daily papers of December 6th, there does not appear to have been any scientific investigation to ascertain whether cyanogen, or carbonic acid, gas had escaped through the fissure in the wall of the furnace.

Recovery after taking a large Dose of Prussic Acid.—Mr. W. H. BURMAN, of Wath-upon-Dearne, has communicated a very interesting history of the recovery of his father from accidental poisoning by prussic acid. We regret that our limits compel us to confine our notice of this instructive case to a short abstract. Mr. Burman, sen., took by mistake a drachm of Scheele's acid instead of diluted acid. In a few seconds, he perceived by the bottle the mistake he had made; he immediately swallowed half an ounce of aromatic spirit of ammonia, with a little water; and then called to his son, and told him what had occurred: he spoke hurriedly, breathed deeply. Mr. W. H. Burman immediately administered some solution of crystals of sulphate of iron, trusting to the ammonia swallowed previously for the formation of an insoluble compound of the acid with the oxides of iron. This was two minutes after the poison had been swallowed; from this time, for twenty minutes, Mr. Burman had no recollection of anything that was taking place. Respiration became deeper and slower. Four minutes after taking the poison, cold douche was freely employed, and more solution of sulphate of iron with spirits of ammonia administered. Vomiting took place; a slight convulsive shudder occurred; the cold effusion was persevered in, with the occasional administration of spirits of ammonia. In twenty minutes he began to exhibit signs of returning consciousness. In about fifteen minutes later, he was able to walk upstairs to bed. Perfect recovery took place. The patient was about sixty years of age, and of a strong constitution.

By chemical analysis, Mr. W. H. Burman found that the quantity of the acid which his father had taken contained 2·4 grains of anhydrous acid. Mr. Burman observes, that this is the largest recorded quantity taken and followed by recovery. It is also a matter of interest in this case, that the time at which insensibility came on is so exactly known,—viz., two minutes after the poison was swallowed.—*Lancet*, Jan. 14.

Experimental Investigation of the Poisonous Quality of the Oil of Bitter Almonds when freed from Hydrocyanic Acid.—A servant-girl having been poisoned by half a drachm of essential oil of almonds, taken by mistake, Dr. Douglas Maclagan has been led to investigate the poisonous quality of the simple hydruret of benzule, which constitutes, with more or less of hydrocyanic acid, the commercial essential oil of almonds. The following are the conclusions:

"1. The marked difference between rectified and unrectified oil of bitter almonds shows that the poisonous character of the latter is essentially due to the hydrocyanic acid which it contains. 2. That the oil, really free from hydrocyanic acid, in doses of a few drops, does not act as a poison on animals generally; and that the instances of fatal effects on man and animals of such doses of the unrectified oil, must be referred entirely to the hydrocyanic acid. 3. That experiments on rabbits with quantities of half a drachm and under, invariably show that, if quite free from prussic acid, such doses do not cause fatal effects. That in larger doses (a drachm and upwards) it does, even when quite free from hydrocyanic acid, prove fatal to rabbits, but with great variation as regards the rapidity of the death, which variation is due to the physiological peculiarities of these animals. 4. That on dogs, whose organization renders them much better subjects for testing the probable effects of the substance on man, doses even so large as three drachms of the oil, entirely or nearly free from prussic acid, produce no other effect than a little vomiting, and do not cause death, or even dangerous symptoms. 5. That experiment shows, that if this substance is to be called a poison at all, it must be regarded as one of no great activity; but that, in reality, it cannot, even on the ground of its effect on rabbits, be styled a poison, without including under this denomination many other substances, such as oil of cloves, which cannot be regarded as poisonous, in the common-sense acceptation of the term. 6. That the use of the purified oil to make flavouring condiments is open to no objection which would not apply to ordinary aromatic volatile oils; and that the spirituous solutions sold for this purpose, if made of properly purified oil, are not dangerous. 7. That since, by due care, the oil can be so entirely freed from hydrocyanic acid as to deprive it of active poisonous properties, great culpability will attach to the sale of preparations made with unrectified oil."

The author has given a short notice of the opinions of different toxicologists on the properties of this oil, with the details of his own experiments; forming a very valuable contribution to toxological science.—*Edinburgh Monthly Journal of Medical Science*, January.

Death following on the Inhalation of Chloroform.—The following case is abridged from the narration by JOHN HARRISON, Esq., Senior Surgeon to the Bristol Royal Infirmary:

Jane Morgan, æt. 59, moderately stout, pale, but otherwise not of an unhealthy appearance, was placed under the influence of chloroform previously to attempts at the reduction of a supposed dislocation of the humerus forwards, the result of a fall eight weeks previously. Nothing in the general condition of the patient, or in the stethoscopic indications of the state of the thoracic organs, appeared to forbid the administration of chloroform. It was afterwards learnt that the patient had been subject to syncope. The anæsthetic was breathed from a cupped sponge, care being taken to ensure a due passage of atmospheric air. No food having been taken since breakfast, the patient commenced the inhalation of one drachm of chloroform at 2 P.M., in bed, under the superintendence of the house-surgeon. Nothing unusual occurred during this inhalation. In about five minutes, the inhalation of a second drachm was commenced. The chloroform was almost immediately withdrawn, as the patient's breathing became stertorous; immediately afterwards, the pulse, which had continued pretty firm, was suddenly imperceptible, the respiration ceasing at the same time. The pupils were not dilated. Cold water, cold air, galvanism, artificial respiration through an opening in the trachea, were all promptly had recourse to without avail, a few convulsive movements of the muscles of respiration having been their only results. The post-mortem examination revealed extreme congestion of the lungs; the cavities of the heart were nearly empty; the blood, fluid. The coronary arteries were dilated and studded with atheromatous and bony deposits. The muscular structure of the heart was pale, and in the right ventricle seemed, under microscopic examination, to have undergone incipient fatty degeneration. Other organs presented nothing abnormal.

It was found that a fracture of the humerus through the greater tuberosity had existed. The head of the bone lay in contact with the coracoid process, and to its inner side.—*Association Journal*, Feb. 4.

Death, Forty Hours after the Administration of Chloroform.—The coroner for Hull, æt. 49, had several polypi removed from the right nostril by Dr. KELBURNE KING. The patient had pertinaciously insisted upon the administration of chloroform, in opposition to the advice of Dr. King, who had urged that the operation did not involve sufficient pain to require the use of an anæsthetic agent. During the administration of the chloroform, which was given to the amount of six drachms, on a handkerchief, the patient became so violent that an additional assistant was required. The state of insensibility having been obtained, was continued during eight minutes. Consciousness was completely restored after the operation, upon the freedom from pain &c. during which, the patient freely conversed. In the course of the same day, he experienced unpleasant sensations in his head. He passed a restless night. The next morning his pulse was 100, and rather full, and he seemed drowsy; in the afternoon he became comatose, and, in spite of all means, died about forty hours after the administration of the chloroform.

On making an examination of the body, about twelve hours after death, the principal morbid appearances were extreme sanguineous congestion of the brain, with some serous effusion; the surface of the greater part of the summit of both cerebral lobes was covered by a thin layer of pus adherent to the visceral layer of the arachnoid.

In the remarks which Dr. King has appended to the details of the above case, the author reviews the latest opinions on the causes of death from chloroform, and suggests that it is possible that asphyxia or syncope may occur, even after an apparent entire cessation of the effects of anæsthetics. The present case, Dr. King is of opinion, is not one of death from chloroform, but from obscure meningeal inflammation, having some connexion with the existence of the polypi.—*Edinburgh Medical and Surgical Journal*, Jan. 1854.

Sulphuric Acid and Per-oxide of Manganese as a Test for Morphia.—A suspicion exists that opium is contained in Manilla cheroots. In order to ascertain the truth of the suspicion, the "Lancet Analytical Sanitary Commission" has instituted a very careful analysis of twelve samples, with the result that none of these contained opium. It is further stated, that tobacco contains a principle, probably nicotine, which gives nearly the same reaction with nitric acid as morphia. The action of concentrated sulphuric acid and per-oxide of manganese on morphia produces a characteristic *violet* tint, by which so little as a quarter of a grain of opium in a single cheroot may be detected.—*Lancet*, Dec. 10.

Poisoning by Darnel Seeds.—The *Lolium temulentum*, or darnel grass, when taken in sufficiently large doses, exerts a local action upon the alimentary canal, with remote action on the brain and nervous system. There is heat, pain in the stomach, nausea, vomiting, diarrhœa, followed by languor, loss of vision, ringing in the ears, and vertigo. There is, however, no recorded instance of their having proved fatal to man. (Taylor on Poisons, p. 745.) These effects have usually arisen from the accidental mixture of the seed with food grain. The following newspaper paragraph records an instance of this kind:

"The town and neighbourhood of Roscrea was thrown into the greatest consternation on Christmas-day, during divine service, by intelligence having been communicated to the police that several families had been poisoned by having eaten whole-meal bread at breakfast. Medical assistance was immediately rendered the unfortunate sufferers, when it was ascertained that they laboured, to an intense degree, under the violent symptoms produced by the seeds (flour) of the bearded darnel, rye-grass, commonly called ryley, which was mixed with the whole-meal. Over thirty persons received medical aid, and presented symptoms as if from intoxication. Remedies having been applied, on the following morning all the sufferers were found convalescent, but much debilitated."—*Leinster Express.*

Poisoning from Œnanthe Crocata.—Dr. BIRKBECK NEVINS, of Liverpool, related the following cases at a meeting of the Liverpool Medical and Pathological Society:—On the 9th of September, four children ate some roots of the œnanthe crocata (hemlock water dropwort) at 2 P.M.; and about half-past 6 P.M. they were taken to the Royal Infirmary, and admitted under the care of Dr. TURNBULL. At that time one of them, a boy, at the age of casting his milk-*teeth*, was perfectly insensible; the face was livid and turgid. He had previously vomited blood, and bloody mucus oozed from his mouth on admission. The prominent symptom was *violent convulsions solely affecting the flexors throughout the body.* The trunk was powerfully bent forward; the hands were firmly clenched, even after death; and the jaws were so rigidly closed, that some of the front teeth were broken in opening the mouth to introduce a stomach-pump. The spasmodic contraction sometimes gave way for a few minutes at a time, but the extensors were never affected. The respiration was spasmodic till death. The pupils were at first contracted, but afterwards dilated; they acted very feebly under the stimulus of light. The pulse was almost imperceptible. There were no involuntary discharges. This state continued until death, which occurred twelve hours after taking the poison. There was no return of consciousness, and the spasmodic contraction continued, with slight intermissions, as long as he lived. There was no vomiting after his admission, and the stomach-pump removed nothing but a portion of a blackberry.

The quantity eaten in this case was about the size of a man's thumb.

The treatment consisted in the application of cold to the head, and the administration of diffusible stimuli internally. Under this treatment the other children recovered. One of these was insensible and convulsed, and appeared as ill as the former. A third child had only abdominal pain, and no cerebral symptoms. The quantity eaten was unknown in the latter instances.

Dr. Nevins quotes from Woodville's 'Medical Botany' several cases of poisoning with this root, in which vomiting is specially noted as having been absent. The vomiting in the above case is explained by the boy having also swallowed several dulcamara berries, which caused vomiting.—*Association Journal*, Dec. 2.

Case of Poisoning by Ruta Graveolens.—Dr. Cooper, of Savannah, Georgia, U.S., relates the case of a negro, thirty-five years of age, who, recovering from dysentery, being dissatisfied with his rate of progress, procured a handful of rue, and after bruising it, swallowed it with about half an ounce of brandy. When seen by Dr. Cooper he found that violent symptoms had been developed in the course of an hour; he was suffering from nausea, vomiting, violent pains in and distension of the abdomen, tenesmus, frequent thin bloody dejections; the stools were so incessant that it was hardly possible to use sufficient despatch in removing and changing the vessels; troublesome irritation of the neck of the bladder, giving rise to painful efforts at micturition; the skin was of the natural temperature, and perspirable; thirst not urgent; tongue clear; pulse 90 to 95, very irregular; countenance anxious. Venesection, opium, mercurials, sinapisms—afterwards, nitrate of silver with opium—were administered. These means appeared to control the symptoms, the patient nevertheless died five days afterwards.—*Dublin Medical Press*, Jan. 11, 1854.

Death after an Enema of Tobacco.—J——, suffering from excruciating pains in the uterine and lumbar region, which were considered to be of a nervous character, not having experienced relief from the means employed, a lavement consisting of a decoction of thirty grains of tobacco was prescribed by Dr. B——. The administration of this remedy, instead of removing the symptoms, caused them to be greatly aggravated, with the addition of purging of blood;—at the end of thirty-six hours the patient died. No post-mortem examination took place. The woman was about eight months pregnant. The question was officially put, whether the enema had caused death. M. Tardieu answered that tobacco has frequently been administered in large, or larger, doses, without harm, under similar circumstances; and therefore that, in the absence of a post-mortem examination, it was not possible to affirm the cause of death.—*Annales d'Hygiène, &c.*

Chemical Analysis of Six Powders, alleged to be Homœopathic Medicines.—Dr. Niemann, of Magdeburg, was officially called upon to analyze a packet of powders said to have been prepared according to the homœopathic doctrine—infinitesimal doses. It was found that they consisted of thirty grains of sugar of milk, and three grains of hydrochlorate of quinine, very carefully mixed, and divided in unequal doses. Each powder should therefore have contained, if accurately divided, five grains of sugar of milk, and half a grain of the salt of quinine. The first powder, however, weighed $5\frac{47}{64}$ grains; the second, $5\frac{2}{64}$ grains; the third, $5\frac{62}{64}$ grains: these three collectively weighed $16\frac{47}{64}$ grains, which, for six powders, would have been equal to $33\frac{30}{64}$ grains. Further, in the second powder, there was of muriate of quinine $\frac{120}{320}$ of a grain; in the third, $\frac{210}{320}$ of a grain. In six powders this would have been equal to $3\frac{3}{32}$ grains.—*Casper's Vierteljahrsschrift*, January.

V. Miscellaneous.

The Kirwan Case.—Our readers may remember that at the beginning of the year (1853) this celebrated case occupied much of the public attention. The principal facts were briefly these:—Kirwan's wife was found dead on a small island in the Bay of Dublin. An inquest was held on the body, and a verdict returned, "found drowned." A rumour having been raised that the deceased had been murdered by her husband, the body was exhumed, and examined by Dr. Hatchell. The opinion of that gentleman was, that death had been caused by a combination of strangulation and drowning. The accused was found guilty of the murder. A very strong impression, nevertheless, remained on the minds of many persons that Kirwan had been wrongly convicted,—that his previous character for conjugal infidelity was his ruin. The medical facts of the case were therefore submitted to Dr. Alfred Taylor, who concluded an elaborate report in the following words:—"I assert, as my opinion, from a full and unbiassed examination of the medical evidence in this case, that so far as the appearances of the body are concerned, there is an ENTIRE ABSENCE OF PROOF that death was the result of violence at the hand of another." Dr. Taylor's conclusion was that deceased had died from syncope, epilepsy, or apoplexy, while bathing. The result of emphatic remonstrances from many quarters, medical and legal, was that the sentence of death has been since commuted to that of transportation. Under this strange proceeding, Kirwan is detained as a convict in Spike Island. It is clear that he either murdered his wife, or is innocent of the charge. In the one case the law decrees death—in the other he is surely entitled to his liberty. That death was accidental, we consider amply proved by Dr. Taylor and others. For a complete examination of all the facts of the case, we refer to a pamphlet by J. Knight Boswell, Esq., of Dublin; also to a pamphlet entitled 'The Kirwan Case, illustrating the danger of Conviction on Circumstantial Evidence.'

Notwithstanding all the weight of medical and legal reasoning by which the innocence of Kirwan has been established, Dr. Field ('Some Remarks on the Medical Evidence given at the Trial of W. B. Kirwan,' by John Field, M.D., with a Commentary by F. C. Skey, Esq., F.R.S. &c.) supposes that the convict is the murderer, and that he committed the crime by thrusting a sword up his wife's vagina. This supposition is based on the observation by Dr. Geoghegan, that the os uteri presented "an ulcer covered with yellow pus, and extending into the cervix." A grave charge, to be grounded on such indefinite premises as the alleged existence of an ulcer of the os uteri in a body that had lain two months in a wet grave, and the supposed existence of a sword-stick which is not forthcoming! The 'Commentary,' however, of Mr. Skey, suggests sufficient improbabilities to deprive the supposition of all its points. Among other doubts, Mr. Skey very forcibly suggests that an ordinary stick-sword could hardly have been introduced in the manner surmised by Dr. Field, without the victim becoming conscious of the object before it was too late to prevent the perpetration of the crime. Dr. Field is compelled to make the unaccountable assumption that Mrs. Kirwan *voluntarily* (!) placed herself in the recumbent posture on her back, before her husband attempted to insert the sword through the organs of generation. This supposition is so entirely gratuitous, and so utterly unlikely to have been the case, under the circumstances, that it forms one of the weakest points of Dr. Field's feeble hypothesis. Still more stubborn obstacles exist, however, to the admission of the view advanced by Dr. Field. Dr. Hatchell, the principal medical witness, stated at the trial,—"*Having been told that instruments had been run up the body, I examined to see if there were traces of such having been done,* BUT I DID NOT FIND ANY WHATEVER." This should appear conclusive of the question. Has Dr. Field overlooked this evidence of Dr. Hatchell? A still more serious obstacle, however, is encountered in the fact that Pat Nangles, the boatman, the most active witness against Kirwan, only deposed to Kirwan's having had with him a stick *like a sword-stick.* This supposed sword-stick, Dr. Field informs us, on the authority of this same veracious Pat Nangles, long after the trial, "Kirwan had with him when he embarked to return to Howth, but he had it not on his arrival at Howth. *He must, therefore, have sunk it secretly during the passage.*" This would have been superfluous, as he might have thrown it into the sea before the arrival of the boat to convey him to Howth, according to his appointment.

An objection, however, altogether fatal to the whole of Dr. Field's explanation of the cause of death, exists in the simple facts, that *Kirwan never had a sword-stick at all with him, but a common walkingstick; and that so far from this stick having been secretly consigned to the depths of the sea, it was safely in the hands of the police at the time of the trial.* (See Mr. Boswell's defence.)

Dr. Field has evidently been misled by Pat Nangles, whose veracity and honesty stand so much higher in the doctor's estimation than in that of his neighbours. A more careful examination of all the facts would have saved Dr. Field from having put forward a totally unfounded hypothesis.

Mistaken personal identity.—Dr. Kinlock, of Drumoak, Aberdeenshire, relates a case of mistaken identity under extraordinary circumstances. The body of a man, between sixty and seventy years of age, was found slightly imbedded in the sand on the bank of a river; both eyes had been picked out by hooded crows, but decomposition had made no progress. The left ear, and the first finger of the left hand, were wanting, having the appearance of having been lost in early life. The body was conveyed to a suitable place, and persons were requested by advertisements to come and identify it. After some time two young women claimed it as the body of their father, who, they stated, was a sawyer, that he was in the habit of leaving home for two or three weeks at a time without informing them where he went, and that he had lost the left ear and first finger of his left hand. They apparently recognised the clothes and the body, and gave vent to expressions of grief on the event. Subsequent doubts in the mind of one sister were overruled by the confident affirmations of the other. The funeral took place accordingly, and was attended by the daughters and friends of the supposed deceased sawyer. Returning from the funeral, the boatman of the ferry which they had to cross asked them for whom they were in mourning, and upon receiving their answer, laughingly informed them that he had, only half an hour before, ferried their father over alive and well, and directed them where they would find him—which, to their great joy, proved to be true. Whose was the body they had buried in the churchyard at Drumoak has not been discovered.—*Edinburgh Monthly Journal*, Feb.

THERAPEUTICAL RECORD.

Aneurism.—See Perchloride of Iron.

Angina Pectoris.—M. Carrière (Bull. de Thérap., i. p. 7) has recommended the inhalation of chloroform, at the commencement of the paroxysm. Duchenne (Ibid.), in addition to this measure, has employed with advantage the "electro-cutaneous excitation" in the mammary region.

Cataract.—M. Lopez (Bull. Gén. de Thérap., 1854, ii. p. 89) has employed with advantage iodide of potassium taken internally, and vesication on the temples, in cataract. The treatment was persevered in for five or six months, and in 3 cases out of 4 was productive of great benefit.

Chloroform.—Dr. Hardy (Dublin Journal, Nov.) relates cases to show the efficacy of chloroform vapour directed upon the part in uterine affections. The vapour is applied by means of an instrument consisting of a metallic chamber, to one end of which a pipe with a valve is attached to the gum-elastic bottle. A sponge dipped in chloroform is placed in the metallic chamber, and then by pressing on the elastic bag, the vapour is expelled through the pipe. In cases of carcinoma and simple ulceration of the os uteri, this plan appears to be very efficacious; but it is useful also in pruritus pudendi, in sore nipples, and in other painful affections of the skin.

M. Nelaton (Gaz. des Hôp., and Med. Times and Gaz., March 4) relates a case in which the vapour of chloroform directed (by Hardy's apparatus) upon an abscess in the axilla, produced complete insensibility, so that the entrance of the knife was unperceived.

Diabetes Mellitus.—Dr. Basham (Lancet, Jan.) relates two cases treated by the permanganate of potash, in doses of 10 grains three times daily; the amount of urine was very slightly diminished; the sugar was augmented; the other symptoms were unaffected. Dr. Basham has employed alkaline treatment with advantage; the sulphite of soda was found to be useless. In one case opium was carefully administered, to the amount of 3 grains daily, but no striking effect was produced.

Fever, Continued.—Dr. Brinton recommends at the commencement of fever, an emetic of ipecacuanha (f℥j of the tincture); afterwards a stimulant plan is adopted, consisting of the administration of *small quantities* of brandy with water, beef tea, &c. In great abdominal pain and tympanitis, turpentine stupes and enemas are used. (For the enema the following prescription is given:—Spirits of turpentine, ♏xxx; tincture of catechu, ʒij; tincture of opium, ♏xv; decoction of starch, ℥ij.) The rate of mortality was 10·4 per cent.

Fever, Intermittent.—Dr. Harting (Schmidt's Jahrb., 1853, ix.) has employed quinoidine with alcohol and sulphuric ether in ague, and, from twelve years' experience, states that it is superior to common quinine He considers the quinoidine to be an amorphous quinine, (an opinion which has been strongly opposed by Mülder.)

Dr. Castiglioni (Schmidt's Jahrb., 1853, ix.) has used the tannate of cinchona; it requires to be given in larger doses than quinine, but is much less expensive.

Fever, Typhoid.—M. Vrancken has recommended in typhoid fever, ablutions with vinegar and water. M. Van Dromme (Rev. Med. Chir., Jan. 1854) has employed this treatment largely, and with great success. He uses 1 part of vinegar to 3 of water, applied with a sponge over the whole body night and morning. The diet is very low; pure or slightly acidulated water is permitted to be drunk *ad libitum.* Of 20 cases treated in this manner, 1 died. M. Vancken also employed the acetate of ammonia internally; but this is regarded as useless by M. Van Dromme. Chomel has also recommended the vinegar ablutions.

M. Mazade (Rev. Med. Chir., Fev., p. 95, et Rev. du Midi) has employed the sulphate of quinine, in doses of $15\frac{1}{2}$ grains daily, in 71 cases of typhoid fever, and comes to the following conclusions:—That this remedy is eminently useful when the fever assumes a remittent form; that it is also useful, but less so, when there are less regular remissions; that it is seldom useful, and often hurtful, in typhoid fever of continued type. M. Dupré (same journal) thinks that quinine is useful in continued typhoid fever, when of very adynamic type.

Gonorrhœa.—Dr. Boinet (L'Un. Méd., Sept.) speaks highly of the effect of tincture of iodine when applied to the mucous membrane of the vagina in the gonorrhœa of women; a *single* application was sometimes sufficient. At the same time a solution of equal parts of tincture of iodine and water was injected into the urethra, but was not allowed to penetrate into the bladder. Dr. Boinet has employed the local application of iodine in inflammations and ulcerations of other mucous membranes, and with great success

Gout.—Dr. Goolden (Med. Times & Gaz., Nov.) uses with good effect, as a local application, spirits of wine. The relief to the pain is said to be sometimes very great. In the same journal the utility of an old remedy, the carbonate of soda, as a local application in gout and rheumatism, is referred to. A drachm of the carbonate is mixed with a hot bread poultice, and applied over the joint.

Lead Poisoning.—Dr. Goolden (Lancet, Dec.) records a case of lead-palsy successfully treated by the iodide of potassium and by galvanism. After the commencement of the treatment the urine was examined for lead by Dr. Gladstone, who believes that lead was present, though the examination was not perfectly decisive.

Dr. J. R. Nicholson (Lancet, Jan.) relates a case of lead-colic and palsy successfully treated by iodide of potassium and galvanism. The urine was examined for lead before treatment, without any being detected; during treatment, however, the lead passed off abundantly in the urine.

Menorrhagia.—In cases of abundant menstrual flow without physical uterine lesion, Dr. Tanner speaks highly of the effect of the tincture of cinnamon, in drachm doses in cinnamon water every six hours.

Neuralgia.—Periodic neuralgia of the face and head have lately been common in Paris, and have occasionally withstood the action of quinine. M. Aran (Bull. Gén. de Thér., 1854, ii. p. 84) has employed in such cases the aconite in large doses. The preparation employed was the extract, of the French Codex.

Perchloride of Iron.—As it is possible that some of our readers may be disposed to experiment with this substance, in the treatment of vascular tumours, we may remind them that the strength of the solutions used in France is regulated according to the degrees of Baumé's hydrometer. Thus, a solution is said to be 45° or 30°, and so on. Now, a solution of 45° (Baumé), 55° Fahrenheit, is of the specific gravity 1·455; one of 30°=sp. gr. 1·26; one of 20°=sp. gr. 1·16; and one of 15°=sp. gr. 1·114. It has been shown, by M. Burin du Buisson, (Bull. Gen. de Thér. t. xlvi. p. 73), that to obtain a solution of 15°, it is not sufficient to add two parts of water to one of a solution at 45°, but it requires more than two and a half parts of water. He finds that 100 parts of the solution at 45° (sp. gr. 1·455), contain 43 parts of perchloride of iron, and 57 of water. Moreover, he states that a careful estimate of the strength of the several solutions, gives this general result—5 parts of the solution at 45° equals 10 parts at 30°, 15 parts at 20°, and 20 parts at 15°—so that any given quantity of the solution at 45° may be easily converted into either of the other strengths.

In the absence of our Surgical Report in this number, we may mention that some better success has attended the use of the perchloride of iron in the treatment of aneurism, since the introduction of the question, in November last, at the Academy of Medicine in Paris. It was then stated by M. Malgaigne, that out of 11 cases, 4 had proved fatal, and 5 or 6 had failed. A successful case, however, was then mentioned, in which M. Valette had cured a false aneurism of the brachial artery by injecting a larger quantity of solution at a higher strength than advised by M. Pravaz. He used fifteen drops at a strength of 30° Baumé, and was careful to compress the artery, for some time, above and below the point of injection. The patient has since died of some other disease. The artery was obliterated: and the sac of the aneurism filled with firm clot, containing particles of chloride of iron.

Experiments in the zygomatic, external plantar, and carotid arteries of the horse, have been made by MM. Debout and Leblanc, with success, and without any bad consequences. They employed, at last, a solution at 15°, and insist on the necessity for temporary compression of the vessel. The perchloride solution has also been injected into varicose veins by M. Follin, and into a *venous* tumour (query erectile) by M. Giraldis, both with success. It has also been employed, by M. Thierry, externally upon varicose veins, and varicose ulcers, as well as in a case of lupus of the face, and in another of nævus. In all cases the results were good. Where the epidermis is entire, a blister is first applied. Finally, the solution has been applied to internal hæmorrhoids; and, with complete success, by M. Miergues, in a case of fistula in and communicating with the bowel. We shall await further trials with this agent, and consider the subject again hereafter.

Phosphate of Lime.—M. Mouriè's (Compte-rendu de l'Acad. Jan.) states that in the French towns the food is deficient in phosphate of lime. He affirms this after examination of the food and of the urine of women; the ingestion and egestion of this substance are diminished by a fourth. He also affirms that in the milk of nursing women, fed on this diet, there is a great deficiency of phosphate of lime, and consequently the children born in the great cities are insufficiently nourished. He recommends, therefore, the administration of phosphate of lime, and in nursing women he has given it with benefit to the amount of ʒiii per diem. [M. Mourié's conclusions appear to be based on an insufficient number of observations, but they accord with the researches of Beneke, (Zur Phys. and Path. des Phosphorsauren Kalk, Gött. 1850), who has also rendered extremely probable the relation of phosphate of lime to the formation of cells.]

Pityriasis Capitis—Dr. May (Lancet, Sept.) recommends a lotion of biborate of soda (℥ss.), camphor (ʒij.), and water (℥xxxij.). Twice a week the scalp is gently wiped with a soft flannel saturated with the solution.

M. Duplex (Lancet, Oct.) advises the nitrate of mercury ointment, mixed with a little olive oil, to make it more manageable.

Mr. O'Connor (Lancet, Oct.), who has tried the biborate of soda without effect, recommends washing the head in cold water, and the administration, by the mouth, of the sesquicarbonate of soda in some bitter infusion.

Mr. Wingar (Lancet, Oct.) speaks highly of the following lotion:—fresh sulphuret of potassium (ʒj), water (℥iij); to be used daily.

Phthisis.—M. Piorry (Bull. Gén. de Thér. 1854, ii. p. 86) has employed, in 31 cases of phthisis, inhalations of iodine. About thirty grains of iodine are placed in a large jar with a wide opening, and the patient places the mouth over the opening, and inhales deeply one or two hundred times in the twenty-four hours, or even more often than this. At other times a larger quantity of iodine is exposed in the room, so as to impregnate the atmosphere. M. Piorry declares that not only are the cough, expectoration, &c., improved, but that the signs on percussion (of which method M. Piorry is, as is well known, an ardent votary) also improve. The editor of the 'Bulletin de Thér.' expresses doubts of the validity of these conclusions.

Pneumonia.—In old decrepit persons, in tuberculous subjects, in those who are suffering at the time from diarrhœa, Dr. Fieling (Schmidt's Jahrb. 1853-4, ix) recommends acetate of lead (three to six grains) with digitalis and opium.

Heusinger (Deutsche Klinik, 1853, xxiv.) has employed digitalis at the very commencement, or after one or two doses of tartar emetic. In twenty-four to thirty hours the physiological effects of the digitalis appear,—viz., weakness, purging, vomiting, cold moist skin, slowness and intermission of the pulse, &c. When these symptoms appear, the pneumonic process is arrested, and in a few hours resolution commences. The digitalis is given as infusion, in moderate doses, every hour or every two hours.

M. Aran (L'Union Médicale, September, et Bull. de Thérap.) has employed *veratrine* in inflammatory affections, in typhoid fever, in acute rheumatism, and especially in pneumonia. M. Aran had employed veratrine in rheumatism after the manner of Piedagnel and Trousseau (vide Therap. Record, July, 1853, p. 282), and being struck by the enormous decrease in the rapidity of the pulse (from 112 to 64), determined to employ the medicine in pneumonia. Six cases are related. One died; the rest were rapidly cured. After 5, 10, or 15 milligrammes ($\frac{1}{14}$, $\frac{1}{7}$, and $\frac{2}{7}$ of a grain) of veratrine given in divided doses, the general symptoms began to abate. The effect on the pulse was extremely marked; it fell in an extraordinary way. The number of respirations diminished; the action on the nervous system was marked by extreme debility, prostration, pallor of the face, &c. M. Aran, however, does not pronounce exclusively in favour of this treatment, but would employ it in obstinate cases. He gives five milligrammes ($\frac{1}{14}$ of a grain) every four or six hours the first day, and in gradually lessening quantity the three succeeding days.

Priapism.—Dr. Debont (Gaz. des Hôp., 1853, p. 61) calls attention to the efficacy of the tincture of hops, in priapism. The effect seems heightened by combining sugar with it.

Ptyalism.—Dr. Erpenbeck (Schmidt's Jahrb., 1853, p. ix.) relates a case of severe mercurial salivation arrested by the internal use of belladonna. The salivation returned when the remedy was discontinued, and was again checked by it.

Sciatica.—Mr. Hancock believes that most cases of sciatica are caused by pressure on the nerve within the pelvis, either by accumulation in the cæcum and colon, or by tumours. He recommends croton oil internally to remove fæcal accumulations, in doses of half a drop combined with blue pill. Quinine is to be given after the croton oil has fully operated.

Spermatorrhœa.—Dr. Laroche (Bull. Gén. de Thér., 1854, II. p. 76) records a case of spermatorrhœa treated with success by digitalis, as recommended by Corvisart. The dose is one or two grains of the powder of the plant, gradually elevated to eight grains.

Spleen, Tumour of.—In a case of splenic tumour, Dr. Gurtrac (L'Un. Méd., 1853, No. 69) has employed the sulphate of manganese with good effect, in doses of one to one and a half grains twice a day, in the form of pill.

Strabismus.—Du Bois Reymond (Müller's Archiv. and Bull. Gén. de Thér. 1854, II. p. 95) recommends the *stereoscope* for the cure of squinting. The principle is the same as in the case of the prismatic spectacles, advised by Mr. Spencer Wells.

Syphilis.—Dr. Heyfelder (Medical Times & Gazette, Oct.) has employed the bichromate of potash in secondary syphilis, with good effect. The dose is two-thirds of a grain daily for a few days, and then the quantity is gradually increased to 3 and 4 grains, until altogether about 130 grains have been taken.

Tape-worm.—Dr. Mackinnon (Indian Annals of Medical Science, No. I., p. 284) recommends a remedy called in Northern India the "Kameyla," and used as a vermifuge for dogs. He has given it in 16 cases, and finds it more useful than turpentine or kousso. The dose is 2 or 3 drachms, which purges rather copiously. The "Kameyla" is a powder attached to the fruit-capsule of an Euphorbiaceous plant, the "Rothera Tinctoria;" it is brushed off and collected when the fruit is dry; its price is moderate.

Ulcers.—In callous ulcers, Mr. Hainworth has successfully adopted the plan of paring away the ring of hard condensed cuticle which surrounds the old ulcer. The operation is painless, for the cuticle is sliced off without the cutis being wounded.

Vomiting.—MM. Clertan and Galante (Bull. Gén. de Thér., ii. p. 91) have employed, in cases of obstinate chronic vomiting which had resisted all other treatment, capsules containing sulphuric ether. In a case of apparently hysterical vomiting, which is related as evidence, ether taken in the ordinary way did no good, but the disengagement of the ether in the stomach was at once successful.

BOOKS RECEIVED FOR REVIEW.

The Anatomy and Diseases of the Prostate Gland. By John Adams, Surgeon to the London Hospital, &c. Second Edition. London, 1853.

Handbook of Inorganic Analysis, &c. By F. Wöhler. Edited by A. W. Hoffmann, Ph.D., F.R.S. London, 1854.

Ueber ärztliche Bildung und Bildungsanstalten. Von Dr. J. M. Leupoldt, Ord. Professor der Medicin an der Universität zu Erlangen, &c. Frankfort, 1853.

Guy's Hospital Reports. Second Series. Edited by E. L. Birkett, M.D., and Alfred Poland, F.R.C.S. Vol. VIII. Part II.

Reminiscences of a Medical Life, with cases and practical illustrations. By Jonathan Toogood, F.R.C.S., Founder of, and late Surgeon to, the Bridgwater Infirmary. Taunton, 1853.

Medico-legal Observations upon Infantile Leucorrhœa, arising out of the alleged cases of felonious assaults on young children, recently tried in Dublin. By W. R. Wilde, F.R.C.S.

The Odontalgist. By J. Paterson Clarke, M.A., Dentist Extraordinary to H.R.H. Prince Albert. London, 1854.

Unsoundness of Mind, considered in relation to the question of responsibility for Criminal Acts. By Samuel Knaggs, M.R.C.S. Eng. London, 1854.

Registration of Births, Deaths, and Marriages in Massachusetts, for the year ending December 31st, 1851. By Amasa Walker. Boston, 1852.

The Nature of Cholera Investigated, with a supplementary Chapter on Treatment. By J. G. French, F.R.C.S. Second Edition. London, 1854.

The Retrospect of Medicine. Edited by W. Braithwaite. Vol. XXVII. July—Dec. 1853.

Memoire della Società Medico-Chirurgica di Bologna. Vol. V. Fasc. 3. Bologna, 1853.

Traité Elementaire des Maladies de la Peau. Par Maurice Chausit, M.D. Paris, 1853.

Practical Observations on the History, Nature, and Treatment of Cholera Asphyxia. By John Coghlan, M.D., M.R.C.S. Eng. Dublin, 1853.

Traité des Maladies du Sein, et de la Region Mammaire. Par A. Velpeau, Membre de l'Institut, &c. Paris, 1854.

Remarks on the Examination of Recruits, &c. By H. H. Massy, A.B., M.B., 4th Light Dragoons. London, 1854.

Half-Yearly Abstract of the Medical Sciences. Edited by W. H. Ranking, M.D., Cantab., and C. B. Radcliffe, M.D. Lond., L.R.C.P. Vol. XVIII. July—December, 1853.

De la Meteorologie dans ses Rapports avec la Science de l'Homme, et principalement avec la Medecine, et l'Hygiène publique. Par P. Foissac, M.D. Paris, 1854. Tomes 1 and 2.

The London and Provincial Medical Directory. 1854.

The Diseases of the Rectum. By Richard Quain, F.R.S., Professor of Clinical Surgery in University College. London, 1854.

Ueber die Cholera, und ihre Behandlung. Von Dr. C. Dworschak (aus dem Russischen). St. Petersburg, 1853.

Surgical Anatomy. By Joseph Maclise, F.R.C.S. Second Edition. Fasc. III.

The Common Sense of Cholera. By a Practical Practitioner. London, 1854.

Handbuch der Allgemeine Arzneimittellehre. Von Dr. J. F. H. Albers. Bonn, 1854.

The London University Calendar for 1854.

The Modern Treatment of Syphilitic Diseases. By Langston Parker, Surgeon to the Queen's Hospital, Birmingham. Fourth Edition, entirely re-written. London, 1854.

On Indigestion, and certain bilious disorders often conjoined with it. By G. C. Child, M.D., Consulting Physician to the Westminster General Dispensary. Second Edition. London, 1854.

Researches on the Conduct of the Human Uterus after Delivery. By Dr. R. Heschl. (Translated from the German) Dublin, 1853.

A Treatise on the Diseases, Injuries, and Malformations of the Rectum and Anus. By T. J. Ashton, Surgeon to the Blenheim Dispensary. London, 1854.

The Circle of the Sciences. Part I. London., Jan. 1854.

Comparative Anatomy. By C. Th. v. Siebold and H. Stannius. Translated from the German, with notes and additions, by Waldo J. Burnett, M.D. 1854: London and Boston. Vol. I.

A Letter to the Right Hon. W. E. Gladstone, M.P., Chancellor of the Exchequer, on the Formation of the Initiative Board in the University of Oxford. By H. W. Acland, M.D., F.R.S. Oxford and London, 1854.

The Elements of Natural Philosophy, &c. By Golding Bird, M.D., F.R.S., and Charles Brooke, M.A., M.B., F.R.S. Fourth Edition. London, 1854.

The Question considered: is it justifiable to administer Chloroform in Surgical Operations, after its having already proved suddenly fatal in upwards of fifty cases, when pain can be safely prevented, without loss of consciousness, by momentary benumbing cold. By James Arnott, M.D. London, 1854.

A Clinical Introduction to the Practice of Auscultation, &c. By H. M. Hughes, M.D., Assistant Physician to Guy's Hospital. London, 1854.

Die Vaccination und ihre neuesten Gegner. Von Dr. H. Haeser, Professor der Medicin au der Univ. zu Greisswald. Berlin, 1854.

Erster Versuch einer Allgemeinen und Vergleichenden Thier-chemie. Von J. E. Schlossberger. (Erst. Sief.) Stuttgardt, 1854.

The Laws of Epidemic and Contagious Diseases, &c. By James Bird, M.D., F.R.C.S., late Physician General Bombay Army. (Pamphlet.) London, 1854.

The Varieties of Pock Delineated and Described. By Walter C. Dendy, Surgeon to the Royal Infirmary for Children. London, 1853.

Handbuch der Speciellen Pathologie und Therapie. Redigirt von Rud. Virchow. (Ersten Bandes Zweite Hälfte.) Erlangen, 1854.

Practical Observations on Gout and its Complications, and on the treatment of Joints stiffened by Gouty Deposits. By Spencer Wells, F.R.C.S.E. London, 1854.

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