THE BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW

OR

QUARTERLY JOURNAL

OF

PRACTICAL MEDICINE AND SURGERY.

VOL. XI.

JANUARY—APRIL 1853.

LONDON:

SAMUEL HIGHLEY, 32, FLEET STREET;
AND
JOHN CHURCHILL, 46, PRINCES STREET, SOHO.

MDCCCLIII.

LONDON:
SAVILL AND EDWARDS, PRINTERS, CHANDOS STREET,
COVENT GARDEN.

NEW FEATURE

IN

THE BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

NOTICE.

SOME changes have been introduced into the current Number of the Review, which demand a few words of explanation.

The names of the reviewers are, in several instances, affixed to the reviews. It is hoped, by this plan, to make the criticisms even more valuable than formerly—since the reviewer will appeal more authoritatively to his own experience. A review is simply an inquiry into the validity of an author's statement by the aid of previously-admitted facts, or by the assistance of others known only to the reviewer as the result of his proper observations. To give these last their due weight, they must be authenticated. A writer who merely builds up his argument with well-known truths, or with the opinions of other men, may remain anonymous; but whenever he resorts to his own experience, he must guarantee it with his name. An unavowed statement is dead to science, and no one recognises a veiled authority.

By far the most valuable kind of criticism is that in which a reviewer starts from the same point as his author, and treading in his footsteps, explores afresh the same country, confirms what is true, and corrects what is inaccurate. We have seen this mode of reviewing carried in this country to as high a degree of perfection as the employment of the incognito can ever permit. It remains to be seen whether it may not be made still more authoritative, and therefore more valuable; whether, in fact, it may not be possible to permit no statement to pass into the currency of general belief until a second name has been affixed to it, as a guarantee that the evidence on which it is made has been tried, and not found wanting.

By the adoption of this plan, also, it will be possible to bring more frequently before the profession the matured views of experienced men in various departments of medicine. At present, such reviews lose much of their weight, because their authors are unknown, and they are less numerous than they might be, since no one wishes to put the labour of years into an article which carries with it no evidence of its source.

It is not intended, however, to lay aside altogether the incognito. In some cases the addition of the name may, perhaps, be unnecessary, in others inexpedient. Experience alone can show how far the interests of truth and science may demand publicity, or warrant secrecy.

In order to maintain the high character of the reviews, the aid of the most competent men will be sought; and no assistance that is worthy and honest will be disdained. This journal has never been, and will never be, the organ of a section or a party; it is a catholic work, in which all who love their science will be invited to join.

Some Original Communications are now, for the first time, introduced. A new and advantageous medium of publication is thus afforded, and the journal itself will gain in variety, interest, and utility. Occasionally, it is intended, also, to translate foreign papers of peculiar merit, of which an example appears in the present number.

The term "Chronicle" has been substituted for that of "Periscope," as expressing more fully the intention of this part of the Review, which is, to record as concisely, though as faithfully, as possible, the progress of Medical Science.

CONTENTS OF Nº XXI.

OF THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JANUARY, 1853.

Analytical and Critical Reviews.

§

Bibliographical Record.

Original Communications.

Chronicle of Medical Science.

CONTENTS OF No. XXII.

OF THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1853.

Analytical and Critical Reviews.

§

Bibliographical Record.

Original Communications.

Chronicle of Medical Science.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JANUARY, 1853.

PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *Handbuch der Pathologie und Therapie.* Von Dr. C. A. Wunderlich, &c. In drei Bänden. Zweiter Band. Erste Abtheilung. Die Anomalie der Constitution. Elfte Lieferung.—*Stuttgart*, 1852.
Manual of Pathology and Therapeutics. By Dr. C. A. Wunderlich. In Three Volumes. Vol. 2, part i. 'Constitutional Affections.'

2. *A Treatise on the Practice of Medicine.* By George B. Wood, M.D., Professor of the Theory and Practice of Medicine in the University of Pennsylvania; President of the College of Physicians of Philadelphia; one of the Physicians of the Pennsylvania Hospital; one of the Authors of the 'Dispensatory of the United States of America,' &c. &c. Third Edition. Two Volumes.—*Philadelphia*, 1852.

Treatises on the Practice of Medicine are inevitably voluminous, whatever may be the method adopted by the writers. The perfect freedom of opinion enjoyed by the profession renders it necessary to appeal to the judgment of the reader, and the statement of premises and grounds of opinion inevitably involves more or less of theory or of questionable observation. It is not surprising, then, that Dr. Wood's treatise is spread over seventeen hundred closely-printed pages, royal octavo, or that Wunderlich's Manual, to be published in three thick volumes, is only half completed, although the work has been three years in progress. If the reader will glance round his library, he will see that voluminousness is the characteristic of all the systematic writers, for the shelves groan beneath the weight of modern thick octavos, or less modern quartos, or older folios. A refuge from the evil has been sought in 'Libraries,' or 'Cyclopædias,' or 'Dictionaries,' but sought in vain, for these are more voluminous still. 'Conspectuses' and 'Manuals' have been tried, and have met with some success; but the generalities of a *conspectus* are too abstract for any than the most abstract mind, and the dry summaries of a manual too dogmatic and general for the practitioner, unless edited with masterly tact.

There is another difficulty the reader has to encounter, of no little importance. Having selected his standard works of reference, they quickly depreciate in value, because, with every year, they less and less represent the existing state of medical science and art. The science of life is too little elaborated to supply fixed general principles of pathology and therapeutics, and the practitioner must be content to gather from the medical periodicals, from discussions at medical societies, and from the perusal of systematic treatises, as they issue from the press, that knowledge which it is absolutely essential he should possess, if he would keep well forward in the race of life It is not the "portentosa morborum phalanx' which he has alone to encounter; the sharp rivalry and intellectual competition of his brethren is no slight difficulty in his way, and no unimportant stimulus (fortunately) to his efforts; not to mention those less important, but not less troublesome, annoyances and injuries inflicted upon him by empirics. The medical practitioner, then, must make up his mind to work.

It is not quite certain, however, whether all has yet been done which might be done in the elaboration of *fixed general principles* of pathology and therapeutics; or at least, whether the time has not now come for a more definite and direct attempt. It is not possible to remain indifferent to the vast and secure strides which medical science has made of late years. If some of our more recent systematic writers be compared with those popular a quarter of a century ago, and these latter be compared with the writers of the twenty-five years preceding them, it will be seen that medical knowledge has recently advanced in a very accelerated ratio. A more refined visual and chemical analysis has been so applied to physiology and medicine by the aid of the microscope and improved chemical science, as to constitute an entirely new instrument of research, and thereby obscure phenomena have been elucidated, and their hidden relations brought to light. Large general laws of nutrition and development are being propounded; the chemical and microscopic anatomy of the blood (if the phrase may be permitted) is being brought to bear on an improved system of physiology; morbid and morbific changes in that fluid are consulted more and more in relation with structural change and functional disorder; and above all, the foundations of the science of vital dynamics are being securely laid on a better knowledge of vital forces, from whence fixed principles in pathology and therapeutics will eventually arise. The medical profession may, most justly, congratulate itself upon the cheering prospects before it, and may reasonably hope that systematic writers will not be wanting to re-cast and combine the vast mass of materials which the labours of the past and present generation of medical inquirers have placed at their disposal. The time is favourable for the attempt. In the first place, the number of the self-styled "practical" men is rapidly decreasing and giving place to a class with a better knowledge of what is really and truly practical. This class more clearly perceives that the crudest theories are quite compatible with an arrogant disdain of the theoretical, and that, in fact, the assumption of the title of practical is only a stroke of policy to hide a profound ignorance of the literature and science of the profession. This largely increasing class is also aware that an assiduous cultivation of the reasoning, as well as of the observing, faculties is necessary to the formation of the sound practitioner, and that a course of study, limited to manuals and medical reading-made-easier, can never develop the intellect or enlarge the mind; while it will almost inevitably lead to the empiricism of which the homœopathic sect offers the most perfect type. But perhaps there is nothing more satisfactory in the whole course of recent events in the medical world, than the good which has resulted and will result from the evil of licensed empiricism. With unrivalled sagacity, not a few have acted on the principle "Fas est et ab hoste doceri," and have turned the doings of various empirics to practical account. The influence of the nervous system, and of the mind through the nervous system, has been better recognised; the experience of Currie and others as to the simpler treatment of fevers and febrile diseases, has had the attention it merited; and the importance of dietetics and hygiène, in the practice of medicine, more clearly perceived and acknowledged. The scepticism which, on the one hand, despises medical art, and the polypharmacy which, on the other, humiliates it, have alike received a check. It may be now more than ever truly said, in the words of a popular writer of the last century—"Ratum enim apud omnes manet NATURAM esse optimam morborum medicatricem: et frequentiores affectus ipsi commissos, annuente SUPREMO VITÆ ET NECIS ARBITRIO, sponte sanari; in aliis verò haud exiguam opem conferre artis præsidia, dummodo dosi congruâ præcipiantur, et tempestivè, vel lentâ festinatione exhibeantur."*

This full recognition of the curative efforts of "nature," and the adoption of methods of cure which have for their object the assistance of those efforts, will hardly constitute an important step in advance, unless more accurate information be acquired as to the forces and the dynamics of the machinery by which these beneficial results are attained; or, in other words, unless an improved physiology and pathology supply *principles* for our guidance, so that we can discriminate readily between phenomena originating from the proper exercise of the powers of the organism, and those resulting from the direct operation of morbific agencies; for it is on a due discrimination between these two groups of phenomena that a rational and scientific treatment can be based. These curative efforts of "nature" have been recognised more or less in every age, and methods of treatment have been successful in proportion as they have been formed in reference to the *vis medicatrix;* but the adoption of such methods has been due rather to great instinctive sagacity, than to a full philosophical appreciation of the properties of organized matter. The inventors have closely followed nature, but just as a workman would follow instructions in the management of a machine, while still ignorant of its principles of construction; or they have copied nature with a knowledge of her general aim and ends, but ignorant of details. No more striking instance of this class of great physicians can be mentioned than Sydenham: it was to the intuitive sagacity with which he observed and appreciated the course of nature in disease, that his persistent fame is due. With admirable judgment and precision, he recognised and applied that great principle of perfect adaptation by a universally operating Providence, which is, and must

* Synopsis Universæ Praxeos Medicæ, &c. Nova Editio. Auctore Jos. LIEUTAND, &c. Parisiis 1770. Prœmium, p. xiii.

be ever, the only basis alike of psychology, physiology, and pathology; and with no less sagacity, he discriminated between the results of that principle in operation, and the results of morbific agents. Thus,– to mention one of many examples,—in his discourse on the Pestilential Fever (the great Plague of London), he observes, "From all this it clearly follows, that the physician who, in the cure of other diseases, is *bound carefully and closely to follow the path and conduct of Nature,* must here renounce her guidance;" and then goes on to argue for active treatment by bleeding and diaphoresis. In the same discourse, he fully defines the meaning he attaches to the term Nature, and with such instructive lucidity, that the passage merits quotation:—

"One concluding observation I have yet to add, and I do so, lest any one twist my meaning by an improper interpretation; or at any rate, lest he misunderstand it. In the foregoing discourse, I often use the word *Nature,* and attribute to it various effects; just as if I pictured to myself under this name something universally diffused throughout the whole framework of the world; something that ruled, as it were, and regulated, all substances reasonably and with intelligence; something, in short, like the *animus* [*anima*] *mundi* of some philosophers. Now, for my own part, I dislike innovation, both in fact and language; so that throughout my pages I have used the term *Nature* as an old word with a limited meaning, which I believe all understand, and some adopt. Hence, as often as I apply it, I mean the whole complication of natural causes—causes which, in themselves, are brute and irrational, but which, nevertheless, are regulated by the highest reason, and which, under its guidance, perform their functions and exhibit their operations. The Supreme Deity, by whose power all things are produced, and upon whose nod they depend, hath, in His infinite wisdom, so disposed all things, that they betake themselves to their appointed works after a certain order and method; they do nothing in vain; they execute only that which is most excellent, and that which is the best fitted for the universal fabric, and for their own proper natures. They are engines that are moved, not by any skill of their own, but by that of a higher artificer."*

Physical philosophy, as well as anatomy and physiology, have largely added to the proofs of the existence of this great force; but it could hardly be defined in clearer language. We now know that this principle of adaptation is as universal as the force of gravity, as constantly in operation, as fixed and definite in its results. To the physiologist it is a familiar fact,—so familiar as to lose much of its weight.—that the simplest microscopic organism displays the operation of this principle of beneficent adaptation as much as the most highly developed. To the astronomer it is equally as familiar a fact, that the same principle of beneficent design is in operation amongst the vast masses of the universe. By the calculus of probabilities, La Place has shown that the sun's rising again on the morrow of any given day is two million times less probable than that the motions in our planetary system are the result of perfect design, with a view to the *stability* of that system. Since the time of Sydenham, moreover, anatomists and physiologists have done much to elucidate the modes of action of this principle; in particular we now understand better than in his day, the pathology of that portion of the nervous system (and its appendages) which has recently been denominated "the true spinal system," and which is the seat and instrument of a large number of automatic actions, the end

* The Works of Thomas Sydenham, M.D. Sydenham Society's Edition, vol. i. p. 119. (Dr. R. G. Latham's Translation.)

and aim of which is the conservation of the individual or of the species. This law of adaptation to beneficent ends—the care and preservation of the individual—is so fully recognised on all hands as the guiding principle of the "reflex function," that no additional testimony need be adduced on this point. The extension of the same law of action to the encephalic portion of the nervous system is rapidly in progress; and the time is not far distant when it will be discovered to be the only sound basis for cerebral physiology, as well as of any new systems of psychology which will necessarily arise therefrom. The application of the same doctrine to all the various vital processes, which go on *independently of a nervous system*, as well as in connexion therewith, although necessary to progress, is not so probable; nevertheless, as the advantage to practical medicine of further researches in this direction becomes appreciated, they will be undertaken, and the *desiderata* for medical art of well-founded general principles be attained. Histology, in this respect, will add largely to our knowledge, for it will enable us to understand the more recondite operations of the great principle, as well in *vegetable* as in animal organisms; but whether or no, researches into these operations can no longer be limited to the nervous system, or even to animal tissues. This extension of the doctrine of "reflex function" will be a large stride in advance; for rich as have been the results to practical medicine of the application of comparative anatomy and physiology to pathology, much more will be gained by a diligent study of the processes of *vegetable* life, and of the forces by which they are carried on.

But, indeed, the time has already arrived for a wider and more philosophical generalization of vital phenomena in relation to medical art. The resuscitation, in the nineteenth century, of the leading doctrines of the HUMORAL PATHOLOGY current in the seventeenth, is not without deep meaning, nor of trifling importance. It not only indicates a more general recognition of the existence of that fundamental vital force upon which that humoral pathology was based, but it will also render the vast stores of experience and of learning, accumulated during the period when it was current, available to modern uses, inasmuch as the great truths they contain will now be readily sifted from the errors and crude theories with which those truths are mixed up. Looking at the mass of printed pages before us, we have felt that an abstract, or even a critical analysis, of their contents, was a hopeless attempt, if anything like justice was to be done either to author or reader; we therefore thought it better to sketch out some such wider and more philosophical generalization, taking as our guides the mediæval and more recent views; and as illustrations may be more useful than abstract considerations, we propose to consider the two large groups of diseases to which the doctrines of the humoral pathology are now being more particularly applied—*fever* and *gouty diseases* Dr. Wood has given a short history of the theories of fever, and we quote his statement of the doctrine of the humoral pathology as an introduction.

"Not only in the case of fever, but of all other diseases, it was supposed that the blood becomes contaminated or depraved, either by the reception of noxious substances from without, or in consequence of changes going on within the body; that the system is excited by the presence of these impurities, either to direct efforts for their expulsion, or to a course of elaborate action, so as to fit them for expulsion; and that, in the language of Sydenham, a disease is no more than a vigorous effort of nature to throw off the morbific matter, and thus recover the patient. This is the sum and substance of the humoral pathology."

Grafted upon this general doctrine, and flowing out of it, were various theoretical notions of fermentation, concoction, &c., which lead to injurious and unnatural methods of treatment. The more sagacious of the school repudiated these notions. Thus Sydenham remarks: "I cannot bring my brain to comprehend the meaning of those physicians that are continually talking about the administration of remedies that promote the concoction of the febrile matter—points which they insist upon when called in at the beginning of the disease. Why! the fever itself is Nature's instrument. . . . . *The concoction of the matter of fever simply means the separation of the sound from the unsound.*"*

Sydenham expressly repudiated the use of the terms *fermentation* and *ebullition*, when treating of continued fever, preferring the term *commotion*. "I begin with observing," he remarks, "that the inordinate commotion of the volume of the blood, whether cause or concomitant of this fever, is excited by nature in order that either some heterogeneous matter, incorporated with our humours, and inimical to them, may be eliminated, or else that the blood itself may be transmuted into a better diathesis."†

Dr. Wood appears to think that it would be quite supererogatory to attempt to refute doctrines of this kind. The "fanciful notion of a vis medicatrix naturæ," to use his words, has no part in his pathology; and as to the more recent humoral views, while he admits that the blood is often greatly altered in the bilious, remittent, and yellow fevers, as well as in others, and that it is possible, in some instances, that this alteration is primary, and the direct cause of the other changes which occur, he insists that, "in the present state of our knowledge on this point, the assumption that all fevers, or even the particular fevers mentioned, are dependent exclusively on the morbid state of the blood, is quite unwarrantable." Upon examining more in detail the views of Dr. Wood, as to the nature of fever, we find that the word "exclusively" largely qualifies his meaning, and that there is much in which we agree with him, in common, we think, with the majority of practitioners in this country. Dr. Wood considers fever to be a disease of the whole system; that it is sometimes essential and idiopathic, sometimes symptomatic; that when symptomatic, the cause is local, as when there is local inflammation. As to the idiopathic group, and as to the general disturbance which occurs in the symptomatic as well as in the idiopathic fevers, Dr. Wood is not able to state anything distinctly, and frankly confesses his inability. He remarks: "Together with disorder of the solids, there is very frequently an altered state of the blood; but the question has not yet been settled whether this is an essential part of the disease, and important in the chain of causation, or a mere incidental effect. We are thus in a condition of uncertainty upon almost all points." We need hardly remark, that general principles like these are of little practical value. They neither supply rules of art, nor explain the curative operations of nature, which all experience proves to be the best guide. In our judgment such remarks hardly do justice to existing medical science. Wunderlich is much more definite and explicit. Recognising the constitutional character of fevers, he notes that there is a

* Op. cit. vol. i. p. 54. † Ib. p. 44.

certain habit of the functions generally—a normal condition—and that deviations from this condition are disease as important as those dependent upon structural changes. An abnormal susceptibility of the functions in relation to external impressions and local disorders constitutes constitutional irritability. Increased excitability, and rapidity and disorder of the functions, not momentary, nor quickly passing away, is *constitutional irritation*, or FEVER. Retarded, imperfect, or paralyzed functional action, is torpor. The relations of these three forms are numerous. As to causation, constitutional irritability, or torpor, is an habitual condition. It is a defect dependent upon hereditary conformation, age, diseases previously experienced, modes of life, and the like. It may have its origin in the nervous or nutritive system, &c. Under given circumstances, this condition may be developed into constitutional irritation, or fever. 1. There may be a predisposition to this form of disorder, from causes specified. 2. Changes in the external relations of the organism, as of temperature, barometric and hygrometric conditions, the winds, climate, and the like. 3. Every deviation from the normal condition in the blood will induce fever, or constitutional irritation, provided it be immediate and sufficiently extensive. All morbid conditions of the blood predispose to disturbance of function and febrile action, but most certainly of all, any considerable increase in the fibrin; febrile phenomena are manifested in the highest degree, and with early symptoms of adynamia and paralysis, *when the fibrin is diminished in quantity, when foreign and poisonous substances enter into the circulation, or when abnormal transforming processes are set up in the blood.* 4. Violent muscular actions, violent exertions, excessive sexual indulgence, more or less develop constitutional irritation and febrile phenomena of the adynamic type. 5. Painful affections, sensorial irritability, the occurrence of secretions for the first time, as the menstrual and lacteal, or the sudden suppression of such secretions, strongly predispose to, and sometimes induce, febrile phenomena. 6. Local disorders, classed by Wunderlich under six heads,—namely, whether acute or chronic; as to the tissue affected; as to the extension of the chronic disease; as to the sympathetic irritation; as to the changes in the blood which it induces; and as to the kind of organ which is the seat of the local disease. Like Dr. Wood, Professor Wunderlich thinks it not possible, in many cases, to determine whether fever is idiopathic or dependent on local causes, nor whether the local changes are primary or secondary, inasmuch as we are so much in the dark as to the origin of the phenomena, or their causal connexions. Like Dr. Wood, Professor Wunderlich takes no note of the great basis of all vital dynamics, that all-pervading "something" of Sydenham, in virtue of which "all things betake themselves to their appointed works after a certain order and method," and in virtue of which, therefore, the phenomena of disease are subject to that same determinate order and method. It is in thus ignoring this great and most fundamental principle of vital dynamics that we think the authors before us are defective. The "fanciful notion" of a vis medicatrix is so far from being fanciful, that the reparation of injury to the organism is as much a part of its nature and mode of existence, as its development from the primary cell through all the phases and grades of its existence. It is not possible to institute any *fundamental* difference between the vital force which produces a limb for the good of the animal, according to the normal and fixed order of development, and that force which reproduces a limb equally according to a normal and fixed order, and equally with the same object when the limb has, by accident, been severed. To our minds, the process of nutrition by which growth is maintained is not *essentially* different from that process of nutrition by which waste is repaired; neither can we think that if the great principle of adaptation to beneficent ends be considered as the basis of vital dynamics, it is logical to maintain that after providing in every way for defence against or for the removal of noxious agencies generally, it stops short at the removal of those which cause special forms of injurious action. The minor proposition is surely included in the major.

In adopting more particularly the humoral pathology of Sydenham, and of the great thinkers of the seventeenth and eighteenth centuries, it is by no means necessary to adopt their errors or to neglect the truths of modern pathology. The blood is undoubtedly the primary constituent of the body, as the capillaries are the primary mechanism. It is upon the histological change in the blood and in the tissues that all vital phenomena essentially depend. The heart and vascular system at large are but an hydraulical machinery for its due distribution: the pulmonary system is its aërating apparatus; the gastro-intestinal supplies the solid and fluid materials; the nervous systems, both voluntary and involuntary, are but special instruments constituted to put in motion, co-ordinate, and harmonize, all this varied machinery of the organism, and to defend it and them, so that the great object of the universally innate principle of Providence—the well-being of the created thing—may be fully attained. When this great principle works in a simple cell, it works with as admirable adaptation in all these respects as in the most complex of organisms; but inasmuch as in it there are no machines to co-ordinate other than the cell-wall and the granules, it provides no nervous system; as the circulating fluid is limited to the cell-cavity, aëration and nutrition are direct, and there is therefore no need of stomach, heart, or lungs; equally without distinct excreting organs, the circulating fluid is still duly depurated; and if it be not depurated, or poisons be introduced, the cell dies. Histology, while it establishes the great fact that the law of more complex development is simply a continual specialization of function, shows to us also that the organic elements essential to vital action are the containing tissue and the contained fluid; the former constituting the analogue of the apparatus whereby nutrition, aëration, and depuration, are perfected, the latter the analogue of the blood, and fulfilling its functions. From this point of view it is seen that the blood is the primary and principal seat of vital action; that for the maintenance of its purity all the varied processes of animal life are instituted; and that without that purity, they must all languish.

The great pervading principle of adaptation recognises to the full extent this fundamental and primary importance of the blood in the economy. With more complex development, every imaginable precaution is taken for the supply of material to it, perfectly elaborated and fit for the functions which the blood has assigned to it. Machines for procuring, seizing, tearing, and lacerating the raw materials of the most diversified design and most ingenious construction are developed; apparatus expressly fitted to prevent the entrance of improper materials is provided, manifesting not less ingenuity and perfection of adaptation; if unfit materials by chance escape these arrangements, a further provision is made for their expulsion from the recipient viscus, and should they escape these inner guards of the system, and make good their entrance into the citadel of life, the depurating organs come into active operation and effect their expulsion—with what rapidity and success, under ordinary circumstances, experiments in toxicology and therapeutics abundantly demonstrate. Not less admirable prevision and masterly skill preside over the due aëration of the blood, and so important, in short, in the scheme of LIFE, is the regular supply of solid, liquid, and gaseous elements, to the circulating fluid. that by means of the co-ordinating apparatus—the nervous system—the whole powers of the individual are concentrated, if the supply be defective, upon its attainment. Thus it is that hunger, thirst, and the desire for air, are the most urgent and most uncontrollable of the instincts. But it is of great importance to the pathologist, to notice the fundamental fact, that it is the morbid condition of the blood which excites the whole organism to desperate efforts, and not local and minor changes in the organs themselves. The dry throat is not the sole cause of the sensation of thirst and the concomitant agencies; it is only a secondary result of a diminished supply of fluid to the blood, in virtue of which its depuration is prevented, and the salts and used-up materials that ought to have passed off by kidneys, skin, lungs, and intestinal mucous surface, are retained, because the solvent is no longer present. Hunger is not, in its full intensity, caused by an empty stomach, but by that condition of the blood which a check in the supply of nutrient material causes. Hence in *tabes mesenterica*, more or less hunger is felt, however full the stomach may be. Great facts like these throw a flood of light upon the relations of all those diseases in which exalted functional activity of the nervous system is conjoined with blood-disease; the latter cannot possibly occur in any degree and not the former result in the same proportion, simply in virtue of the conservative efforts of that great instinctive principle we have referred to.

It would, however, be a most fatal error, if the attention of the pathologist were limited to what may be termed the phenomena of normal re-action. The due performance of the functions of all these diversified machines is certainly dependent upon a healthy condition of the circulating fluid; if this be wanting, the heart and vascular system labour from abnormal stimulus; the innervation of the nutrient, depurant, and aërant apparatus is imperfect, from imperfect action of the co-ordinating apparatus, and thus general functional disorder supervenes, constituting true disease, and giving rise to further morbid complications and re-actionary changes. To discriminate correctly between the phenomena of normal reaction and the phenomena of diseased reaction, is of the highest importance in practice, and the necessity of such discrimination has been recognised by the leading minds of all ages. There is indeed no more profound principle in therapeutics, nor a principle upon which more refined observation and a sounder physiology will throw greater light.

In applying this doctrine to fevers, we see at once that just as there is healthy and unhealthy inflammation, so there is healthy and unhealthy fever, paradoxical as the use of the terms may appear. What, then, are the phenomena of healthy fever, or rather, of that normal reaction of the organism against poisonous agents moving along in the current of the blood, in those fevers in which it is fully allowed that the presence of a febrific poison is the cause? Obviously, the symptomatology of fevers looked at from this point of view is nothing more than a branch of toxicology; indeed, pyretology might be termed pathological toxicology. Now we know that it is through the depurating organs that a poison is carried out from the blood, and that, consequently, any increased action of these organs occurring in the course of blood-fevers, may be considered as a part of the normal reaction, or of the healthy phenomena of fever. To this group, then, we can certainly refer the diaphoresis, diuresis, and diarrhœa, occurring in the course of blood-fevers; probably also increased pulmonary transpiration (in cases in which the respiration is hurried), and increased salivary and biliary discharges. We may also refer to the same group two other leading phenomena of fever—namely, increased vascular action and thirst; the latter developed to acquire an increased supply of diluent fluid; the former, to transmit the blood more rapidly through the depurant organs. Whether the increased heat be a part of the healthy or the morbid phenomena is not so clear. It is usually attributed to the increased rapidity of the circulation, and the consequent increased oxidation of the tissues; in so far as it is indicative of excessive waste, it is morbid; but it is not altogether improbable that the increase of temperature is a part of the curative process, for this may demand for its more perfect performance a higher degree of heat in the blood and tissues. Be this as it may, if in the course of a blood-fever these symptoms disappear—that is to say, if the pulse becomes feeble and slow, the thirst ceases, the body cools, and the secretions are arrested, the healthy phenomena have become unhealthy.

Toxicology teaches us that poisons have a *local action* upon tissues and organs as well as a general action through the blood. This local action differs in degree according as circumstances differ. To take mercury as an illustration. If this poisonous metal be administered therapeutically, even in very small and carefully-watched doses, in cases of nephria, in which the excreting tissue of the kidneys is destroyed, it is difficult to prevent dangerous ulceration of the mouth and fauces supervening. This appears to depend upon two distinct causes; firstly, the drug is not carried off from the depurating organ so rapidly as under ordinary circumstances; and secondly, the pre-existent morbid condition of the blood facilitates adynamic inflammatory action. So in fever-poisoning, the extent, at least, of local action is determined by the previous condition of the blood, and thus it is that local adynamic inflammation, indicated by sloughing, gangrene, &c., is apt to occur in all fevers, in which there is a morbid impoverished condition of the blood at the time when the poison is received into it, such as occurs in scorbutus, in the malarious, syphilitic, and uræmic cachexiæ, and the like, or in all in which the blood is rapidly devitalized. These phenomena characterize the *malignant, typhoid*, and *adynamic* forms of febrile diseases. Again: all things being the same, the *amount of poison* admitted determines the degree of both general and local action, so that a large dose is much more influential than a smaller. This has an important bearing on the history, etiology, and semeiology of febrile phenomena. Throughout the whole group of malarious fevers, to mention an illustration, we have a very great variation of the phenomena, induced simply by this difference in the amount of dose of poison taken into the blood. It is evident, indeed, that just as with drugs generally, the reaction against febrific poisons, and the morbid and morbific effects they induce, depend upon *several* circumstances other than the dose, those concerning the *individual* being, however, the more important to remember. Precisely as in therapeutics, the effects of the same dose of opium given to several individuals will be very different, so it is with febrific poisons. Thirdly, the circumstances which determine action in any *particular* tissue or organ, are very various, as well as the susceptibility to morbid action in the different organs. Thus the fact, whether the poison is solid, fluid, or gaseous, is important; so, also, the great fact that organs, often under circumstances of this kind, put on a depurant vicarious action, whereby poisons differing in their nature, and exciting a *different* series of reactionary phenomena under ordinary circumstances, may, under these circumstances, excite *similar* phenomena. But whatever may be the exceptional conditions, it may be stated, generally, that a local inflammation occurring in the course of a blood-fever, is the direct *result* of the blood-poisoning, and does not belong to the group of the normal or healthy reactionary phenomena of fever. We can therefore say, that the cutaneous inflammations occurring in the large group of exanthematous fevers, are not essential to the depuration of the blood, but are morbid phenomena in the exact sense of the word. The old humoral pathologists adopted a contrary view, and upon that view founded the hot and stimulant method of treating variola and other fevers of that group. Modern therapeutics have wholly discarded the method of treatment, although, under the term *zymosis*, the theory is still entertained more or less. These eruptions, therefore, as well as all the inflammations of mucous surfaces, such as the buccal, pharyngeal, pulmonary, gastric, and intestinal, which occur in the course of fevers, must be considered as phenomena, not essentially resulting from the curative instinct, but morbid effects of the febrific agent, and themselves giving rise to secondary series of instinctive reaction and other febrile phenomena, or, in other words, to inflammatory fever. It would certainly be a mistake, we think, to take even these latter out of the general category. The febrile reaction set up in the reparative process, after injuries or local derangement, is really a part of the general movement towards health; and it appears to be by no means improbable, that the phenomena thereof are dependent upon changes in the nutritive constituents of the blood, but especially (as is now more generally thought, and stated by Wunderlich) upon an increase in the fibrine. Hence, inflammatory fever not only takes its place among blood-diseases, but its phenomena may be divided into the purely reactionary, or healthy, and the purely morbid. A more careful analysis of the phenomena of inflammation and of inflammatory fever in relation to what have been usually termed idiopathic fevers, will lead to highly beneficial results. It is a conclusion not less flowing from theory than from observation, that the secondary results, or complications of fevers, are much more important because much more morbid than the fever itself, except when the co-ordinatory apparatus—the nervous system —is directly involved. The experience of the observant practitioner is clear as to the greater fatality of the fevers with inflammatory sequelæ or complications, and as to the greater danger of the local than the general disorder. Hence the importance of a more perfect natural history of fevers, so that these inflammatory complications may be prevented. Inasmuch as prophylaxis is all-important in treating them successfully, it is to be regretted that this great practical point has been so much lost sight of, or misapprehended, in discussing the question as to whether fever is a general disease, or dependent only upon local inflammation or irritation. A philosophical analysis of accurately observed phenomena will do much, both towards sound theory and sound practice in this respect.

To secure, then, the best results, it will probably be found necessary to consider febrile pathology as a branch of toxicology. This step would have one most excellent effect—namely, to determine more accurately the *etiology of fevers*, and the *modus operandi* of febrific poisons, by principles drawn from the general facts of toxicology. It is truly lamentable to see how much misunderstanding might be avoided by larger views of this kind, as well in regard to the general pathology of fevers, as to the doctrines of contagion, and the generation *de novo* and reproduction of the *materies morbi.* According to the present method of discussing them, the whole group of the *chronic blood-diseases*, induced by febrific poisons, and so predisponent to the acute forms, is either lost sight of altogether, or occupies but a subordinate position. Yet nothing is more certain, we think, than that the chronic blood-diseases of this kind, whether considered as distinct affections or as predisposing causes, are equally important as the acute. The chronic action of malaria upon the nervous system, although so admirably demonstrated by Macculloch, has by no means had that attention given to it which this widely-spread and very insidious form of disease requires, while the chronic influence of animal and fæcal effluvia, and of retained *excreta*, is hardly recognised at all. In treating of the humoral pathology as applicable to gout, we will endeavour to illustrate this point. Whether we consider the acute or the chronic groups, it is obvious that as a branch of toxicology we must determine the etiology; or, in other words, ascertain the specific fever-poisons, and the phenomena they excite.

As to the etiology of fevers, we are much indebted to our American brethren. The vast extent of territory in the new world occupied by the English race comprises almost every climate; and although it be not so varied or extensive as the British empire, the fevers to which the inhabitants are liable are felt *at home*. To the American physician, whether of the States or Canada, the entire and extensive group of *malarious fevers*—comprising diseases of every degree of intensity, from simple ague to the most pernicious remittent, in which the nervous system is primarily affected, and which are characterized by phenomena not unlike those of Asiatic cholera —is presented, under circumstances of great interest, while of late years the vast crowds of emigrants from Europe have introduced and spread widely the *ochletic* class. We are inclined to think that one of the best nosological arrangements on record of febrile and contagious diseases is due to Dr. Hosack, and his commentator Dr. Smith, in whose work (1825) on the 'Etiology and Philosophy of Epidemics,' it may be found. The arrangement of Wunderlich is certainly much less clear and suggestive. In the former arrangement there are five genera. The first comprises diseases communicable by contact only, and constitutes a very natural group, if scabies hydrophobia, and vaccinia, be omitted, inasmuch as it includes syphilis, sibbens, yaws, the laadda of Africa, and elephantiasis; to which, possibly, radesyge and pellagra might be added. The second comprises the acknowledged exanthemata, influenza, pertussis. These are communicable as well through the atmosphere as by contact, and generally attack a person but once. To this genus some recent researches enable us to add, exanthematous typhus, probably Asiatic cholera, one form of yellow fever, plague, the 'relapsing' fever, and dengue. The third genus includes fevers caused by the exhalations from the sewers and filth of cities, and the soil of marshes, &c. These fevers are the intermittent and remittent fevers, and those characterized by yellowness and black vomit. Modern researches would suggest a division of this into two groups, the one comprising the purely malarious, induced by vegetable effluvia, and the other, those induced by animal effluvia, especially civic filth—the true miasmatic. The fourth includes jail, hospital, and ship fevers; these result from the effluvia given off by the pulmonary, cutaneous, and other excreta of the human body, accumulated in small and unventilated places, and acted upon by heat. This group should be limited to those fevers caused by *ochletic* miasm only, to the exclusion of the fæcal, included in the previous genus. The fifth is the mixed class of fevers produced by the union of the two preceding febrific poisons.

It will not be unprofitable to show how this arrangement, modified on the principles of a reformed humoral pathology, may be made the basis of an entirely new nosology of fevers, which shall provide for, and explain, many of the apparent anomalies so puzzling to the student and practitioner. The first genus is a group which might be left wholly undisturbed, with the exception stated. Hydrophobia and vaccinia might be transferred to a new genus, to include those fevers communicated from brutes to man—the epizootic. The second genus might also remain, with the additions suggested; but the three remaining genera might be broken up etiologically, and the species re-distributed. We would, in the first place, allot a genus to those arising exclusively, or principally, from the effluvia of decaying *vegetables*, or their *excreta*, and term it the *malarious*. Their phenomena are characterized by periodicity, and the action of the poison is manifested on the nervous system generally, and on the stomach, liver, and spleen locally. The *chronic* diseases it induces are splenic, gastric, and hepatic congestion and inflammation, malarious chlorosis, spanæmic cachexiæ, local neuralgiæ, centric diseases of the nervous system, of an adynamic type. The *acute* diseases this poison induces, are the great family of intermittent and remittent fevers. We would group another class of fevers, manifesting as a leading symptom gastro-enteric irritation, under the term *fæcal*, or true *miasmatic*. In this group the poison derived from the decomposition of the intestinal excreta of man and animals, acts upon the depurating surface from which it is normally excreted, and excites the non-contagious forms of gastro-enteritis, gastric fever, dysentery, diarrhœa, and summer cholera. In these the local irritation is due, in the first instance, to the operation of the instinctive curative process of elimination, so that the diarrhœa and gastro-intestinal effusion are depurative. When conjoined with local inflammation, ulceration, and perforation, it is to be considered as a morbid, and not a curative symptom.

The pulmonary, pharyngeal, and cutaneous surfaces are extensive excreting surfaces, and want of personal cleanliness (that is to say, the retention upon its surface of the matter excreted from the skin), and imperfect ventilation, with overcrowding (that is to say, the retention of the pulmonary and tonsillar excreta in the air, and their constantly repeated respiration in large quantity), induce a group of fevers which may be termed *ochletic*—a term first applied by Dr. Gregory, and well adapted to designate the group. It comprises those forms of non-contagious typhus and typhoid, in which pharynx and tonsils suffer, the lungs manifest a low form of pneumonia or bronchitis, and there is more or less inflammatory change in the skin, shown either as rose-coloured eruptions, petechiæ, vibices, or as a dusky redness. The phenomena of neither of these two groups exhibit periodicity in the same way as those of the malarious, and, like the latter, they may recur again and again in the same individual, provided he be the recipient of a sufficiently large dose of the poison. It is, moreover, a characteristic common to all, that the true fever paroxysm, or entire series of paroxysms, has a definite duration; modified, however, very variously in this respect, by the secondary fever, and by the character, seat, and extent of the local complications. A general principle of toxicology is also common to all—namely, that the constant presence of the poison in the blood renders the system much more tolerant of large doses; a condition which, in reference to the poisons of these groups, is termed *acclimatization.* Whether there be provision made for a more regular or more efficient elimination of the poison, or whether it is that that great instinctive principle of adaptation to external agencies, which causes so large a number of changes in the external form and the internal mechanism of organisms, comes into operation under these circumstances, is doubtful; but the fact is certain. Just as persons become inured to large doses of poisonous drugs, as opium, tobacco, alcohol, &c., so the born dwellers in marshy districts suffer less than immigrants, and the seasoned denizen of the town than the stranger from the country.

The febrific poisons of these groups are gaseous; but there is another group holding a somewhat indeterminate position—namely, that comprising fevers in which putrescent animal (and perhaps vegetable) matter is introduced into the blood, either in the fluid state *directly* through a wound, or *indirectly* in a gaseous state, by absorption through the lungs or skin. It is probable that there are two divisions in this group—namely, those derived from this source when the fluids of the putrescent organism contain a specific fever-poison (as in death from variola or erysipelas), and those in which there is putrefaction simply. Distinct from all, however, is the great group, in which, with tolerably well-marked periodicity, definite *inflammation of the skin* or *mucous surfaces* is a leading phenomenon: in which there is a something generated in the blood, such that when introduced, under suitable circumstances, into the blood of a healthy person, it induces a similar fever; and which, having attacked an individual once, seems to be incapable of inducing again in him during his lifetime the same kind of diseased action, or at least in the same degree. These fevers, from their local complication, have been termed *exanthemata;* but since it often happens that the local inflammation is so slight as to be almost invisible, or being limited to the inner surfaces is never seen, it is doubtful whether the group would not be better designated *non-recurrent;*

and the local complication considered as a secondary and corroborative characteristic, rather than a primary. The cutaneous inflammation observed in plague, miliary, gastro-enteric, relapsing, and exanthematous typhus, in *one* form of yellow fever, in Asiatic cholera (Indian typhus), in influenza, dengue, and occasionally in pertussis, taken with the fact of the alleged non-recurrence and contagiousness of these epidemical fevers, would warrant us in adding them to this group. We should thus have three principal groups, the specific, or non-recurrent fevers; the miasmatic, or typhoid; and the malarious, or periodic.

Dr. Wood will probably have no difficulty in conceding to us the *general* correctness of this arrangement; the principal fevers of the groups lie, however, on debatable ground, and it has been found difficult to assign to typhus, plague, yellow fever, Asiatic cholera, &c., their exact nosological position. This difficulty is, we think, to be found in the great fact, that where men congregate in masses, no one poison is found in the blood alone at the same time, or, in other words, that in the great majority of fevers, and in all of the epidemic class, there are two or more poisons in the blood, and that, consequently, the febrile phenomena are of a complex character. Thus malaria may be combined with sewer or fæcal poison; the latter with ochlesis; or the whole three with a specific poison, as variola. The combination of malaria and fæcal miasm is very common, and gives rise to all those forms of intermittents and remittents which are characterized by the supervention of abdominal symptoms, as dysentery, diarrhœa, ulceration of the intestinal mucous surfaces. The combination of fæcal miasm with the poison of the exanthemata is also no uncommon occurrence, and is marked by the same complicating symptoms of gastro-intestinal irritation or ulceration. Hence it is that the etiology of fever is an important element of diagnosis. Without a clear perception of the nature and origin of the febrific poisons, and of their *modus operandi*, accurate diagnosis is in fact impossible, except in cases in which the characteristic phenomena are unmistakably manifested, as in regular intermittents, bubo plague, or examples of the exanthemata with a distinct eruption on the skin. Hence also it is, that no practitioner will overlook the prevailing type of fever, or, in other words, what specific poison is in operation, while he also bears well in mind the important circumstances of climate, temperature, and public and personal hygiène as pointing out the source and nature of the complicating febrific poisons.

It is not altogether impossible to classify the phenomena arising out of these complex combinations. Malaria, when acting independently of local inflammation, is purely intermittent in its influence; in proportion as local inflammation is excited, the paroxysmal character is lost, and the fever is remittent or continued; consequently specific poisons exciting local inflammation mask the true malarious phenomena. These are, however, also characterized by a *depressing influence* on the co-ordinating apparatus—the nervous system—and on the vitality of the blood. Hence in all cases of complex specific fever in which there is a large malarious element, there are highly-developed adynamic phenomena. To this group of complex specific fevers may be referred the epidemic cholera (Indian typhus) of Asia, the yellow fever of the Western Continent, the Levant plague, and those epidemics which ravaged Europe in the middle ages, previously to the advancement of public hygiène, and which were sometimes influenza, sometimes exanthematic typhus, bubo typhus, scarlatina, and the like. The *fæcal miasm* predominating, develops intense *abdominal* symptoms; complex fevers of this group appear, therefore, in camps, and in all large bodies of men where no provision is made for the removal of fæces, in cities of temperate climates during the heats of summer, and especially after a period without rain, when a large accumulation of fæces in the sewers is acted upon by a high temperature. In these instances, diarrhœa, summer cholera, and dysentery, complicate the fevers arising from a specific poison, or usher in epidemics of abdominal typhus, or of continued fever with abdominal symptoms. When *pulmonary* and *cutaneous ochlesis* predominates, as it will in all races, nations, and individuals, that neglect due ventilation and cleansing of the surface, the specific fevers, accompanied with pulmonary and cutaneous inflammation, assume a malignant type. Hence, in some degree, the fatality of Irish typhus; hence, also, the destructive ravages of the exanthemata, especially scarlatina, rubeola, and variola, amongst semi-civilized and wandering tribes. In these it is probably the ochletic miasm which gives the adynamic character and destructive extension to the local inflammation. When *pulmonary ochlesis* especially prevails, the specific febrile poisons which specially act upon the aërating surface, are more prevalent and malignant; and as it is in cold countries that the pulmonary system attains its highest development, and during the cold seasons of temperate climes that the necessity for the exclusion of the atmosphere from dwellings arises, so it is in these countries and seasons that rubeola, pertussis, influenza, and the pulmonary complications of the exanthemata especially, and of fever generally, are most developed, and lead to the greatest mortality.

The fevers of *epizootic* origin have received so little attention, and their course and nature are so little determined, that it would not be possible to analyse them without entering into numerous details, for which we have now no space. It may be stated, however, that the whole question is closely connected with the mode of origin of the *specific* poisons, and with the question whether they originate *de novo*, and if originating *de novo*, whether they commence in brutes or in man. We are inclined to think that the more typical forms, as variola and scarlatina, may, in the first instance, have been epizootic, but that all the forms of fevers into which the miasmatic element enters, may originate *de novo* either in man or brute. When we consider the curious transformations and phases of life in the monads, in entozoa, and epiphytes, in cancerous growths, and in histological morbid changes in the tissues, it appears by no means improbable that the presence of the ochletic and miasmatic poisons in living blood, already predisposed to abnormal transformation by defective nutrition, aëration, or depuration, may induce such transformations in the elementary germs, that specific forms of cell-life may be developed from them within the circulating fluid, and pass off through those great depurating channels to which the atmosphere has access, or from which the excreta are eliminated; and that impinging upon like surfaces and entering a circulating fluid equally as favourable to parasitic life as that in which they were formed, they may be reproduced therein. The singular, and indeed almost incredible, minuteness of the germs of known *fungi*, as well as the natural history of the latter, favours the hypothesis, which has been broached by several observers lately, as to the *fungous* character of the malarious, and specific poisons. If, then, from exposure to the conjoined operation of malaria, fæcal miasm, and ochlesis, new febrific poisons can be developed in the blood of man, we may conclude that the doctrine is applicable to brutes, and fairly infer that epizootics may thus arise, the special cause of which, when communicated to man, may induce specific febrile diseases in him not necessarily similar in their pathognomonic phenomena, but transmissible from man to man as well as from brute to man. We need hardly mention glanders and hydrophobia as instances of communicable epizootics; we have seen cases of *murrain* in man, in which the communication could be as clearly traced as it is possible to be—namely, in farriers, who had to introduce their hand and arm into the mouth and throat of diseased cattle, in treating them medically. The *carbuncular* fever is also transmissible; and it is not at all improbable that the influenza itself is only a modification of the phenomena of a pulmonary ochletic fever constituting the epizootic pleuro-pneumonia, or lung-disease of horses and cattle. The relations of epidemic pharyngeal inflammations to the latter and to erysipelas are of importance. These questions, as well as how far the *sheep*-pox is the parent of some of the varioloid fevers, remain to be determined by a well-conducted series of observations and experiments.

Having thus applied the humoral pathology to the elucidation of the natural history and pathology of fevers, it remains to apply it to treatment. It is obvious that there are two great principles to be had in view—namely, the removal of the poison, or its destruction by antidote, and the treatment of its sequelæ—the same principles, in fact, as guide us in poisoning by arsenic or other poisons, mineral and vegetable. We can only mention a few illustrations of the mode in which these principles of treatment have been, or may be, carried out. Inasmuch as the primary phenomena of fever must be looked upon as an effort of the curative instinct to remove the poison, any remedial means expressly adapted to this end must be used with careful reference to these curative efforts. In this respect the *principles* of treatment laid down by the older humoral pathologists have great value; nor is their recorded experience of the results to be slighted, for they were far from indulging in that helpless system of looking-on, which practically leaves the patient to the worse than unaided efforts of nature, and so destroys all the value of medical art. Since, however, our knowledge of the intimate working of the mechanism is very imperfect, we need not, except in certain instances, do more than watch, and carefully attend to, nature's indications. Thus if there be a great desire for any particular thing, that desire is to be indulged. If there be urgent thirst, water or simple diluents are supplied abundantly; if there be a clearly expressed desire for salt food or stimulants, they are not withheld. If the skin be hot, it may be cooled by cold affusion, ablution, or free aëration, and the pure cool atmosphere may be freely admitted to the lungs. If there be total anorexia, food is not forced upon the patient; if there be any free excretion, as a diarrhœa, or diaphoresis, it is not unduly checked, but rather encouraged. In the treatment of 'continued fever,' Sydenham adopted a perfectly philosophical method little different from the modern. Noting the "concoction" of the blood which constitutes fever, he observed, "we must keep it within its limits, so that it coincides with the intentions of nature. If, on the one hand, it rage too much, perilous symptoms will set in. If, on the other, it slacken into insufficiency, either the expulsion of the morbific matter will be hindered, or the efforts of the blood to reduce itself to a better state will be weakened."* He prescribed bleeding or omitted it, in accordance with these views. In persons of imperfect health, or with blood "of a weak character," as children, invalid youths, or men in the decline of life, "I keep my fingers," he says, "from the lancet. If I order venesection, the blood, weak enough even without being diminished, would be rendered incompetent to the work of despumation." In those "of an athletic habit and sanguine temperament," he says, "venesection is my leading remedy." And he adopts it with exclusive reference to the sequelæ—the local inflammations. "Neglect it, and you run the risk of frenzies, pleurisies, and such like inflammations." His next step is equally based on philosophical principles. It is so instructive that we will quote at length.

"After the bleeding, presuming it to have been necessary, I anxiously and carefully inquire whether the patient, at the onset of the fever, has been distressed by either vomiting or ineffectual efforts to vomit. If he have, I prescribe an emetic at once, unless, indeed, extreme youth or evident debility counter-indicate. Very necessary is an emetic, where there has been this previous tendency to vomit; since, unless the offending matter be cleared away, it will serve as a sink for all sorts of mischief. These will embarrass the physician during the whole of his attendance, and add greatly to the dangers of the patient. Of these, the most important, as well as the most usual, is diarrhœa. . . . . . As Nature partially corrects the malignant humours in the stomach, she passes them on to the intestines, and these become corroded by the continuous flow of acrid humours which the belly serves as source to. Diarrhœa *must* follow. . . . . Now the danger of the diarrhœa lies in this. The patient, weakened by the disease, grows weaker still."†

Sydenham here closely followed the indications of nature, for after "eyeing curiously the matters brought up by the emetic, and finding them neither remarkable for their quantity nor notably bad in their quality," he often wondered that the patient was "so much and so suddenly relieved." He administered the emetic by preference, "at the very beginning," when the vomiting, or desire to vomit, is manifested, giving late in the evening an anodyne draught. As to all these points, Sydenham's practice did not differ from that recommended, under similar circumstances, by Dr. Wood, in the treatise before us. Sydenham protested against attempting to check the vomiting in cholera morbus for the same reasons that he administered an emetic at the onset of continued fever. If successfully attempted, "fevers of a malignant disposition" supervene. "Such skill," he says, "is mischievous; stop the vomiting, and you replace it with a worse series of symptoms." He nevertheless strongly reprobated meddlesome medication. "The concoction of the matter of fever simply means the separation of the sound from the unsound. To accelerate this, we are not to meddle with attemperants, and I know not what . . . . . your evacuants and refrigerants are but hindrances; they procrastinate recoveries; and when health is coming of its own accord, they frighten it away. This I have seen often." His poorer patients got off better than the richer, for "as their purses could ill bear to be drawn upon for a long charge of physic," after the preliminary emetic and bleeding (if required), he enjoined them to keep

* Op. cit., vol. i. p. 46. † Loc. cit., p. 47.

their beds closely, and "drink oatmeal-gruel, barley-broth, or something of that sort;" to quench their thirst they were allowed "a little small beer with the chill off," and every other day took an enema of milk-and-sugar. "In this manner," Sydenham adds, "without any further complication of practice, beyond the mere addition of a purge towards the conclusion of the complaint, I sent them out of hand safe and sound."*

In treating the complications we find the same regard to principles. The bronchitic cough Sydenham treated simply by oil of almonds, "for cough medicines would only overload the stomach, which is already weak enough." After prescribing the best means of arresting *hæmorrhages*, he adds, "Now this must not be taken as if I recommended every hæmorrhage to be cured in this off-hand manner. At times it must be left alone altogether; since it may help the patient considerably, both by controlling the excessive ebullition, and by carrying off the disease critically. In reality, it does little good to check this symptom at all; unless it has either gone on for some little time, or blood has been drawn from the arm. This must be remembered."†

These extracts may suffice to illustrate the *methodus medendi* by removal of the febrific poison, according to the natural process. Sydenham knew little of antidotes to these poisons; nor were the so-called "febrifuges" anything more than means used for moderating the febrile symptoms; but convinced, himself, of the utility and safety of the Peruvian bark, he spoke out boldly and wisely in opposition to the prejudices of his contemporaries. He frankly declared his inability to explain its efficacy, but he maintained that previously to the discovery of its uses, agues were justly called the *opprobria medicorum*. No remedy has so constantly maintained its position, so that at this day it is the most available *antidote* we possess to the malarious poison; and during the last few years it has been tried, and with reported success, in the typhus class. The extent to which this fever-antidote is administered in the United States, as a cure for all forms of intermittents, has no parallel in this country. Dr. Wood informs us, that from thirty to sixty or even one hundred grains have been given during twenty-four hours, although he thinks from twelve to twenty-four grains is all that is necessary to be given between the paroxysms. Nor is the occurrence of local inflammation, if it be of a low type, a contra-indicant. "When typhoid pneumonia supervenes upon, or becomes complicated with, intermittent fever, it will be proper not to wait for a distinct intermission, but to administer the quina as soon as the nature of the disease is clearly ascertained. I have seen," says Dr. Wood, "the happiest effects result from this treatment, and have been informed by physicians residing in miasmatic regions, that they habitually employ it with great advantage. Not only is the intermittent interrupted, but the inflammation itself puts on a more favourable character." Equally beneficial results follow the use of this antidote in intense bilious remittents. "When a paroxysm of great virulence has occurred, from which the patient has been saved only by the most strenuous exertions, and there is every reason to fear that a similar one will prove fatal, recourse should be had to the sulphate of quina in the remission, however imperfect or short it may be. . . . . I am entirely confident that I have seen lives saved by this treatment, which must have

* Op. et vol. cit., p. 56. † Ibid., p. 64.

been inevitably lost under any other." The great contra-indicant is cerebral congestion or inflammation, for the antidote acts powerfully on the encephalon. In the 'pernicious' form of malarious fever, in which the poison acts so intensely on the nervous system that the paroxysm is not unlike an attack of epidemic cholera, the sulphate of quina is the only known antidote, and the sheet-anchor in treatment. "As soon as a remission or intermission has been obtained, there is but one course of treatment, and that is all important. . . . . No matter whether the patient has been under treatment during the paroxysm or not; no matter how partial the remission, provided it be a remission; no matter at what period of the interval the practitioner may have been called; his first, his last, almost his only thought, should be sulphate of quina. This is the remedy for the disease, and only this. . . . From thirty to sixty grains of this salt should be given, from the commencement of one paroxysm to that of the next." The experience of the French physicians in treating the paludal fevers of Algeria in every way corroborates these views.* But what is more remarkable is, that the remedy has been given with success in the ochletic yellow fever. At New Orleans from fifteen to thirty grains of the sulphate are given at once, combined with adjuvantia and corrigentia suitable to the particular case, and the same or smaller doses repeated if necessary. The febrile action is said in many to subside very speedily, and the patient frequently to enter immediately into convalescence; but in other cases it only modifies the phenomena, and the disease marches steadily onward to black vomit and a fatal issue. Cases are also reported in which the quina has cut short an attack of abdominal typhus, and as the former is only, we think, a variety of the latter, the statements on this point are worthy consideration. It must be remembered, however, that the fevers of this group are complex, and it may be readily granted, that while the *malarious* element of the compound poison may be counteracted, the *fæcal*, or *ochletic*, or specific fever-poison may remain uninfluenced. In this way we can explain the powerlessness of quina in those specific forms of yellow fever which should be classed with the exanthemata, and in all others of this group, as exanthematous typhus, variola, &c. It will perhaps be found useful in some degree as an antidote to the fæcal poison; but still, even if powerless over the miasmatic element, the counteraction of the malarious is a great point gained in the complex-fever group: it would be better, however, to give the quina as a prophylactic rather than a curative agent. There is a destructive class of infectious and contagious fevers, especially those of ochletic origin, as epidemic cholera and yellow fever, which derive much of their fatal energy from the previous entry of the malarious poison into the blood, so that the way to disarm them of their virulence and render the attack milder and safer, is to administer quinine as a prophylactic. This plan we adopted during the cholera epidemic with marked success.

The experience of the antidotal properties of quina should encourage practitioners to seek for an antidote for the fæcal and ochletic miasms, and for the specific contagions, since the probability that such antidotes exist is demonstrated by that experience. Two centuries ago, an antidote for the paludal poison was much less probable. "Specific medicines," Sydenham remarks, "in the restricted sense of the word, are by no means of everyday occurrence. They do not fall to every man's lot. Nevertheless, I have

* Vide vol. x. of this journal, p. 362.

no doubt, but that out of that abundant plenitude of provision for the preservation of all things wherewith nature burgeons and overflows (and that under the command of the great and most excellent Creator), provision also has been made for the cure of the more serious diseases which affect humanity, and that near at hand and in every country."*

The fact that so many persons are exposed to the specific poisons without injury or any apparent impression whatever on their health, merits close investigation; for it cannot be denied that at least hundreds of medical practitioners receive them into their blood daily without harm. The discovery of the mode in which nature protects would be a great triumph of art. It ought not to be forgotten, in all inquiries into contagion and infection, that the blood of a healthy person may not only be a fomites, but for anything known, there may be the same rapid generation of the poison as in the sick, only with an equally rapid elimination. Nature is undoubtedly equal to the task.

After all, the greatest triumph of art would be the utter removal of the sources of these fatal poisons. In a system of public hygiène is to be found the great means of prophylaxis; but even in the adoption of hygienic means the great protecting instinct has anticipated man's highest wisdom. The instinct of personal cleanliness in animals is nothing more than another manifestation of that same guiding principle which provides for the depuration of the blood; and it is curious to watch how, in animal communities with social instincts, a system of public hygiène is developed. Thus the bees of a hive provide for its efficient ventilation by mechanical means, as perfectly applicable to its purpose as it is ingenious; or if the dead body of an enemy cannot be removed from the hive, they provide against the evolution of putrescent effluvia from it by sealing it up hermetically in a coffin of wax. Numerous similar illustrations of instinctive, social, and personal hygiène are afforded by natural history. It has been observed, for example, that certain of the feline carnivora adopt exactly the same method of disposing of fæcal excreta which was enjoined by Moses on the Jews in the Wilderness, with the view of preventing dysentery breaking out in the camp. So it is that in the smallest and most trivial, as in the greatest and noblest, of God's works, there is the same incessant operation of a beneficent principle; it supplies the example which man can most safely follow, and its works are those which it is the height of human knowledge to know. It is very sure, that in medical science and art, as in all others, the grand principle of Bacon is strictly true: "Homo, NATURÆ *minister et interpres*, tantum facit et intelligit quantum de NATURÆ ORDINE re vel mente observaverit; nec amplius scit aut potest."

Having in some degree indicated the method whereby a modern humoral pathology, based on vital dynamics, may be made available to the better illustration of the pathology of fevers, and to the discovery and perfection of better methods of treatment, we will turn to that other large class of blood-diseases dependent primarily upon changes *within* the organism, and of which gout and rheumatism are the great typical forms. By the phenomena of this group, the relations of the humoral pathology to the laws of development, to histology, and to bio-chemical forces, are best illustrated. By selecting them we can also compare the two systems of Wood and Wunderlich. Dr. Wood divides his pathology and therapeutics into

* Op. cit., vol. i. p. 22.

two parts, the first, *general*, the second, *special*. The special pathology is divided into three classes—namely, general diseases, or fevers proper; constitutional diseases, or gout and rheumatism; and local diseases, or diseases of organs, or systems of organs. This arrangement is, we need hardly say, confessedly empirical, for Dr. Wood by no means denies that struma and other similar diseases are not constitutional; he excludes the latter simply as a matter of convenience. Wunderlich grapples at once and successfully with the difficulty from which Dr. Wood shrinks, and this wholly by applying the principles of the humoral pathology. According to his arrangement, so far as it is given in the part before us, the constitutional affections are as follow:—(A.) Fevers.—(B.) Dyscrasiæ, not caused by a specific cause: in this are included—1. Those dependent upon morbid nutrition.—(i.) General cachectic state; (ii.) Marasmus, or tabes; (iii.) Chlorosis; (iv.) Struma; (v.) Polysarcia, or fatty degeneration.—2. Those characterized by a tendency to hæmorrhagic effusion: (i.) Scorbutus; (ii.) The transient hæmorrhagic diathesis; (iii.) Morbius Maculosus (Werlholfii); (iv.) Habitual hæmorrhagic diathesis.—3. Those characterized by abnormal excretions or exudations: (i.) Dropsy; (ii.) Azoturia; (iii.) Melituria (Diabetes mellitus); (iv.) Icterus; (v.) Pyæmia; (vi.) Ammoniacal excretion (uræmia), suppression of urine. To this group Wünderlich would add, general tuberculosis, general cancerous degeneration, diphtheritic products, with phosphatic and the so-called arthritic deposits; but he proposes to treat of all these subsequently.—(C.) Constitutional diseases, characterized by structural changes in special tissues or organs. This includes: 1. A group in which the changes are induced by specific agents, but without reference to the close connexion of the structural with the functional disorder, and which comprises the metallic or mineral cachexiæ—namely, those induced by lead, copper, mercury, arsenic, phosphorus, and iodine.—2. A group caused by the entrance of vegetable substances, and which (rather paradoxically) has for its first species alcoholismus; the second being the opium cachexia. With the consideration of this uncompleted, the part before us ends. The fragment we have given suffices, however, to show the more comprehensive character of Wunderlich's arrangement.

Gout, according to Dr. Wood, is a constitutional affection, exhibiting itself in a peculiar irritation or inflammation in various parts of the body, of which, probably, no one vital portion or tissue is at all times exempt. He considers it under the three heads of *acute*, *chronic*, and *nervous gout*. As to the nature of the constitutional affection, he remarks:

"The humoral pathology taught that the phenomena of gout were owing to a peculiar peccant matter existing in the system, which it was the business of the paroxysm to eliminate. This opinion, abandoned after the times of Cullen, is again gaining ground, and ranks at present among its advocates men of high name in the profession, of whom not the least conspicuous is Dr. Holland. The fact which lends the strongest support to this opinion, is the tendency evinced in gout to the excessive production of uric acid. Not only is that acid thrown out in excess in the urine, giving rise to frequent lateritious sediments, but it is also often deposited in the joints, or their vicinity, in the state of urate of soda or of lime. Dr. Garrod has proved, by chemical examination, that the blood in gout contains urate of soda, which can readily be separated from it in a crystalline state. He has also shown, that before the occurrence of the gouty paroxysm, there is, along with this excess of urate of soda in the blood, a deficiency of uric acid and its salts in the urine.

The use of an excess of animal food, with insufficient exercise, which disposes to an excessive production of uric acid, predisposes also to gout; and the uric-acid lithiasis, or gravel, not unfrequently alternates with that complaint. But these arguments, as well as others, drawn from the moveable characters of the local disease, as if it depended upon an offending matter carried everywhere with the blood, and from the relief experienced after the paroxysm, as if the offending matter had been discharged, appear to me to be inconclusive. Uric acid, I think, certainly cannot be the matter in question. This substance is generated in excess, and thrown out by the kidneys in many diseases in which no sign of gout is exhibited; and cases of gout often exist, especially in its irregular forms, in which there is no evidence whatever of an excess of uric acid; nor can I exactly perceive how the production and elimination of this matter, if not essential to gout, should lend any support to the idea of the existence of some other unknown and concealed matter, which is the real offending cause. It is possible there may be such a substance; but its existence, not to mention its nature and properties, has certainly never been demonstrated." (vol. i. pp. 479, 480.)

Dr. Wood might have added, that colchicum, the specific for gout, increases the quantity of uric acid in the urine, as shown long ago by Chelius. The paragraph is a good example of Dr. Wood's style; he states the facts succinctly and fairly in support of a given doctrine, and when you expect the conclusion as a matter of course, some indefinite negatives are advanced to meet the positive facts, doubts are expressed as to the facts themselves, and the reader is pushed into a dark closet of inferences, to find his way out as he best can, or accept the indefinite conclusions to which the author has led him. Let our readers compare Dr. Holland's lucid pathology of gout, and especially his general deductions, with the following conclusions of Dr. Wood:

"All of a general nature that we can fairly deduce from the symptoms is, that there is a morbid state of the system which probably involves all its essential constituents, and which evinces itself now and then by peculiar local phenomena, which may be either purely nervous, purely functional, or inflammatory. That the inflammation is peculiar or specific is proved by its shifting character, and indisposition to the secretion of coagulable lymph or pus." (vol. i. p. 480.)

Or, in other words, we know nothing about gout. Now, this is so far from being the necessary deduction from recorded experience, that we would almost venture to say, that there are few constitutional diseases that are better understood, even if we go no further than the facts as stated by Dr. Wood. It suffices to remark, in answer to Dr. Wood's negative arguments, that the excessive production of uric acid is by no means necessary to induce gout. An excessive production is a characteristic of the gouty *diathesis;* but so long as the acid is freely eliminated, the individual is free from *gout,* just as he would escape the effects of other poisons, which are harmless if carried off as rapidly as received. Mercury must be prevented "running off" by the intestinal canal if we would induce its constitutional effects; and comparatively small doses will produce febrile action and local inflammation (ptyalism) if the whole that is administered be retained. If there be no evidence of lithic acid in the urine of persons of a gouty constitution suffering from gout, we may conclude that it is retained in the blood, not as uric *acid,*—that would be a conclusion not warranted by the facts,—but possibly as a *urate* of soda. It must be remembered, also, that the sudoriferous glands take on a vicarious action in the gouty, and are auxiliary to the kidneys. It is, however, to be noted, that Dr. Wood adopts the humoral doctrine in practice, although he rejects it in theory.

"The paroxysm in gout is certainly not the whole disease. There is undoubtedly a morbid state of system to which the paroxysm is owing, and which it has a tendency to relieve, if allowed to run its course. It is no less true, that the removal of the paroxysm has no effect in removing the state of the system alluded to. If, therefore, by remedies addressed exclusively to the former, we succeed in cutting it short, we may very possibly leave the latter still in existence, and ready to display itself by some assault, it may be upon one of the joints as before, or it may be upon one of the interior or vital organs. The most prudent procedure, therefore, would appear to be, to allow the inflammation of the joint to complete its course, contenting ourselves with moderating the violence if excessive, and endeavouring to render the patient as comfortable as possible." (*loc. cit.*)

This is just the old prescription of "flannel and patience." Sydenham's advice is not widely different from Dr. Wood's. "The elimination of the *causa continens* is," he observes, "the work of Nature, and it must be done according to Nature's own method." Again:

"In gout, however, it seems as if it were the prerogative of Nature to exterminate the peccant matter after her own fashion, to deposit it in the joints, and afterwards to void it by insensible perspiration. In gout, too, but three methods have been proposed for the ejection of the *causa continens*—bleeding, purging, sweating. Now none of these succeed."*

He discusses the three methods, and shows how it is they fail. Purgatives, for example, cause the peccant matter meant for the joints to take hold of the viscera.

"Nature, diverted from her own good and safe method of depositing the peccant matter in the joints as soon as the humours are solicited towards the intestines, instead of acute pains with little danger, induces sickness, griping, fainting, and other irregular symptoms, which will nearly destroy the patient."†

It is not possible to watch an exquisite case of gout—that is to say, a case, in which all the most striking characteristics of the disease are manifested—without coming to the conviction that it is blood-disease. Nothing is more extraordinary in pathology than to see the chalky-looking matter (the urate of soda) flowing from the ulcerated bursæ mucosæ situate about one or more of the joints (as the elbow, wrist, &c.), and to note with what sensible benefit to the sufferer the discharge continues. Although he be crippled in every limb, advanced in years, and unable even to move in bed, all the vital functions go on well so long as there is a free exit for the chalk-like matter. Being a blood-disease, the pathology of gout is to be considered as a branch of pathological toxicology: and just as fevers illustrate the acute and chronic blood-diseases arising from poisons derived from without; so gout illustrates a large group of similar diseases arising from poisons derived from within. As in the former, so in the latter, we have to consider the blood-poison etiologically; then the results of its action on the organism generally, in inducing a curative reaction; and on the organism locally, as either acute or chronic affections. In considering these points, Dr. Wood's division into acute, chronic, and nervous gout may be adopted.

The decomposition and recomposition of the histological constituents of the organism is the final cause of the whole mechanism; it is the essence

* Op. cit., Treatise on Gout, p. 131.

† Ibid.

of vital action. The bio-chemical forces by which this process is effected have been sufficiently elucidated, to establish at least the general fact that the effete or used-up matter passes out of the body in the form of *excreta*, and constitute the various nitrogenous, hydrogenous, and other compound inorganic products given off from the excreting surfaces of the kidneys, skin, lungs, and glands in connexion with the gastro-intestinal canal. By far the largest supply of excreta must be sent into the blood from the *motor* apparatus, whether voluntary or involuntary, and by the co-ordinatory apparatus thereof—the nervous system. This is the necessary result as well of the extensive histological surface as of the degree of vital action. The exact result of the bio-chemical changes going on is not yet determined, but it is generally allowed that part is elaborated as carbonic acid from the lungs, and part as urea and other urinary excreta from the kidneys. It is very probable that the skin also acts as an excreting organ, but to what extent and under what circumstances has not yet been ascertained by physiological chemistry. The facts of pathology render it certain, however, that it takes a larger share than is usually allowed. The acid and fœtid excreta of the skin which constitute such striking symptoms in some diseases, have had little or no light thrown upon them by physiology, except what is included in the general principle, that one part of the great system of excreting surfaces may take on the functions of another part, when the adaptive principle of the organism indicates the necessity. In this way fœtid excreta from the tonsils, the pulmonary mucous surface, the skin of the feet, &c., may be considered to depend on a vicarious action eliminating those compounds of sulphur, or phosphorus, or carbon, with hydrogen, usually transmitted through the intestinal glands. So also the acid sweats in rheumatism, miliary fever, &c., may be due to the elimination of effete materials, which, under other forms, should have passed off by the liver or kidneys. Amidst all this uncertainty there is no doubt but that the effete residue of the histological transformations in the muscular and nervous systems have their outlet, to a great extent, by the kidneys, and that in gout and gouty diseases, these transformations and the elimination of the products are of primary etiological importance.

The excreta enter the blood only to be eliminated. This process of depuration goes on incessantly, for the necessity is as continuous as vital action itself. If they accumulate in the blood faster than they are eliminated, they excite the protective principle into activity, and special means are used for their elimination. In this way retained excreta induce febrile action, the intensity of which will be in relation to the amount of the poison accumulated and the force necessary for its elimination. It is thus that checked perspiration is supposed to induce febrile movements. The amount of the excreta-poison accumulated will depend upon two principal circumstances—namely, the rapidity of its production and the relative activity of function of the organ appointed to eliminate it. Applying these views to urea and the urinary salts, and adopting the theory that they are poisonous and induce disease (as well those of a gouty character as others), and are to be eliminated by the skin and kidney, it is evident that there may be an accumulation from so excessive a transformation of the nutrient materials, that the kidneys or skin, although healthy, are not equal to their elimination; or that there may be only a normal, or even an inferior activity of transformation, but that an accumulation supervenes from functional or structural disease of the kidneys or skin, whereby the elimination is impeded or arrested. Such being the probable facts as to the origin of the poison (or peccant matter), it is obvious that the pathology of gout and of renal disorders generally must be considered from a much wider and more comprehensive point of view than hitherto. We at once see the necessity of inquiring more particularly into the nature of these transformations, into the functions of the viscera and tissues in which they take place, and as to the operation of the retained poison upon the tissues from which it is derived. We get a glimpse, also, of an entirely new nosological arrangement, and see how it is possible to include a larger number of apparently widely dissimilar diseases in one great group, comprising morbid transformations of the vascular and muscular systems.

It has been already noted as a general fact, that fever poisons received into the blood from without have a special local action, and that it is the tendency of those derived from animal excreta to exercise a morbid influence first upon the blood and tissues, and then, or concurrently, upon the excreting surface from which they are normally eliminated. Thus the fæcal miasm induces diarrhœa and dysentery; the *ochletic* causes pulmonary, pharyngeal, and cutaneous inflammation. If we apply this general principle to the urinary excreta, we should conclude that they would act injuriously, first upon the tissues, from contact with which it is a primary principle of the organism to remove them; and secondly, or concurrently, upon the excreting organs allotted for their removal. The latter are the kidneys or skin; the former are principally the motor apparatus. The motor apparatus differ in their intensity of action and their extent of surface. The voluntary system is the most extensive, but it has long intervals of inaction; the cardiac involuntary system, less extensive in surface, is more intense in action, because it never ceases. The pulmonary is next to it in degree, and then the abdominal tubular muscles. We should therefore infer, *à priori*, that the accumulation of renal or cutaneous excreta in the blood, would manifest itself in histological change of structure, in modification of function, and in capillary derangement of those organs and tissues in which the production of those excreta is most active, or from which they should be eliminated. This is, in fact, what occurs in gout, in rheumatism and rheumatic gout, in arthritic and rheumatic inflammation by metastasis, and in those affections of the kidneys and skin which impair or arrest their eliminating function. The voluntary muscular system and its appendages, the heart and vascular system, and the kidneys and surface, are, in all these diseases, the seat of the pathognomonic phenomena. It was on these general grounds that we were led to notice and teach the connexion between uræmia (caused by Bright's disease) and the inflammations of serous membranes which are developed during its course, for some years before that connexion was so ably demonstrated by our late lamented friend, Dr. J. Taylor. It is probable, however, that the poison which excites gouty erysipelas or chronic gouty eruptions, differs from that which gives the acid or straw-like odour, to the perspiration in rheumatic fever, miliary fever, syphilis, &c., and it is obvious that, in addition to the isomerism of these excreta, and the facility with which their constituent elements are recombined so as to mask their true character, there is the difficulty of ascertaining the chemical relations of the cutaneous excreta in consequence of the impossibility of collecting them for analysis. Nor could the determination of the urates to the bursæ mucosæ and tendon-sheaths be easily explained, even on the principle of vicarious action, until lately, since good evidence of a secreting structure was wanting; but Mr. Quekett has demonstrated (what had been already hypothetically inferred from these pathological phenomena) that these tissues are in their histological structure identical with secreting surfaces, having a series of processes like villi upon their surface, which project into all parts of the cavity of the joint or of the sheath, so that the true nature of the articular inflammation in gout is now more apparent. In support, therefore, of the opinion that it depends upon the local action of urinary constituents retained in the blood, we have, in addition to the ample evidence of accumulated experience, the philosophy of vital dynamics, improved chemical research, the general doctrine of vicarious action, and the most recent and trustworthy researches into histological structure.

These views place us in a position to ascertain the relations of the antecedents to the arthritic condition of the blood and the tissues, and to consider the phenomena by which it is manifested. The two great predisposing antecedents are high living and an hereditary conformation of the system. The relations of the former are sufficiently obvious; those of the hereditary state are more obscure. Experience establishes this, that the gouty predisposition shows itself in two classes of persons, the one comprising those with bilious, sallow complexions, small limbs, feeble vitality, tolerant of stimulants, and subject to asthenic forms of disease. The other group, and by far the most numerous, includes persons of a sanguine temperament and portly figure. These have large limbs, broad thorax, firm, regular, well-set teeth, florid complexions indicative of a highly developed capillary system, and a blood rich in globules. Advancing years modify the complexion, for hepatic disorder is apt to give a sallow tint (leaving, however, the characteristic mark of numerous small bloodvessels meandering over the cheek), and fat becomes deposited, so that corpulence shows itself; in many the hair becomes grey early, not however falling off from the crown even at an advanced age, and the teeth remain sound until late. As regards the general habits, the most noticeable are mental and bodily energy, and a love of " the pleasures of the table," rich food and wine being desired and enjoyed. The pathological conditions are urinary deposits of lithic acid, renal diseases, and analogous transformations of the serous and sero-fibrous tissues. In particular, the left heart, its valves, and the arterial system, undergo patchy calcareous degeneration or spurious ossification, causing cardiac diseases of various kinds, and consequent pulmonary congestion, with its sequelæ, hypertrophy and dilatation of the right ventricle, hepatic and renal congestion, and sanguineous apoplexy. This is a general description of the most *typical* form of the arthritic diathesis, and in such gout in any of its forms may be readily developed, whenever the appropriate exciting causes are brought into action. There are numerous instances of a latent arthritic diathesis in which the temperament is not purely the sanguine, but compounded with states in which the nervous system is highly developed, or there is a strumous taint, or a leading predisposition to hepatic disorder. These may modify the general characteristics considerably; nevertheless the practised eye can detect the latent arthritic conformation.

It is an important point in the history of gout that it rarely attacks females, although we know instances in which girls under twenty, with a strong hereditary taint, have suffered. Nor are men affected until the middle of life, except in young men (as in girls and women) with a very decided hereditary tendency. It is probable that when it occurs under unusual circumstances as to age or sex, the principal predisposing cause is the too great or rapid transformation of the tissues, but that when it occurs (as is usual) about the age of forty, the predisposing cause is to be found, mainly at least, in imperfect renal action. The great dependence of this function upon due innervation of the kidneys, is probably one of the reasons why excess in sexual indulgence, as well as excess in eating or drinking, is so often followed by an attack of gout. It is quite certain, too, that ovarian influence modifies the renal function very considerably in females with a gouty predisposition, and although a paroxysm of gout, or even articular inflammation, is very rarely induced thereby, yet one or more of that group of diseases termed nervous gout is induced, and the anomalous forms of hysteria excited. This doctrine, as to the *ovarian origin* of arthritic hysteria, which we promulgated thirteen years ago, has made progress of late years, in proportion as the modern humoral pathology has been developed, and is now fully adopted by eminent physicians. Its best example is that state of the system which we termed *neuræmia.**

The *exciting* causes of a paroxysm of gout (the acute form) do not differ essentially from those of fevers. Exposure to cold or other depressing agencies, as watching, fatigue, and the operation of other febrific poisons on the blood, are some of these. The points more particularly to be noticed in the paroxysm are, firstly, that the pain is essentially neuralgic, although aggravated, doubtless, by the local inflammation; secondly, that the inflammation is specific; thirdly, that the phenomena are periodic. This last point is little noticed by later systematic writers, yet it is of considerable importance in determining the treatment, for we learn thereby that the paroxysm (like an attack of fever) will have a natural termination. It is, in fact, a fever of the tertian type, lasting fourteen days; it is nevertheless eminently a recurrent fever, for there is a constant reproduction of the febrific poison, and the organism appears never to become habituated to its presence in the blood. Hence if its elimination be prevented, the febrile phenomena recur, but with each recurrence, especially if they be frequent, there is less power of reaction, less perfect elimination by the natural process, and, finally, the chronic disease.†

* The doctrine of a gouty hysteria is not new. "Of this gouty kind, also (observes Dr. Cheyne), is the nervous Sharpnesses of hysterick Women, which sometimes is a Pain in the *Forehead*; a violent Stitch and Pulsation, fix'd over one of the *Eyes*; a Coldness in the *Top* of the Head, on the *Temples*, and almost all other Parts of the Body by Turns, a Pain in the *Teeth*, the *Jaws*, the *Stomach*, the *Sternum* and the *Guts*, is a *Strangury*, *Tenesmus*, a *Colic*, the *Gout*, and the Gravel: In short every Thing."—An Essay on the True Nature and Due Method of Treating the Gout. By Geo. Cheyne, M.D. and F.R.S. 7th Edition, 1725, p. 130.

† "Hence likewise we may see the Reason, why the first Fits, of otherwise healthy Persons, enrage and remit once in four and twenty Hours. The Access being at Sunset, and the Remission about Daybreak: why the whole Fit in such lasts about fourteen Days. And lastly, why the particular Accesses and Remissions of Persons advanced in Years, and broken with the *Gout*, happen only once in fourteen Days. And why the whole Duration of the Fit is made up of several such Accesses and Remissions in equal Times."—Cheyne in *op. cit.*, p. 7.

In treating paroxysmal gout of the normal type, we inquire, first, what is the antidote to the poison? secondly, through what organs does nature eliminate it? It can hardly be conceived that it is eliminated into the seat of inflammation, except in those instances in which concretions form, or the chalky fluid is poured out from the suppurating surface of the ulcerated bursæ and tendinous sheaths. There are some good grounds for the opinion that the cutaneous surface as well as the kidneys gives exit to the poison. Of the two, the skin is certainly the safest point for artificial aid, for the kidneys participate already in the morbid action, and are indeed, possibly, its primary seat; but even powerful diaphoresis is not quite safe during the paroxysm, however valuable in the interval. The maxim of the old humoral pathologists is very applicable to gout, and should never be forgotten in treatment, whether we wish to excite the local inflammation in a joint as a counter-irritant to metastatic inflammation, or as prophylactic means when such inflammation is feared, or whether we wish to cut short the fever by rapidly eliminating the poison. In all these, "*Ubi irritatio ibi fluxus*" is a cardinal maxim. As to the antidote to the poison, the most approved is colchicum, although the ranunculaceæ have been used. In some instances quinine is very available, more particularly when a malarious poison is co-existent. There is much truth in Dr. Wood's doubts as to the *modus operandi* of colchicum, and as to the amount of our knowledge on that point. Dr. Wood states it has no decided narcotic property, and therefore cannot act as an anodyne; and in the ordinary sense of the word, this may be true. It is very possible, however, that it has a sedative effect on the *vital actions going on in the tissues themselves*, so that the rapidity of the transformations is diminished, and in this way the generation of poison is stopped. For the same reason, it may facilitate renal and cutaneous excretions. The free use of diluent fluids may (as in other fevers) accelerate the elimination of the *materies morbi*. It is in prophylaxis that colchicum may be given most safely; administered in small doses, so as not sensibly to affect the bowels and in combination with chalybeates, the haloid salts, or such other remedies as the special habitus of the individual may indicate, it is undoubtedly a very valuable remedy in almost every form of chronic or masked gout.

The *cutaneous* gouty diseases have scarcely had so much attention directed to them as their importance merits. Their true nature is overlooked by the practitioner, unless there be an efflorescence or deposit of urates, or the presence of the arthritic diathesis arrests his attention; for they will occur in gouty persons who have never had a paroxysm. Prurigo, and the desquamative inflammations of the derma (as psoriasis inveterata), are the most usual forms. Although concretions of urate of soda have been found in the mammæ, the ears, and as a saline efflorescence, on the skin of gouty persons, it is by no means certain that the excreta eliminated are compounds of urea; in some instances they have been suspected to be oxalates. The determination of the urinary constituents to the derma is an interesting pathological fact, because the derma is to be considered but a portion (inverted) of the great gastro-intestinal system. Until lately, this was only an hypothetical deduction from the general principles of embryology and morphology; but Kölliker, having established the existence of the non-striated class of muscular fibres in the integument, has set this point at rest.*

Urinary salts have been discovered in the saliva, and all analogy would lead us to the inference that they may appear vicariously in any of the inner mucous surfaces as well as in the skin. In this way we comprehend gouty coughs, gouty asthma, colic, and various other spasmodic diseases arising in the course of chronic gout. Patients of this class are also subject to gouty flatulence, when vast volumes of nitrogen are given off. Wunderlich thinks it is derived from the air which has been swallowed with the food, and not an excretion from the gastric mucous surface; but this explanation is not sufficient to account for the enormous quantity poured out in a very short time in cases of this kind, and it seems more reasonable to infer that it is due to some bio-chemical action on the urinary excreta, which are for the most part rich in nitrogen.

Nervous gout is as proteiform as hysteria; there is, indeed, no functional disorder of the nervous system which may not appear under this form. In this respect the arthritic poison is closely analogous to the malarious, which induces a great variety of nervous disorders, from simple neuralgia to coma and catalepsy. The extreme grade of morbid action in urinary poisoning is seen in the coma, epilepsy, or amaurosis, developed during the progress or towards the termination of Bright's disease. The most rapidly fatal is that in which extreme mental labour determines the inflammation or irritation in a paroxysm of gout to the cerebrum, when furious delirium, mania, or encephalitis supervene. Neuralgia is an early symptom in Bright's disease. Very rightly Dr. Wood impresses upon his readers the necessity of tracing out the arthritic relations of the patient in all cases of obstinate neuralgia. Probably lead, malaria, and the urinary poison, are more frequently the causes of neuralgia than all others together; and when we consider the general pathology of neuralgia, it is to be feared that hypochondriasis and hysteria are not the only cerebral (mental) disorders induced by the gouty blood, but that even some of the more unmanageable forms of insanity may be referred to this cause.

There is one other important point in the pathology of gouty disorders, which a humoral pathology, conjoined with histological research, may strongly elucidate. The two great series of tissues affected with gouty disease are also subject to transformations and degenerations, under circumstances which point out their relationship to the same primary morbid condition. It is a well acknowledged fact, that the hereditary predisposition to gout is little amenable to the same methods of treatment as the acquired; but beyond this, our knowledge is imperfect. We know nothing of that congenital condition of the germ and of the nutrient *nisus* on which the predisposition depends, but that it is closely connected with some more recondite condition than is suspected, seems to be indicated by several facts. In the first place, the greater development of that predisposition in males is not, probably, to be attributed wholly to habits of life, or to the physiological influence of the sexual organs, but rather to the same vital force which determines the difference between the germ-cell and the sperm-cell. There is a condition of the bloodvessels, for example, which is hereditary,

* Vide vol. ix. of this journal, p. 210.

but strictly limited to males, although *transmissible* through the female; in these cases the hæmorrhagic diathesis seems to depend on imperfect contractility of the vessels. Angina pectoris is a disease of the vascular system, traceable to the gouty diathesis, but manifested almost exclusively in males: it has lately been attributed to fatty degeneration of the cardiac fibres, as well as to calcareous transformation of the coronary arteries; in either case the histological morbid condition and the sex are not without some relation. Fatty and calcareous degeneration in the muscular system is most usually met with in males. Mr. Quekett mentions a most instructive instance of this, in which, of a family of nine children, the six girls were perfectly healthy; but all the boys, on arriving at the age of three or four years, began to lose the use of their limbs. The disease was inexplicable, until one of these (the eldest) having died, the muscles were found to have undergone fatty degeneration, the brain and spinal cord being perfectly healthy.* It is not altogether irrelevant to notice another circumstance—namely, that fatty degeneration of the kidney in men is not unfrequently associated with the gouty diathesis, or with chronic gout, and that a general tendency to deposit fat (or corpulence) is associated equally with an hereditary and acquired gouty habit. Whatever may be the theoretical value of these facts, they have undoubtedly a practical bearing upon the treatment of the entire group of diseases in which there is abnormal nutrition of the tissues, inasmuch as they point out new relations to functional and structural diseases which are in close relation to the products of nutrient transformation. Nor is the theoretical value small, for in this direction—namely, the combination of vital dynamics with histological and biochemical research and pathological experience—lies the way of escape from the vicious circle to which medical science and art have been so long restricted.

As an opinion may be expected on the value of the works before us, we can confidently state that they may both be considered standard works, and interesting to the English practitioner as treatises presenting diseases under new aspects. Dr. Wood's cannot be compared, however, with such a work as Dr. Copland's, even as a compendium of the best current medical doctrines and modes of treatment. But it has gone into a third edition, and this is praise enough. In the present edition the additional pages are principally occupied with the relapsing fever described by Dr. Jenner; the dengue (unaccountably omitted in the last edition, and having points of resemblance to the relapsing fever); certain cutaneous affections, as trichosis, pellagra and lupus; and the more recent views as to Bright's disease, hæmorrhages, &c. We have not given, nor attempted to give, any abstract of the matters contained in either of these authors; we felt, indeed, that such a method of criticism is not applicable to systematic works, if it were possible to adopt it with satisfaction either to the author or the reader, which it is not. It seemed to us a more useful method to compare, critically, current pathology with present physiology and past principles of treatment, so as thereby to determine in what respects we have advanced or can advance; in what respects we have remained stationary or even retrograded. We adopted Sydenham as the best modern

* Lectures in Histology, p. 197.

instance of the school which treats disease according to the natural method; and the extracts we have given (which might have been multiplied indefinitely from other writers as well as from Sydenham) will enable the reader to judge for himself in these matters. We cannot but think that more philosophical views will arise even in our day. The phenomena of disease (to use the words of Sydenham), "if carefully collated with each other, lead us, as it were, by the hand, to those palpable indications of treatment which are drawn—not from the hallucinations of our fancy, but —*from the innermost penetralia of nature.* By this ladder and by this scaffold did Hippocrates ascend his lofty sphere—the Romulus of medicine, whose heaven was the empyrean of his art. He it is whom we can never duly praise. He it was who then laid the solid and immovable foundation for the whole superstructure of medicine, when he taught that *our natures are the physicians of our diseases*—Νούσων φύσιες ἰητροί. . . . . . Herein consisted the theory of that divine old man. It exhibited the legitimate operations of nature, put forth in the diseases of humanity. The vain efforts of a wild fancy, the dreams of a sick man, it did not exhibit.*

T. Laycock.

Review II.

1. *Mikroskopische Anatomie; oder, Gewebelehre der Menschen.* Von Dr. A. Kölliker, Professor der Anatomie und Physiologie in Würzburg. 2ter Band.—*Leipzig*, 1852. Art., *Milz.* pp. 253.

Microscopical Anatomy; or, Description of the Tissues of the Human Body. By Dr. A. Kölliker, Professor of Anatomy and Physiology in Würzburg. 2nd Volume.—*Leipzig*, 1852. Article, *Spleen.* pp. 253.

2. *Ueber runde Blutgerinnsel, und über pigmentkugelhaltige Zellen.* Von R. Remak. *Müller's Archiv.*, February, 1852. pp. 115.

On Round Clots of Blood, and of Cells containing Pigment Granules. By R. Remak. (In *Müller's Archives* for February, 1852.) pp. 115.

Nucleated cells, from $\frac{1}{2250}$th to $\frac{1}{740}$th of an inch in diameter, containing in their interior a number of blood-corpuscles, varying from one to twenty,—such are the bodies on the alleged existence of which in the spleen considerable weight has lately been laid in reference to the function of that organ. Of the signification of these blood-corpuscle-holding cells, as they have been called, two opposite interpretations are given. According to the one, the contained blood-corpuscles are *new* formations which become free on the solution of their parent cell. According to the other, on the contrary, the contained blood-corpuscles are *old*, and about to be destroyed; and preparatory to this, they are presumed to have the cell wall formed around them.

The latter is the opinion enunciated and advocated by Professor Kölliker, who has especially taken the doctrine of blood-corpuscle-holding cells under his protection.

As the first view of the signification of blood-corpuscle-holding cells has been little worked out, we shall not consider it further. Confining our

* Op. cit., vol. i. p. 16. Preface to the Third Edition of Medical Observations.

attention to the second view, we shall have to discourse of pigment cells, as alleged phases in the history of blood-corpuscle-holding cells, and of extravasations of blood as the soil in which these bodies are said to be especially produced,—extravasations of blood not in the spleen alone, but in the brain, liver, and kidneys.

In extravasated blood, then, the distinguished Würzburg professor teaches, that the blood-corpuscles, at the same time that they become smaller and darker (and round in the case of the oval corpuscles of the oviparous vertebrata) agglomerate into small round heaps, which, with some blood-plasma, become, by the formation of a nucleus in their interior and a cell-membrane on their exterior, blood-corpuscle-holding cells. When thus enclosed, the blood-corpuscles are broken up and resolved into pigment granules of a gold yellow, brown red, or black colour. Pigment cells, met with in the localities and under the circumstances above indicated, are thus to be viewed as stages, on the road to destruction, of blood-corpuscle-holding cells.

In respect to the development of blood-corpuscle-holding cells, Professor Kölliker considers it certain, that they are formed, not directly round a nucleus, but by the deposition of a membrane around a small mass of coagulated blood, like the membrane around the last subdivisions of the fecundated yolk. Whether the nucleus, which is subsequently found in these cells, without exception, exists before the formation of the cell-membrane or not, he leaves undetermined, though seemingly disposed to think that it does, and that it is the *blast* or germ of the membrane.

It may be proper here to observe, that, along with blood-corpuscles, other matters may be included in the cells; for of blood-corpuscle-holding cells, met with in an extravasation of blood in the *commissura mollis*, Professor Kölliker mentions his having found some which contained *pieces of cerebral substance!*

But it would appear that it is not in extravasated blood alone that blood-corpuscle-holding cells are met with. In the *Triton*, Dr. Kölliker has found such cells within the capillaries of the semi-transparent spleen, arranged often in linear series, and admitting of being pressed onward into the veins, so that one of these vessels may frequently be found filled with nothing but such peculiar elements. Whether this is of constant occurrence in the triton, Kölliker does not know, nor whether the same thing is to be met with in other batrachians. He can, however, affirm that in the triton, frog, toad, and black salamander, he has found blood-corpuscle-holding cells even in the trunks of the splenic and portal veins; and in Bufo cinereus, Triton igneus, and the salamander also, he has traced them in the hepatic branches of the vena portæ, as far as the capillaries of the liver. In the salamander he has even found them in the inferior cava and heart.

It was necessary, in the preceding account of blood-corpuscle-holding cells, to allude to their occurrence elsewhere than in the spleen; but the comments which we now proceed to make will have reference to them, only in connexion with their supposed subserviency to the function of that organ. And as introductory to this, we would beg the reader's attention to a brief outline of the intimate structure of the spleen, the result of an examination to which we subjected the organ some years ago.

To the naked eye, or to the eye assisted by a magnifying glass, the parenchyma of the spleen, as every one knows, is perceived to consist of two different substances: a red-pulpy looking matter, and the whitish Malpighian corpuscles.

The red pulpy matter presents indications of a subdivision into lobules, owing to the tuft-like arrangement of the vessels leading to and from the capillary network with which it is pervaded.

The Malpighian corpuscles, of a roundish shape, and about $\frac{1}{50}$th of an inch in diameter, lie imbedded in the red substance adhering to the smaller divisions of the arteries before they open into the capillary network, and not among the capillaries. In the spleen of the sheep, injected with chromate of potass and acetate of lead, the Malpighian bodies were not found penetrated by the coloured deposit, but they appeared plump and distended looking.

Examined microscopically, the red pulpy substance is found to consist of nucleated corpuscles and short nucleated fibres held together by a finely granular intercellular substance. Most of the nucleated corpuscles are destitute of an evident cell-wall; the nuclei, which are about the size of the red corpuscles of the blood, being merely surrounded by a granulous substance of irregular outline. Those corpuscles which possess a distinct cell-wall are about $\frac{1}{1600}$th of an inch in diameter. Besides their nucleus, which is similar to, though slightly larger than, that of the corpuscles destitute of cell-wall, these cells may or may not contain a little granulous matter. The nucleated fibres are fusiform, and generally straight; but individuals occur circularly coiled, the coil being maintained by a tenacious intercellular substance filling up the middle space. Besides the elements now described, larger corpuscles are met with, but in much smaller number, in the form, viz., 1st, of nucleated cells of about $\frac{1}{1000}$th of an inch in diameter, with a rather thick and not very smooth cell-wall, which resists the action of water, a large nucleus and some granules in their interior; 2nd, finely granular corpuscles of the same size, but in regard to which it cannot be very clearly ascertained whether they have a distinct cell-wall or not.

All the elements now enumerated are without colour. The proper colour of the pulp appears to reside in the intercellular substance, in which pigment granules are dispersed, partly free and partly contained in cells.

Examined with a low magnifying power, the Malpighian corpuscles present the appearance of thick-walled glandular vesicles, with contents. The thick walls are not defined and homogeneous, but are, on examination with a high power, found composed of nucleated fibres and nucleated corpuscles, similar to those of the red pulpy substance; between which, indeed, and the exterior surface of the Malpighian corpuscles, there is no very distinct line of demarcation, other than is produced by the condensation of the wall of the Malpighian corpuscles, and the absence in them of coloration.

The contents of the Malpighian bodies are nucleated granular corpuscles and nucleated cells, similar to those of the red substance, cohering together in a mass by means of a diffluent intercellular substance, and interspersed among them a few somewhat larger nucleated cells. We also observed a very pale capillary tube: but whether bloodvessel or not it was not easy to say. Dr. Sanders and Professor Kölliker appear to have made a more decided observation of bloodvessels.*

The above description, which is in general agreement with the account given by Dr. Kölliker, was drawn up from the spleen of the sheep. In the human spleen we have not found the Malpighian corpuscles entire. We have found, in their situation among the divisions of the small arteries, merely a shapeless, soft, whitish substance, composed of the same microscopical elements as the Malpighian corpuscles. That this shapeless, whitish substance was the remains of softened and broken-up Malpighian corpuscles, appears from what may be observed in the sheep's spleen—viz., the existence of all intermediate gradations between this shapeless white substance and the still perfectly entire Malpighian corpuscle. The softened remains of the Malpighian corpuscles now passed into the red pulp without any line of demarcation as regards consistence; nor was there any longer a line of demarcation as regards colour. When it is remembered that the microscopical elements of the red substance and of the Malpighian corpuscles are the same, the softening of the latter may be viewed as indicating their resolution into the red substance.

Having thus examined the structure of the spleen, we next proceeded to a comparison of what enters with what issues from the organ—i. e., a comparison of the blood in the splenic artery with that in the splenic vein.

The blood in the splenic artery was found to be similar to that in other arteries; but the blood in the splenic vein was peculiar, inasmuch as it contained, besides the ordinary blood-corpuscles, a large number of nucleated corpuscles and fibres identical with those above described as composing the red pulp of the spleen, together with free nuclei similar to those of the nucleated corpuscles. On the occurrence of these nucleated corpuscles and free nuclei, the statements as to the great number of 'colourless' or 'lymph' corpuscles in the blood of the splenic vein appear to have been founded.† Numerous small masses of several of these corpuscles agglomerated together occurred in the blood, as well as in the broken-up pulp. These elements were traced as far as the vena portæ, but in the venæ hepaticæ they had mostly, though not entirely, disappeared.

The anatomical inference which might be drawn from the facts now related is, that some of the venous radicles of the spleen have a connexion with the red pulp of the organ analogous to that which exists between the radicles of the hepatic duct and the parenchyma of the liver; whilst the physiological inference is, that the materials thus derived by the blood from the spleen may concur in fitting it for the secretion of the bile.

To return to the question of blood-corpuscle-holding cells.—We have above seen that, according to Dr. Kölliker, blood-corpuscle-holding cells are met with in the batrachians, within bloodvessels. To this it is to be added, that they have been found, by Dr. Ecker, in the blood of the splenic

* We take this opportunity to mention that in a grayish pulpy matter, exactly similar in general appearance to the contents of the Malpighian corpuscles, which was evacuated from one of those vesicular granulations which are often found on the palpebral conjunctiva, in chronic ophthalmia, capillary vessels, filled with red blood-corpuscles, were discovered among the cells, in different stages of development, composing the matter.

† Funke. Ueber das Milzvenenblut. Henle Zeitschrift, Bd. 1. (Neue Folge) 1851.

vein of calves and swine sometimes, and of the horse once; in the blood of the splenic vein of which animal also, a single one was once found by Dr. Funke, who had before frequently met them in the spleen itself. We shall not, however, enter into the consideration of such intravascular cells, further than to remark, that supposing blood-corpuscle-holding cells to be formed in the parenchyma of the spleen, they will as readily make their way into the splenic vein as we have just seen the other constituent elements of that substance do.

The basis of Kölliker's main theory is, that extravasation of blood into the parenchyma of the spleen is of such constant occurrence, that it may be viewed as much a physiological, as a pathological phenomenon. That extravasations of blood may and do take place in the spleen, of course there can be no doubt; but we must confess, that we do not see what evidence there is for any such constant occurrence of extravasation, as that which Kölliker assumes. We cannot say that we have observed appearances in the red pulp of the spleen, which could be put down as unequivocal indications of extravasation having occurred during life. A piece cut out for examination from such a soft vascular parenchyma as that of the spleen, must necessarily be infiltrated with blood from the cut vessels.

The condition adduced by Professor Kölliker, on which the alleged extravasations depend, is "a kind of stagnation of blood, of course without stoppage of the circulation"! A stagnation of blood without stoppage of the circulation is rather a contradiction in terms; but if it be meant that there is congestion of blood-corpuscles within the vessels of the spleen, we can only say, that such a condition, though quite possible and likely enough, is not, and cannot be, an observed fact; and therefore is, in the present instance, of no more value than an hypothesis erected to support the other hypothesis, that extravasation of blood occurs so frequently in the spleen as to constitute a normal process.

But there are the blood-corpuscle-holding cells in the parenchyma of the spleen, at any rate, it may be said. This, however, is a point not so fully established. Many observers have failed to detect them; among others, Dr. Remak, of Berlin, who first drew attention to the subject, but who has since denied that blood-corpuscle-holding cells have any real existence, affirming that what has been taken for such are merely round clots of blood, and pigment cells. *And our own attempts to discover blood-corpuscle-holding cells have ended in a similar negative result.* To say nothing of our former investigation of the spleen, which was undertaken specially to ascertain what physiological relations exist between it and the corpuscles of the blood; in the course of which we never noticed anything like blood-corpuscle-holding cells, we have recently devoted several sittings exclusively to a search for blood-corpuscle-holding cells in the spleen. We have examined for this purpose three spleens of the frog (one frog was newly caught, one had been kept without food for a week, and the third for about three or four weeks), two spleens of the sheep, one spleen of a bat (Kölliker acknowledges never having found blood-corpuscle-holding cells in the bat), two human spleens, and one rabbit's spleen.

As the subject from which Professor Kölliker has taken his examples of blood-corpuscle-holding cells for delineation, we in particular subjected the spleen of the rabbit to a very searching investigation, but without discovering a single object which could be called a blood-corpuscle-holding cell.

We saw, indeed, numerous aggregations of red corpuscles, such as present themselves in a viscid plasma; many being rounded, and of the size ascribed to blood-corpuscle-holding cells. But, of course, we cannot, with Dr. Remak, suppose that Professor Kölliker could mistake such aggregations for 'blood-corpuscle-holding cells.'

We found numerous pigment granules, both free and enclosed; but nothing like a transition from *blood-corpuscle-holding* to *pigment-holding* cells.

Under these circumstances, it may be useful to examine somewhat closely the description which has been given of blood-corpuscle-holding cells.

In the mammifera, it is said that blood-corpuscle-holding cells are not readily detected in the spleen, on account of the small size of the red corpuscles, and the readiness with which they give out their colouring matter. Small size of the red corpuscles! Surely in the present state of microscopy, there can be no difficulty on this score. Again: the blood-corpuscle-holding cells must be looked for without the addition of water to the piece of spleen under examination, as the water dissolves the cell-membrane, and bleaches the corpuscles. The cell-membrane must thus be different in chemical composition from that of ordinary cells. However this may be, we have succeeded as little without as with water. As to bleached corpuscles, an eye familiar with their appearance would have no difficulty in seeing such within or without a cell with a good microscope and good light. Kölliker's figures of blood-corpuscle-holding cells, from the spleen of the rabbit (here copied from page 267), might, for anything delineated to the contrary, be simply blood-corpuscles, embedded in small oval or round masses of lymphy substance.

a

We have above spoken of the occurrence of small aggregations of nucleated corpuscles in the pulp of the spleen, and in the blood of the splenic vein. Had those aggregations had a well-defined outline, and presented indications of a cell-wall, they might have corresponded with the above description. In connexion with this point, we may observe, that Professor Kölliker describes the nucleated fibres frequently met with coiled up, as contained within a cell, which, he says, becomes dissolved on the addition of water; but the annexed figure (copied from page 257) gives no indication of a cell-wall; and we never could determine the existence of anything more than a tenacious intercellular substance maintaining the fibre in its coil, and filling up the interior, and which we have distinctly observed in various degrees of laceration.

b

In the frog tribe, Dr. Kölliker says that the blood-corpuscle-holding cells are beautifully seen; and that, on account of the large size of the blood-corpuscles, the metamorphosis of the latter into pigment granules admits of being most distinctly traced. The existence of pigment cells in the spleen of the frog, as well as in the spleen of mammalia, is very certain; but we have not, as above stated, been more successful in discovering blood-corpuscle-holding cells in the spleen of the frog, than in that of mammalia: and in regard to the pigment cells, we have to remark, that their average size was only equal to that of the red blood-corpuscles of the animal; and therefore unfitted to be a receptacle for them; some, indeed, were larger, but many were much smaller. Moreover, it is to be observed, that the walls of these pigment cells were not so susceptible to the action of water as those of the blood-corpuscle-holding cells are represented to be.

Though insisting so much on the existence of blood-corpuscle-holding cells, and their office of enclosing blood-corpuscles, preparatory to the breaking up of the latter, Prof. Kölliker still would have it understood, that he does not lay too much stress on the formation of a cell-wall around the little heaps of blood-corpuscles to be destroyed, as he finds that the blood-corpuscles may be broken up in the spleen, without their having been previously so enclosed in cells. And though he considers the spleen an organ in which blood-corpuscles are broken up, he admits that is not the only organ.

With these concessions, Dr. Kölliker virtually gives up the whole point of his theory of the function of the spleen; and we may conclude that *if blood-corpuscle-holding cells have a real existence* in that organ, it must be under unusual conditions, such as have never occurred to us; and that their physiological signification remains to be determined; for, assuredly, there are no grounds for admitting that attributed to them by Professor Kölliker.

T. Wharton Jones.

Review III.

Ueber Krisen und Kritische Tage. Von Dr. L. Traube.—*Berlin,* 1852.
On Crises and Critical Days. By Dr. L. Traube.

The views, which our most ancient authors, and principally *Hippocrates* and his commentator *Galen,* held on crises and critical days, have been frequently rejected in modern times as perfectly groundless; but since we have acknowledged again accurate observation of the process of nature to be one of the fundamental principles for the progress of medicine, we have learned that there is some truth in the old doctrine. A very interesting essay on this subject has been lately published by Dr. Traube of Berlin; it is the result of the careful observation of a large number of febrile cases, in which he had accurately fixed the commencement of the disease, and had marked twice on every day (during the time of remission and that of exacerbation) the temperature under the tongue, the pulse and other symptoms of importance. From the analysis of his cases he draws certain inferences which we shall proceed to quote and to remark on.

"I. *Fever consists essentially in an increased temperature of the blood.*"

It is scarcely necessary to remark here, that by the word '*fever*' the author does not mean a special disease, but only that complex of symptoms, which accompanies inflammatory diseases, acute exanthemata, &c., &c., and to which the Greeks very properly gave a distinct name πύρεξις, πυρετός, sometimes simply πῦρ, avoiding by this the confusion between '*fever*' as a real disease, and '*fever*' as a mere symptom or shadow of many different diseases.

The above-cited opinion of Dr. Traube is quite in accordance with the most ancient of all theories on fever, that of Hippocrates, Galen, Avicenna, and others—"Essentia vero febrium est—præter naturam caliditas" (ἡ παρὰ φύσιν θερμασία). Traube thinks that all the other symptoms accompanying the increase of temperature (fever-shivering, frequency of pulse, thirst, &c.) are caused by the abnormal heat of the blood, and he promises to adduce the arguments for this view, in a future and longer work. For the present, however, we do not venture to look at one of the symptoms as producing all the others, but would consider the whole group of them, called *'fever'* (the increased temperature not less than the frequent pulse, the burning thirst, &c.), as the effect of a common cause, which we do not yet accurately know, but which we must look for in the reciprocal processes and changes taking place within and between the solid parts of the different organs and the *altered* blood in their capillaries, under the special influence of the nervous system.

"II. *The change from the abnormally increased to the normal temperature takes place either abruptly** (*within* 12—36 *hours*), *or gradually within a larger or smaller number of days.*"

An example of each kind of termination will clearly show the meaning of this inference. For the abrupt mode we may cite the following

"Case of pleuro-pneumonia in a man, aged 40 years; admitted during the exacerbation on the third day of the disease:

Day of Disease.	Exacerbation.		Remission.	
	Pulse.	Temperature.	Pulse.	Temperature.
III.	104	105°44	91	105°08
IV.	99	105°35	92	104°54
V.	97	105°08	80	101°66
VI.	72	99°95	56	98°78" (p. 6.)

For the gradual decrease we will cite,

"A mild case of typhoid fever in a weakly-framed female patient, aged 20 y.; the commencement of the disease on December 16th, at about 8 o'clock P.M., with intense shivering; admitted on the evening of December 26th—i.e., toward the end of the 10th day of the disease. The treatment was merely expectant.

Day of Disease.	Remission.		Exacerbation.	
	Pulse.	Temperature.	Pulse.	Temperature.
X.	..	..	112	105°98
XI.	100	102°74	108	103°64
XII.	100	101°48	104	104°54
XIII.	92	100°94	100	103°82
XIV.	92	100°4	100	104°18
XV.	84	100°22	100	103°82
XVI.	84	99°68	96	103°46
XVII.	80	99°14	88	102°74
XVIII.	88	98°78	80	102°2
XIX.	92	98°24	80	100°04
XX.	72	98°24	68	99°32
XXI.	76	98°645	68	99°50
XXII.	68	98°78	80	99°50"
XXIII.	80	98°42		(p. 6.)

* In the German original, 'sprungweise,' i.e. with a jump.

Several other cases are given for the explanation of § ii., the truth of which every one has opportunity of testing in daily practice. Pneumonia, acute cynanche tonsillaris, simple erysipelas, furnish frequently instances of the former, typhus and typhoid fever of the latter manner of termination.

We must, however, not understand Traube to assert that there does not exist another termination of acute diseases into health besides the two just described; in § viii. he speaks of a third kind as standing between the two, which he might have properly mentioned in this place, and which, in our as yet limited experience, has been found to be the most frequent one.

The abnormal heat becomes almost suddenly, within 12—36 hours, considerably diminished, but the temperature shows still an increase over the healthy standard of the individual, and this only gradually disappears during the subsequent decline of the disease.

Amongst 6 cases of pleuro-pneumonia and pneumonia, in which we have noted the temperature, in 2 only did it sink to the normal standard within 24—36 hours; in the 4 others it decreased considerably within a short space of time, but the remaining increase disappeared slowly. In one case, for instance, between the end of the 5th and that of the 6th day the temperature decreased from 105°5 to 98°5, then gradually during the following six days it fell to 97°0, the normal standard of that individual. To make this third manner of termination more evident, we shall give the outlines of one of these cases, which offers also some interest for other questions connected with the subject of crisis. The patient was a sugar-

Day of Disease.	Remission.			Exacerbation.			Urine.			REMARKS.
	Pulse.	Temp.	Resp.	Pulse.	Temp.	Resp.	Quantity in 24 hours	Specific Gravity.	Quantity of Lithic Acid in 24 hours.	Stool—Urine—Perspiration.
							Oz.		Grains.	
III.	..	..	..	110	105°0	30				
IV.	105	104°5	34	112	104°5	28	46½	1·0195	12.2	One stool.
V.	109	104°5	28	118	106°2	29	40	1·023	16·5	One stool.
VI.	90	101°0	20	116	104°0	28	35	1·0205	16·5	No stool.
VII.	90	105°0	30	108	106°4	34	12	1·018	..	Two fluid stools.
VIII.	108	105°5	28	124	106°4	.	10	1·0205	..	Two fluid stools; sedmt. of lithates.
IX.	110	105°5	26	120	106°0	28	31	1·0168	29·0	Much perpiration; no sediment.
X.	80	101°0	24	82	100°0	20	39½	1·0185	24·0	Much persp.; no stool; no sediment.
XI.	75	100°0	18	80	100°0	..	38	1·023	..	Two fluid stools; no sediment.
XII.	..	..	..	80	99°8	..	39	1·0195	22·5	Slight persp.; sediment of lithates.
XIII.	70	..	20	75	99°8	18	..	..	..	Two stools; no urine saved.
XIV.	70	99°0	..	74	..	18	..	..	..	No urine saved; perspiration.
XV.	76	99°8	..	82	100°0	17	45	1·0175	16·5	One stool; sediment of lithates.
XVI.	72	99°6	..	80	99°8	18	48	1·0165	18·0	Perspiration; no sediment.
XVII.	72	99°6	16	70	99°0	17	52	1·0165	..	Perspiration; no sediment.
XVIII.	68	..	..	..	..	..	42	1·023	..	Perspiration; large sediment.
XIX.	..	98°8	..	68	99°0					
XX.	65	..	..	65	98°8	..	49	1·0185	14·5	Perspiration; sediment.
XXI.	62	..	..	68	98°8	..	40	1·024	..	No perspiration; no sediment.
XXII.	62	96°6	16	65	97°6	16				
XXV.	62	98°2	..	66	98°6	..	40½	1·025	10·5	Sediment.
XXVI.	62	..	..	62	97°8					
XXVII.	60	98°2	15	64	98°2	..	37	1·0195	..	Perspiration; no sediment.
XXVIII.	60	97°8	15	62	98°2	16	..	..	7·2	Perspiration; no sediment.
XXIX.	58	97°8	..	60	97°8					
XXX.	..	..	..	58	97°8	..	38	1·0195	..	Slight perspiration.
XXXVI.	56	97°8	16	.	97°6					
XLII.	..	..	..	60	97°8	..	36½	1·016	7·0	

The urine was more or less acid as well during the whole of the disease as during the convalescence.

baker, aged 22 y., of phlegmatic temperament, well-developed muscular system, but of a rather hydræmic appearance (probably through his lowering occupation); three or four days after having been exposed to noxious influences he was seized with rigors on January 3rd in the morning, was admitted into the hospital on January 5th—i.e. in the first part of the third day of the disease.

On 6th day, in the morning, patient was much better in all symptoms; the dulness of percussion and bronchial respiration were confined to the lower part of the right side of the back, reaching upwards only to the middle of the scapula; on the 7th day, in the morning, the local phenomena had considerably extended as well upwards as towards the front; from the 9th day, gradually, the morbid phenomena disappeared, but on the 24th day, slight dulness with decreased vesicular respiration was still perceptible.—The treatment had consisted in small doses of antim.-pot. tartr. with nitr. of potash and opium in the beginning,—in hydrochlorate of ammonia (scrup. dim. quater die) after the 11th day of the disease.

"III. *The more rapid decrease of temperature is very often accompanied by considerable perspiration, less frequently by urinary sediments of lithates. In some cases both phenomena appear after the decrease of temperature, in others they do not appear at all.*"

Traube promises a fuller detail on these questions in his larger work. It is to be hoped, that he will then take also into account, not only the salts of the lithic acid, but also the lithic acid itself, which he does not at all mention in the present essay. From our own observation, which is, however, not yet sufficient to form a certain opinion, the excretion of the *lithic acid* appears closely connected with the process. In two cases of pneumonia and two of rheumatic fever the quantity excreted in 24 hours was found much increased during the days next to the critical change, as well before as after this, even to 29 grains, which is at least three times more than during the state of health (8 grains in 24 hours—*Becquerel*). In the one case of rheumatic fever and the one of pneumonia, in both of which the recovery was protracted and the temperature sank after a sudden considerable decrease only gradually to and under the normal standard, the quantity of lithic acid in the urine remained likewise abnormally great till the time of recovery. The numbers of the one case have been marked on the previous table. It may be also clearly seen from that table, that the appearance of the sediment of lithates in the urine does not depend on the quantity of lithic acid contained in the urine; on the 9th and 10th day 29·0 and 24·0 grs. of lithic acid were excreted in *only* 31 and $39\frac{1}{2}$ ounces of urine, and in spite of this no sediment had appeared; on the 19th and 25th day it amounted only to 14·5 grains and to 10·5, which were contained in 49 and in $40\frac{1}{2}$ ounces, and yet both times a large sediment of lithates had been formed soon after the urine was discharged.

"IV. *With the abrupt decrease of temperature, leading to recovery, a speedy and considerable diminution of the abnormal frequency of the pulse is almost always coincident.*"

To this inference Traube adds, that, on the other hand, sudden and considerable decrease of the pulse is *never* observed, but with coincident remarkable sinking of the morbid heat, excepting in those cases which are under the influence of large doses of digitalis, and those affected with idiopathic disease of the brain and medulla oblongata. Although we have not yet observed a sudden decrease of the morbid frequency of the pulse, without coincident decrease of the abnormally augmented temperature; yet we think we ought to be cautious in stating such a coincidence as a rule without exceptions.

"V. *The sudden decrease of temperature may take place ere the process of inflammation, which was accompanied by the decrease of temperature, has ceased to spread.*"

As an instance of this, Traube gives a case of erysipelas faciei, in the decline of which the temperature considerably decreased on the beginning of the fifth day of the disease, although the local erysipelatous process had at the same time extended from the left to the right side of the face.

Without, as yet, contesting the assertion contained in this inference, we cannot help remarking, that we should have wished to see another case for its corroboration, as it is well known, that with the disappearance of the local phenomena in erysipelas, not only the temperature but also all the other constitutional morbid symptoms strikingly decrease.

"VI. *In protracted acute diseases, where the abnormal heat gradually disappears, towards the end the type of the fever becomes frequently that of the febris hectica, that is, the temperature is, during the period of remission, almost normal, or even abnormally low, but is considerably increased during that of the exacerbation of the same day.*"

The following table of a severe case of typhoid fever clearly shows the meaning of this paragraph:

Day of Disease.	Time of Remission.		Time of Exacerbation.	
	Pulse.	Temperature.	Pulse.	Temperature.
XIV.	108	105°08	104	104°81
XV.	100	103°28	100	105°62
XVI.	104	103°10	100	104°54
XVII.	101	104°0	104	104°67
XVIII.	104	103°82	104	104°90
XIX.	96	102°92	112	104°72
XX.	100	102°11	96	103°82
XXI.	92	103°86	104	104°54
XXII.	100	104°13	104	104°81
XXIII.	88	101°12	92	102°38
XXIV.	84	98°06	100	103°19
XXV.	84	98°37	92	102°83
XXVI.	76	98°51	108	104°36
XXVII.	80	99°14	96	104°90
XXVIII.	76	99°32	88	102°92
XXIX.	76	99°50	84	101°75
XXX.	68	96°755	76	101°03
XXXI.	76	98°37	84	101°03
XXXV.	84	99°41	76	98°19
XXXVII.	80	98°87	76	99°23

The same phenomenon we find well marked in one of two cases of *typhoid fever*, attended by Dr. Parkes, in University College Hospital, and in four out of six cases of the same disease attended by us in the German Hospital. The three of the eight cases in which the difference between the morning and evening temperature is not so striking, are all mild ones; in one of the five other cases, the typhoid fever was likewise mild, but the subject was debilitated by previous disease and starvation; in the four remaining cases the typhoid fever was severer, attended in one by frequent profuse perspiration and slight diarrhœa; in two others, by very copious diarrhœa during more than seven days; in the fourth, by considerable diarrhœa and loss of blood in the stools. The same phenomenon we observed lately in a case of peritonitis from a perforating ulcer of the small intestines, in which, during several weeks, almost no food was allowed through the mouth. From these and other cases we are inclined to look at this phenomenon as a consequence of starvation, or rather of the want of matter to effect and to undergo the warmth-creating processes, partly from the deficiency of nourishment taken and digested; partly from the accelerated loss of substance during the course of the disease: and we quite agree with Traube in considering it as an indication for support by diet and medicine, as far as the circumstances will permit it.

As these cases affirm also some other points of interest for our subject, we will shortly give the outlines of the two cases communicated to us by Dr. Parkes, the first of them showing a very considerable difference of the morning and evening temperature during several days; the second showing only a slight one.

Day of Observation.	Morning.			Mid-day.			Night.		
	Pulse.	Temp.	Resp.	Pulse.	Temp.	Resp.	Pulse.	Temp.	Resp.
XIII.	...	...	...	104	101°5	...	100	101°0	
XIV.	100	...	40	98	97°0	28	108	102°5	24
XV.	92	...	34	116	97°5	26	92	101°0	28
XVI.	98	99°0	30	85	94°0	24	108	97°0	24
XVII.	112	93°0	28	112	99°0	28	112	101°5	30
XVIII.	92	94°0	26	...	...	...	104	102°0	26
XIX.	96	100°0	36	84	98°6	27	104	102°0	24
XX.	96	96°0	28	96	98°0	24	106	103°0	28
XXI.	100	98°0	24	75	97°0	28	94	102°0	36
XXII.	90	96°5	28	84	98°5	28	116	...	32
XXIII.	84	96°5	20	80	96°0	24	110	101°5	30
XXIV.	76	96°0	20	74	97°5	22	104	101°0	30
XXV.	75	97°0	28	...	...	...	98	100°5	
XXVI.	90	98°5	28	84	98°5	28	98	98°5	

The patient was a woman, aged 25 years; considerable diarrhœa had been present from the time of admission into the hospital. During the night preceding the 17th day of the disease, a large quantity of blood was passed with the fluid stools; a smaller quantity during the course of the 17th day.

After the 18th day the diarrhœa ceased; after the 26th day of the disease, the difference between the morning and evening temperature was very slight; the convalescence appeared fully established, only a slight cough continuing for a few days longer.

Dr. Parkes' second case was a very mild one, in a male patient, aged 35. In the beginning there was some diarrhœa, well-marked rose-spots appeared between the 5th and 15th day; at first rather copiously, then sparingly; they had entirely disappeared on the 22nd, when the patient could be considered as convalescent.

Day of Observation.	Morning.		Night.	
	Pulse.	Temperature.	Pulse.	Temperature.
V.	88	101°5	84	102°1
VI.	76	101°0	88	101°5
VII.	88	101°5		
IX.	98	99°5	...	100°5
X.	92	98°5	84	102°0
XI.	80	99°5	86	102°0
XII.	78	100°5		
XIII.	76	100°5	84	100°0
XIV.	74	99°0	80	99°5
XVI.	84	98°5	68	98°5
XVII	76	98°5	78	98°5
XIX.	..	98°5	60	97°5
XX.	80	98°5	...	97°5
XXII.	72	98°0	76	98°5

"VII. *If in acute diseases the abrupt sinking of temperature, leading to recovery, begins, as it generally does, within the first fortnight, then it is always either on the 3rd, or 5th, or 7th, or 9th, or 11th day, that this occurs.*"

Of the 52 cases analyzed, the temperature suddenly decreased in 30 cases; in 2 of these 30, the beginning of the disease could not be accurately fixed; in 1 the change took place on the 17th day; of the remaining 21, in 4 cases on the 3rd day, in 9 on the 5th day, in 11 on the 7th day, in 2 on the 9th day, and in 1 on the 11th.

Almost always, the sinking began within the 24 hours of an *odd* day, only 4 times between an *even* and *odd* day, never on an *even* day.

Traube thinks himself, according to inference IV., entitled to corroborate § vii. by 32 other cases of pleuro-pneumonia, in which he had not only noted the abrupt decrease of the frequency of the pulse, but that of the temperature.

It must be remarked here, that by the word '*day*' is not meant the period of 24 hours beginning after 12 o'clock at night, but (according to the example of Galen) "*a day of the disease,*" i.e. a period of 24 hours, beginning with the appearance of the first symptoms of the disease. A patient, for instance, went to bed on the 19th of November in the feeling of health; he awoke on the morning of the 20th November, at 8 o'clock, with uneasiness, followed soon by fever-shivering;—the first day of the disease is considered in this case to commence, not at 12 o'clock in the night, but at 8 o'clock A.M. Every one will feel immediately how difficult, and frequently impossible, it is to ascertain at what time of the day the first symptoms of the disease made their appearance; and yet this is indispensably necessary, if the observation is to be of any value for the proposed question. Amongst 62 acute cases, into which we lately accurately inquired with this purpose, in 18 only could we ascertain the commencement of the disease; in 5 of these cases the notes on the temperature are not sufficient to be analyzed for the present subject; of the remaining 12, the change took place rapidly in 8 cases, and of these—on the 5th day in 2 cases; between the 5th and 6th day in 1 case; on the 7th day in 2 cases; between the 7th and 8th day in 1 case; on the 9th day in 1 case; between 9th and 10th in one case.

The difficulty of accurately tracing the disease to its very commencement is still greater than in private practice, in those patients who can be made subjects of such examinations, as they are generally not accustomed to think much of their health, and to care for trifling uneasiness; and yet we all know that severe acute diseases frequently begin with a feeling of slight indisposition. And further, even if we know the hour when the first rigor occurred, are we always entitled to consider this as the outset of the disease? If we observe ourselves, or inquire from our friends, we frequently find that the fever-shivering, if there have been any, had been preceded for a greater number of hours by languor, uneasiness in the head, and other morbid symptoms. Quite in accordance with this is the observation of *Baerensprung*,* that the *temperature is already increased before the commencement of the rigor.* Once only we had the opportunity of examining almost immediately (about 8 or 12 minutes) after the beginning of a rather vehement shivering, in the case of a severe angina tonsillaris, which a female convalescent had contracted during her residence in the hospital. The temperature was 103°5 under the tongue, and 103°0 in the axilla, to which height we can scarcely suppose it had risen within 8 to 12 minutes; the normal temperature of that individual before and after the attack of angina was 96°—97°5. This statement of Traube's requires, therefore, additional and rigorous testing.

"VIII. *Not rarely during the decline of acute diseases, on the 5th, 7th, 9th, or 11th day, a sudden and remarkable sinking of temperature spontaneously takes place, which, though not leading immediately to recovery, is followed by a considerable and permanent decrease of fever. Never, as yet at least, have I met with such an occurrence on one of the intermediate* EVEN *days.*"

This would constitute a third manner of termination, to which we previously alluded. Five of the thirteen cases mentioned in the preceding paragraph belong to this head, but also of the eight others, four might be mentioned here, as after the abrupt sinking of the abnormal heat a small increase remained for several days longer. Of the five cases not analyzed in the preceding paragraph the change took place in one between the 5th and 6th day, in one on the 7th, in one between the 7th and 8th, in one on the 9th, in the fifth of the cases (erysipelas faciei et capitis) the decline was rather uncommon. The temperature decreased between the 7th and 8th days from 105°0 to 100°0, rose again on the 9th to 105°5, sank then on the 12th to 99°5, and in the following four days to 97°0. It appears to us, however, that the change taking place on the 12th day must not

* Müll. Archiv., 1852, pp. 217—286.

necessarily make us look at this case as at a proof against the inference of § viii., but the decrease after the 7th day may be considered as an incomplete crisis, the sudden increase on the 9th as a relapse, the crisis of which occurred on the 12th day—i. e., the 3rd day of the relapse. The following outlines of the case will permit every one to form his own opinion.

The subject was a rather delicate young man, aged 22; fever-shivering in the afternoon of June 1st; admitted on June 5th during the morning—i. e., towards the end of the 4th day, when the local erysipelatous affection was limited to a part of the right side of the face, gradually spreading during the three following days over the right ear and right side of forehead; on the 8th day, local symptoms slightly diminished, constitutional almost disappeared; on the 9th day, and on the two succeeding, spreading of the local affection to the left side, which gradually disappeared after the 12th day. Treatment expectant with saline draughts (Sodæ sulph. ℥ss., Aq. font ℥ij.) on the 5th, 6th, 9th, and 10th days.

Day of Disease.	Time of Exacerbation		Time of Remission.		Urine.			REMARKS.
	Pulse.	Temp.	Pulse.	Temp.	Quantity in 24 hours.	Spec. Grav.	Lithic Acid in 24 hours.	
					Oz.		Grs.	
V.	105	102°5	109	103°5	36	1·0185	..	One stool.
VI.	109	104°5	104	104°0	12	1·0215	..	Three fluid stools.
VII.	106	105°5	96	105°0	38	1·0205	26·5	No stool.
VIII.	85	100°0	84	99°5	44	1·019	24·5	Profuse persp.; sedmt. of lithates; 1 stool.
IX.	86	99°0	108	105°5	..	..	..	No urine saved; 1 stool.
X.	112	105°5	104	105°0	10	1·0225	..	Four fluid stools.
XI.	110	105°0	98	104°5	11½	1·024	..	Three fluid stools.
XII.	105	105°5	94	99°5	40	1·0205	29·5	No stool; perspiration.
XIII.	92	98°5	88	98°0	41	1·021	25·0	No stool; much persp.; slight lith. sedmt.
XIV.	90	98°0	..	..	..	..	..	No urine saved; 2 stools.
XV.	85	97°5	82	97°0	34	1·024	..	Two stools; much perspiration.
XVI.	80	97°4	76	97°0	39	1·018	..	Much perspiration.
XVIII.	76	97°0	74	97°2	42	1·0195	10·5	One stool; slight persp.; slight sediment.
XXI.	75	97°0						

"IX. *If during the decline of an acute inflammation attended by fever the increase of temperature is disappearing abruptly, and if it began to do so on one of the above-named days* (§ vii.), *the process of inflammation at the same time ceases to spread.*"

The best proof for this proposition may be found, according to the author, in cases of pneumonia confined to the upper lobe; after the sudden disappearance of the morbid heat, the dulness of percussion *never* increases in circumference or intensity; on the contrary, all the physical symptoms indicate a speedy resorption of the inflammatory exudation. On this point we have no evidence to offer.

"X. *The inflammation can, however, continue* (*though generally only for a short time*) *after the disappearance of the abnormal increase of temperature, in that part of the organ which was already previously affected, and in this continuance may remain a predisposing cause for a later spreading of the inflammation.*"

The author, in explanation of the first part of this paragraph, relates a case of *angina faucium*, in which the temperature sank during the 5th day from 103°28 to 99°32, and on the following day even under the normal mean of that individual, to 97°52, although in the local condition of the fauces, no change was observed before the second half of the 6th day, and, "even on the 10th day the inflammation had not yet entirely ceased."

"XI. *The abrupt disappearance of the increase of temperature (within 12–36 hours), in the decline of acute diseases, is in many instances not immediately succeeded by the normal degree of warmth, but by an abnormally low one, which only gradually passes into the normal state. The same is very often observed concerning the frequency of pulse.*"

This remark, as far as it regards the temperature, is in accordance with the experience of *Baerensprung* (l. c.), and we have had opportunity of corroborating it; more often, however, we have observed the decrease according to the manner described in § viii., and alluded to in the note to § ii.; in that case an abnormally low temperature frequently followed during the latter part of convalescence, and principally in weak people, or after a more protracted decline of the disease. Traube is much inclined to generalize the inference contained in § xi., partly induced by his own observation, partly by the view, based on *Chossat's* experiments on 'inanition,' that after the fever-exciting cause has disappeared, in consequence of the want of nutriment supplied during the fever, the normal medium of animal warmth cannot be produced; he considers, therefore, the temperature during the pyrexia as the product of two factors: the one tending to increase, the other to lower, the animal warmth, the former being the effect of the fever-exciting cause, the latter that of the insufficiency of food.

As to the frequency of the pulse, we have often observed it during recovery below the mean of the individual, but in several cases, where the temperature was too low, we found the pulse to be weak and more frequent than during health.

"XII. *There are probably two kinds of critical excretions:* (a) *such as form the cause of the sudden disappearance of fever,* (b) *such as are to be considered as the mere consequence of this disappearance.*"

Previously to examining the different excretions, Traube refers to the definition of '*crisis.*' The one given by Galen, in his third book on 'crisis,' is: "Crisis is called only the sudden change into health" (perhaps better, "towards health"). "This in general takes place through some manifest excretions or remarkable abscesses. And such like excretions and abscesses are preceded by an unusual ('not a little') perturbation in the body of the diseased." (*Μόνη τοίνυν ἁπλῶς κρίσις ἡ εἰς ὑγείαν ὀξύῤῥοπος μεταβολὴ προσαγορεύεται, καὶ γίνεται μὲν πάντως ἐπὶ φανεραῖς τισιν ἐκκρίσεσιν ἢ ἀξιολόγοις ἀποστάσεσιν. . . . . Ἡγεῖται δὲ τῶν ἐκκρίσεών τε καὶ ἀποστάσεων τούτων οὐ σμικρὰ ταραχὴ κατὰ τοῦ καμόντος σῶμα.*) Although Traube highly values this definition, he has three objections against it. He does not admit the existence of a *crisis of the disease*, but only of the *fever* (pyrexia) accompanying the disease, as there does not occur a sudden disappearance of all the morbid phenomena, but only of the abnormal heat, by the decrease of which the principal condition for a speedy recovery is given.

In favour of Galen it can be said, however, that the words μεταβολὴ εἰς ὑγείαν might be interpreted as meaning the change into the way towards health; the Latin translation in the edition by Kühn (vol. ix. p. 703), saying, "ad sanitatem conversio."

Traube's second objection is, "that complete crisis may take place, without any remarkable phenomena in the different apparatuses of secretion."

In corroboration of this objection, he gives the outlines of a case, which we shall copy here, in order that every one may form his own opinion.

"Case of *angina faucium* in a strong girl, aged 21. The disease began in the morning with pain in the left submaxillary region, which was already on the following day so intense, that patient could not move the lower jaw. When admitted during the exacerbation of the 7th day the condition was as follows: The space to which the lower jaw can be removed from the upper one, measures scarcely ¾ of an inch; tonsils much swollen and very painful; submaxillary lymphatic glands of left side enlarged and tender; constant flowing out of saliva, great difficulty in deglutition, not in respiration. Upwards to the 9th period of remission the treatment was merely expectant; only when at that time, in spite of the decrease of temperature, spreading of the inflammation from the lymphatic glands to the adjacent intercellular tissue had been observed, twenty leeches were applied to the left submaxillary region; internally also then no medicine was administered.

Day of Disease.	Exacerbation.		Remission.	
	Pulse.	Temperature.	Pulse.	Temperature.
VII.	...	...	100	101°12
VIII.	92	102°74	104	104°0
IX.	76	101°8	84	102°11
X.	64	99°64	68	100°04
XI.	64	99°05	64	99°68

"The abrupt decrease of temperature, which in this case took place on the 9th day, was neither accompanied by perspiration nor by urine disposed for the deposition of sediments of lithates; also after the decrease of temperature none of these phenomena occurred." (p. 21.)

In defence of the definition of *Galen*, we must remark that he does mention, not only manifest excretions, but also remarkable abscesses (ἐπὶ φανεραῖς τισιν ἐκκρίσεσιν ἢ ἀξιολόγοις ἀποστάσεσιν), and we are inclined to think, that he does not mean merely abscesses in the common sense of the word, but also visible depositions, eruptions, &c., which in some diseases accompany the change toward health; as, for instance, the appearance of the local phenomena (redness, swelling, bullæ, &c.,) in the erysipelatous process, of those in the process of herpes zoster, as that of an eruption on the skin in several other morbid processes, accompanied by pyrexia (exanthematic diseases), as also in some others, that of inflammatory infiltration of lymphatic glands and cellular tissue. Interpreting Hippocrates and Galen in this manner, we are inclined to suggest whether "the spreading of the inflammation to the adjacent cellular tissue" in the above given case was not a so-called critical deposition or an ἀξιολογος ἀπόστασις. At any rate, we can scarcely consider ourselves entitled to conclude, merely from the absence of perspiration, or of sediments of lithates, &c., that during the period of change no critical excretion of any kind has taken place.

The last assertion is thus stated:

"Finally, I consider the perturbation in the nervous system preceding the crisis as being likewise unimportant, even more so than the critical excretions, as I have extremely seldom observed it." (p. 21.)

In favour of Galen we may say, however, that he does not speak merely of "a perturbation in the nervous system," but "of not a little perturbation in the body of the diseased" (*οὐ σμικρὰ ταραχὴ κατὰ τοῦ καμόντος σῶμα*), by which, according to our opinion, he may have meant the fever-heat, the frequency of pulse, the restlessness, &c.; and certainly every one of us has sufficient opportunity to remark such symptoms of perturbation of the system as frequently preceding in the stadium acmes of acute diseases the more or less sudden change towards health.

Traube's definition of crisis is the following:—

"*Crisis* is that termination of acute diseases attended by pyrexia, in which the change to convalescence is introduced by the sudden disappearance of the pyrexia (Fieber), i. e. of the abnormally high temperature. Opposite to this stands the *lysis*, in which the increase of temperature disappears under gradual continual decrease. As it were in the middle between the two, stands that manner of termination which is described in § viii., the abnormal heat sinking abruptly on one of the critical days, but not completely, so that a moderate augmentation of temperature remains for a few days longer."

We have remarked already, that we cannot yet look with Traube at fever and abnormally increased temperature as being identical; we think that he is quite right in not admitting the sudden change towards health to be induced *by the so-called critical excretions and abscesses*, but we cannot yet find any sufficient proof, that it is induced *by the abrupt sinking of the high temperature.* We must at present consider all the different phenomena of pyrexia to be equally dependent on the change in the morbid processes within the system, as yet so little known to us. True, it may be, that in these phenomena the change is earliest, most constantly and most easily observed in the temperature, but by accurate examination we shall find it also in the action of the heart, in the excretion through the lungs, the kidneys, and other organs, in the condition of the skin, &c.

Returning to Traube's classification of the so-called critical excretions, we find that he considers the *perspiration* and the *urine* disposed to the spontaneous formation of sediment as decidedly *after-critical* (nachkritisch), i. e., appearing *after* the crisis is already effected. The arguments for this inference are, that, 1, in cases terminated by a "*crisis completa*," he never saw the critical urine and perspiration *precede* the decrease of temperature; 2, he saw several times, in acute diseases, copious perspiration *without* being followed by decrease of temperature; 3, that artificially produced copious perspiration (by Priessnitz's method) *had no effect* on the temperature. In some diseases, however (principally in *rheumatismus articulorum acutus*), he admits that the perspiration can be "the cause of crisis, i. e., of the sudden decrease of temperature."

Traube attributes a really *critical* influence to *bleedings* and to *alvine* *evacuations.* Concerning the former he has constantly found even small detractions of blood to be followed by considerable decrease of temperature. After three large venesections (amounting to 14, 16, and 20 ounces), we have likewise observed a sinking of from 0°8 to 2°0 Fah. (in cases of pleuro-pneumonia), but we have not been able to ascertain the same phenomenon after small local bleedings (4 to 8 ounces). Only in two cases out of seven we found a decrease of 0°6 and 1°0 Fah., but as this decrease coincided with the beginning of a gradual change towards health, we were not sure, whether to attribute it to the loss of blood or to the natural progress of the case. Also Von Baerensprung, who, in accordance with Nasse, observed a decrease after bleeding in dogs, has not noticed the same in men, except after considerable venesections. As a proof that spontaneous bleeding has the same effect, Traube gives a case of pleuro-pneumonia, in which an hæmorrhage from the lungs (amounting to about 6 ounces) occurring on the 4th day was followed by a remarkable decrease of temperature (1°8 Fah.), of the frequency of pulse and respiration, although the crisis did not take place before the 5th day. In a case of ulceration in the lower part of the small intestines in a young man, aged 22, a profuse hæmorrhage through the bowels (amounting to almost 2 pounds, without any diarrhœa), occurring on the 23rd day, was followed almost immediately by a rapid decrease of temperature (from 103° 0 to 91° 0 within two hours); after several hours' sleep the warmth had not returned to the amount noted on the night previous to the hæmorrhage, but almost to the normal standard, when a second smaller loss lowered it again considerably; the same sinking after new accesses of bleeding we remarked in this case three times, but in less than 18 hours after each access the temperature had increased again over the normal standard, except after the first one, which was so speedily followed by the second.

Also in Dr. Parkes' first case, previously given, we are inclined to attribute the very low degree of the morning temperature of the 17th and 18th day, to the loss of blood through the bowels; although this was accompanied by diarrhœa, we cannot consider the latter to be the only cause of the remarkable sinking of temperature, as it existed already on the previous days, without being attended by the same phenomenon in such a degree.

Traube ascribes a similar influence to the *alvine evacuations,* in case they occur shortly before or on the critical days.

The lowering power of profuse diarrhœa on the temperature we have likewise had an opportunity of observing, but we have not as yet been able to ascribe to it a critical influence. *Baerensprung's* (l. c.) researches show the same phenomenon in cholera, and not rarely we meet with it in delicate persons suffering occasionally from diarrhœa.

"XIII. *There are* REMEDIES, *by which the abnormally high temperature and the symptoms dependent on it can be considerably diminished; among the remedies, whose action I know by my own experience, I consider as such,* BLEEDING, DIGITALIS, CALOMEL *(in large doses), and* WATER, *provided its temperature be lower than that of the body. But never have I observed, that these remedies were able to induce a complete crisis on a non-critical day. Bleeding I have seen several times followed by a complete crisis, when* *it had been instituted shortly before or at the commencement of a critical day."*

To show the influence of CALOMEL in large doses so as to produce copious evacuations, the author gives two cases of typhoid fever and one of pleuro-pneumonia. As the nature of the first case of typhoid fever might be doubted, we shall give the translation of the outlines, to allow every one to form his own opinion.

"Case of typhoid fever (Abdominal-typhus)* in a strongly-built man of twenty years. Beginning of the disease at 9 A.M., with rigors, followed by pain in the head and in the lumbar and sacral region. Admitted during the exacerbation of the seventh day (August 5th, 1850); an emetic had been administered on the fourth day."

Day of Disease.	Time of Remission.		Time of Exacerbation.	
	Pulse.	Temperature.	Pulse.	Temperature.
VII.	...	...	109	104°99 [1]
VIII.	103	103°46	106	104°0 [2]
IX.	84	99°86 [3]	98	103°28 [4]
X.	76	101°30 [5]	84	101°66 [6]
XI.	76	99°86 [7]	88	102°38
XII.	76	100°22	78	100°94
XIII.	68	99°86		

[1] Soon after the time of this observation, a cold half bath (71° Fahr.). Remained in it during eight minutes and a half.
[2] At half-past 3 P.M. 5 grains of calomel had been administered, the same dose to be repeated three times in intervals of three hours.
[3] Between yesterday, 9 P.M., and now three copious greenish stools.
[4] Had another dose of 5 grains of calomel at half-past 11 A.M.; is to have the same once more at 6 P.M.
[5] Since yesterday, 9 P.M., two copious green stools.
[6] During the afternoon, two more.
[7] Since yesterday evening, again two such evacuations.

"There can be no doubt concerning the diagnosis, as the patient, who never had suffered from ague, had considerable enlargement of the spleen when admitted; besides this, he had been affected with epistaxis during the three days previous to his admission; and, in addition to this, intense cerebral affection was present." (p. 32.)

After having explained his opinion, that the cold half-bath of the 7th day had no influence on the change after the 8th day, and after having added, that a tablespoonful of castor oil had been administered on the 10th, on the 11th, and 13th days (to promote the action of the calomel), Traube continues:

* The disease, which is called *abdominal-typhus* by many of the German physicians, is the same affection which *Louis* describes as "*Fièvre Typhoide*" (Paris, 1841), and *W. Jenner* distinguishes as "*typhoid fever,*" from "*typhus fever.*" ('On the Identity or Non-Identity of Typhoid and Typhus Fever.' London, 1850.) Although, in the present case, the symptoms described are not characteristic of "typhoid fever," yet the word "Abdominal-typhus" cannot be translated otherwise.

"From all this we learn, that *at least* those changes in the temperature and in the pulse, which took place between the evening of the eighth and the afternoon of the tenth day, are to be attributed to the calomel, and this the more certainly, as I have at that period, even in the mildest cases of typhoid fever, never observed such a remission of the fever (pyrexia) when the treatment had been expectant." (p. 33.)

Without doubting Traube's diagnosis, we may remark, that we have never attained such a striking effect from calomel in large doses, although we have not rarely administered it in a similar manner in cases of typhoid fever; the calomel produced the well-known green stools, but never quite so favourable a change as in this case.

In the second case of *typhoid fever*, the temperature on the 9th day, as well during exacerbation as remission, was 105°35. After several copious calomel stools between the 9th remission (Feb. 18th, morning) and 10th exacerbation (Feb. 18th, evening), the temperature at the latter period had decreased to 102°2; then it gradually rose again to 104°72, at the 12th exacerbation, after which, during the night, some more calomel stools ensued on the following morning—i.e., at the time of remission of the 12th day—by a new decrease to 101°61; in the evening of the same day, however, i. e., at the time of exacerbation of 13th day, the temperature was 104°32, in spite of another copious green stool.

The third case given in corroboration of § xiii. is one of *pleuro-pneumonia* in a strongly-built female patient.

At the exacerbation of the 7th day the temperature was 103°10; after two copious calomel stools (during the night) the temperature of the following (8th) remission was only 98°24, of the 8th exacerbation 98°60, then increasing again to 100°04 at the 9th remission and at the 10th exacerbation.

It can be scarcely doubted, that in this case the remedy had produced the decrease of the temperature; but it may be asked, whether this lowering influence was a specific effect of the calomel, or merely that of the copious alvine evacuations; or, in other words, whether another remedy, say magnesia sulphas in a slightly purging dose, would have produced the same effect or not? The circumstance that profuse spontaneous diarrhœa is not rarely attended by diminution of temperature must make us suspicious. We have not personal experience enough to form a decided opinion; two cases which we lately met with make us inclined to think, that *saline aperients*, in purging doses, exercise, under some conditions at least, a lowering influence on the temperature.

Of the one case we will give a short outline:

A strong-looking young man, aged 25, was attacked on September 7th, towards midnight, with headache, rigors, &c.; on the 11th September, in the morning, he was admitted into the hospital suffering from *angina faucium* with intense pyrexia. The treatment was merely dietetic, except that the patient took two ounces of *sodæ sulphas* in eight ounces of water, between the end of the 4th and that of the 5th day of disease (9 P. M. of Sept. 11th to 9 P. M. Sept. 12th), which was followed in the night from Sept. 12th to Sept. 13th by 4, and during the course of the 13th Sept. by 5 copious stools; after Sept. 13th the alvine evacuations became rare.

Day of Disease.	Time of Remission.		Time of Exacerbation.	
	Pulse.	Temperature.	Pulse.	Temperature.
IV.	95	103°5	101	105°2
V.	98	104°0	98	105°4
VI.	92	99°5	88	99°0
VII.	90	100°5	92	103°0
VIII.	85	101°5	90	102°0
IX.	80	98°0	70	97°5
X.	62	96°4	60	96°8
XI.	58	97°0		

We may be almost certain in this case that the decrease on the 6th day was effected by the remedy, the more so as the temperature rose again soon after the relaxation of the bowels had ceased.

Similar, though not quite so striking, was the effect in another patient, suffering likewise from *angina faucium* complicated by *bronchitis,* to whom two ounces of *magnesiæ sulphas* were administered between the end of the 3rd and the first half of the 4th day. The temperature at—

10 A.M. on 4th day was 104°5 (pulse 105).
6 P.M. " 101°5 (after four fluid stools).
9 A.M. on 5th day was 98°0 (pulse 58, irregular, after seven fluid stools).
5½ P.M. " 100°0 (pulse 95, after one stool more).
9 A.M. on 6th day was 104°0
6 P.M. " 103°5

After the 8th day the temperature gradually decreased.

We must add, however, that in a case of acute rheumatism, attended by a high degree of pyrexia, we have seen no decrease of heat after two ounces of magnesiæ sulphas, administered during the second half of the 6th day, although the consequent relaxation of the bowels was considerable on the 7th and first part of 8th day.

Concerning the important question, whether there is any difference in the power of the remedies to effect a complete crisis according to their being administered immediately before and on an *odd* day or on an *even* day, we are not yet able to give an opinion either in favour of or against Traube's assertion.

"XIV. *By the frequency of the pulse we often cannot judge of the intensity of the fever (pyrexia). Sometimes I have found the former abnormally increased with normal temperature, at other times normal or even abnormally low with increased temperature; and thirdly, I have often observed a considerable evening exacerbation of the temperature, when at the same time the frequency of the pulse was only so slightly increased, that the difference between this and the morning pulse was scarcely perceptible.*" (p. 35.)

Several of the tables of cases contained in the author's essay can serve as arguments for the truth of § xiv.; also Dr. Parkes' two cases of typhoid fever (note to § vi.) may be used for the elucidation of its correctness. So in the first of these cases, on the 17th day, in the morning, the temperature was as low as 93°0, at night 101°5, and yet the frequency of the pulse at both periods was the same (112); on the 21st day in the morning, pulse 100, temperature 98°0; at night, pulse 94, temperature 102°0.

It is satisfactory to remark, that by this observation the view of our oldest observers is fully corroborated; as *Galen* clearly expresses his opinion, that "the essential nature of fever does not consist in the motion of the arteries .... but in the abnormally increased heat." It was *Boerhaave* who held the opinion that the frequency ('velocitas') of the pulse was the most essential symptom of fever, saying in his aphorisms, "Quæ quidem symptomata in omni febre adsunt (horripilatio, pulsus velox, calor vario febris tempore vario gradu), sed *sola velocitas pulsus* adest ex his omni febris tempore, ab initio ad finem, eâque solâ medicus præsentem febrem judicat." *Boerhaave* certainly was not right in this opinion, neither was he correct in adducing the "stadium frigoris" of ague as an argument that fever may exist without increased temperature, as the observations of *De Haen*, *Gavarret*, *Nasse*, *Schmitz*, and *Baerensprung*, sufficiently prove, that *also during the 'stadium frigoris' the temperature is much increased.* But we must also be careful in adopting *Traube's* aphorism: "*Solus calor* adest ex his omni febris tempore, ab initio ad finem, eoque solo medicus præsentem febrem judicat." Circumstances may occur during the course of any disease attended by fever, in consequence of which the temperature may be for a shorter or longer period of time much lowered. After hæmorrhage I have repeatedly observed the temperature during several hours below the normal medium of health, and of this Dr. Parkes' case is also a proof; we can scarcely agree with the view, that there was no fever at all during those hours. On that account we can also not adopt without modification *Traube's* first inference, that "fever essentially consists in an increased temperature of the blood." From our limited knowledge we might venture perhaps to express our view concerning the connexion between *fever* and *increased temperature* in the following manner:

The animal temperature is in its largest amount the product of the chemical metamorphosis of parts of the blood and organic tissue. This chemical metamorphosis being more or less accelerated during the course of fever, the temperature must be more or less increased. As the increase of warmth is almost proportional to the increase of decomposition (or better, of the chemical processes), and as the latter is to a great degree dependent on the intensity of the disease, the temperature may in *general* be looked at as a scale for this intensity; but it does not follow, that as long as the disease is intense, the temperature must be *always* increased; —no, the chemical change of matter is constantly under the special direction of the *nervous system*, and it may be almost suddenly increased or decreased through influences acting on the latter; instances for the lowering influence are given in the loss of blood through hæmorrhage, or in any power producing a state approaching to syncope.

If, however, any one considers *fever as equivalent to morbidly increased decomposition of substance during acute diseases*, then we quite agree with him in saying, that the degree of heat is a scale for the degree of fever.

In taking leave of Dr. Traube's essay, we highly recommend its careful perusal to every one who is interested in the science of medicine; we look forward with much pleasure to the promised more elaborate work of the able author on the subject of crisis. Certainly these observations may be of the greatest importance, not only for the pathology, but also for the therapeutical management of acute diseases. Their investigation is not so easy as it might appear; careful and continued observations, according to a certain plan, are utterly indispensable. We shall mention here a few points which appear to us particularly requisite.

1. The exact commencement of the disease must be as accurately fixed as possible.

2. The temperature and the pulse should be registered at least twice on every day—i. e., at the period when both are in general the lowest, and at that when they are highest (period of *remission* and *exacerbation*). It must be borne in mind that the typical change in the temperature is not the same during the course of acute diseases and during health; while there is in the normal state a double sinking and rising within 24 hours, with the highest number at about 11 o'clock A.M., and 6 to 7 o'clock P.M., and lowest at about 4 A.M., and 2 P.M. ("Baerensprung in *Müller's Archiv.*" 1851, p. 126), we observe during the most acute diseases the lowest between 7 and 9 A.M., the highest between 5 and 7 P.M.*

3. The greatest accuracy is necessary in taking the temperature and frequency of the pulse; it is scarcely necessary to remark, that everything that has an influence on the latter must be excluded.

It would be advisable, not only to mark the degree to which the mercury rises, but also the time which elapsed before it reached the highest point; a comparison of different cases, measured by the same thermometer, will show interesting differences.

4. Besides the pulse and the temperature, the phenomena presented by the *urine* are to be registered—the quantity secreted in 24 hours, the specific gravity, the physical appearance, the acidity or alkalescence, with approximative degree of the one or the other, if possible the quantity of the uric acid, that of the urea, of the phosphates, the sulphates, &c.

5. The phenomena of the *skin* are to be noted under another head.

6. Those of the *stools* under another.

7. The outlines of the progress of the *physical* and *functional phenomena* might have a place on the same table.

8. The *therapeutical* and *dietetic* influences ought not to be forgotten.

9. The notes on these points are to be continued during several days, at least, after the decided commencement of the convalescence.

Only by carefully recording all these points, by comparing and analyzing a large number of them, can we venture to form conclusions on subjects so delicate as those treated of in the preceding essay.

In private practice the opportunity is not often given for executing such investigations; in hospitals we meet more frequently with cases which,

* It is scarcely necessary to remark that ague forms an exception; immediately before the beginning of the paroxysms in ague the temperature begins to rise, increases rapidly during the stadium frigoris, until it reaches the height at the close of this stadium and at the very beginning of that of dry heat, remains then for a time unchanged, begins slowly to sink towards the commencement of the stadium of perspiration, during the course of which it decreases rapidly, showing, however, at its close, still an increase over the normal standard, which only gradually becomes diminished during the time of apyrexia, until it reaches its minimum shortly before the stadium frigoris of the following paroxysm.

Since this review was written, we have received a number of Schmidt's Jahrbuch, containing an abstract of an important paper on the normal temperature of the body, by Lichtenfels and Fröhlich. It will be found in the Chronicle at the end of this number.

when accurately observed, may gradually throw light on these obscure subjects; but, unfortunately, the number of medical men employed even at larger hospitals is far too small, and they are so overloaded with other duties that little time remains for the investigation of such matters, which, nevertheless, form the most necessary links in the great chain of phenomena through which we gradually may be led to a more thorough understanding of the laws of our organism—an understanding not less important for practice than for science.

Hermann Weber.

REVIEW IV.

1. *Beiträge zur Kenntniss der Bright'schen Krankheit.* Von Dr. REINHARDT.

Contributions to the Science of Bright's Disease. By Dr. REINHARDT. (From the 'Annalen des Charité Krankenhauses.')—*Berlin*, 1850.

2. *Die Bright'sche Nierenkrankheit und deren Behandlung.* Von Dr. FRERICHS.—*Braunschweig*, 1851.

Bright's Disease and its Treatment. By Dr. FRERICHS.

3. *Zur Pathologie der Bright'schen Krankheit.* Von J. F. MAZONN. Erster Theil. Pathologisch-Anatomische Beobachtungen.—*Kiew*, 1851.

On the Pathology of Bright's Disease. By J. F. MAZONN. First Part. Anatomico-Pathological Observations.

4. *Ueber Uræmia und Uræmischen Krankheitscharakter.* Von Dr. EISENMANN.

On Uræmia and the Character of the Uræmic Disease. By Dr. EISENMANN. (From the 'Verhandlungen der Physikalisch-Medicinischen Gesellschaft in Würzburg, 1852.)

THE paper by Reinhardt, which we have placed at the head of our list, is devoted to a consideration of the morbid anatomy and pathology of Bright's disease. Its author describes, with great particularity, the various morbid appearances which the kidneys present to the naked eye, and the minute structural changes which are revealed by a microscopical examination; he endeavours to trace the connexion between these various morbid conditions, and the result of his inquiries is the conclusion that the different outward appearances in the kidney which many pathologists agree in referring to distinct forms of renal degeneration, are, in fact, different stages of a single morbid process, which he calls a diffuse inflammation of the kidney. We propose now to examine the grounds of this opinion. The subject is not one of those which have a merely speculative interest for the scientific pathologist, but it is one respecting which it is of the highest importance that we should have definite and accurate notions. In the examination of this question we shall have frequent occasion to refer to the opinions of Frerichs, whose work on Bright's disease, now well known to British pathologists, has already received an extended notice in the pages of this journal.*

The definition which Reinhardt gives of Bright's disease is sufficiently

* Vol. ix. p. 285.

precise and comprehensive. It is characterized, he says, during life by a definite series of symptoms, and especially by anasarca, which, however, is not constant; by an albuminous condition of the urine; and by certain striking changes in the kidneys observable after death, either a granular condition, or a considerable enlargement of the cortical substance. This definition will include all the various morbid conditions of the kidney which are described and depicted in the far-famed *Reports of Medical Cases.*

Reinhardt and Frerichs agree in describing what they consider three stages of morbid change in the kidney; and their divisions, though slightly different, agree in all essential points. Reinhardt's first stage he calls the *simple inflammatory stage,* that of Frerichs is *the stage of simple hyperæmia and of commencing exudation.* The second stage of Reinhardt is *the stage of fatty infiltration of the kidney,* that of Frerichs is *the stage of exudation and of commencing metamorphosis of the exuded material.* The third stage of both authors is *the stage of atrophy of the kidney.* The following, therefore, is the order of phenomena as interpreted by Reinhardt and Frerichs: an engorgement of the renal bloodvessels, an effusion of inflammatory products, a more or less complete and general metamorphosis of these products into fat, and finally atrophy and wasting of the kidney. The small contracted granular kidneys have once been fat; the large, pale, fat kidneys are in continual progress towards atrophy and contraction.

The first observation which we have to make, with reference to this systematized description of renal disease, is, that there is no proof whatever that hyperæmia or over-fulness of the bloodvessels, is either a cause or an antecedent of those exudations into the uriniferous tubes, which constitute an essential feature of the inflammatory forms of renal disease. We refer now to the rapid formation of epithelium within the convoluted tubes, and to the occasional replacement of the normal epithelium by puriform cells. These changes in the tubes, it is true, are accompanied by engorgement of the bloodvessels, and the phenomena occur almost, if not quite, simultaneously; but in the order of causation, the changes in the secreting cells stand first. The circulation is impeded in consequence of morbid changes primarily affecting the secreting cells, and retarding their functions. Engorgement of bloodvessels implies, not in increased afflux of blood, or a more rapid circulation, but a retarded and impeded circulation, the impediment being shown by the frequent occurrence of hæmorrhage from the Malpighian capillaries in the early stages of acute renal disease, and by tortuosity of the arteries, with great hypertrophy of their muscular coats, after long-continued morbid changes affecting the secreting cells.

The theory of the *oneness* of Bright's disease has apparently had its influence in leading our authors to overlook the importance of distinguishing the various kinds of exudation into the tubes, which occur during the inflammatory forms or stages of the disease. If these varieties of morbid products were appreciable only after the death of the patient, their distinction would have little practical value; a moderate amount of clinical observation, however, will show, first, that the precise nature of the pathological changes which are occurring in the kidney may, with few exceptions, be as readily detected by a microscopical and chemical examination of the urine during life, as by the most searching post-mortem inspection of the kidneys; and, secondly, that the various kinds of morbid products observed in the urine have a widely different significance when viewed in relation to prognosis. With reference to this point, it is of the first importance to ascertain, in any case of recent acute albuminuria, whether the urine is clear and free from sediment, or whether it deposits morbid materials, and what is the nature of these materials—whether there are any forms of tube-casts, and what is their appearance—are they composed of blood or of pure fibrin? or do they entangle organic cells of any kind, and what is the nature of these cells? have they the character of renal gland-cells; and do they contain oil, or are they free from that material? or, lastly, do they more nearly resemble pus-corpuscles?

Reinhardt's paper is defective in the results of the clinical study of renal disease; his opinions appear to have been chiefly deduced from the examination of the dead body. No one who has directed his attention to the subject can have failed to perceive that in many cases a careful microscopical examination of the urine during the patient's lifetime affords important aid in determining the minute structural changes in the kidney, so that the investigation of the diseased organ is much facilitated by a previous examination of its morbid secretion. This observation may be illustrated by reference to certain appearances in the tube-casts during the inflammatory stages of renal disease. In a large proportion of cases some of the renal gland-cells are entangled in the fibrinous material of the casts, while in other instances the casts are transparent and homogeneous, apparently composed of pure fibrin, and do not entangle epithelial cells. Frerichs offers the following explanation of these different appearances:—"The fibrinous cylinders are covered with epithelial cells when the effusion occurs in those uriniferous tubes which yet retain their epithelial lining, while those casts which escape from tubes which have been previously deprived of their epithelium are homogeneous and transparent, and entangle only some nuclei or oil-globules." (p. 57.) This statement has an element of truth, but it involves an important error. Frerichs has taken no note of the diameter of these transparent wax-like casts. It will be found, however, that while some of them measure $\frac{1}{500}$ inch in diameter, there are others which do not exceed $\frac{1}{1000}$ inch. The larger casts have a remarkably sharp, well-defined outline; their average diameter is that of the uriniferous tubes, and they are formed in those convoluted tubes which have no epithelial lining, as may be readily shown by an examination of the cortical portion of the kidney. The small homogeneous wax-like casts have a less definite outline than the larger; and so far from indicating that the tubes in which they are formed have been deprived of their epithelial lining, their diameter, which corresponds with that of the free canal in the centre of the uriniferous tube, sufficiently indicates that they have been moulded within that space, and that the tubes from which they have escaped possess a complete lining of epithelium. This point we have verified by the repeated examinations of the kidneys after death, and by a comparison of their condition with the microscopic appearances which had been observed in the urine during the patient's lifetime.

Frerichs doubts the existence of any morbid process in the kidney, to which the term *desquamative nephritis* can be applied, and the same doubt has been expressed in this country. The appearance of renal epithelium in the urine is, Frerichs says, a result of the cells becoming entangled in the fibrinous effusion which coagulates in the tubes, and escaping thence, drags away the epithelial lining. We are curious to know what explanation Frerichs and other supporters of this hypothesis would give of the not uncommon cases to which we have just now referred—cases in which there is an abundant serous and fibrinous effusion with very numerous tube-casts, and an entire absence of renal epithelium in the urine. In what way would they explain the modifications in the appearance of the cells which are cast off in other cases? And how would they account for the occasional replacement of epithelium by pus? What account is to be given of the undoubted fact, that many of the tubes after death are crowded with epithelial cells, which are at least *twice as numerous* as in a healthy tube? *How is this multiplication of renal epithelium to be explained, if not by a rapid formation and shedding of new cells?* The supposition that the appearance of desquamation in the tubes after death is the result of *post-mortem* decomposition of the tissues, is entirely inconsistent with the *positive* observation that the epithelium is present in abnormal amount. The cells have, in fact, increased in number, in the same way as can be observed on other mucous membranes, whose epithelium is rapidly formed, and as rapidly thrown off.

The stage of fatty degeneration.—In considering the subject of fatty degeneration of the kidney, it is very important to bear in mind that the morbid condition occurs in two distinct forms. The large *granular fat kidney*, which is represented in the third figure of Dr. Bright's third plate, is, in a large proportion of cases, a secondary condition, which has been preceded for a longer or shorter period by an inflammatory state of the organ. We have observed the approach of this form of disease under the following circumstances:

1. An acute attack of general dropsy, with scanty high-coloured, albuminous, and bloody urine, and an abundant desquamation of epithelium, is followed, after a period of three or four weeks, by an appearance of oil in some of the cells; and as the disease makes progress, the total amount of epithelium in the urine diminishes, while the proportion of the cells which contain oil is increased, until at length nearly all the cells are more or less distended with oil, many of the cells, as well as scattered oil-globules, being entangled in small transparent wax-like casts. After death the kidneys are found enlarged, the cortical substance pale, and having scattered through it the characteristic yellow granulations, which very much resemble the minute atheromatous spots which are often seen in the arteries. These granulations are found, on microscopical examination, to be composed of convoluted tubes distended with oil, which is partly free and partly contained in cells. In other tubes the epithelium appears opaque, but contains no oil, and the central canal is free from deposit, while in others again there is an accumulation of epithelium, or a fibrinous effusion, or both combined.

2. In other cases the approach of fatty degeneration is different. The disease is chronic from the commencement; the urine is highly albuminous, but frequently of the natural colour, and either free from sediment, or it deposits a light cloud, which contains some of the small transparent waxy casts before mentioned. After a period, which may vary from a few weeks to many months, these casts entangle oil partly in the form of

scattered globules, and partly contained in cells. The oily casts and cells continue until the fatal termination, and after death the kidneys present essentially the same appearances as in the cases last mentioned.

The second form of fatty degeneration of the kidney differs in many important particulars from the preceding. The kidney is enlarged, but the cortical substance wants the granulations which are characteristic of the first-mentioned form, and it has instead a mottled appearance. On a microscopical examination *all* the tubes of the cortical substance are found to contain an excessive quantity of oil, which is, for the most part, contained within their epithelial cells. This condition of the kidney is sometimes found both in the human subject and in the lower animals—in cats and in dogs—unconnected with albuminuria, or with other functional symptoms of renal disease;* while in other cases of dropsy, with albumen and oil in the urine, this mottled form of fatty degeneration has been the only anatomical change observable in the kidneys after death. It will be seen, therefore, that the two forms of fatty degeneration differ in these important particulars : 1st, that in the granular form of disease the fatty degeneration is partial, while in the mottled form it is general throughout the tubes of the cortical substance; 2nd, that in the first form of disease, albuminuria and what may be considered an inflammatory stage precede, sometimes for a considerable period, the signs of fatty degeneration, while in the second form a great degree of fatty degeneration may exist unassociated with an albuminous condition of the urine.

The stage of Atrophy.—We come now to the consideration of that condition of the kidney, the most remarkable outward feature of which is a diminution of size and weight. The atrophy affects primarily the cortical substance, the surface of the kidney usually becomes uneven and coarsely granular, and its vascularity is much diminished. The first and second figures in Dr. Bright's third plate are probably familiar to most of our readers.

We have already stated that Reinhardt and Frerichs agree in considering that these small granular kidneys have passed through the stage of fatty degeneration, and that atrophy of the gland is only a later stage of the same morbid process as that of which inflammatory effusion and fatty degeneration constitute the first and second stages. In this opinion Eisenmann and Mazonn also concur, although they differ from Reinhardt and Frerichs in respect to some points of less importance. Now, with reference to this question, we dissent entirely from the opinions of these pathologists. True it is that a careful and extended series of observations upon morbid urine and kidneys had led us to form a judgment upon the point in question before we had any knowledge of the opinions referred to, but we trust that we were not, on that account, less open to conviction by any evidence which might be adduced to prove the transition from fatty degeneration of the kidney to that contracted condition of the organ with which every pathologist is familiar. But as we have met with no such evidence in the course of our own study of morbid phenomena, so we find none in the writings to which we have access. None of our authors

* This form of fatty degeneration of the kidney is frequently found after death from diabetes. We may observe, that in the able review of Frerichs' work, before referred to, in this journal, the existence of a fatty kidney, *par excellence*, is admitted.

attempt to prove the transition in question by clinical observation of the urine, but their opinion appears to be based upon what they consider the various stages of morbid change in the kidneys, as determined by post-mortem inspection.

Our limits will permit us to indicate only some of the principal facts, which tend to prove, as we think incontestably, that the large granular fat kidney and the small contracted kidney are the result of two morbid processes as essentially diverse as is consistent with the fact of the two diseases affecting the same tissues.

The most characteristic feature of that form of disease which leads to atrophy of the kidney is a disintegration of the epithelial cells, which appear in the urine in the form of granular casts of the tubes. In consequence of this washing away of disintegrated epithelium from the tubes, the basement-membrane is left denuded, and subsequently the tubes, having lost their epithelial lining, either waste away entirely, or, as we believe, they may continue to secrete a serous liquid, and so become dilated into cysts. In consequence of the atrophy of the tubes, the meshes of the matrix, in which the tubes are packed, become narrowed, and the fibres appear relatively thicker. Frerichs describes a development of new fibrous tissue as an occasional occurrence, and Mazonn considers it to be a constant and a characteristic condition. As Frerichs doubts the very existence of the normal fibrous matrix, his evidence upon this point is of little value to those who believe in the existence of such a tissue.* Mazonn recognises the normal intertubular tissue, and believes that he can distinguish this from the newly-formed fibres which are the product of disease.

A very few of the tubes may usually be found to contain oily matter, and this occurs more frequently in the denuded tubes than in those which still retain their epithelial lining. The thickening, and finally the obliteration, of the Malpighian capillaries, and the hypertrophy of the arterial coats, occur in this as in all forms of chronic renal disease.

The points of contrast between the fat granular † kidney and the contracted granular kidney are chiefly in respect of the tubes, with their epithelial lining. In the contracted kidney the disintegrated epithelium is swept away in the form of granular casts, and the tubes thus left denuded either waste or grow into cysts. In the fat kidney, the epithelium, for the most part, retains its position, and undergoes fatty degeneration, the tubes neither become denuded nor waste, as in the contracted kidneys, nor do they, except in very rare instances, grow into cysts. The combination, in the same subject, of the characters of the fat and the contracted kidney, are so rare as to prove that, while the two forms of disease are not absolutely incompatible, they are by no means allied; indeed, their relation is rather one of antagonism, since, in the one case, the epithelium is disintegrated and swept away, while in the other it remains, and undergoes fatty transformation.

* We must confess that we are surprised at the doubts which have been expressed on this point. Kölliker has recently published a *Handbuch der Gewebelehre*, in which, at page 481, it will be seen that this accomplished microscopist has had no difficulty in finding the tissue in question.

† Some confusion has doubtless arisen from the application of the same term, 'granular,' to two essentially different appearances. The granulations in the first form of fat kidney are local accumulations of oil, but the firm projecting granulations on the surface of a contracted, or, as it is sometimes called, a *cirrhosed* kidney, are composed of atrophied tubes, thickened arteries, and fibrous matrix, often blended with a hardened fibrinous effusion, but rarely with fat in any form.

That the fat kidney has no tendency to pass into the contracted one, is shown by the post-mortem examination of cases which have been for a long time under observation. Not long since we examined the kidney of a man who had been nearly four years ill. He first had dropsy in the autumn of 1848; the urine was highly albuminous, and contained only casts and cells in November, 1849, the same in January, 1850, and again in December, 1851; after that we have no note of the urine. He died in March of the present year (1852). One kidney had been destroyed by a calculus in the ureter, the other was more than double the natural size and weight, and had all the characters of a granular fat kidney, but not a trace of denuded or atrophied tubes. In another case, which had been nine months under observation, the urine, from first to last, contained oily casts and cells. The kidneys were much enlarged, and presented all the characters of fatty degeneration, but not a trace of the process which leads to atrophy.

But the essential difference between the two forms of disease may be proved by evidence of another kind, which may appear more conclusive to some of our readers who, perhaps, have less confidence than ourselves in the results of microscopical observation. The chronic desquamative disease which causes the disintegration and destruction of the epithelium, and finally extreme wasting of the kidney, is in many cases one of the most insidious of maladies, and it may proceed to the extent of destroying a large portion of the epithelium of the kidney without the occurrence of dropsy or any other formidable symptom; when perhaps, suddenly, in consequence of some accidental cause, the most urgent symptoms of suppressed secretion arise, the patient soon dies, and the kidneys are found wasted, and many of their tubes denuded and atrophied. We have before us the notes of four such cases; one patient was suddenly seized with peritonitis, and died in a few hours, a second died with apoplectic symptoms, a third with delirium and epilepsy, and a fourth with obstinate vomiting, consequent upon suppression of urine. The first two patients were supposed to be in good health until the period of their sudden seizure. In the last case alone had there been any dropsical symptoms, and these were only very slight and transient. In all the cases the kidneys were in an advanced stage of that chronic form of disease which is characterized by denuded and atrophied tubes. In contrast with these cases, which are by no means rare,* we place the fact, that, according to our experience, the *granular form* of fat kidney *never* destroys life without the previous occurrence of dropsy, which is usually one of the most prominent and distressing symptoms. Again, we have very rarely met with a case of inflammatory disease of the kidney—such as Reinhardt and Frerichs agree in referring to the first stage of Bright's disease—unaccompanied with dropsy in some degree, and for a variable period. Now, according to the opinion of these pathologists, the kidneys of the four patients whose cases we have briefly mentioned, must have passed through an inflammatory stage, and a stage of fatty degeneration, before they finally arrived at the stage of atrophy; yet in three of the cases there had been no dropsy whatsoever, and in the fourth case a slight and transient œdema of the ankles—such as might occur in any debilitated subject—had formed the

* Besides the four cases referred to, others have occurred to us, and, if necessary, we could collect a numerous series. A similar observation has been made by Rapp—(Virchow's Archiv., vol. iv.)

only dropsical symptom. This supposition is so improbable that we should hesitate to admit the doctrine which is based upon it, even without what we consider the conclusive evidence of the minute structural changes in the kidneys. We therefore feel bound to dissent from the doctrine of the oneness of Bright's disease, as propounded by Reinhardt and Frerichs. The apparent simplicity of the doctrine is not in accordance with nature, and it therefore tends to cause confusion. While we recognise an inflammatory form of the disease, we must, for the purposes of accurate diagnosis and prognosis, distinguish between the various kinds of effused products, which are chiefly, besides serum and blood, epithelium, pus, and pure unorganized fibrin. We must distinguish the granular form of fat kidney, which may be a consequence of a previous inflammatory stage, from the mottled form of the disease, which is analogous to ordinary fatty degeneration of the liver, and not a consequence of inflammation. Finally, we must recognise the fact, that the small contracted kidney, although an occasional consequence of an acute inflammatory attack, is more commonly the result of a disease which is chronic from the commencement, and never, as we believe, a consequence or a later stage of either of the forms of fatty degeneration.

We purpose, on a future occasion, to discuss the important subject of uræmic poisoning, with especial reference to the hypothesis of the decomposition of urea, which has been recently put forth by Frerichs.

George Johnson.

Review V.

1. *The Practice of Surgery.* By James Miller, F.R.S.E., Professor of Surgery in the University of Edinburgh, &c. &c. Second Edition. —*Edinburgh*, 1852. 8vo, pp. 687.
2. *A System of Practical Surgery.* By William Fergusson, F.R.S., Professor of Surgery in King's College, London, &c. &c. Third Edition.—*London*, 1852. 8vo, pp. 846.

Most of, if not all, the readers of this journal, it may be presumed, are familiar with the previous editions of the works whose titles are above transcribed, and it is, therefore, scarcely necessary to make any apology for not examining their contents in detail. Indeed, such an examination would be almost impracticable, for a recapitulation of the subjects discussed in these volumes, with the most meagre commentary upon some of the more leading and prominent topics, would occupy greatly more space than could possibly be spared. The only convenient course that lies open is to institute a comparative examination of the manner in which some few important surgical questions are treated of by Mr. Fergusson and Mr. Miller; and in attempting to do so, it is not our intention to criticise, but rather to compare, the works of these two very able and eminent surgeons, and to endeavour to convey a fair idea, both of the point of view under which each writer regards his subject, and of the nature and extent of the information which, in conformity with their respective plans, it is their object to convey. With that view, passing over Mr. Miller's brief opening chapter on operations in general, we shall commence by comparing some of the doctrines set forth in the succeeding chapters on injuries of the head, with those contained in the corresponding portions of Mr. Fergusson's work.

Mr. Miller considers injuries of the head both more methodically and at greater length, than Mr. Fergusson. For example, Mr. Miller devotes a separate chapter, occupying seven pages, to "Injuries of the Scalp," while Mr. Fergusson, when considering fractures of the cranium, incidentally disposes of injuries of the scalp in the few following lines:—"If there be a wound in the integument, it must be treated as one would be in any other part; it may be *stitched*, strapped, or poulticed, according to circumstances." (p. 549.) The word "stitched" has been underlined in the foregoing extract, to contrast the practice it seems to authorize, with the rule laid down by Mr. Miller, that in scalp wounds "sutures are, if possible, to be avoided; experience having shown that here they are especially liable to prove the exciting cause of erysipelas." (p. 9.) Mr. Miller, however, does not object to tying arteries of the scalp; on the contrary, having premised that "simple incised wounds of the scalp are apt to prove troublesome by bleeding," he proceeds to say—

"The arterial point or points are to be exposed and secured by ligature. Pressure may, in some instances, succeed, but in general it is decidedly inferior to the use of ligature; being not only less certain as a hæmostatic, but also liable to induce sloughing, or at least troublesome ulceration in the compressed part." (p. 9.)

It is, we apprehend, commonly thought that ligature of an artery of the scalp is much better avoided, and not very frequently needed. A ligature on these vessels usually causes much pain, and, what is more important, is just as likely as a suture in the scalp to excite erysipelas. Moreover, there are few regions of the body where pressure can, in ordinary cases, be so easily and efficiently applied; though of course cases occur from time to time, in which it may fail, or be inapplicable. Neither Mr. Miller nor Mr. Fergusson notices the troublesome complication of hæmorrhage with erysipelas, or the difficult and embarrassing, but fortunately rare occurrence of obstinate bleeding from a wound of the temporal artery deep in the temporal fossa; nor is it necessary here to discuss the practice proposed in such cases by Mr. Mayo and by M. A. Berard.

Mr. Miller introduces his chapter on "Injuries of the cranium and their consequences," with an account of "concussion of the brain," extending over nearly ten pages. Mr. Fergusson devotes, altogether, to the same subject, about half a page, from which we extract the following passage respecting the treatment of concussion of the brain:

"At first the practitioner's object should be to rouse the patient from this condition; cordials, stimulants, warmth, and *bloodletting*, are the ordinary means; the latter plan is generally resorted to at the earliest period possible, and though, in so far as the mere state of the pulse goes, the method is in contradiction to the usual doctrine of not bleeding in a state of shock, I believe that in some instances it may be of the utmost value. The late Professor Reid, of St. Andrews, found that after producing concussion in a rabbit, the right auricle of the heart became unable to act in consequence of over-distension, and that the animal died under these circumstances; if, on the contrary, another was injured in a similar way, and a vein in the neck opened, to lessen the quantity of blood in the vessels leading to the heart, its action continued, and recovery took place." (p. 547.)

Mr. Miller shall serve as a commentator upon the preceding extract, and it were, indeed, superfluous to add a word to his excellent directions for the treatment of the first stage of concussion of the brain. The passage is a long one, and does not, it is true, set forth anything new; yet its quotation is justified, both as it affords a fair specimen of Mr. Miller's matter and manner, and subserves our purpose of comparing the two works before us.

"*Treatment.*—This necessarily varies according to the severity of the injury and the intensity of its results; but more especially is it different at different periods of the case. In the first stage—that of depression—if we act at all, it will be with the view of favouring at least the commencement of reaction. An opposite procedure were plainly at variance with common sense; but, unfortunately, it is found to be not equally at variance with common practice. A man stunned by a blow or fall, and labouring under simple concussion, is often bled on the instant—or an attempt, at least, is made to bleed him—by the rash and thoughtless practitioner. In other words, a fresh and powerful agent of depression is exerted on the general circulation, when such depression is already great, and has probably brought life to the very verge of extinction. If blood flow from the wound in venesection, under such circumstances, perhaps life is lost; at all events, the direct untoward result of the injury is aggravated; and the case is rendered both more urgent and more protracted than it otherwise would have been. The lancet is certainly not to be used during this stage. In many cases we should be little more than passive spectators. The depression is not extreme, nor giving indications of long continuance; signs of reaction, on the contrary, are slowly manifesting themselves: and we await the natural progress of events. Not altogether idle, however. Although not engaged in active treatment, we are prepared for activity, when circumstances shall call for our interference. The patient is stripped and put to bed. His whole body is carefully examined. He cannot tell us whether or not other parts have been injured, besides the head. Besides an anxious investigation as to the existence or not of other internal injuries (Principles, p. 101), we must ourselves carefully examine each joint and bone; detecting fracture or dislocation, and having it immediately rectified, while circumstances are also peculiarly favourable for the required manipulations. (Principles, p. 732.) On recovering his senses, he has not to complain of a painful and distorted limb, now for the first time observed; but finds what was distorted duly replaced, and already some way advanced in the process of repair. The head is carefully shaved; and is placed on pillows, considerably elevated. If wound of the scalp exist, hæmorrhage is arrested, if need be; and approximation is effected in the ordinary way.

"Should the depression prove great and continued, plainly indicating risk to life by syncope, something more is required of the practitioner. He endeavours gently to originate reaction. Warmth is applied to the surface; and friction is used over the chest and abdomen. If this be not sufficient to turn the course of the symptoms, a stimulant enema of turpentine is given. If still the progress be downwards, an attempt is made to convey to the stomach some warm tea, or soup, or wine-and-water; and stimulants are held to the nostrils, for insufflation. These last, however, are always to be warily managed, so as to avoid risk of injury by their too free application to a patient at the time insensible of pain; and the giving of fluids by the mouth, too, must be effected with care, lest they pass into the air-passages, and produce asphyxia. So soon as reaction has begun, we cease from our auxiliary efforts; and again become passive onlookers; completion of the second stage being always safest in the hands of Nature.

"If stimulants are used at all internally, it must be only in urgent circumstances, and with much caution; begun with a sparing hand, and repeated warily. And in general, we are well content to do nothing in this way; knowing that moderate depression is a favourable occurrence; and that premature cessation of it, especially when followed by abrupt and marked reaction, is apt to prove most injurious. For, at first, we can never be certain that the case is one of pure concussion. There may be a lesion, by laceration, of the brain's substance. During the existence of concussion's first stage, the case remains, practically, one of concussion still; circulation is weak in the torn part, as elsewhere; extravasation of blood does not take place from the open vessels; valuable opportunity is afforded for their closure by natural hæmostatics; and when at last—it may be after some hours—the natural reaction slowly sets in, and circulation is proportionately restored, still no escape of blood occurs; and the symptoms may remain those of mere concussion to the last. Whereas, had the period of depression been abridged, and reaction rendered not only premature, but also abrupt and active, circulation would have been restored in the injured part ere the open vessels had closed, blood would have been extravasated, and compression of the brain must have ensued. Or, even if no lesion of the brain have occurred, the case being in all respects one of mere concussion, still premature and excessive reaction is most hazardous; by tending not only to kindle an inflammatory process in the brain or its membranes, but also to secure its being of an aggravated and perhaps uncontrollable character.

"Thus, then, it is plain that two great errors may be committed in the treatment of the first stage of concussion. Blood may be drawn prematurely; lowering the vital powers still further; unnecessarily, untowardly, perhaps fatally. Or stimuli may be imprudently employed; too soon, and too freely; hurrying on reaction; and endangering life, either by compression in consequence of extravasation of blood, or by an inflammatory process of an urgent and untoward character. Let both errors be studiously avoided; for each is of a most grave nature. While we take care that the depression does not proceed too far, let us beware of doing anything to effect either a premature or an excessive reaction. And when we attempt to fulfil the former indication, let us beware both of inducing asphyxia, by the misconducting of ingesta; and of causing troublesome excoriation and subsequent inflammation in susceptible and important parts, by the spilling of irritant stimuli upon them." (pp. 16—18.)

Mr. Fergusson says nothing respecting the mechanism of concussion of the brain: Mr. Miller quotes from one of our predecessors,* and we presume adopts the account, there given, of M. Gama's experimental explanation of the manner in which concussion of the brain is produced: on that head we may be pardoned for observing that M. Nélaton and M. Desnonvilliers admit, that M. Gama's explanation is probably the correct one; but they both state, that they failed to obtain similar results on repeating M. Gama's experiments. It so happens, that the experiments in question were also repeated by the writer of this notice, and it may not be out of place to say, that he succeeded in distinctly reproducing the more essential phenomena described by M. Gama. Be the mode of production of concussion of the brain, however, what it may, the more interesting and important question remains, whether concussion is the consequence of, or is attended with, any appreciable physical alteration of the brain. Neither Mr. Miller nor Mr. Fergusson touch upon that point, and with so saying, any reference to it on our part should perhaps end, but as another opportunity for the purpose may not soon occur, we are unwilling to let the present one pass, without asserting the priority of a distinguished English physician, Dr. Bright, in having observed and described a condition of the brain after concussion, which there is much reason to conclude, though as yet the fact can scarcely be considered established, reveals the intimate nature of the injury sustained by that organ.

Two eminent French surgeons, M. Nélaton, in his 'Elements of Surgical Pathology,' and M. Desnonvilliers, in the 'Compendium of Surgery,' com-

* British and Foreign Medical Review, No. xx. p. 16.

menced conjointly with the late M. A. Berard, and, since his lamented death, continued along with M. Gosselin, have each given an account of certain morbid appearances observed after death from concussion of the brain. M. Nélaton, it will be observed, attributes to Sanson the merit of having first pointed out the appearances described in the following extract:

"Sanson, who had a predilection for studying every question relating to injuries of the head, was the first to indicate an alteration of the substance of the brain, which he drew our attention to several times. The alteration in question consists in minute extravasations of blood, as large as the heads of small pins, disseminated in the substance of the brain. Since our attention was directed to this fact, we have discovered, under similar circumstances, these small miliary extravasations, which might be confounded, were we not upon our guard, with minute drops of blood presenting at the orifices of the vessels when a slice of brain is cut away. But that mistake is easily avoided; for in the first case the minute clot of blood can be removed with the point of the knife, while in the second it is fluid blood that presents at the extremity of the vessel; and, moreover, it is possible to remove the little speck of blood and cause it to reappear several times in succession at the same point, by gently compressing the substance of the brain. It requires, no doubt, great care to always discover so slight a lesion; yet it will seldom escape observation, if we have the patience to first remove the pia mater and then pare away the brain in very thin slices. These extravasations of blood are found at the periphery and towards the centre of the brain; they occur equally at the point percussed and at points adjacent to it, and at others more or less remote. The small extravasations are sometimes numerous, sometimes few, and but five or six of them may be disseminated throughout the brain." (Nélaton, t. ii. p. 575.)

M. Nélaton then goes on to observe, that the alteration above described constantly exists in contusion of the brain also, and he thence infers that *concussion* must be regarded as a slight degree of *contusion*—an inference not warranted by the premises, as the appearance in question only shows, at the utmost, that bloodvessels are ruptured in both cases; and finally, M. Nélaton says, it by no means follows that the minute extravasations of blood above described are present in every case of concussion of the brain, because it is quite conceivable that a shock may suffice to rupture some of the cerebral fibres, without being sufficiently energetic to rupture the bloodvessels also, and the opportunity of examining the brain only occurs in fatal cases of the accident, in which the severer shock capable of rupturing the bloodvessels has been inflicted. The second volume of M. Nélaton's work was published in 1849, and we do not know any earlier notice of Sanson's having made the observation, respecting the condition of the brain in concussion, attributed to him by M. Nélaton. There is no allusion to the subject in the 'Nouveaux Elements de Pathologie Med. Chirurg.,' by Roche and Sanson, in which the latter writer expressly treats of injuries of the head.

In the 'Compendium of Surgery,' by MM. Desnonvilliers and Gosselin, two fatal cases of concussion of the brain, with the *post-mortem* appearances, are recorded. The first of these cases was communicated by M. Bayard to M. Desnonvilliers, and is peculiarly interesting, because it adds another to the extremely few cases in which any account is given of the condition of the brain, where death occurred very soon after the accident; and in which, consequently, the appearances cannot be referred to changes produced by consecutive complications. The circumstances, under which the injury was inflicted, were almost identical with those in Littré's celebrated case. A man, aged twenty-three, was arrested in the suburbs of Paris, and having threatened to commit suicide, was confined in a cell with his hands tied behind his back; when left alone, he dashed his head violently against the wall, fell senseless, and died in about *three quarters of an hour*. On dissection, there was no fracture of the skull, no extravasation of blood, either between the dura mater and the bone, or on the surface or in the substance of the brain. Neither was there any contusion or laceration of that organ. The brain, moreover, presented its natural consistence; but throughout its substance were interspersed small specks of blood, and, on gently compressing the sliced brain between the fingers, fluid blood exuded from the vessels. In the second case death occurred *eight hours* after a fall from a considerable height. Several complications, however, existed in this case. There was fracture of the cranium with depression, and slight laceration of the brain at one point, with slight contusion of the organ at the point diametrically opposite, but no blood was extravasated, either on the surface or in the substance of the brain. On slicing the brain, however, appearances similar to those seen in the preceding case were discovered.

M. Desnonvilliers admits the accuracy of the account given by Littré of the condition of the brain, in the well-known case recorded by that writer; and he thence concludes, that when concussion of the brain is immediately fatal, the only cognisable alteration is a contraction or shrinking of the organ; with that point, however, we are not at present concerned. But M. Desnonvilliers further infers, from the two cases of which an outline has been given above, that when the patient lives for some time, the anatomical characters of concussion consist in congestion of the vessels of the brain. And when the patient survives the affection, he thinks it may be fairly assumed that the organic changes are the same in kind but less in degree.

We need not stop to point out the differences between the anatomical characters described by M. Nélaton, and by M. Desnonvilliers; but it will be seen that Dr. Bright's account of the appearances observed after concussion of the brain, embodies those that are given by both the French writers; one of whom he anticipated by eighteen, and the other by twenty years.

We have not space to quote as fully from Dr. Bright as we could desire; but it is, perhaps, enough to say, that in his *Reports of Medical Cases*, &c., vol. ii. part 1, 1831, Dr. Bright describes at page 408 the "small clots from laceration . . . . of the size of large pin-heads," occupying "the internal part of the brain," &c. and that at page 410 he writes as follows:

"What then is the immediate state of the brain producing the symptoms of concussion? Of this we can only judge from the nature of the injury traceable after death. . . . . and almost the only appearances which can be considered peculiar are the minute lacerations of the brain and vessels, which occur both upon the surface and deep in the substance of the brain; and we are led to conclude that the violence done to the brain, if it does not always go the length of producing these appearances, has at least such a tendency, and that it is this rupture of the brain, or an approach to it, with some consequent congestion in the vessels, which gives rise to the peculiar symptoms of concussion."

And a little further on Dr. Bright comes to the conclusion, which harmonizes so well with practical experience, that " a process of repair"—a " mode of healing,"—is required for the cure of concussion, probably " analogous to the repair of apoplectic injuries, requiring therefore much time, and calling for great caution in the management of the invalid for a long period after the first symptoms."

The condition of the brain in concussion requires, no doubt, further investigation ; but the existing evidence, so far as it goes, strongly tends to show the connexion between concussion of the brain and the morbid changes above mentioned ; and if further researches establish that connexion more completely, the merit of having first elucidated one of the most obscure and debated points in surgical pathology, will certainly belong to Dr. Bright. It is scarcely necessary to remark how much more satisfactorily all the phenomena of concussion can be understood by tracing them to the physical alterations indicated by Dr. Bright, than by the vague expedient of referring them to " functional derangement." It will probably, also, be admitted, that M. Gama's experimental illustration of the mode of production of concussion of the brain, quite accords with the appearances that have been observed in fatal cases. But this digression has made a formidable inroad on our space, and it is time to return to Mr. Miller and Mr. Fergusson.

Mr. Miller thinks the operation of trephining is generally inapplicable " in the case of compression by extravasated blood," because it is uncertain whether we shall hit upon the site of the extravasated blood, and if we should succeed in doing so, the blood may lie beneath the membranes of the brain ; and, moreover, we may be unable to remove the extravasation even if we have exposed it ; while, on the other hand, we are tolerably certain to greatly increase the danger of inflammation by the operation. To this general rule Mr. Miller admits the following exceptions :

" The exception consists in those cases of injury applied in the course of the middle meningeal artery, immediately followed by urgent symptoms of compression, with or without fracture of the skull, in which we can have little doubt of the following circumstances :—1. That the compression is caused by extravasation of blood ; 2. That the blood has been extravasated at or near the point struck ; 3. That the extravasation is situate exteriorly to the dura mater ; 4. That the blood is yet mainly fluid, and therefore likely to escape readily outwards, on an aperture of communication being established ; 5. That even if it have coagulated, extrusion may yet be effected, without necessarily exciting inflammation, either in the brain or in any of its membranes. Under such circumstances, we need not hesitate to apply a trephine to the injured part—when the symptoms of compression are sufficiently urgent to demand direct interference—with the full hope of affording most important and salutary relief.

"We can also conceive it possible, that an injury may be sustained at a part of the cranium not connected with the course of the meningeal artery ; that the symptoms of compression by extravasation may be both very urgent and very plain ; and that the surgeon, after careful examination and consideration of the case, may feel satisfied that the site of extravasation corresponds to the part struck. The trephine is applied. If blood be found at that part, exterior to the dura mater, the issue is most fortunate. But if no blood be found, two questions naturally arise : Are the membranes of the brain to be perforated ? or is another part of the cranial contents to be exposed by reapplication of the trephine ? The latter question is certainly to be answered in the negative ; the former, in the affirmative, only when the dura mater is elevated through the trephine-hole, tense, comparatively non-pulsating, perhaps fluctuating, or otherwise affording tolerably distinct evidence of the sought-for blood being lodged beneath." (p. 31.)

With reference to trephining in "compression by accumulation of pus between the cranium and the dura mater," Mr. Miller lays down the following rules of practice :

"If, on removing a portion of skull by the trephine, matter is not found, a question arises whether our efforts at direct relief are to cease, or whether further exploration is to be attempted. Is the dura mater to be perforated, in the hope that the site of abscess may prove to be beneath? Not, if the membrane present its usual normal characters at the part exposed; level, moving synchronously with the cerebral mass, smooth, of a brownish hue, and showing something of a silvery lustre. But if it be protruding through the cranial aperture, flocculent, non-pulsating, and either too dark or too pale in colour—and, more especially, if it afford anything of a feeling of fluctuation when touched—we need not hesitate to puncture, and need not doubt to find an issue of purulent or other fluid from the wound. If the dura mater appear sound, and its puncture consequently be unwarrantable, are we permitted to re-apply the trephine; either at the site of *contrecoup*, or in the immediate vicinity of the first application? Either of these procedures may be warrantable, if the symptoms of dura-matral abscess are peculiarly marked, and the surgeon is thoroughly convinced of its existence. But, as can readily be understood, the latter site of the reapplication is preferable. And, as already stated, a large size of trephine should be employed at first, to anticipate the necessity of such repetition. Only in very extreme cases should the site of *contrecoup* be trephined. Having failed in the indicated spot, we proceed to other explorations with great uncertainty. Fortunately, however, it is comparatively seldom that the site of abscess is elsewhere than at the injured part." (p. 35.)

But what are "the peculiarly marked symptoms," in virtue of which the surgeon is to be "thoroughly convinced" of the existence of "*dura-matral* abscess"? The most significant indications are the local, coupled with the general symptoms, so very well described by Mr. Miller, but which it is unnecessary to quote. But these indications are not conclusive. If they were, there would be no need of the precept to search for pus at the site of *contrecoup*, or elsewhere, after having failed to discover it at the point where the local, but fallacious, signs existed. And again, how is it to be known that there was any counter-stroke? or if one did occur, how is its exact site to be determined? for the counter-stroke does not uniformly take place at the point diametrically opposite to that which received the blow; and, very often indeed, when a counter-stroke has occurred, it corresponds to some part of the base of the skull.

Mr. Fergusson is much less reserved than Mr. Miller in the use of the trephine in compression by extravasation of blood, or by pus; and, probably for that very reason, he is much less precise in specifying the conditions under which its employment is indicated. Mr. Fergusson draws no very clear distinction between compression by extravasated blood, and compression by pus—that is to say, so far as his rules of practice are concerned. He thus briefly disposes of the two questions :

"If a person had received a blow on the temple, and if the symptoms indicated effusion of blood from the meningeal artery, I should not hesitate to perforate the cranium over the supposed collection; and in the event of compression coming on in a number of days, and when suppuration might be suspected, the same method might be adopted. Here, however, it might be difficult to determine the part on which the trephine should be applied. Where blood has been suspected, I have seen great hesitation on the latter point, and so also there may be in the supposition of an abscess being present. In the latter case, the wound on the surface would probably be selected; more especially if the bone around seemed diminished in its vascularity, or altogether deprived of circulation. [Here intervenes the description of the manner of performing the operation of trephining.] In the instance of fracture the operator can scarcely go wrong as to the proper part for applying the instrument; but in other cases he may discover that he has erred in his diagnosis, or he may then (not finding that which he has expected) think of opening another part of the cranium, either immediately contiguous or at some distance. Such proceedings are now rarely ever heard of; yet, under the desperate circumstances of the patient, I should consider the surgeon justified in making further search: provided always, that he has some degree of reason on his side (such as that the patient is not actually *in articulo*, for I have heard of a zealous practitioner persisting in the use of the instrument until it was hinted that his patient was dead); and there is no lack of precedent for such a course. Two, three, and four pieces—even a larger number—have often been removed with success; and Vander Wiel relates one instance where the trepan was applied twenty-four different times with success." (pp. 549—551.)

Reaction is liable to occur in the practice of surgery as in everything else, and mediæval tendencies are now-a-days pretty strong. Modern surgeons very properly deviate widely from the over-cautious maxims of Desault and his school, yet we scarcely anticipate a revival of the heroic performances of the age of old Stalpartius Vander Wiel.

There is no material difference in the precepts laid down by Mr. Miller and Mr. Fergusson for trephining in *fractures* of the cranium.

Mr. Miller notices dislocations of the cervical vertebræ with great brevity; Mr. Fergusson considers them at greater length, but mixes up dislocations caused by external violence with those consequent upon disease of the spine. With reference to the treatment of traumatic dislocations of the cervical vertebræ, Mr. Miller says:—"If life, or the hope of life, remain, replacement is to be effected by careful extension and coaptation." (p. 302.)

Mr. Fergusson on this subject writes as follows:

"The cervical vertebræ are occasionally displaced, either as the result of accident or disease. When the former happens, it is customary to suppose that the spinal marrow is so affected that immediate death is the result, especially if the injury is above the origin of the phrenic nerve. I have not myself seen any case of this kind, but I believe that there are few in the profession who doubt the truth of the current doctrine: it has been asserted, however, that such displacements may be treated like luxations in the extremities—that is, by extension and counter-extension, and cases of the kind have been recorded in modern journals. The practice which has been proposed in such instances is not so novel as some people imagine. Heïster states that 'what is vulgarly called a broken neck is generally no more than a luxation, though sometimes the vertebræ are fractured. If life should remain after such a luxation, which very rarely happens, the patient's head is commonly distorted, with his chin close down to his breast, so that he can neither swallow anything nor speak, nor even move any part that is below his neck; therefore, if speedy assistance be not had, death ensues from the compressure or hurt of the medulla. But to repulse this unwelcome messenger, the patient is to be immediately laid flat upon the ground or floor; then the surgeon kneeling down, with his knees against the patient's shoulder, is to bring them together so as to contain the patient's neck between them; this done, he quickly lays hold of the patient's head with both his hands, and strongly pulling or extending it, he gently moves it from one side to the other, till he finds, by a noise, the natural position of the neck, and the remission of the symptoms, that the dislocation is properly reduced."

"That the neck may be straightened in this way I believe, but I should imagine that the injury to the spinal marrow would be such that this 'unwelcome visitor' would keep his place. Heïster is certainly a very different authority on professional matters than Rabelais, yet by the above account one is forcibly reminded of the exploit of Panurge in replacing the severed head of his friend Episthemon." (pp. 540, 541.)

Heïster's account of the matter, no doubt, besides being somewhat grotesque, is quite too vague to satisfy the exigencies of modern science; but the cases "recorded in modern journals," and in some modern books also, deserve a more serious examination. We cannot pass those cases in review here, but we may say generally that they establish—*First*, what Heïster affirmed (though he completely reversed the relative frequency of dislocation and fracture), and what was subsequently long denied, that simple dislocation of the inferior cervical vertebræ may occur; *secondly*, that "immediate death" is by no means the necessary result of such dislocations; and, *thirdly*, that these dislocations have been reduced, and that the patients have recovered. On this latter point anatomical evidence is, of course, wanting, the patients having survived; but the cases recorded by Leveille (Desault's case), Rust, Newmann, Walther, Schuh, Willefeld, J. Guerin, Mignonneau, and Drs. Harrison and Ellis, leave no reasonable doubt upon that score.

Heïster made another proposal, respecting which Mr. Miller and Mr. Fergusson do not entirely agree—we mean, trephining in fracture of the spine; for Heïster, and subsequently Viguerie, suggested that operation, which was first performed by Mr. H. Cline. Mr. Miller thus unreservedly rejects the proceeding:

"In the obviously displaced spinal fracture, with symptoms of compression of the cord, it has been proposed to employ the trephine, with the view of relieving the injured medullary matter. Reason and experience, however, have decided against the procedure; inquiry having shown that the compressing agent is usually the fore part of the body of the vertebra, which cannot be reached and dealt with from without." (p. 301.)

Mr. Fergusson, in the following judicious observations, is less absolute in condemning the operation:

"The analogy between such injuries [of the spine] and those of the head, when there is depression of bone, though in some respects close, is widely different in others; thus, there may be extensive depression on one side of the cranium without any such condition on the other; but in the spine, when the column is broken, the irregularity will probably be as much on one side of the canal as on the other, consequently, pressure on the spinal cord may just be as great and as destructive in front as behind. However, it is possible to imagine an instance where a lamina is driven in without the body of the vertebra being affected; and, as the case may be in any way almost hopeless, it might be deemed advisable to give the patient the advantage of this poor chance." (p. 714.)

In fractures of the spine, Mr. Miller recommends "careful reduction of the displacement" (p. 301); and Mr. Fergusson does not object to making an attempt at reduction, though he questions its utility. "No harm, in my opinion," he says, "can arise from a moderate attempt at extension, although its ultimate utility may be doubted; but the idea of forcing the protrusion into a proper position seems preposterous." (p. 714.)

The admissibility and value of the practice in question have certainly yet to be determined by future experience; but Mr. Crawford's case, referred to by Mr. Fergusson, and those published by Mr. Tuson, and by M. Malgaigne, are calculated to encourage surgeons to give it a more extended trial. Neither Mr. Miller nor Mr. Fergusson mention the use of Dr. Arnott's hydrostatic bed in the treatment of fractures of the spine; and yet its value can scarcely be overrated in the management of many cases of that accident.

It does not fall within Mr. Fergusson's plan to consider wounds of the thorax and abdomen, and he merely makes the most general possible allusion to them, especially the latter. The following passage respecting wounds of the abdomen is equally applicable to the description of notice taken by Mr. Fergusson of wounds of the thorax:

"I maintain that in this department, as in all others pertaining to surgery, the properly-educated surgeon should here have his knowledge intuitively, as it were. A fixed rule for all such cases cannot be laid down; the management must be left to the discretion of the surgeon, and a reliance upon the general principles of surgery will be more likely to produce good results than a reliance on a dogma which may possibly not be applicable to the case. Perhaps I may err in thus referring to such injuries, and possibly this may arise from my want of experience in cases of the kind. I believe I am not wrong, however, in stating that instances of the sort occur but rarely in civil practice, and when they do come under notice, the circumstances are usually such as need no special directions beyond those precepts which belong to a knowledge of the science of surgery, founded on anatomy and physiology." (p. 708.)

Rational and scientific practice must, unquestionably, be founded on sound general principles; but it is equally true that an important, nay, an indispensable, element of successful practice lies in a knowledge of details—that is to say, in knowing how to apply general principles in particular cases, and when to deviate from general principles under peculiar circumstances. But how is the most gifted student to lay the basis of that familiarity, not only with general principles, but with their application also, which, with accomplished surgeons like Mr. Fergusson, ultimately ripens into something occasionally resembling intuition? One of the most important means of so doing consists in the oral and written instruction of such men as Mr. Fergusson; and as comparatively few can have the advantage of the former, it were to be wished that Mr. Fergusson had indicated to the student the connecting links between principles and practice in these particular cases.

Mr. Miller, in enumerating the signs of intra-thoracic hæmorrhage, says: "The patient lies only on the affected side, and the corresponding cheek has often been observed of a purple colour." (p. 316.) We acknowledge we were not previously aware of the latter sign of intra-thoracic hæmorrhage; but a purple discoloration of the integuments over and below the most depending part of the thorax, resulting from transudation of blood—Valentin's sign, in short, as it is called—has often been observed in those cases.

In the treatment of hæmorrhage into the pleura from a penetrating wound, Mr. Miller says:—" The wound is to be kept open, means are to be taken to arrest the hæmorrhage at its source, and at the same time to assist the respiration." (p. 317.) All very well if the hæmorrhage comes from an accessible source, such as the intercostal artery, for example. But if the source of the hæmorrhage should be inaccessible, if the lung itself were wounded and bleeding freely, is it not the received and well-founded opinion, that so long as the blood can find a passage through the external wound, the bleeding is likely to continue; and that the best chance of arresting the hæmorrhage is had by closing the external wound as accurately as possible, so that the blood, being confined in the pleural cavity, may compress the bleeding vessels, and act like a plug, as it were, in arresting the hæmorrhage? It is true, as Mr. Fergusson so well remarks in the passage last extracted from his work, that no "fixed rule" can apply to every case alike. If the fixed rule fails in its application, the surgeon must, if he can, devise some new application of general principles, or find resource in his anatomical or physiological knowledge. It was thus that, in a case of penetrating wound of the thorax, involving the lung, when death appeared imminently impending from excessive hæmoptysis and bleeding into the pleura (the external wound being closed), M. Duret, of Brest, considering the difference between a small aperture in the parietes of the thorax, merely sufficient to let the blood effused in the pleural cavity escape, and a free opening, which would probably produce immediate collapse of the lung, and consequent contraction of its vessels and diminution of the quantity of blood circulating through it, enlarged the external wound, in the direction of the intercostal space, to the extent of three inches. The lung collapsed; the hæmorrhage ceased on the instant; and the patient recovered.

The operation for establishing an artificial anus having, of late years, attracted a good deal of attention, a statement of the opinions held by Mr. Miller and by Mr. Fergusson on the subject will probably be acceptable. Mr. Fergusson says on this head (the first part of the extract, it is scarcely necessary to premise, relates to congenital imperforate anus):

"When the infant cannot be relieved by an incision in the perineum, the sigmoid flexure of the colon, or any other portion of the large intestine, which may happen to be prominent, may be cut into through an opening in front, as was originally proposed by Littré. The descending colon, as was recommended by Collison, perhaps even the sigmoid flexure, may be reached behind where not covered by the peritoneum, and thus that membrane may be avoided; but such proceedings have been attended with indifferent success, and considering the condition in which the patient is afterwards left, with an artificial anus in the side, constantly permitting the escape of the contents of the bowels, fatal results are scarcely to be regretted. I have repeatedly, now, on being consulted in such cases, stated the particulars fairly to the male parent, and have left him to decide; the decision has invariably been against any operation under such circumstances.

"In the adult the lower part of the intestine, in some individuals, becomes completely obstructed, by the contraction of a stricture, or in the progress of scirrhus and cancer of the rectum. Amussat, in such a case, has strongly recommended the formation of an artificial anus higher up, and has succeeded in relieving several patients in this way. Mr. Teale, of Leeds, Mr. Alfred Jukes, of Birmingham, and others, have performed such operations. The latter gentleman has published some drawings of the parts, in an instance where he opened the descending colon behind the peritoneum in the lower part of the lumbar region; his patient died on the sixteenth day: and one upon whom Mr. Teale operated in March, 1842, died on the seventh day after.

"This subject has been extensively brought under the notice of the Royal Medico-Chirurgical Society, by Mr. Cæsar Hawkins and others, during the present session, 1851-2, and the success of such an operation, though of a temporary character, is such as might lead one unhesitatingly to resort to such a proceeding in all cases which seem to demand this mode of treatment." (pp. 740, 741.)

Mr. Miller agrees so nearly with Mr. Fergusson that his observations need not be quoted in full, especially as the extract would be a very long one. In congenital imperforate anus, if the passage cannot be restored by an operation performed in the perineum, Mr. Miller, like Mr. Fergusson, deprecates forming an artificial anus elsewhere, thinking "it were better to leave such [children] to perish by the original obstruction of the bowels, than to force on them a more miserable and scarcely less brief period of existence." (p. 394.) In the case of adults affected with malignant disease of the rectum, Mr. Miller would leave the decision to the patient after "having had the danger of the operation, and the almost disgusting result of its success explained to him." In insuperable obstruction from non-malignant disease of the rectum, Mr. Miller thinks "the expediency of the operation may be safely urged upon the patient." Mr. Fergusson, it will have been observed, says, "*Perhaps* even the sigmoid flexure of the colon may be reached behind." Mr. Miller goes further, and asserts, that "the sigmoid flexure of the colon is *plainly* the part of the intestinal canal to be reached, and it may be opened either before or *behind*." (p. 395.) Mr. Miller expresses no decided opinion respecting the relative merits of Littré's and Callisen's (or Amussat's) operations. The former, he says, is more dangerous, and leaves the patient in a condition more offensive both to himself and to others; but, on the other hand, it is the simpler and easier proceeding, and, after some time, the opening gets "something of a sphinctral power," and the escape of fæces may be prevented by a good truss, which the patient can easily manage himself. The only objections urged by Mr. Miller against M. Amussat's operation are—that the opening is liable to contract inconveniently, and that it is out of the reach of the patient; while the proceeding is admitted to be safer, less offensive in its results, and less liable to the risk of prolapse of the bowel.

The exceedingly fair and judicious parallel drawn by Mr. Miller between Littré's and Callisen's operations, seems decidedly in favour of the latter proceeding. But, so far as we can collect, Mr. Miller thinks the balance between them is pretty nearly equipoised, if it does not, indeed, rather incline in favour of Littré's method. We cannot but think that the one element, of Callisen's operation, being admittedly the less dangerous to life, should alone outweigh every other consideration, even if nothing more could be alleged in its favour; but, in addition to that, Callisen's method has the comparatively minor, but positively very important, advantage of greatly diminishing, and, in some instances, almost completely exempting the patient from the loathsome annoyances experienced by patients with artificial anus on the anterior parietes of the abdomen. Mr. Fergusson and Mr. Miller have, perhaps, been somewhat hasty in condemning Callisen's operation in congenital imperforate anus, when relief cannot be given by incisions in the perineum. We are not, indeed, aware whether any of the more recent operations of the kind proved ultimately successful; but it is known that, in at least one instance, the result was favourable. The individual mentioned by Sanson attained the age of manhood, and was capable of engaging in the occupations, probably laborious, incident to his humble position in life. As to endeavouring to reach the sigmoid flexure of the colon from behind without injuring the peritoneum, the suggestion is, we believe, novel, and not very likely to be acted on. Indeed, bating an anomalous condition of the part, the thing would be quite impracticable.

In considering stricture of the urethra, Mr. Fergusson apprehends he is venturing "on ticklish ground, for most writers on stricture seem to be particularly pugnacious." (p. 191.) Mr. Fergusson is not obstinately wedded to any favourite method of treating the disease, but varies his practice according to the circumstances and varying phases of each particular case. Progressive dilatation is, he considers, the preferable plan in the majority of cases; but "the method by cauterization," he thinks, "is perhaps too much neglected in the present day." The lunar caustic, Mr. Fergusson thinks, is probably useful by merely allaying irritability, "but the caustic potash, besides this, undoubtedly destroys tissue, and so permits a more rapid access along the urethra with a large instrument." (p. 786.) Mr. Fergusson also frequently incises strictures in the anterior part of the canal, and occasionally scarifies them all round with "excellent effect;" but "in the deeper part of the urethra, this plan" (says Mr. Fergusson,) "is not to be recommended, and should be ventured on solely by one fully acquainted with the anatomy of the parts." (p. 791.) As regards this practice, Mr. Miller agrees with Mr. Fergusson that "for very tight and unyielding contractions anterior to the suspensory ligament, the method is not unsuitable, though, even there, it is not unattended with some risk of infiltration of urine through the cut parts; but Mr. Miller, very properly, in our opinion, altogether condemns the use of the lanceted catheter deep in the urethra, "under any circumstances whatever." (p. 528.) Mr. Miller tolerates the use of the caustic bougie "as a corrector of irritability;" but, with respect to its use as a caustic proper, he makes the following observations, than which nothing could be more sound and judicious.

"To prove successful as an escharotic, in clearing away obstruction, the mucous membrane must first be sacrificed; and though, for a time, ample space may thus be obtained, yet in the end recontraction is obviously inevitable; partly by reason of the plastic deposit which surrounds ulceration, and partly by reason of the contraction which invariably attends on cicatrization of a sore." (p 529.)

Mr. Miller considers the "perineal section" in the treatment of stricture of the urethra very briefly, but very fairly and judiciously. He believes, as almost all surgeons do, that "some few strictures are really impermeable;" and in such strictures Mr. Syme's particular operation cannot, of course, be performed. On the other hand, Mr. Miller is persuaded that the vast majority of 'penetrable' strictures can be well and safely cured without the use of cutting instruments. But some few cases of permeable stricture do prove intractable under the ordinary method of treatment, and in such cases Mr. Miller thinks that Mr. Syme's operation is "very suitable." (p. 530.)

Mr. Fergusson's estimate of the "perineal section" appears in the following extract, which is the more interesting, as in it are stated some facts, derived from Mr. Fergusson's own experience, respecting the immediate and ultimate results of an operation, which has unhappily occasioned so much and such embittered controversy.

"Every surgeon of experience must have met with examples of stricture, with little or no indication of mischief otherwise in the perineum, where the bougie has been productive of mischief, or where probably the disease, having been temporarily improved, has speedily returned, and where again and again there have been the same treatment and results. It is chiefly in such cases that the division of the stricture has been recommended, and in advocating the operation Mr. Syme sets down as fundamental rules for its due performance certain doctrines totally at variance with those generally held by surgeons. An instrument must be passed through the stricture, otherwise the perineal section cannot be performed. Surgeons have heretofore considered this the commencement of certain success with the after use of the bougie. Mr. Syme makes it essential that an instrument, however small, should be passed through the stricture, to guide the knife and to insure that the canal is laid open, and not any false passage that may be near it. To those who ask what is to be done when an instrument cannot be introduced? Mr. Syme replies, that there is no such thing as an impermeable stricture, and that it is only the surgeon's want of skill which prevents the proper introduction. Every-day experience shows that one surgeon will introduce an instrument where another has not succeeded, but there are few who maintain that they never fail in this proceeding, and it is certain that the most expert operators have failed to pass instruments where there has been positive proof at the time, by the dribbling of the water, that the canal has been in some degree permeable. . . . . .

"Mr. Syme's reports of this practice, as published in the 'Edinburgh Monthly Journal' and in the 'Lancet,' are most favourable, and I have, myself, repeatedly seen the best results follow, but, like all other cutting operations, it is not devoid of danger. Several deaths from it have been recorded, and I have myself had one fatal example, the patient sinking soon after the operation without any other seeming cause than the wound. I have seen alarming hæmorrhage, and one young man nearly lost from this cause, which continued from a wound in the bulb for twelve days, and brought the patient to the lowest ebb of life. No such mishaps have occurred in Mr. Syme's hands, however, although various other surgeons have not been so fortunate. . . . . From my own observation I can positively state that this mode of division of the stricture with the knife is not always followed by the entire relief from after annoyance that some have been led to expect, and whilst I think highly of the practice in certain instances, I am of opinion that, like all others in surgery wherein the knife is required, it is not free from hazards, over which the surgeon has no control." (pp. 787—789.)

Neither Mr. Miller nor Mr. Fergusson mention Mr. Colles' operation for the permanent cure of stricture at the orifice of the urethra consequent upon ulceration surrounding the outlet of the canal; a form of stricture almost uniformly unmanageable by any of the methods previously employed. The accidents that are liable to occur during the treatment of stricture are very slightly noticed both by Mr. Miller and Mr. Fergusson, but the rigors, heat, and sweating, that so frequently supervene after the passage of an instrument, are thus alluded to by Mr. Miller:

"Other patients are liable to suffer from agueish attacks, after the use of bougies; such are generally elderly persons, who have lived freely and been abroad. They benefit greatly by the use of quinine." (p. 526.)

It is needless to dwell upon the radical distinction between urinary fever and ague; or to observe that opium, whether as a preventive or curative means, is greatly preferable to quinine in these attacks, which cannot be termed agueish; although they have, indeed, been occasionally mistaken for remittent fever, because of their presenting a cold, hot, and sweating stage with tolerable regularity.

We at first read with something more than surprise the following passage in Mr. Fergusson's observations on the treatment of aneurism:

"In the practice of surgery, the application of a ligature to the vessel with which the disease is connected, has a powerful and specific effect; and although the operation for its accomplishment is not without danger, both at the time and subsequently, it possesses such advantages over all other modes of treatment, that the surgeon who, when other circumstances are favourable, hesitates to adopt or recommend it, may with justice have either his skill or his sincerity called in question." (p. 145.)

On reading further, however, it appeared that the preceding passage had been allowed to stand as it appeared in the original edition of the work, for the purpose, it may be presumed, of giving additional weight, as undoubtedly it does, to Mr. Fergusson's present opinion respecting the comparative merits of ligature and compression of the artery, in cases where the latter proceeding is practicable. What Mr. Fergusson's opinion *was*, we have just seen; what his opinion now *is*, may be learned from the following extracts. The quotations we are about to make are of considerable length, but Mr. Fergusson occupies a foremost place as a dexterous, and what is of greatly more value, a scientific and sagacious surgeon; and his matured judgment respecting one of the most momentous questions in modern surgery will be read with interest, and carry with it the weight it is so well entitled to.

"Since these remarks were published in the last edition of this work, the treatment of aneurism by compression has attracted further attention, and the results, in the hands of the Dublin surgeons especially, have placed the success of the practice beyond doubt. Much credit is due to Dr. Bellingham, of Dublin, for the attention he has paid to this interesting subject, and Mr. Tuffnell, of the same city, is not less worthy of praise. The latter gentleman has published a most admirable treatise upon it; and Dr. Bellingham's most recent views have been published, in a short paper on the Treatment of Popliteal Aneurism by Compression, in the thirty-fourth volume of the 'Transactions of the Royal Medical and Chirurgical Society, 1851.' From this paper it appears that 36 cases of external aneurism had been treated in this manner in Dublin, during the preceding seven years. To use Dr. Bellingham's own words, 'In 29 of these a cure was effected by compression; of the remaining seven cases the artery was tied in two, the patients recovering. In one, pressure was discontinued, the aneurism subsequently diminished in size, and the patient had the perfect use of the limb for three years, when symptoms of aneurism of the aorta supervened, and compelled him to give up his employment. In two, the limb was amputated, the patients recovering; and in the remaining two, death occurred, in one from pulmonary disease, in the other from a severe attack of erysipelas; but, in both, the local disease was very nearly cured, the aneurismal sacs being almost completely filled by fibrine deposited in concentric layers.' In not one of these cases can it be said that evil resulted from pressure. If erysipelas was the result of the interference of the surgeon, it was more likely the sequence of the galvano-puncture, which was resorted to in this instance in conjunction with compression, than from the pressure. These 36 cases were treated by 21 different surgeons, and both numbers give fair practical inference of what may be expected from a continuance of the practice among surgeons generally. The method has been tried frequently by other than the Dublin surgeons, but as yet we have no data from their cases to form any positive conclusions regarding it. Many instances have been mentioned to me wherein it has failed, but I have heard of many more in which it has been successful; and if Dr. Bellingham's Table—which, as we learn from the 'Dublin Medical Press,' for December 3, 1851, has since been increased to 62 cases—be contrasted with some of those showing the results of ligature of the femoral artery in the hands of various surgeons, the balance seems greatly in favour of compression. It has been ascertained by Dr. Norris, that of 188 cases, in which the operation was performed, 46 died, the majority of this number being from causes directly attributable to the use of the knife. In six of the cases of recovery in the above list, amputation of the limb was required. Of 119 cases of popliteal aneurism, collected in a tabular form by Dr. Crisp, 16 died, and in six of those wherein recovery took place, amputation was resorted to. So far as our comparatively limited experience in the method by pressure, as followed by the Dublin surgeons, will enable us to form an estimate of its value, it seems in many respects, if not in all, preferable to that by deligation of the main artery; and there seems these great advantages in it, that if it does not act satisfactorily, the Hunterian operation may still be resorted to with as much probability of success as ever, while by its application none of those formidable dangers are incurred, which are the well-known consequences of the application of the ligature. The difficulties and immediate dangers of a cutting operation are avoided. It may seem strange to make use of such language in the present day, as applicable to ligature of the superficial femoral artery—for that, I assume, to be the vessel meant by the unfortunately vague term of 'femoral;' nevertheless, when it is known that great difficulties have been experienced in such a proceeding, even by hospital surgeons and teachers of surgery, and that the accompanying great vein has been wounded by different operators, the facts cannot be overlooked. Mr. Syme states in the 'Edinburgh Monthly Journal of Medical Science,' for November, 1851, that he has tied the superficial femoral artery twenty times without a fatal issue, and with perfect success, and both he and his patients may be congratulated on such satisfactory results; but the tables above referred to show no such average success: and when it is borne in mind that the attempt at cure by pressure does not preclude the resource of the ligature, it seems to me that, with the ample evidence before us of the great success which has attended the modern practice by this means, the surgeon should undoubtedly give it trial ere he resorts to the knife. Unquestionably the evil result of the Hunterian operation, in regard to ligature of the superficial femoral artery, has in many instances resulted from the defective or injurious style of operation, but the same may be said of certain examples where pressure has failed. Granting both of these statements to be correct, it cannot be overlooked that ligature of the superficial femoral artery, done to the perfection of human skill, has nevertheless been followed by the worst possible results. I have seen Mr. Syme perform the operation repeatedly with admirable skill and precision in all points, and the results have been all that could be desired; but I have seen many others, and among them I may name the late Mr. Liston, perform the same operation with an equal amount of tact and judgment, yet the results have been very different. With pressure, surgery has still further resources, but with the ligature the fate of the case is, for a time, placed almost, if not quite, beyond human power; and doubtless the surgeons of Dublin who have resorted to this practice (many of whom stand among the highest of those who have graced the annals of the profession) have duly considered all these points." (pp. 150—152.)

"For either the popliteal or femoral aneurism the pressure may be made on the common femoral artery, or on the superficial. Sometimes one point may be chosen, sometimes another, and not unfrequently in one case both may be tried alternately.

"The principal feature in all the instruments which have been used for such practice is, that, while effectual pressure may be kept up on the main artery, the collateral branches throughout the greater part of the limb have free action, and the part below is sufficiently supplied with blood. Instruments similar to those which were used in the last century have been revived, and some modern improvements have also been called into play. I have seen the tourniquet of Signoroni (see p. 34) used with admirable effect; and two of the successful cases in the tables given by Dr. Bellingham were treated under my observation by my friend and former pupil, Mr. Robert Storks, with this instrument alone. But it is difficult to keep the pad steady on one point, and an instrument which fairly encircles the limb, without, however, compressing all the surface, is to be preferred. . . . . An ingenious addition to the common screw force has been applied here by means of bands of caoutchouc, whereby a certain resiliency is acquired, which, while it keeps up effectual pressure, may possibly obviate injurious effects from the screw. . . . . Whatever instrument is used, it is of importance to bear in mind that the absolute stoppage of the circulation is not essential, as it has been proved by experience, that a diminished force of circulation is sufficient to effect the desired end. If, however, it is found that pressure strong enough to arrest the flow of blood in the main vessel can be applied, then doubtless the favourable effects may be expected more speedily." (pp. 433, 434.)

Mr. Miller's observations on the same subject well deserve to be quoted along with those of Mr. Fergusson, and, like them, supply an excellent example of sound and candid surgical reasoning.

" *The Popliteal* is probably the most common of all external aneurisms; and, hitherto, the Hunterian application of ligature, to the superficial femoral, has been the only approved mode of treatment. Latterly, however, as elsewhere explained (Principles, p. 580), the application of pressure, instead of the ligature, has been employed. And experience is, almost daily, giving direct and undoubted testimony to the efficacy of the practice. There are some patients, doubtless, who may prove intolerant of pressure; and there may be others who prefer the apparent certainty of the knife and ligature, to the apparent uncertainty and delay of the compressor. But the greater number of cases are assuredly capable of cure by pressure properly applied; without risk, with but little pain or inconvenience, and without any wearisome amount of privation or confinement. The skin, which is to bear the pressure of the instrument, is protected by a layer of thick soap-plaster; and that, again, may be covered by leather. More than one compressor is used; or, at least, pressure is made at different parts, at different times; so that the burden of it may not all be thrown on one point, but, by being subdivided, may be rendered more tolerable. Using several instruments, along the course of the vessel in the thigh—they may be slackened and tightened alternately; or the same instrument may be shifted in its site, with a like effect. It is never to be forgotten, that all severity of pressure is unnecessary; and that it is not essential to arrest the arterial flow, at the compressed point. And it is also important to remember, that should this mode of treatment fail, it by no means interferes with the subsequent performance of the ordinary operation; but, on the contrary, the constitutional treatment suitable for pressure renders the success of subsequent deligation all the more probable. Those surgeons who obstinately adhere to the old operation may adduce, as their apology, a series of successful cases so treated. But this is very plainly a contracted view of the subject; and as well might such practitioners prefer successful amputation of the hand to amputation of a finger, for a simple affection of the latter only. A surgeon of the olden time, who had succeeded in curing several successive cases of popliteal aneurism by amputation of the thigh, might very naturally entertain a distrust and dislike of the proposal to treat the same disease by ligature of the femoral; but the naturalness of such an aversion to the minor and modern practice, would not render it one whit the more reasonable or praiseworthy. And an impartial observer will not consider any one justified, in subjecting his patient to serious risk of life, by hæmorrhage, suppuration, and gangrene; while he has it in his power to effect a cure by a minor means, comparatively devoid of risk, and the failure of which will not militate against subsequent recourse to the major procedure—if necessary. Why should a mode of treatment, which causes little or no risk, always be passed by; or why should an operation always be had recourse to, which may, and not unfrequently does, result in direct loss of life? And the question comes in much force, if it be admitted—

and statistics will scarcely warrant even feeble contradiction of this any longer—that the two methods are at least equally successful for the cure of aneurism.

"Recorded facts seem to prove the following conclusions:—1, That, in popliteal aneurism, skilful compression of the femoral is capable of curing the disease, and that with comparative, and almost absolute safety to life and limb; 2, That the time expended in cure is, on an average, not greater than in the treatment by ligature; 3, That failure by compression does not compromise subsequent recourse to deligation; 4, And that, consequently, compression, when skilfully employed, being equally certain, far more safe, and not more tedious than the ligature, should in the great majority of cases be preferred. The only disadvantage of compression is the care and trouble necessary on the part of the attendant, with irksomeness and sometimes suffering on the part of the patient. The obvious and only advantage of deligation, on the other hand, is the facility and dispatch of its execution, with probable exemption from suffering afterwards by the patient, in the successful cases. The formidable disadvantage is, its proved risk to life and limb. (pp. 594—596.)

Our extracts must terminate here, though pages might readily be filled with quotations as creditable to Mr. Miller and Mr. Fergusson as they would, doubtless, be interesting to our readers. It were superfluous to express any opinion respecting the general merits of the works before us; for we could only express our concurrence in the favourable verdict long since pronounced by the profession, and we do not affect to suppose that we could add any weight to the decision of the authority most competent in the matter. At a time when new books daily fall still-born from the press, Mr. Miller's and Mr. Fergusson's works have, respectively, reached their second and third editions, and no eulogium need be added to such tangible evidence of substantial and acknowledged merit. It is only necessary to say, that these publications are not mere reprints; they are new editions in the proper sense of the term, a large amount of new and valuable matter having been added to both works, to bring them fully up to the existing state of science. Both works, it need scarcely be added, are written with the same generous and candid tolerance of the opinions of others which distinguished the former editions, and which stands in such strong and favourable contrast with the style of some surgical writers of the present day.

R. C. Williams.

REVIEW VI.

Lehrbuch der physiologischen Chemie. Von Prof. Dr. C. G. LEHMANN. Dritter Band.

Physiological Chemistry. By PROFESSOR LEHMANN. Third volume. —*Leipzig*, 1852. 8vo, pp. 518.

LEHMANN'S 'Physiological Chemistry' is now completed, and the third and concluding volume, which now lies before us, is in no respect inferior to its two predecessors, which were noticed at some length in the recent numbers of this journal. The first and second volumes were respectively devoted to "the organic substrata of the animal organism," and to "the animal juices;" the third volume embraces two distinct subjects—namely, "Histochemistry," and "Zoochemical Processes."

In the introduction to the "Histochemistry," we have a brief general sketch of the leading steps by which this department of science has been advanced to its yet very imperfect state. Müller was the first to adopt the plan of examining the action of chemical re-agents in microscopic preparations. This method in the hands of Mulder and Donders, and of other experimentalists, has already led to valuable results. Thus, it was only by this method of microscopico-chemical analysis, that we first attained any clear idea of the structure of the different varieties of horny tissue, or that all doubt regarding the axis-cylinder of the nervous fibres was removed. The numerous sources of fallacy, and the precautions by which they may be (as much as possible) avoided, are well pointed out in these introductory remarks, which are concluded by an attempt to establish the law that *the chemical nature of the tissue always corresponds to its function.* Our author endeavours to deduce this law from the following considerations. It has been a long recognised fact, that every tissue which is serviceable to the body almost solely from its physical properties, as its hardness, toughness, pliancy, &c., contains, as its most essential substratum, a substance which on boiling yields gelatine; we know, moreover, that those textural elements, which are especially distinguished by a high degree of elasticity, as the nucleated fibres of areolar tissue, and the true elastic tissue, exhibit perfectly similar chemical re-actions; and (as Lehmann proceeds to show in a subsequent part of the volume) all the more vitally active tissues—those namely, which, in addition to a certain amount of elasticity, possess the property of contracting in consequence of certain influences transmitted to them by the nerves—contain, as their most essential constituent, muscle-fibrin or syntonin; moreover, the mode of arrangement, and the chemical characters of the substrata constituting the nervous system, confirm the truth of the law, that the materials of which the tissues are built up present a chemical conformity with their vital capacities.

The *osseous tissue* is the first of the individual textures treated of by our author. In his microscopical description he almost entirely follows Kölliker; in the chemical department he freely avails himself of the labours of von Bibra, Stark, Ragsky, Marchand, Nasse, &c. In addition to a full account of the chemistry of the bones of man, and the different classes of animals, we have an excellent abstract of all that is at present known regarding the chemical changes occurring in primary sclerosis osteophyte, exostosis, osteoporosis (dilatation of the medullary cells and of the Haversian canals), osteomalacea, including the craniotabes of Elsässer which has been so admirably investigated by Schlossberger, in caries and in necrosis. There is nothing, however, in this section, or in the following one, *on the teeth*, specially demanding notice.

Cartilage, areolar and *elastic tissues, horny tissue*, and *hair*, are duly considered in their appropriate sections.

Contractile fibre-cells—a term probably new to many of our readers—next claim our attention. For most of our knowledge of these cells, which in a condition of aggregation have been known as organic or smooth (unstriped) muscular fibre, we are indebted to Kölliker. They usually occur as elongated, thin fibres, with pointed extremities, but sometimes as four-sided or club-shaped plates, with occasionally jagged edges. They usually present a distinct nucleus, which becomes more apparent on the addition of acetic acid. The substance of the cell sometimes exhibits granules linearly arranged along the axis of the fibres, but in other respects is homogeneous. These fibre-cells, when arranged laterally, form the bundles of smooth or unstriped muscle which are recognisable even with the naked eye in the intestinal canal. Kölliker makes two divisions of *pure* and *mixed* unstriped muscle; in the former the cells are closely grouped together, so as to form bundles, or even membranous patches; in the latter they are only scattered over other tissues; the former occurs in the muscular coat of the lower half of the œsophagus, and in the muscular coat of the stomach and intestinal canal, in the nipple, the prostate, and the vagina; the latter is most obvious in the trabecular tissue of the spleen, but may also be seen in the tunica dartos, in the middle arterial coat, in veins and lymphatics, in the corpora cavernosa, the prostate, the Fallopian tubes, the uterus, and the urethra; in the trachea, the bronchi, ureters, and vasa deferentia, the tissue presents an intermediate character approximating to the pure type. Both these forms are more or less imbedded in, and intermixed with, areolar tissue. The unstriped muscles are never enclosed in a true sarcolemma.

The use of these textural forms does not depend, like that of the elastic fibres, with which they are so often associated, on their physical character; but it is to them that the tissues in which they occur owe their contractility, under the influence of the nervous system. Ed. Weber, in his admirable article on "muscular motion," in the third volume of 'Wagner's Handwörterbuch der Physiologie,' has clearly established the differences of action between the contractile organs and the voluntary muscles. He has found that almost all organs provided with these fibre-cells undergo a very gradual, and, at first, a very limited contraction, when a mechanical or chemical irritant is applied to them. He chiefly employed a galvanic current obtained from a rotation machine, and he found that the movement very gradually passed from the nearer bundles of fibres to the more remote ones, and did not disappear for a considerable time.

The micro-chemical reactions of acetic, hydrochloric, sulphuric, nitric, chromic, and phosphoric acids, and of solutions of alkalies and alkaline salts, in various stages of dilution, on these fibre-cells, have been carefully studied by Donders,* Schultze,† Paulsen,‡ and Lehmann. From these experiments it appears that the substance of which these fibre-cells are composed closely resembles, although it is not identical with, the fibrin of the blood.

Lehmann observes that much importance should probably be attached to the circumstance, that those organs which present most vital activity are moistened by a fluid, which is very different from an ordinary transudation, or from the plasma of the blood. Liebig's discoveries have shown that the striped muscular fibres are surrounded by a fluid very different from the blood-plasma, and the same is the case with the fibre-cells of the unstriped muscles. Thus Schultze, in examining the middle arterial coat, found that it was permeated by a fluid which was very rich in casein. In 100 parts of the dried circular fibrous coat of the thoracic aorta, he found 23·1 of soluble constituents, of which 7·24 were casein; while in the middle coat of the carotid, which contains fewer elastic fibres, but far more

* Holländische Beiträge, and Mulder's Vers. einer phys. Chem.
† Ann. d. Ch. u. Pharm. vol. lxxi. p. 277.
‡ Observationes Microchemicæ; diss. inaug. Dorpati, Liv. 1848.

contractile fibre-cells than the aorta, he found 39 p. c. of soluble constituents, of which 21 were casein. This interstitial fluid had a faint alkaline reaction, and besides casein and salts, contained small quantities both of a substance coagulable by heat, and of a non-coagulable substance. Lehmann himself found that the fluid which moistens the muscular coat of the stomach of the pig has a distinctly acid reaction, although less intense than the juice of striated muscle; while the analogous fluid of the middle arterial coat (of different parts of the aorta and carotid of the ox), only faintly reddened litmus, and that from the tunica dartos exerted no action on vegetable colours. He endeavours to account for the circumstance of Schultze finding the juice of the middle arterial coat alkaline, by the supposition that it was mixed with some of the alkaline fluid yielded by the areolar tissue, or that decomposition had commenced in it. There is more casein and less albumen in the middle arterial coat and in the tunica dartos than in the muscular texture of the pig's stomach; the latter is also as rich in albumen as the juice of the voluntary muscles.

Creatine and inosite are contained in this juice in far less quantities than in the juice of the striated muscles; indeed, it is only by crystallometric admeasurements, made with the microscope, that we can convince ourselves of their presence. Lactic, acetic, and butyric acids are also present in minute quantities. The ratio of potash to soda was $= 38:62$ in the juice of the unstriped muscle of the stomach, and $= 42:58$ in that of the middle arterial coat; the ratio of the soluble to the insoluble phosphates $= 82:18$ in the muscular juice from the stomach, and $= 79:21$ in that from the middle arterial coat.

From these relations, and from others, fully described by Lehmann, but which our limited space forbids us to notice, it follows that there is, at all events, a very close analogy between the juice of these fibre-cells and that of striated muscular fibre. Even if we entertain any doubts (as some of our leading histologists still do) regarding the existence of the fibre-cells in the middle arterial coat, there is unquestionable chemical evidence that the striated and the unstriped muscles, and the contractile tissues, not only contain a solid material which is chemically identical in all three, but also that they are bathed by a fluid which differs essentially from all other animal juices in its acid reaction, its abundance of potash-salts and phosphates, and in its containing creatine, inosite, &c.

We regret that we have no space to notice the microchemical investigation by which it is proved that the three morphotic elements which occur in *striated muscular fibre*—namely, the substance of the fibrillæ, the substance of the nucleus, and the sarcolemma, are chemically different from one another; we must, however, extract Lehmann's remarks on muscle-fibrin, or syntonine, the true substance of the fibrillæ:

"From the preceding micro-chemical investigations, we must agree with Liebig in regarding the material extractable from the muscles by dilute hydrochloric acid as the true elementary substance of true muscular fibre. We have already remarked that this material, although it possesses the most essential properties of the protein bodies, and in many respects comports itself like blood-fibrin, is not identical with it. The muscle-fibrin or syntonin extracted from striated muscles by diluted hydrochloric acid, and precipitated from the solution by neutralization of the acid, forms, while moist upon the filter, a coherent, somewhat elastic, snow-white mass, which may be detached from the filter in plates or membranes;

on applying tension to the more delicate plates, they present under the microscope a fibrous appearance not unlike blood-fibrin. This substance, while still moist, is very readily soluble in lime-water, as well as in dilute alkalies; it coagulates from its lime-water solution on boiling, like albumen; and is precipitated both from this and the alkaline solutions by concentrated solutions of neutral alkaline salts; when placed in a moderately concentrated solution of carbonate of potash, the mass swells, becomes gelatinous and opaque, but does not dissolve; it is only after very considerable dilution that any solution commences. In this respect it perfectly coincides with the experiments I have made with carbonate of potash on flesh in large quantities, and likewise in the micro-chemical examination of the fibrillæ of primitive fibre. On adding chloride of calcium, or sulphate of magnesia, to the alkaline solutions of this substance, no precipitate is thrown down, unless the mixture be boiled; if, however, the alkaline solution be previously boiled (which at most renders it opaque), solutions of the above-named salts at once induce a flocculent precipitate. Nitric acid throws down a white flocculent precipitate from the alkaline solutions of syntonin. Chromic acid, or acid chromate of potash, and hydrochloric acid, precipitate this substance in flakes, both from its alkaline and acid solutions; pure hydrochloric acid, if added in excess, merely induces a turbidity in the alkaline solution. Uncoagulated syntonin does not dissolve in a solution of nitre (6 parts of KO,NO_5 in 100 of water) even after five days' digestion at 86° Fahr. It has been already mentioned that the primitive muscular bundles, even when digested for a longer period with a solution of nitre at from 86° to 104°, present no change under the microscope, which would lead to the supposition that there was even a partial solution of the finest muscular fibrillæ. Since, however, the above experiments were, for the most part, made with beef and veal, and, as is well known, the fibrin of ox-blood is almost perfectly insoluble in a solution of nitre, while that of other animals dissolves readily when digested with that fluid, other experiments were made with pork, from which the fat was thoroughly removed, the meat being finely chopped, and treated with distilled water till the expressed fluid no longer contained traces of albumen. This flesh-mass thus perfectly freed from soluble protein-compounds, was digested from two to five days, in a solution of nitre of the above strength; but this fluid did not dissolve even a trace of a protein-compound, coagulable by heat or acetic acid, or precipitable by any other reagent. The syntonin naturally contained in the fibrillæ of muscle is therefore just as insoluble in a solution of nitre, as the syntonin artificially obtained from the muscles by hydrochloric acid." (vol. iii. pp. 86, 87.)

Strecker's analyses show that this substance differs slightly in its ultimate composition from blood-fibrin.

The colour of the muscles is usually supposed to depend upon the quantity of blood which they contain. Lehmann, however, inclines to Kölliker's opinion, who believes that the muscles possess a pigment of their own, which is very similar to, but not identical with, hæmatin. Kölliker's argument, that this pigment is not contained in the vessels and blood-corpuscles, but adheres to the fibrillæ, is founded, not so much on chemical as on the following physiological grounds. During contraction, the muscles retain their colour; comparatively colourless muscles are often as rich in bloodvessels as strongly coloured ones; and, lastly, even under the microscope, some bundles may be observed to have a decidedly yellow colour.

We shall conclude this subject with the following remarks on the light which chemical inquiries have thrown on the physiology of muscle:

"The protein-substance extractable from the fibrillæ of muscle by extremely diluted hydrochloric acid, is the most essential element of animal motion; it is one and the same in the striated muscular fibres, in the smooth muscles, and in those tissues which were formerly termed contractile; it is, however, peculiar to those organs only whose power of movement is dependent on the nervous system. What changes in its physical properties this substance undergoes in the contraction of the tissues, we are unable to explain chemically.

Both in the voluntary and the involuntary muscles there is, moreover, a fluid which bathes and moistens the tissues in which the contractile fibres lie, and is distinguished by its acidity, and altogether different from the plasma of the blood. Liebig has calculated that the voluntary muscles alone contain more than a sufficient quantity of acid to neutralize the alkalinity of the blood. We have seen that wherever contractile fibres or fibre-cells occur, the permeating fluid or juice always contains a preponderating quantity of potash-salts and phosphates, while, on the other hand, the blood-plasma is poor in these salts and rich in alkaline chlorides and soda-salts. The antagonism between the intercellular fluid of the blood-cells and the interstitial fluid of the contractile fibres cannot be a mere accident. Liebig had already shown that this antagonism must give rise to an electric current, or must be occasioned by such a current; and since that time we know that Du Bois-Reymond* has thrown much light on the electrical phenomena of muscular contraction, and obtained many brilliant results, clearing up the preceding labours of Matteucci. It is sufficiently obvious that the development of electricity which accompanies muscular contraction is intimately connected, on the one hand, with the acid of the muscular juice, and on the other, with the alkali of the blood; and that the chemical constitution of the muscular juice is of special importance in relation to the function of the organ, from the well-known and very striking fact, that all muscles, voluntary as well as involuntary, (striated and smooth), very rapidly lose their contractility in water. This experiment, which any one may confirm for himself who has seen contractions induced by a rotation apparatus in the striated muscles, in the stomach, intestinal canal, or bladder of an animal just killed, appears to be opposed to the results obtained by the younger Liebig,† who found that muscles from which the blood had been as completely as possible removed by the injection of water into the vessels, retained their capacity for contraction as long as muscles which contained blood. There is, however, no real contradiction between these two observations; for if the muscles do not lose their contractility on the removal of the blood, but do so after the addition of water, this only proves that they are deprived of their capacity for contracting by the dilution of the muscular juice. To establish a connexion between the development of electricity during contraction with chemical forces, we must seek the causes of the polarity less in the antagonism of the alkali and acid, than in that of the solid substance of the muscular fibrillæ (syntonin) and the muscular juice." (vol. iii. pp. 98, 99.)

As the views of the younger Liebig, referred to in the above extract, may not be familiar to the majority of our readers, we will briefly give his principal results. His most important conclusion is, that a muscle cannot contract unless oxygen be present; he found that the muscles of the frog retained their capacity for contracting in an atmosphere of oxygen, longer than in a non-oxygenous atmosphere; that is to say, than in carbonic acid, nitrogen, or hydrogen. He further ascertained, that as long as the capacity for contraction existed, oxygen was absorbed, and a corresponding quantity of carbonic acid exhaled—facts which establish the view that at all events a great part of the carbonic acid produced in the animal body is formed externally to the capillaries in the parenchyma of organs, and that it is principally dependent on muscular action.

* Unters. üb. thier. Electricität. Berl. 1848-9.
† G. v. Liebig, in Ber. d. Akad. d. Wissensch. zu Berlin, 1850, pp. 339—347.

The succeeding twenty-five pages are devoted to *the Chemistry of the Nerves and Brain;* and although they contain all that is known on the subject (and perhaps something more), they leave much still unexplained.

The first portion of the volume concludes with *Exudations and Pathological Formations.* After alluding to the almost innumerable difficulties which surround this department of zoo-chemistry, he proceeds to point out the dependence of "the phenomenology of the process of exudation" on mechanical conditions, and especially notices "the memoirs of Jolly* and Ludwig† on endosmosis and endosmotic equivalents; C. Schmidt's‡ beautiful investigations regarding the relation of the coefficients of density of saline solutions to the diffusion-equivalents, and the remarkable discoveries of Graham§ on the diffusion of liquids."

Our author adopts Rokitansky's mode of arrangement, and considers exudations under the following heads:

1. As fibrinous, these being subdivided into plastic and croupous.
2. As albuminous; and,
3. As purulent, with which must be classed diffluent and hæmorrhagic exudations.

In connexion with tuberculous exudations, which are a mere subdivision of the fibrinous species, Lehmann observes that cystine has recently been discovered as occurring in old tubercles; he has not, however, had an opportunity of personally confirming the observation. We may here also direct the attention of our readers to the circumstance that Dr. Struthers has recently detected large quantities of crystals of urate of ammonia and triple phosphate in the contents of a pulmonary abscess, consequent on the lodgment (for four years and a half) of a foreign body in the air-passages. "The crystals of urate of ammonia," says Struthers, "were formed, no doubt, on the spot by the destruction and decomposition of the surrounding tissue."‖

We regret that we can afford little more than a passing remark to the excellent section on *Pus.* In accordance with the views of the best recent pathologists, Lehmann believes that no difference whatever exists between pus-corpuscles (or, as Henle terms them, cytoid-corpuscles), lymph-corpuscles, colourless blood-cells, and mucus-corpuscles. He mentions a case of catarrhal icterus, in which one of his pupils discovered glyco-cholate and tauro-cholate of soda in the contents of a large abscess in the leg, and a case of diabetes, in which sugar was found in a purulent discharge.

"The differences," he observes, "between pus and mucus, on which physicians formerly laid so much stress, are no longer of any importance, since a more recent physiology has taught us that there is no line of demarcation between these fluids, and that they gradually merge one into the other, and that the mucus in inflammatory affections of mucous membranes gradually presents large quantities of cytoid-corpuscles, and that at the same time its albumen increases, so that both in a physical and chemical point of view it thoroughly resembles pus, if it be not actually the same thing: even the quantity of fat in the purulent fluid secreted by inflamed mucous membranes—a point which Güterbock regarded as of the greatest diagnostic value—is often precisely the same as in genuine pus: in most cases the pus secreted from mucous membranes retains its mucous property of gelatinizing on the addition of water or of acetic acid." (vol. iii. p. 165.)

* Zeitschr. f. rat. Med., vol. vii. pp. 83—148. † Ibid. vol. viii. pp. 1—52.
‡ Charakteristik der Cholera, pp. 22—28. § Transactions of the Royal Society for 1850.
‖ Monthly Journal of Medical Science, Nov. 1852, p. 455.

We have now arrived at the second division of the volume—at the portion treating of *Zoochemical Processes*, which includes the consideration of *molecular forces, the formation of organic matter in the vegetable kingdom, a general review of the metamorphoses occurring in the animal body, digestion, respiration*, and *nutrition*.

Passing without comment over the first two sections, we would direct attention to our author's views regarding the differences which present themselves in the metamorphoses of the two great kingdoms of organic matter. Liebig, in one of his earlier writings, contrasted the functions of plants and animals, in the following terms :

"The PLANT *produces* neutral nitrogenous bodies, fats, sugar, starch, and gum; *decomposes* carbonic acid, water, and salts of ammonia; *developes* oxygen; *absorbs* heat and electricity; is a *reducing apparatus*, and is devoid of the power of motion.

"The ANIMAL *consumes* neutral nitrogenous bodies, fats, starch, sugar, and gum; *produces* carbonic acid, water, and salts of ammonia; *absorbs* oxygen; *developes* heat and electricity; is an *oxidizing apparatus*, and possesses the power of motion."

We shall endeavour to show how far these statements require modification. There can be no doubt that the organic matter which is produced or formed in the vegetable world, is for the most part decomposed or reduced to compounds of a lower order in animals; but the assertion that animals merely consume the protein-compounds, fats, and carbo-hydrates, and cannot in any degree produce them, is not only incapable of proof, but to a certain degree is unquestionably false. It is established beyond all doubt, that flesh can be formed in the animal body, whether the elements from which it be produced are protein-bodies, or carbo-hydrates. (This subject is fully discussed in the first volume; see pp. 254—258 of the English translation.) Moreover, it is still an open question whether, under certain conditions, protein-substances may not be formed in the animal organism (see vol. i. p. 346 of translation of the first volume).

"Although," says Lehmann, in reference to this subject, "if we turn to the lower animals, we must deny that they possess the power of forming starch or cellulose, yet sugar and dextrine are being perpetually formed in the organisms of the herbivora during digestion from other carbo-hydrates, by the action of saliva and pancreatic juice; and even in the bodies of carnivora we have a factory for sugar in the liver, where in all probability this substance is formed solely from the metamorphosis of nitrogenous matters, as careful investigations have recently convinced me. And how many substances there are formed in the animal body, which never occur in the vegetable kingdom! It may, indeed, be urged, that these substances are only the products of a process of oxidation; but what an essential difference there is between xanthine or uric acid, and theeine and theobromine, which are homologous to them! And no one unacquainted with the origin of cystine or of taurine, could say whether these substances were products of the animal or the vegetable kingdom. We have nothing in the vegetable kingdom analogous to those complex combinations, the salts of the biliary acids; in short, we cannot deny that the animal body possesses the power of forming new compounds; but both in the animal organism and in the laboratory of the chemist, already formed organic matter is requisite for the formation of new substances not occurring in the vegetable world, in short, for imitating vegetable products. Since many of the excretions of the animal body contain tolerably complex atoms of organic matter, the statement is only in part true, that the animal gives off to the external world carbonic acid, water, and ammonia; even if we should regard the urine as representing an ammoniacal salt, and if we considered the taurine of the excrements and the formic, butyric, acetic, and caproic acids of the sweat, as equivalent in this view to carbonic acid and water, we still could not deny that both men and the lower animals daily give off no inconsiderable quantity of protein-bodies directly to the external world, since the solid excrements are never free from mucus, and since the epithelial desquamation and waste of other horny tissues are sufficiently great to admit of calculation. . . . . . We have already remarked that in plants there are several processes of oxidation proceeding simultaneously with the general de-oxidation; so also in animal life, besides the oxidation in the blood of the capillaries, we perceive a number of reducing processes, which, in their intensity, hardly yield to the corresponding processes in plants; thus we have seen that substances whose reduction requires the most powerful agents,—as, for instance, the sulphates,—are deprived in the *primæ viæ* of their oxygen; and the peroxides of iron and mercury, and similar substances, are de-oxidized in the intestine; we have previously had occasion to mention, that the fats and lipoids which are formed in the animal body can only be produced by a process of de-oxidation. Even if we should assume that oleic and margaric acids were produced from starch or sugar, by a process of cleavage (Spaltungsprocess) such as occurs when alcohol is formed from sugar, so that the reduction would be only apparent (as when a body separates into a highly oxygenous portion, as carbonic acid, and a portion poor in the oxygen, as alcohol, fousel oil, margarin, or olein); must not the stearic acid, which is only very rarely taken by animals as a constituent of vegetable food (for as yet it has only been found in cacao-butter), be formed by a direct process of de-oxidation, since its composition and its chemical qualities indicate that it can only be regarded as a lower stage of oxidation of the radical of margaric acid? And can we suppose that a substance which is so poor in oxygen as cholesterin, is formed by a simple disintegration of other organic matters? The oxidizing power of the animal organism is confined within tolerably narrow limits; sulphuret of potassium, when taken by an animal in not too small a quantity, passes in part unoxidized into the urine, through the highly oxygenated blood; salicine, in its passage through the blood, is not even converted into salicylic acid. Can the highly sulphurous cystine be produced by any other process than one of de-oxidation? When we consider the richness of many of the horny tissues in sulphur, and further, that they contain a group of atoms perfectly similar to albumen, we can hardly explain its formation, otherwise than by a local de-oxidation. If it be true that the iron is extracted from hæmatin by sulphuric acid and water, with a development of hydrogen gas, there must be a reducing apparatus somewhere in the animal body by which the iron which enters the organism with the vegetable food solely in an oxidized state, is deprived of its oxygen." (pp. 213—216.)

These illustrations are quite sufficient to show how impossible it is to erect chemical barriers between the organic processes in plants and animals: and we might increase them to almost any extent.

On reviewing the chemical substrata of the animal body, treated of in the first volume, we perceive that there are four groups of substances in which the vital processes are most intensely manifested; or, in other words, which most actively participate in the metamorphoses of the animal tissues. These are:

1. *The albuminous substances or protein-bodies, and their derivatives.*
2. *The fats.*
3. *The carbo-hydrates, and*
4. *The inorganic salts.*

That *albumen* is one of the most important substances in the animal body is sufficiently obvious from the positions in which it occurs: we find it in the greatest quantity in the blood, and in all those animal juices which principally contribute to the nutrition of the organism; however, a chemical investigation of various tissues shows us that albumen only requires slight modifications to enter into other forms; as, for instance, that of syntonin or muscle-fibrin, the essential constituent of the solid contractile parts by which alone both the voluntary and involuntary movements of the animal body are accomplished. We find it both in its fluid and solid form in that most complex of all structures, the nervous system, both in the nerve-tubes and in their contents. In association with a little fat and traces of sugar, the ovum consists merely of albumen and casein holding salts in solution; and there can be no doubt that with the co-operation of the oxygen conveyed by the blood, all the tissues are formed from the protein-bodies, although we are not as yet in a position to explain with certainty the exact nature of the changes by which the gelatigenous and certain other structures are produced.

The fats next claim our attention. Their physiological value and their mode of origin have been noticed at some length in the first volume (see translation, vol. i. p. 248—272); we will here simply mention, that without the intervention of fat no colourless blood-cells, and therefore no red corpuscle, could be formed; indeed, no animal cell or fibre of any kind. While, however, in the normal state, the fat takes an active part in cell-formations in the animal body, we also, in some cases, perceived a tendency to a formation or production of fat in existing cells and tissues whose nutrition has been peculiarly modified. The phenomenon commonly designated as fatty degeneration admits of a double explanation. We may either assume that the pre-existing fat, under the influence of certain molecular forms, is accumulated in the older and less vitally active cells, and replaces the nitrogenous textural particles as they become worn out; or that the fat is produced directly from the nitrogenous textures of the cells or fibres, the nitrogen being developed in the form of ammoniacal salts, and the fat being left as a product of decomposition. Lehmann, in his first volume, supported the former view as the least hypothetical of the two, and as the more probable from its simplicity; since the date of its publication Wagner and Liebig have, independently of each other, instituted certain positive experiments which very much strengthen the second view. Wagner made the remarkable observation that crystalline lenses, pieces of dried albumen from eggs, and other substances poor in fat, which were introduced into the abdominal cavity of birds, were perfectly changed in their texture in the course of from twenty-five to fifty-four days, the residue containing far more fat than existed in the original substance. Liebig has shown that the metamorphosis of the albuminous tissues of the animal body into fat is, in a chemical point of view, not only possible but probable. Both in the putrefaction and in the gradual oxidation (by chemical means) of albuminous substance, the results, under favourable conditions, are ammonia and fatty acids, such as the butyric and the valerianic.

These experiments and observations of Wagner's and Liebig's are, at all events, sufficient to show that there is no chemical absurdity in assuming that, under certain circumstances, fat may be a product of the decomposition of the protein-compounds.

The carbo-hydrates, constituting the third group, are in many respects closely allied to the fats. The substances of this class occurring in the animal body are, dextrine, milk-sugar, inosite, and glucose; to which, perhaps, we should add cellulose, occurring in the investments of the tunicata. Recent investigations have detected sugar, in small quantities, in almost all the fluids subservient to nutrition, as, for instance, in the blood, transudations, lymph, chyle, the albumen of the egg, &c.

"The sugar," says Lehmann, "which we meet with in the intestinal canal of herbivora and omnivora is due to the metamorphosing influence of saliva and pancreatic juice on starch and other carbo-hydrates; but we also find sugar in the blood of carnivorous animals in no very inconsiderable quantities; this must therefore have arisen from some other source than from the carbo-hydrates conveyed into the system from without; from a number of comparative analyses of the blood of the portal and of the hepatic veins, I believe, that I have indicated the probability that the sugar which is found in the liver, where it has also been found by Bernard and Frerichs, owes its origin to the decomposition of albuminates, and especially of fibrin."

When, further, we consider that nature has provided the egg with a small quantity of sugar, and that its amount varies according to the stage of development of the chick, the conviction forces itself upon us that the sugar, like the fats, is intended for some other purpose in the economy than merely to sustain the animal heat by its slow oxidation.

We are still far from being fully acquainted with the carbo-hydrates and the products of their conversion, which occur in the animal juices. Lehmann believes that we shall find indifferent substances similar to Scherer's inosite in the extractive matter. Our knowledge is more perfect regarding the acids which are formed in the animal body from the carbo-hydrates: formic and acetic acids have recently been found by Schottein, a very promising young chemist, in large quantities, in the sweat; butyric acid occurs, not only in the sweat but also in the muscular fluid, in the parenchymatous juice of the unstriped muscular tissue of the stomach, the intestinal canal, and the urinary bladder; and lactic acid is found in the gastric juice as well as in most of the above-named fluids.

We regret that we cannot follow our author through his demonstration of the facts that the presence of small quantities of sugar essentially contributes to the solution and digestion of the protein-bodies, and that the carbo-hydrates, or rather their acid products of metamorphosis, discharge an important function in the intestinal canal, in no way directly connected with the process of respiration.

The most interesting portion of the volume still lies before us, and we hope in an early number to resume our analysis of it.

G. E. Day.

Review VII.

Traité Pratique des Maladies Cancéreuses et des Affections Curables confondues avec le Cancer. Par H. Lebert, M.D., &c. &c.

Practical Treatise on Cancerous Diseases and on Curable Affections confounded with Cancer. By H. Lebert, M.D. &c. &c.—*Paris*, 1851. 8vo, pp. 892.

M. Lebert, well known for a considerable number of years as an ardent cultivator of microscopical histology, has here produced a book of high pretensions and solid worth on one of his favourite themes—Cancer. The ideas of M. Lebert on the intimate structure of this morbid product have been so widely promulgated, through the medium of papers in the French and German periodicals, that our readers must not expect much of actual novelty on this branch of the subject. Still notions, now familiar, are presented in new aspects,—arguments more or less novel are found in their support,—recent objections from various quarters are more or less ingeniously met; so that even in the department of histology an analysis of this volume may not prove valueless, even to those well acquainted with the peculiarity of M. Lebert's cell-doctrines.

The volume is written on a plan sufficiently common now in managing the history of diathetic diseases. In a first part, the pathology of cancer in general is considered; in a second, the disease is described as it occurs in particular structures and organs.

The first part opens with a 'definition' of cancer (in point of fact, and, in logical phrase, merely a brief 'description'), the prominent feature of which is, of course, the specific character of the microscopic elements of the product. The 'definition' is otherwise distinguished by the broad intimation it gives of the author's incredulity as to the cure of cancer being ever effected by surgical extirpation, by its agreement with the doctrine (for several years past taught in this country) which rejects, as unsound, the antiquated division of tumours into the classes 'benignant' and 'malignant,' and by its emphatic expression of the invariably fatal issue of the disease in every instance, where the author has had an opportunity of "following its complete evolution."

Turning to the naked-eye characters of cancer, we find M. Lebert insisting on the importance of its peculiar milky juice; this he holds to be an attribute "almost as constant and characteristic as the presence of the cancer-cell." In the significance assigned to this juice, and without the qualification appended by M. Lebert, British pathologists are probably prepared very much to acquiesce. We have long held this fluid to be *almost* an unfailing test of the cancerous or non-cancerous nature of a tumour; but unfortunately there are instances, rare though they be, in which a whitish fluid, not distinguishable with the unassisted eye from that of cancer, may be expressed from fibrous and simple exudation-tumours. It is, as a single character, the least likely to deceive, but it may deceive.

M. Lebert yields somewhat of his original confidence on the point in admitting that it is not always possible to determine, from the characters of a given *isolated cell*, whether it belonged to a cancerous growth or not. His affirmation now runs thus: from the microscopical examination of any given "morbid tissue," its cancerous or non-cancerous nature may always be established. The type of the cancer-cell is a "small regular sphere, with an elliptical nucleus, placed excentrically, filling nearly the half, or upwards, of the interior of the cell, and containing one or more large nucleoli." But this typical form is often not maintained with purity; the cell-wall assumes an ovoid, elongated, or triangular shape, with acute or obtuse angles. According to M. Lebert it may even be of true 'fusiform' outline—a shape we have never seen; all varieties of 'shapelessly caudate' form, we agree with him in recognising as of occasional, even tolerably frequent, occurrence. To this multiplicity of contour much importance, and with good reason, is attached by the author; and he fairly urges that if similar diversity may be found, on superficial examination, in epidermic tumours, it can always with ease be traced, either to folding or some morbid change of the epidermic cell.

M. Lebert holds to the reality of his so-called "concentric cell:" he has, of late years, frequently met with it in the testicle, lung, mamma, and omentum. "In some of these cells a perfect cell-wall was surrounded by another cell-wall, as regular as, and larger than, itself; in some the concentric character consisted in the superposition of several membranous involucra of lamellar appearance. Few persons have been fortunate enough to see these cells. Henle disrespectfully suggests, that M. Lebert describes them from imaginary forms, or from starch-cells—a suggestion naturally repudiated with indignation by the accused.

The following passage contains so important an admission, that we extract it in full.

"There are circumstances in which microscopical examination does not display the typical characters of the cell in a cancerous tumour. But, in the first place, the occurrence is so exceptional (only in from two to three per cent. of cases) as not to invalidate the general rule. These exceptional tumours contain imperfectly developed cells, in which the cell-wall is commonly wanting; while the nuclei of small size, from 0·005 to 0·006^{mm}, only contain, in rare instances, a characteristic nucleolus. These incompletely developed cells have been chiefly found in very soft cancerous tumours, developed with great rapidity. We have three times observed this condition of things in encephaloid, developed in the interior of bones; and have also established it in several cases of very rapid and very extensive secondary dissemination. As a general statement, it may be affirmed, that while encephaloid of medium consistence, and not over-rapid growth, is the cancer exhibiting the richest variety of cells, so, on the other hand, does it excite the most abundant cell-formation—cell-formation, under these circumstances, qualitatively the most imperfect."

The significance of this paragraph is clinically immense. Here it is formally admitted, that the most active and baneful cancer may vegetate through the system without exhibiting any of the cells we are elsewhere taught to regard as habitually essential to its existence. Here is ample corroboration of the justness of a proposition printed some years ago by ourselves, and which we may perhaps be pardoned for referring to, on account of the importance of the question at stake:—"A tumour may present to the naked eye the characters of encephaloid, be the seat of interstitial hæmorrhage, affect the communicating lymphatic glands, run in all respects the course of cancer, and nevertheless contain no cells but such as are undistinguishable, in the present state of knowledge, from common exudation-cells."* M. Lebert places himself in this dilemma: either cancer exists without the cancer-cell, or simple exudation-matter produces the clinical manifestations of cancer. This at least is the straightforward practical admission, to which we are forced by the facts he admits; that the first of the terms of the dilemma is the one to be accepted, however paradoxical this may seem, cannot be questioned. But we are prepared to concede, as the above extract shows, that with the advance of knowledge, differences will probably be distinguished of a positive kind between simple exudation-cells and those of similar physical aspect, yet dynamically cancerous. Increased experience will probably teach pathologists to distinguish with surety cancerous-cells in unusual conditions of development from the exudation-cells they resemble. In the micro-chemical qualities of different varieties will very possibly be found a guide to their distinction.

Having described the various morbid changes which cancer-cells undergo, (diffluence, thickening of the cell-wall, diffusion, granular and fatty infiltration, and desiccation), M. Lebert turns to the discussion of the objections urged in different quarters to the doctrine of the speciality of the cancer-cell. Unfortunately the argumentation is long, and will not bear condensation. We unquestionably think it triumphant in certain points of view; it shows, as far as reasoning broadly based on observation can, that in *philosophical pathology*, the special cancer-cell is a real entity; but it fails to show (it could not show, if what we said a moment since be true), that in *practical medicine* the nature of growths can always be determined in the present state of histology, by the characters of their cell. We subscribe to the position, that the main source of disbelief in the speciality of cells has been misinterpretation of the various forms they assume in varying phases of development, and under the influence of disease; and we hold absolutely with the author, that the possibility of the direct conversion of the elemental forms of one kind of structure into those of another (as of an epithelial or a liver-cell into a cancer-cell), is negatived by all unbiassed observation.

M. Lebert has performed some very careful injections of cancerous growths with the view of settling the nature of their vessels. Several years since M. Berard made the curious announcement, that these tumours were well provided with arteries, but wanting in veins. Although this statement was based on the results of actual injection, it appeared so inconceivable that blood should be brought to a texture unprovided with the means of returning it, that the opinion of the Paris professor had few converts. M. Lebert, employing yellow injection for the arteries and blue for the veins, has found abundance of vessels of both classes, though in variable proportions, with an intervening capillary rete, tinged green by the union of the two colouring materials. Like his predecessors, the author has failed in finding either lymphatic vessels or nerves in cancer.

To the chemistry of cancer M. Lebert offers no contribution. It is needless almost to remind our readers, that all the analyses hitherto published fail to throw any light on the nature of the morbid product: they are valueless, too, in point of diagnosis: many of the vaunted analyses of

* Cyclopædia of Anatomy, art. "Products, Adventitious," p. 138.

tubercle differ as much from each other, as they do from those of cancer. It is a striking fact, that, as far as we know, analyses of such compound products are never the work of men holding high reputation in their special science,—a pretty significant hint that they regard such attempts, in the present unsettled state of organic chemistry, as at the least bootless, if not positively unphilosophical.

M. Lebert very justly raises his voice against the doctrine of the 'degeneration' of non-cancerous tumours into cancers. The notion, indeed, one of the most flagrant heresies in pathology, could not survive an hour, did it not furnish so convenient a loophole of escape from errors in diagnosis. A given tumour is pronounced by experienced persons, at an early stage of its growth, to be non-cancerous. It enlarges, and is cut out. The qualities of cancer are recognised. But no error, it is held, has been committed; the tumour was, in truth, of innocent nature at first; it subsequently 'degenerated.' Now, in point of fact, degeneration by conversion is an impossibility, and the only way in which the change from non-cancerous to cancerous character could be effected, is by the *de novo* development of cancer within the area, and from the vessels, of a non-cancerous formation. Experience proves, that while there is no *à priori* impossibility of its occurrence, such development is infinitely rare; for our own parts, years ago we wrote that we had searched in vain for an example of it in fibrous tumours, its alleged most common seat, nor have we succeeded since. M. Lebert states that he has twice known cancer form in tumours, primarily non-cancerous, but does not mention their nature. It must, then, be admitted, that in excessively rare cases a change simulating 'degeneration' to the merely practical eye, does actually occur; but this is a very different thing from real degeneration being a common phasis of evolution of non-cancerous growths.

The author agrees fully with many previous writers in regarding cancer as a blood-disease, general from the first, no matter how purely local the malady may appear in its manifestations. The mode of origin of the solid product by cell-germination from a fluid blastema, is described as by writers in this country. But we think M. Lebert disposed to grant too much, in not explicitly denying the possibility of the direct transformation of a coagulum of blood into cancerous matter. The appearances of such conversion seem otherwise explicable. Either *absorbed* blastemal elements of cancer are accidentally evolved in the interstices of a clot; or (an hypothesis supported by numerous facts) blastema, *not exuded from the vessels*, retained within them in fact, germinates in their interior.

Admitting that he has "not been able to study the mechanism of the vascularization of cancer," the author nevertheless ventures to treat as "positively false" the opinion, "that the vessels of cancer form in its interior, independently of the general circulation." We are not aware that any one has professed that the vessels of cancer are *solely* formed on the *de novo* plan. But the opinion that they are in part thus formed, seems warranted, by the analogy of the process in the vascular area of the chick; by the fact that the new cannot at first be completely injected from the old vessels; by the analogical truth, that new blood-particles appearing in lymph in the frog are of spherical shape (as in the fœtal condition), and are therefore not particles previously contained (for these are oval) in the old vessels. Besides, direct observation with the naked eye, pocket-lenses, and the microscope, seems to show that there are some independent vessels. M. Lebert simply affirms that "the vascularity comes from the general circulation from the first." But how does he know this? And granting that he has abundantly injected cancer-vessels from those of the contiguous textures (which no one questions), where is his proof that he can thus inject them *all?* and granting that he does inject them all, where is his proof that the connexion of some of those vessels with the original circulation was not effected by *secondary* inoculation?

How do cancerous tumours enlarge? Of course, essentially by the contribution of new blastema from the blood. But, in assimilating that blastema, do, or do not, already formed cells play an important part? Is the procreative or vegetative faculty of cells a reality or a fable? Is the procreation of each new series of cells wholly independent of those that have gone before? or are the former produced, in part at least, from or through the latter by endogenesis or exogenesis? M. Lebert repudiates utterly the notion of the procreative faculty of cells in general, and cancer-cells in particular. He lavishes light banter and heavy indignation on those who imagine they have "caught nature in the fact, and stood by at the parturition of a mother-cell, and birth of its young." But, except the statements that M. Lebert has studied animal cells much, both before and since the advent of Schwann, and that he is "perfectly convinced" of the non-existence of cell-multiplication, we find nothing tendered bearing even the semblance of evidence that the cell-accoucheurs aforesaid (as the author, in pursuance of his metaphor, would doubtless call them) are wrong in the principle they advocate. M. Lebert should take to pieces, *seriatim*, the facts and arguments of those who maintain that the cells of certain growths are vegetative and evanescent, if he sincerely hope to undermine their belief.

M. Lebert deposes strongly against the importance frequently attached to the characters of cancerous ulcers, as diagnostic of their nature. The hard and callous, thick and everted edges, &c., he justly observes, may be seen in old ulcers of all sorts. This is, unfortunately, too true; were it otherwise, the distinction of cancroid and syphilitic from cancerous ulcers would not be so difficult as it is. The result of the author's observations in general is, that "in several organs cancer ulcerates either very rarely or not at all, and that in the organs where it ulcerates most frequently, it does not undergo this change in a quarter, a third, or half of the cases."

We find described, under the title 'phymatoid,' cancerous substance rendered tuberculous in naked-eye appearance by infiltration with fat; but except in the invention of this word, there is nothing new in the paragraph or the subject. That cancer is given this appearance in the manner mentioned, has long been generally known. Neither in the arguments here given in refutation of the opinion of Bochdalek, that the local cure of hepatic cancer by fatty conversion is of pretty frequent occurrence, do we find anything novel. In his refusal to receive that opinion we wholly agree with the author. This fatty aspect occasionally presents itself distinctly in portions of tumours which are rapidly growing and infiltrating the adjoining tissues with characteristic creamy juice at the very time of its discovery; so much so, that the idea sometimes suggests itself, whether the very destruction, by fat-infiltration, in one part of a tumour may not give a stimulus to its growth elsewhere.

Authors in general admit that cancer destroys life in various ways. The writer of the present volume professes a different doctrine. He has met with such a number of cases of deaths from the progress of a *single* cancer (unattended with secondary formations) "in which there was neither abundant suppuration, nor repeated hæmorrhages, nor serious functional disturbances, to cause death," that he has "come to the conviction that cancer kills, after a certain duration, by *general death of the system*, and not by a purely local affection—by *infection of the entire economy*, and not by incidental disturbance of an important organ. Cancer kills by depriving the blood of the power of sustaining life, not by causing loss of blood either unchanged or altered through exudation." That the condition of blood existing in cancer is capable of killing seems certain; but M. Lebert's notions would lead us to attach too little importance to all morbid conditions beyond that fluid. Still, he argues his case pertinently:

"The reaction produced by secondary cancer, even when this is serious in character, is often so little apparent, that frequently, no matter what attention has been paid to the case, clinically during life, secondary formations are found in organs where no one expected their existence. Again, compare the bodies of two individuals, both cut off by a cancerous growth, which ran its natural course without disturbance, but the one presenting a single cancerous tumour only, the other numerous secondary formations, and you will certainly not find a great difference in the two bodies in respect of amount of discoloration, emaciati atrophy of the tissues, and impoverishment of the blood."

The importance of this doctrine in regard of treatment would be extreme, could M. Lebert afford us the material demonstration of the blood-alteration which he regards as so all-efficient. Here, however, he fails, as all who have gone before him. But who has proved alteration of blood, impregnated with syphilitic virus? And yet who doubts its existence? The day will come, however, and in all probability before long, when actual microscopical and chemical demonstration of the incipient alterations of the blood in diathetic diseases, as a class, will be obtainable, and so a material basis formed for the now prevalent conviction that there is a condition of the fluids capable of being therapeutically handled, which precedes local manifestations of the disease in each diathetic affection.*

The following table represents the amount of frequency with which primary cancer in the different tissues and organs named, was, in M. Lebert's experience, attended with secondary formations; the position of some of these tissues and organs in the scale, and the non-appearance of others, rather surprise us.

* In cases of Bright's disease, which we hold to be indubitably a blood-disease *ab initio*, the failure of treatment probably comes essentially of the existing inability to diagnosticate the affection, before its *local anatomical character in the kidney* has been developed. This anatomical character now absorbs attention; and to it treatment is directed almost alone. It would be just as rational to aim at the cure of phthisis, small-pox, typhoid fever, or scurvy, by local applications to the pulmonary tubercles, the cutaneous pustules, the intestinal deposit with its ulcerative effects, or the cutaneous blotches and spongy gums, which severally constitute anatomical characters of those affections.

Cancer of the bones	0·77
„ liver	0·75
„ lymphatic glands	0·75
„ breast	0·71
„ kidneys	0·58
„ stomach	0·57
„ testicle	0·50
„ intestines	0·50
„ peritoneum	0·50
„ eye	0·45
„ uterus	0·33
„ brain	0·33

The general mean was 0·56.

In a subsequent section on "Cancroid," we find M. Lebert insisting on the importance of limiting that term to "the eating ulcer and to epidermic and epithelial productions, which are analogous to cancer in their mode of progress." Restricted thus in its signification, we see no serious objection to the term; for in very fact a cancroid is not pathologically a cancer, no matter how much it may resemble one in its clinical course, and it is right and proper to intimate the difference in essence by a distinction of terms. We have in this country long been aware how vain were the hopes held out by mere histologists, that cancroid was as profoundly different from cancer in vital effects as in intimate structure; we have long known how fallacious were the assurances tendered, that extirpation of the epidermic mass would prove an absolute cure. But it appears well, as M. Lebert was himself the original source of the erroneous opinions circulated concerning the clinical harmlessness of cancroid, to transfer from his own pages what we may call his recantation.

"The most baneful localizations of epidermic cancroid are in the lower lip and in the penis; while cancroid in these sites readily ulcerates, it is essentially hypertrophous in nature, and is especially destructive, in that the epidermic infiltration has a morbid tendency to diffusion and propagation, at first among the surrounding parts, and subsequently to the bones and muscles, in the case of the lip; to the corpora cavernosa in the case of the penis. Besides, it affects the adjacent lymphatic glands; those of the neck and below the jaw, in the instance of the lip; while, in the case of the penis, the glands of the groin become infiltrated with epidermis, and may break up and form actual putrid ulcers. Among the non-cutaneous sites of cancroid we may mention, as still more noxious, those of the tongue and of the neck of the uterus. Recurrence of the disease is frequently explicable in these cases by its tendency to propagation; so that we oftener meet with a continuation, as it were, of the original disease imperfectly extirpated, than a true relapse. That in these situations cancroid destroys life, is an incontestable fact. Hence there are points of relationship, but too real, between cancroid and cancer."

We repeat, all this teaches nothing of the smallest novelty to observers on this side the Channel; but, on the other hand, M. Lebert does teach us something new, if he be absolutely correct in the importance he assigns to certain differences in the pathological progress of the disease, as follows:

"In several situations cancroid is an affection of much less serious character, of slow progress, leaving the general health unimpaired, of much more limited localization, exhibiting a much less marked tendency to diffused propagation, and consequently holding out the possibility, or rather the certainty, of cure by complete extirpation, and sometimes even promising a state of perfect innocuousness if not interfered with. Further, all our post-mortem examinations, in cases of cancroid, have invariably proved its strictly local nature, even in very noxious sites; we have never met with secondary deposits at any distance from parts in actual anatomical connexion with the original disease."

But it is as obvious as the day, that (with a single exception) every one of these qualities, conditions, or peculiarities are to be found in not a few, but in many, cases of true cancer. That exception, it is almost needless to observe, refers to "the *certainty*" of cure by complete extirpation. But, unfortunately for M. Lebert's argument, we may take leave to doubt whether any such fortunate result can be promised in the instance of cancroid, a whit more positively; with more probability, we admit, but not with more positiveness. Return takes place after the extirpation of cancroid, with which a thick layer of healthy tissue—microscopically healthy—has been deliberately excised.

M. Lebert's notions of the mean duration of cancer in divers organs may be compared with those of other authors.

	Months.
Cancer of the thyroid gland	6·50
" kidneys	8·00
" liver	9·00
" ovaries	12·00
" fauces	12·00
" peritoneum	12·00
" skin	12·00
" bladder	12·50
" œsophagus	13·00
" stomach	13·00
" respiratory organs	13·20
" tongue	14·00
" uterus	16·00
" intestines	18·00
" lymphatic glands	24·00
" osseous system	27·12
" eye	33·50
" breast	42·00
" testicle	42·00

The chapter on the ætiology of the disease leaves us in as complete ignorance of the causes of cancer, as it unfortunately found us. A point or two may, however, be touched upon. M. Lebert examined the question of hereditary transmission in 102 cases, and found evidence of such transmission in 14, or about one-seventh of the number. M. Leroy d'Etiolles formerly found only 1 in every 278 cases ascribed to hereditary influence by the practitioners of France. M. Leroy's informants had, however, in all probability, not investigated the point with the care bestowed on it by M. Lebert. And, even with the proportion of one-seventh of his cases deposing apparently in favour of hereditary transmission, M. Lebert is disposed to doubt its reality. Concerning the direct transmissibility of cancer by inoculation, the author philosophically doubts, although he has observed one fact strongly corroborative of Langenbeck's well-known statement on the subject. Some cancerous juice (proved to be such by microscopical examination) was injected into the jugular vein of a dog; the animal died in a fortnight. The walls of the heart and the liver contained indubitable cancerous formations, varying in size from a pin's head to a small bean. But M. Lebert, knowing that cancer is common in dogs, is ready to admit that the animal may previously have been cancerous. At all events, he shrinks from drawing any positive conclusion from a single experiment.

Of 349 cancerous patients, 131 were men, 218 women; a proportion between the sexes very decidedly differing from that observed in this country, and one which we cannot help thinking must, from some accidental cause, lessen the contingent really furnished by the female sex in France.

The secondary phenomena, non-cancerous in character, which are observed in the victims of cancer, are carefully enumerated by M. Lebert; but there is a mixture, as will appear from the following catalogue, of changes which come really of the nature of the malady, and of those which originate in its mechanical influences, when occupying certain special sites. Thus, " spontaneous coagulation of the blood in the veins, dropsy in the limbs or serous cavities, excessive emaciation, discoloration of the skin, bloodlessness of the organs, fragility of the bones and muscles," give examples of the two kinds of secondary disturbance. A sub-inflammatory state of the alimentary canal is frequent, even in cases of cancer of other parts. Pneumonia is frequently a terminal affection—pultaceous stomatitis and obliteration of veins are also only observed towards the close. M. Lebert found recent tuberculization in 8·6 per 100 of his patients destroyed by cancer; but he has not observed the development of cancer in phthisical persons. As to the absolute excluding power exercised by either of these diatheses over the other we entertain grave doubts; that they rarely coexist in the same individual, especially both of them in an active state, is indubitable, as we long since numerically proved; but, on the other hand, we have in one instance, that of a youth aged about twenty, found encephaloid and tubercle *growing* together in the same lung. M. Lebert appears to us to attach too little importance to the difference of the mean age at which the two diatheses are most active, in explanation of their tendency to mutual exclusion.

The section on treatment pronounces a most unfavourable verdict on all methods of alleged medicinal cure of cancerous diseases. The author has no sympathy with those who, like ourselves, looking to the very considerable number of reputed cancerous tumours that have been cured by the persevering use—pushed almost to poisonous effects—of certain drugs, decline to condemn the statements of our predecessors as altogether erroneous, or altogether mendacious. To pass such a judgment unreservedly on the alleged experience of whole generations of medical men would be harsh, if the foundations of such judgment were solid and unassailable; while the proceeding would, it appears to us, be utterly unjustifiable on any actually attainable evidence. But M. Lebert, armed with the 'discoveries' of 'cancroid,' 'partial hypertrophy of the breast,' and 'fibro-plastic tumours,' denounces all who venture to believe it possible that perchance some real cancers were included in the multitude of putative cancerous tumours, of which past experience records the cure. He expresses himself as really pained that the writer of this article should have lent himself to promulgate a "doubtful or rather a favourable opinion of the curative value of agents hitherto proposed." We think M. Lebert has scarcely acted justly here: in fairness he should have referred to our final estimate of the value of specific agents—an estimate conveyed in these words: "There is no medicine known having claims to the character of a specific in cancerous diseases, nor even endowed with the special attribute of invariably modifying the course of the affection. But this is no reason that such a medicine may not be found: the history of mercury and quinine teaches the folly of absolute scepticism in respect of the reality of specific agents." Now hear M. Lebert. "Although cancer proves incurable by the agents hitherto employed for the purpose, we need not by any means despair of one day finding an agent capable of curing it. The action of bark in intermittent fever, and that of mercury against the syphilitic virus, when become constitutional, bear unmistakable witness to the fact, that there exist in nature agents capable of neutralizing the action of a deleterious morbid principle in the system; and logical induction certainly warrants the hope, that the fortunate day of the discovery of a specific remedy against the cancerous principle will come." What is the difference in the doctrines professed in these passages? In the very illustrations employed, they are almost identical. What is the last-quoted passage but a paraphrase of the first?

Turning to the "surgical treatment of the affection," we find M. Lebert regretting the existing deficiency of satisfactory statistical returns, and especially tearing to pieces the oft-quoted series of results obtained by M. Leroy from various practitioners of France. That accurate statistical returns are wanting, all the world will agree in admitting, with M. Lebert. But in his pleas for the operation ("solely as a palliative, as a means of lessening suffering and prolonging life") he forgets that the very imperfections of M. Leroy's information—the errors of diagnosis by which his estimates are assumed to be utterly vitiated—give additional force to the opinion of those who regard excision, with whatever intent performed, as detrimental in the mass of cases. For it is a starting-point common to those who recommend as a rule, and to those who oppose as a rule, extirpation with the knife, that the chances of beneficial results are vastly greater with non-cancerous than with cancerous tumours. If, then, M. Leroy, or any other person, obtains results positively unfavourable to operation (either as a means of curing the disease or of prolonging life), from the analysis of a group of cases professedly cancerous, but among which non-cancerous growths figure to a considerable amount, it is self-evident that those results would be still more unfavourable, if accurate diagnosis had excluded all non-cancerous products. The greater the number of instances of confusion of fibrous, sarcomatous, and simply hypertrophous masses, the greater the gain to those who affirm, the greater the loss to those who deny, the efficiency of operation. M. Lebert and others, who set great store by the "error of diagnosis" argument, and seem to imagine that it annihilates their opponents, in point of fact simply expose the weakness of their cause by dragging it to light. Why, too, does M. Lebert ignore the telling experience of Scarpa, of Benedict, and Macfarlane? But, truth to say, it matters little which side he espouses, and what experience he selects for its defence; for, as a *curative* measure, excision has been tried, found wanting, and condemned by all but the universal voice; while, that it succeeds "occasionally" as a *palliative* measure, no one, on the other hand, denies. The only function which statistics could now perform in the matter would be to convert the term "occasionally" into so much per cent.; to give us the per-cent. frequency, of palliation. And at the bed-side that crude numerical return, if obtained, would, unfortunately, serve us but little. What answer would it supply—perfect though it were in its way—to the momentous question: Here, in this particular case before us, of A or B, is it better to use or reject the knife? None, absolutely none. That rough per-cent. estimate would not help us an iota in placing them in the category of operable or non-operable patients. True, there are rules given in all quarters for guidance in this difficulty. But, even in M. Lebert's apprehension, these rules are of paltry clinical value. "Who," he observes, "has not seen numerous cases of operation in which relapse did not take place for some years, although the early progress of the case had been rapid, and of ominous character? Nor is it uncommon to find, that of two patients operated on, the one for a cancer of slow progress, the other for one of more rapid development, the latter (in apparently the worse condition) has no return of the disease for a much longer period than the former." The author himself adds no rules for guidance in this matter to those already printed by various of his predecessors. It would appear that the only chance of obtaining trustworthy data to aid us in determining whether the operation be opportune or not, is by *well-digested* numerical returns—that is, let a large number of cases of operation, thoroughly well observed in respect of the local disease and the constitutional state, in a word, of all clinical conditions—cases in which, too, the period of return of the disease is known with precision—be carefully tabulated. The analysis of the table would, in all probability, detect certain circumstances common to cases of early return, certain others, both positive and negative, uniformly occurring in those where operation had secured a protracted release from the co plaint. At least, if the basis of a tolerably sure judgment as to the issue of excision be not supplied in this way, we are without hope for the future; the *à priori* and the *mixed à priori and small-experience* plans have lamentably failed. The man who would devote some years of his life to the collection and analysis of a mass of such cases, all of them either under his own care, or at least observed by himself throughout, would deserve well of his kind.

We have reached the second part of the volume,—that descriptive of cancer in particular tissues and organs. The chapter on the Breast arrests our attention, although this is precisely one of the organs on which it is most difficult to say or devise anything new. Under the title of "elastic cancerous tumours" M. Lebert describes growths intermediate between scirrhus and encephaloid, and forming about one-fifth of the total number of mammary cancers. They are composed of a pale yellow "tissue," homogeneous, elastic, and shining, exhibiting under a common lens a delicate, whitish, fibrous stroma; opaque granulations are also visible in them. The cancerous juice exhibits its usual characters, and in several tumours encephaloid, scirrhus, and this intermediate substance, coexist.

The writer gives an interesting table of thirty-four post-mortem examinations of cases of cancer of the breast, showing the frequency of secondary deposits in cancer of this site.

	Secondary Cancers.		No Secondary Cancers.		Totals.
Females dying without operation	5	...	1	...	6
Females or males dying a certain time after operation	8	...	2	...	10
Females or males dying of the operation	5	...	6	...	11
Insufficient clinical particulars	6	...	1	...	7
	24	...	10	...	34

Hence it appears that secondary cancers had formed in about five-sevenths of the cases. It would further follow, from this table, that the immediate and direct mortality from the operation is something enormous,—one in three, or eleven in thirty-four. This inference is unfair to the operation, however, for five of those fatal cases given in the table, are furnished by a friend; and the author knows not what number of operations may have supplied the five deaths. M. Lebert's own numbers on this point are 6 and 34; 6 deaths immediately in 34 operations, or about 18 per 100. Even this is a most serious contingent of mortality produced by the knife—more serious much than any we have found recorded by others. Of 130 operations (Benedict and Macfarlane), 4 were immediately fatal; "of 25 patients operated on, 2 died from the effects of the operation," according to the late Professor Reid's tables of the practice in the Royal Infirmary of Edinburgh (1825).

M. Virchow, whose name generally appears in association with sound doctrine, has broached a singular opinion on retraction of the nipple,—which, if well founded, would oblige practical surgeons to forget some of the notions they have regarded as most thoroughly established. He holds that "drawing in of the nipple" is assimilable to the contraction of cicatrices, and indicates a curative tendency! M. Lebert disagrees wholly with his "learned friend" on this point, and takes occasion to give the analysis of 27 cases in which adhesion and retraction of the nipple existed: 1st, 10 cases in which the cancer had only lasted from 6 to 15 months, and was making incessant progress; 2ndly, 10 cases in which the adhesion was accompanied with redness on the surface, and all the signs which precede ulceration; 3rdly, in 2 cases the retraction existed in a breast, secondarily affected after the other; a case in which death occurred after the gradual increase of the second cancer; 4thly, in 1 case the retraction was chiefly caused by the development of a cyst in the vicinity of the nipple; 5thly, in 2 patients retraction of the nipple was observed, though the progress of the case was very slow, and of "apparently benignant character;" 6thly, in 2 cases the conditions of retraction could not be determined. From all this it follows, that there are certain rare cases in which retraction of the nipple has no signification either *pro* or *con*, in regard of the serious character of the disease; but not a single instance appears lending support to the paradox of Virchow.

The facts analyzed by the author in reference to the progress and duration of mammary cancer, and the inferences deduced from them, are of such precision, that we extract the substance of his observations on this matter.

"We possess data on the progress and duration of 59 well-observed cases of cancer of the breast: in 27 of the number the duration was ascertained from outset to death; in the remaining 32 it was only known up to the time the patients were lost sight of. But as in the greater number of instances these latter patients had nearly reached the fatal turn, and as they had, for the most part, undergone one, two, or several operations, their cases may also serve us for the statistics of the operation.

"The first point to determine is the time the patients presented themselves to undergo the first operation, and, consequently, the duration of the disease at that moment. We possess information on this point in 34 cases; in about five-sevenths of these the duration had ranged between three months and two years,—in two-sevenths the duration had been greater. Two patients must have been operated on before the end of the third month; but in these cases, as well as in all those at present under consideration, it must be borne in mind that we could only fix the outset at the time of manifestation of tumour, which is some months posterior to the real time of its first formation. Eight patients were first operated on from three to six months after the outset; 12 (6 of them in each three months of the period) between six months and a year; 5 between the twelfth and twenty-fourth month; 4 between two and four years; 3 between four and ten years.

"If we next consider the relapses after a first operation, and the interval of time between the first and second operation, (a matter in which we have 21 cases for our guidance), we learn that the enormous proportion of two-fifths of the second operations were required in from between one to three months after the first,—and of course the time of relapse commencing must have been earlier still. Six of the remaining patients underwent the second operation from three to six months after the first; 4 at the end of one or two years; 2 at the end of from two to three years; and 1 at the end of eight years. If from the 34 patients operated on, we subtract the 6 immediately cut off by the operation, we have 28 remaining,—of which 28 there are 21 in whose cases we have positive information concerning relapse, — and this relapse occurred twice out of thrice within six months,—and in somewhat less than a quarter of the cases in from one to three years. We should be glad to give the remaining 7 cases as examples of cure,—but the probability is small that they were so. They are cases of patients who left the hospital, recovered from a first operation, and who were lost sight of. But nothing justifies us in supposing their fate a better one than that of patients whose subsequent career was watched, and who all had relapses.

"We must, however, here make a qualification favourable to the operation,—to which we are not absolutely hostile under all circumstances. We have, in truth, in some rare cases, seen relapse not take place until some years after a first operation, required at the end of six to nine months after the first manifestation of the tumour,—whereas we have, on the contrary, seen very prompt relapse in patients who had had cancerous tumours in the breast for several years.

"We have also notes, in five cases, of a third operation performed after a second relapse,—three times in the space of from one to three months after the second operation, once in the space of from three to six months, and once between six and nine months. Among the 6 cases of death from the immediate effects of the operation, there are 2 in which it was performed in from three to six months from the outset of the disease, and 4 in which the complaint had existed from one to three years. . . . .

"We reach a second order of cases, 19 in number, of patients not operated on, and of whom 6 are still living, 13 are dead. Among the 6 survivors are 2 in whom the disease has existed from one to two years, 1 from two to three years, 2 from four to six years, and 1 for twelve years. Among the 13 who are dead, the duration was in 1 case from three to six months, in 6 from one to two years, in 3 from three to four years, in 1 case twelve years."

It is unnecessary to enter into any elaborate commentary on these facts; they speak for themselves. True, there is nothing new in the results; they simply corroborate the inferences deduced from observation by men of all times and all climes concerning the inefficiency of operation. But at all events, the eternal objection of "error in diagnosis" (an objection which we have, however, already shown has no real force) cannot be raised against these results by the microscopical school.

The history of mammary cancer is followed by that of "partial hypertrophy of the mammary gland," none other than our old acquaintance, the "chronic mammary tumour" of Astley Cooper. To this section we turned with real interest and curiosity; for as M. Lebert denounces repeatedly, and in no gentle terms, those who confound this disease with incipient scirrhus, we felt assured that rules for effectual distinction of the two diseases would be furnished in number. What was our disappointment to find the clinical aspects of the subject left by M. Lebert exactly as he found them! He who stands puzzled before a breast, doubting whether the nodule he feels is a simple tumour inclining eventually to become cystic, or a very cancer, will not find a syllable to release him from his dilemma in the pages before us. And it is downright absurd to insist, as the author does, on the preservation of the general health as appertaining to the chronic mammary tumour, when, as he admits himself, at the early period of scirrhus (and then alone is the diagnosis difficult) the general health may be excellent—nay, often actually is above par. It is true, M. Lebert gives a very elaborate description of the anatomy of the disease—a description which, if in all points correct (and this we have no reason to doubt), justifies the new name he has given it—and shows that its intimate nature is in some respects unlike the previous conceptions formed of it. But clinically this is valueless. The only anatomical character having important clinical bearing, and of which the author makes much, is the tendency to cyst-formation—a tendency well known to all who have glanced even at the writings of Sir B. Brodie.

We regret to be forced, though not by deficiency of matter, to throw down our pen here. There is yet much in the volume which will repay patient study, and to which we have not been able to make even a passing allusion. Indeed, the present article has no pretensions to the character of an analytical review—being, in truth, little more than a notice of some of the prominent points in the work. Of that work the main characters are fulness and precision of detail, careful avoidance of hypothesis, and sound general views in pathology and therapeutics—characters, it will be admitted, likely to attract a numerous corps of readers to its pages.

W. H. Walshe.

Review VIII.

1. *Présence de Sucre dans les Urines.*
On the Presence of Sugar in Urine. By Alvaro Reynoso.—('Comptes Rendus,' t. xxxiii.—xxxiv.)
2. *Absence de Sucre dans les Urines de Personnes Hystériques et Epileptiques, quelques heures après les attaques.* (Note de M. Michéa.)
Absence of Sugar in the Urine of Hysterical and Epileptic Persons after the Fits. 'Comptes Rendus,' t. xxxiii.
3. *On the Presence of Sugar in the Urine of Aged Persons.* By A. Dechambre. ('Gazette Medicale,' No. 14, 1852.)

Since the interesting researches of Bernard were made known to us, the subject of sugar in the urine has occupied the attention of physiologists both in this country and on the continent. After the experiments of Bernard on the effect of irritating the floor of the fourth ventricle in rabbits and other animals had been repeated, and the startling results obtained by him confirmed by numerous observers, it became an important point to establish a theory which would satisfactorily afford an explanation of the remarkable phenomenon which had been recorded. With this view experiments were repeated and varied according to the notions which presented themselves to the minds of different experimenters, until a vast number of new facts were obtained, which opened new fields for inquiry in various directions, but all bearing more or less on the subject of the presence of sugar in certain secretions, and also in the circulating fluid from which these secretions were elaborated.

Bernard repeated his experiments and extended his researches in those directions which he considered likely to afford new results, but up to the present time the only generalization which he appears to feel himself justified in offering as an explanation of the numerous and important facts he has discovered, is, that the presence of sugar in the urine, and of an undue amount in the systemic circulation, is due to increased activity of the function of the liver dependent upon an unusual excitation of the functions of the great sympathetic system of nerves. At present this is all he considers it right to advance. At the same time he continues the prosecution of his researches, but appears to wish to suspend his explanation of the origin of the changes until the investigations are more complete.

Other observers have attributed the presence of the sugar in the urine to the imperfect action of the respiratory functions, and consider that the results obtained by Bernard are to be explained by the respiratory changes being impaired in consequence of injury to the situation in which fibres of the vagus take their rise, rather than by the supposition that irritation in this part of the medulla stimulates the liver to secrete a greater quantity of sugar than in a perfectly healthy state, through the influence of the sympathetic system. If Bernard's view be correct, when sugar is detected in the urine it is a proof of increased activity in the sugar-secreting organ, more sugar being formed than is capable of being decomposed during its passage through the lungs, and in consequence the excess is eliminated from the circulating fluid by another channel. M. Alvaro Reynoso and others hold, on the contrary, that, instead of an excess, the normal quantity of sugar only may be formed, and that, in consequence of part of this escaping combustion in the lungs, owing to the imperfect action of these organs, it passes into the systemic circulation, and is excreted by the kidneys. Bernard has advanced several weighty reasons against this view; the chief of which is, that after division of the vagi, although respiration is greatly embarrassed, no sugar appears in the urine.

M. Reynoso has lately put forward several statements in the 'Comptes Rendus' in favour of his view; he finds that in almost every case in which the activity of the respiratory changes is interfered with, sugar may be detected in the urine. In the first place, this observer states that he has succeeded in detecting sugar in the urine of persons subjected to etherization, and that of two men under the influence of the vapour of ether, the urine obtained from the most active will contain the greater amount of sugar (vol. xxxiii. p. 416.) Under other circumstances of incomplete asphyxia, it is affirmed that sugar passes off in the urine. Rabbits which had been suffocated or drowned furnished saccharine urine, and living animals which had been prevented from breathing freely became diabetic. M. Reynoso also cites in favour of his view the observation of Bernard, that sugar is to be detected in the urine of fœtuses.* The urine of persons under the influence of the bichloride and iodide of mercury, the salts of antimony, opium, and narcotics in general, or sulphate of quinine, was found to contain sugar. (p. 520.)

In the same volume (xxxiii. p. 606) of the 'Comptes Rendus,' M. Reynoso also states, that dogs which have been submitted to the action of arsenic, lead, or sulphate of iron, excrete sugar in the urine. He also detected sugar in the urine of persons treated with carbonate of iron, and in that of patients affected with tubercle, the sugar being more abundant in those cases in which the malady was in its most advanced stages, and when the inflammatory symptoms were most intense. In pleurisy, asthma, and chronic bronchitis, and in cases of hysteria and epilepsy, sugar was also discovered in the urine. The method employed by M. Reynoso for the detection of the sugar is illustrated in the following example:

"The urine of a healthy man was carefully tested for sugar, and he was then subjected to the influence of ether. About 100 grammes (1543·36 grains) of the urine passed after recovery, was treated with solution of diacetate of lead, in order to separate the lithic acid, &c., and the precipitate was collected upon a filter. The excess of diacetate of lead was decomposed by chloride of sodium, and the mixture again filtered. The clear solution, after being concentrated by evaporation, was treated with the tartrate of potash and copper solution, or the yeast test was applied. Decided indications of the presence of sugar were obtained."

M. Michéa, in the same number, refers to the experiments of Reynoso, and also to the statements put forward in a work on Glucosuria, by Dr. Heller, in reference to the presence of sugar in the urine of patients afflicted with certain nervous disorders. The test employed by M. Michéa was similar to that recommended many years ago by Mr. Moore. Solution of potash was added to the urine, and the mixture was then boiled. If sugar were present, it should assume a dark orange-red colour after ebullition for a short time, and upon the subsequent addition of nitric acid, the solution

* M. Bernard examined the urine of the fœtuses of more than 150 cows and sheep, and he found it to contain sugar in every case. The tests employed were, the liquor potassæ, the tartrate of copper, and the fermentation tests.

would become paler, and at the same time the odour of melassic acid would be developed.

M. Michéa failed to obtain this reaction in the urine of four cases of hysteria, in which the urine passed four hours after the attack was subjected to examination. No indication of the presence of sugar could be obtained upon examining, in this way, the urine of seven cases of delirium tremens during the whole period of the malady; and a negative result was also obtained in six cases of general paralysis, five of mania, and three of partial delirium, in which the urine was examined each day during many weeks. In all—specimens of the urine of twenty-seven patients were subjected to examination by M. Michéa, and in not one could the least vestige of sugar be found.

We should here remark, that in a subsequent number of the 'Comptes Rendus' (vol. xxxiv. No. 1), M. Reynoso reiterates his former statements with reference to the presence of sugar in the urine of hysterical patients and epileptics, and offers to perform the experiments in the presence of M. Michéa.

In the 14th number of the 'Gazette Medicale' for 1852, is a report of some experiments by Dr. A. Déchambre, on the presence of sugar in the urine of aged persons. After mentioning Bernard's experiments and those of M. Reynoso, above referred to, Dr. Déchambre gives the result of six experiments on the urine of old people. The first subject chosen was an old woman of 81 years of age, whose respiration and heart's action were ascertained to be unimpaired by organic disease.

About 100 grammes of the morning urine were collected, and this quantity was treated with a solution of subacetate of lead; after filtration, the excess of lead salt was decomposed with a solution of carbonate of soda; after a second filtration the liquid was treated with the copper and potash solution of M. Barreswil, and an abundant reddish precipitate (suboxide of copper) took place after boiling for a minute.

The same experiment was performed upon the urine of five women whose ages varied from 68 to 81, one of whom had abscess of the arm, another rheumatic pains, the third a chronic affection of the skin, and the two others were suffering from contusions. The morning urine of each of these persons gave a characteristic precipitate when tested.

Next, specimens of the urine from eight healthy old women, of upwards of 70 years of age, with the exception of one who was only 63, were examined. In two cases only a slight yellowish cloud appeared, but in the remainder, a characteristic reddish precipitate was obtained. In a week's time the urine of seven of these cases was again subjected to examination. In two there was no trace of a yellowish cloud, in two others the cloud was slightly apparent, and in the three last cases a precipitate occurred.

The urine of four women from 70 to 92 years of age was examined, and was tested for sugar with the fermentation test. After being treated with acetate of lead and carbonate of soda as before, the solutions were concentrated, mixed with yeast, and placed in a retort, which was gently heated. By distillation at a gentle heat, about a gramme of a colourless liquid was obtained, which was boiled, and upon applying a light the vapour which was given off burnt with a bluish flame, leaving an unequivocal odour of alcohol. A similar experiment was tried upon specimens of the urine of six more women of upwards of 70 years of age, with a similar result. In neither of these experiments was the carbonic acid collected.

In these experiments the quantity of sugar present was not proved to bear any relation to the state of decrepitude of the persons, for in some of the most healthy and most vigorous, sugar was detected in considerable quantity, while in others, extremely aged and very decrepit, only slight indications of its presence were obtained.

From the experiments just referred to, it appears that on one day there may be much sugar in the urine, while in that secreted on the following day, all traces of its presence may have disappeared.

M. Déchambre concludes, from these experiments, that the urine of old people, as a general rule, contains sugar.

The theory of M. Reynoso receives some slight support from an interesting observation of Bouchardat's, which was made about twelve months ago—namely, that in two slight cases of diabetes, the urine was secreted free from sugar upon causing the patients to inhale oxygen.

Dr. Bence Jones obtained evidence of the presence of sugar in the urine of a woman under the care of Mr. Cæsar Hawkins, who had been kept for twenty-four hours and more under the influence of chloroform; two ounces at least of chloroform were taken, and the first urine passed afterwards was examined by Dr. Bence Jones; by the ordinary method no traces of sugar were found, but by following Reynoso's directions, "slight but distinct evidence" of its presence was obtained.

Sugar has also been found in the urine in cases of gout and dyspepsia, by Prout; and in certain abdominal affections and hypochondriasis, by Budge. Lehmann detected its presence in the urine of a puerperal woman, in whom the secretion of milk was suddenly suspended on the fifth day after delivery.

We found indications of the presence of sugar in considerable quantity in the prune-juice expectoration of a patient suffering from pneumonia, which was secreted shortly before his death. Upon boiling a portion of the sputa with the tartrate of copper and potash test, much suboxide of copper was reduced, and fell as a pale brown precipitate. This experiment was originally suggested to us by Dr. Todd, and is mentioned in a paper on 'The Diminution of Chlorides in the Urine, or their Absence from that fluid in cases of Pneumonia,' published in the volume of the 'Medico-Chirurgical Transactions' for the present year (vol. xxxv.), where also a report of the case (case ix.) will be found. The urine of this same patient, which was probably secreted about the same time as the sputa, was of an acid reaction, and specific gravity 1018. It had a highly offensive odour, and upon boiling a portion with the tartrate of potash and copper test, not the slightest change indicating the presence of sugar occurred. This specimen of urine, however, was not removed until the post-mortem examination, which did not take place for forty-two hours after death, which may perhaps, in a measure, account for the absence of indications of sugar. In Bernard's experiments it was found, that although sugar might be detected in a liver removed from the body immediately after death, no indications of its presence could be obtained if the examination was not performed for some hours afterwards.

From the foregoing statements put forward by continental observers, we find it difficult to arrive at any decisive conclusion with respect to the presence of sugar in the urine, in certain cases in which the aëration of the blood may not be carried on with that degree of activity which is consistent with a condition of perfect health. The conclusions of M. Reynoso are, to a certain extent, confirmed by the experiments of Déchambre on the urine of aged persons, while they cannot be said to be in any degree refuted by those of M. Michéa.

This latter observer does not appear to have tested the urine according to the method which was employed by Reynoso, but has used the liquor potassæ test, with the subsequent addition of excess of nitric acid. Although objections may be urged against the employment of those tests which indicate the presence of sugar by the reduction of suboxide of copper (a subject which will presently be considered), the potash test is fraught with still greater objections. A specimen of urine will not often be found, which, when mixed with solution of potash, will not become somewhat darker by boiling, and we should scarcely expect to detect the odour of melassic acid when only very small quantities of sugar were present; moreover, upon the addition of nitric acid other odours are evolved, which, it appears to us, would prevent the recognition of the peculiar smell of melassic acid, even if it were present.

The observation of Dr. Bence Jones on the urine of a patient who had been kept twenty-four hours under the influence of chloroform, is interesting, as it shows that under these circumstances traces only of sugar may be present in the urine, while from the observations of M. Reynoso, we should be led to expect, in so favourable a case as the one referred to, indications of a large quantity; at the same time it must be borne in mind that the presence of sugar may not be constant.

When only small quantities or mere traces of sugar exist in urine, it is a matter of considerable difficulty to detect the presence of this substance with certainty; for whichever method is employed we are in danger of failing to obtain a decisive result. On the one hand we feel that small quantities of sugar may be present in urine, and we may altogether fail to detect a trace, while on the other we may obtain a reaction which might be pronounced to depend on sugar, when, upon the application of other tests, no indications of this substance can be obtained. Again; we find from the researches of M. Déchambre, that although sugar may be detected with certainty in the urine passed upon one day, no traces of its presence may be met with, if another specimen be examined in a short period of time afterwards; moreover, the occasional occurrence of a diabetic state, which may last for a few days, or even for weeks, and then pass off entirely, has been for some time well known to physicians. It is very probable that when traces of sugar only are present in a specimen of urine, the length of time which may have elapsed before it is subjected to examination may materially affect the results obtained upon the application of tests.

It is much to be regretted that M. Reynoso and Dr. Déchambre have not furnished quantitative analyses; for from the results which they have obtained we should be led to expect, that in some of the specimens of urine subjected to examination by them, there was quite sufficient sugar for quantitative examination.

We have obtained, as opportunities have offered, in order to multiply observations on this point, the urine from epileptics, from patients who have been placed under the influence of chloroform previous to undergoing surgical operations; and in some other cases the results of our observations will be found stated in the following cases. It will be observed, that in these observations, the reactions which occurred upon the application of the tests are only mentioned, and we have not ventured to say whether sugar were present or absent. Although the results cannot be looked upon as conclusive, either with respect to the presence or absence of sugar, they may, perhaps, have the effect of drawing the attention of other observers in this country to this very interesting subject.

In the following cases, the urine was tested, as in Reynoso's experiments, except that carbonate of soda was used instead of chloride of sodium.

A certain quantity of urine was treated with a solution of subacetate of lead, until a precipitate was no longer produced. The precipitate was then separated by filtration, and the excess of subacetate of lead neutralized by the addition of carbonate of soda. The mixture was again filtered, and the clear solution concentrated to a small bulk by evaporation over a water-bath. The tartrate of copper and potash test, and, in some cases, Trommer's, the fermentation, or Moore's tests, in addition, were applied to the concentrated solution. In every case in which chloroform had been taken, the first specimen of urine passed after recovery from its effects, was subjected to examination.

1. *Gasken;* removal of foot by Mr Fergusson. Under the influence of chloroform for 15 minutes. Urine clear, pale, containing a very slight flocculent deposit; reaction acid; sp. gr. 1008. 7000 grains were treated, as above described, with subacetate of lead and carbonate of soda. The concentrated solution was treated with the tartrate of copper and potash test. After the greenish mixture had been boiled for a minute, it assumed a light-brown colour, and after the boiling had been discontinued, it became opalescent. Upon standing, a flocculent precipitate collected at the bottom of the test tube. Another portion was boiled with an equal bulk of potash. It gradually darkened until it assumed a dark-sherry tint.

2. *Ann Dyer,* æt. 50; removal of breast by Mr. Bowman. Under the influence of chloroform for 10 minutes. Urine of a lemon-yellow colour; slight deposit; reaction acid; sp. gr. 1022. 700 grains, treated as above. The same character of change occurred, upon boiling with the tartrate of potash and copper test, as in case 1, but the reaction was not nearly so decided.

3. *Smith;* length of time under the influence of chloroform not mentioned. Urine of a yellowish-brown colour; reaction acid; sp. gr. 1024. 500 grains, treated as above directed. Upon boiling with the tartrate solution the change was not so decided as in case 1. Another portion was treated with Trommer's test in the usual way. The mixture became brown, but the yellowish-brown precipitate, which is characteristic of the presence of sugar, was not produced.

4. *F. S., an old lady,* 87 *years of age;* suffering from chronic bronchitis, accompanied with profuse expectoration; otherwise healthy; reaction acid; sp. gr. 1015. A little of the urine was tested with the tartrate solution. Upon boiling, the mixture became brown, but the only precipitate

produced was that of a little phosphate; this, however, was of a brown colour. 700 grains were treated, as in the other cases. Upon boiling the concentrated solution with the tartrate test, the greenish mixture became bright brown, and a flocculent precipitate subsided.

5. *J. W., an old man, aged* 96; suffering from a large sloughing ulcer on the left leg, otherwise in good health. Urine clear; no deposit; reaction acid; sp. gr. 1014. 1500 grains, treated as above. In this case the reaction was as decided as in case 4. It should be remarked, that in cases 4 and 5, the concentrated solution had a brown tint before the addition of the test. When the blue solution was added, the mixture assumed a greenish colour, which, upon boiling, became bright brown. In every case, unless stated otherwise, the mixture was raised to the boiling point, and as soon as decided ebullition had taken place, the lamp was removed.

6. *Charles Hicks, æt.* 9. *Epilepsy;* Urine passed after a fit, of a pale-yellow colour; reaction acid; sp. gr. 1019. 700 grains, treated as in the other cases. Upon boiling the concentrated solution with the tartrate test, the mixture became of a brown colour, and a flocculent precipitate subsided.

7. *Urine of a young, healthy man, æt.* 24. Clear, of a yellow colour; reaction acid; sp. gr. 1016. 1500 grains, treated as above. Upon boiling the concentrated solution with the tartrate solution, the mixture became of a brown colour, darker than occurred in case 6, but not so dark as in 1, 4, and 5. A flocculent precipitate subsided.

8. *Johanna Mead, æt.* 12. *Epilepsy;* generally has three or four fits a day, each lasting from five minutes to a quarter of an hour. The specimen of urine subjected to examination was passed about half an hour after a fit; reaction acid; sp. gr. 1016. 700 grains, treated as above directed. The concentrated solution, mixed with the tartrate test, assumed a greenish hue, which, upon boiling, became paler and opalescent, but no decidedly brown colour was produced.

9. *Urine from a boy, æt.* 8, the subject of an operation upon a cicatrix of the hand; under the influence of chloroform for 10 minutes. The specimen examined was passed within two hours after recovery; reaction acid; sp. gr. 1024. 600 grains, treated as above. Not the slightest change in the colour was produced by boiling a portion of the concentrated solution with the tartrate test for some minutes.

10. *Thompson;* operated on for cancer of the penis, by Mr. Fergusson; a quarter of an hour under the influence of chloroform. Urine contains much lithate of ammonia; reaction acid; sp. gr. 1020. 1000 grains, treated as in the other cases. Upon boiling the concentrated solution with the tartrate test, it assumed a light-brown colour. About a drachm of the urine was also tested in a similar manner, and a similar pale-brown colour was developed. Two drachms of the urine were mixed with yeast, and placed for several hours in a temperature of 80° to 100°, but not the slightest bubble of gas was formed.

11. *Ann Goring, æt.* 20; excision of knee-joint by Mr. Fergusson. The patient was under the influence of chloroform for about half an hour, and did not pass any urine for 18 hours after recovery, when upwards of a pint was voided; reaction very acid; sp. gr. 1030. Two drachms were fermented with six drops of yeast. A bubble of gas was formed which measured ·04 cubic inch, while in a mixture of corresponding quantities of

water and yeast, a bubble of gas not more than half the size was developed This experiment was repeated with the same result; 700 grains were treated with subacetate of lead and carbonate of soda, as above. The concentrated solution, when boiled with an equal bulk of the tartrate test, assumed a bright reddish-brown tint.

Of the specimens of urine from these cases, all but those of cases 8 and 9 became of a brown colour upon being boiled with the tartrate of potash and copper solution, and, in several, flocculent precipitates were produced, but these precipitates had not the peculiar appearance exhibited by that of the suboxide, which is decisive in favour of the presence of sugar. Of these nine specimens which became of a brown colour, five were from cases which had been subjected to the influence of chloroform, two were taken from aged people, one from a child suffering from epilepsy, and one from a young man in perfect health. Of the two remaining cases, one was a case of a child with epilepsy, and the other was that of a boy who had been placed under the influence of chloroform for ten minutes. In case 10 the presence of sugar was not confirmed by the yeast test, but in case 11, indications of sugar were obtained by this test. Is the change of colour from greenish to a brown or reddish-brown upon boiling with the tartrate of copper test, or with Trommer's test, to be taken as evidence of the presence of sugar? If so, sugar was present in the urine of a man, æt. 24, to all appearance in perfect health. If, on the other hand, the production of a precipitate of suboxide of copper is alone to be considered indicative of the presence of sugar, then there is not one of the foregoing instances in which we could assert that sugar was present, for, although in several a precipitate occurred, it differed materially in its character from that obtained by testing diluted diabetic urine, or a very weak aqueous solution of grape sugar in a similar manner.

It becomes necessary, then, to institute a strict inquiry into the degree of reliance which can be placed upon the tartrate of copper and potash solution as a test for small quantities or even traces of grape sugar in the urine, and we have therefore carefully performed the following experiments. The grape sugar employed was obtained from raisins, and by a previous experiment it was ascertained that a very dilute aqueous solution readily yielded the characteristic reaction with the tartrate solution. In many instances the results were confirmed by using a solution of pure crystals of grape sugar, obtained by acting upon starch with sulphuric acid; and corresponding results were obtained when diabetic urine was examined.

1. The precipitate of suboxide of copper was readily dissolved by acetic, hydrochloric, and nitric acids, and also by ammonia.

2. The precipitate was insoluble in a solution of chloride of sodium, but was readily dissolved by a weak solution of muriate of ammonia.

3. The addition of a few drops of a weak solution of muriate of ammonia previous to boiling, entirely prevented the precipitation of the suboxide, the mixture retaining its greenish colour. Upon adding some solution of potash, however, the precipitate was produced, and ammoniacal fumes were given off at the same time. If a moderate quantity of solution of muriate of ammonia was present, the precipitate did not occur upon the addition of potash, or by very prolonged boiling.

4. If a drop of a very dilute solution of the muriate was added to a pretty strong solution of sugar, and after the addition of the tartrate, the mixture was boiled, no precipitate took place, but the solution became of a pale brown tint; the suboxide being immediately thrown down upon the addition of a few drops of a solution of potash, with the development of ammoniacal fumes. In the above cases in which no precipitate took place, it was ascertained that there was excess of alkali present in the mixture.

5. A solution of oxalate of ammonia also prevented the precipitation of the suboxide, but a greater quantity of this salt was required.

6. A neutral solution of lithate of ammonia (artificially prepared) also prevented the reduction of the suboxide, and dissolved the precipitate if added to it. On carrying out this experiment, it was found that

The precipitate of suboxide of copper was dissolved by urine containing an excess of lithate of ammonia, as exemplified by the next observation.

7. A solution of grape sugar in water was prepared, and by a previous experiment it was ascertained that upon boiling with the tartrate test an abundant precipitation of suboxide occurred.

To a portion of the precipitate of suboxide produced in this way, about a drachm of healthy urine, immediately after it was passed, and while yet warm, was added, and the reddish precipitate was instantly dissolved, forming a perfectly clear solution. Upon further boiling, a slight precipitate took place (probably phosphate). The suboxide, however, could not be precipitated by the further addition of potash and prolonged boiling.

8. Upon mixing a small quantity of grape sugar with the same specimen of healthy urine, and boiling the mixture with the tartrate test, no precipitate, except that owing to the presence of phosphates, was produced. About half an ounce of the same mixture of urine and grape sugar was placed in a test tube, mixed with six drops of yeast, and inverted over mercury. The whole was then placed in a temperature varying from 70° to 100° for about twelve hours, at the end of which time the tube was found quite filled with gas, and all the liquid was expelled into the vessel in which it had been placed. The specimen of urine with which the above experiments were tried, was allowed to stand in a still place, and when it had become quite cold, an abundant precipitate of lithate of ammonia was found to be present.

9. A portion of the aqueous solution of grape sugar was mixed with a strong solution of lithate of ammonia (artificially prepared), and then a certain quantity of the tartrate test was added, and the mixture boiled. The characteristic precipitate was not produced, but the mixture became of a pale fawn colour. In a weak solution of lithate of ammonia, the characteristic precipitate appeared after boiling the mixture for some minutes. *So that although much sugar is present, the colour of the mixture may be merely changed to brown, and no precipitate whatever may take place.*

10. A solution of grape sugar was treated with a drop of a dilute solution of muriate of ammonia, and boiled with the tartrate of potash and copper test. The mixture became of a brown colour, but no precipitate occurred. Upon the addition of a few drops of solution of potash, the precipitate of suboxide was produced.

A solution of grape sugar treated with Trommer's test, according to the usual method, behaved in the same way, in the presence of muriate of ammonia, as when treated with the tartrate of copper and potash solution; but in this case a greater quantity of the salt was necessary, for when only traces of the muriate were present, ammoniacal vapours were given off, and the precipitate of suboxide subsided, as before remarked.

From the foregoing experiments the following conclusions, with reference to the practical application of the tartrate of potash and copper test, Barreswil, Fehling, and Trommer's tests, may be drawn.

1. That if the urine contain muriate of ammonia (even in very small quantity), lithate of ammonia, or other ammoniacal salts, the suboxide of copper would not be thrown down if only a small quantity of sugar were present.

2. That unless there be a considerable quantity of the above salts present (in which case the blue colour will remain) the mixture will change to a brownish colour upon boiling, but no precipitate of suboxide of copper will occur. Where only a moderate amount of sugar is present, we have been unable to obtain a precipitate under these circumstances, by the addition of potash to the solution, and prolonged boiling. By observation 8, it appears that a specimen of urine exhibiting this reaction may contain a large quantity of sugar, as ascertained by the yeast test.

3. That in many cases in which the precipitation of the suboxide is prevented by the presence of ammoniacal salts, the addition of potash to the solution, and subsequent boiling, will cause the production of a precipitate, with the evolution of ammoniacal fumes. Hence care should always be taken that there is a considerable excess of free alkali present.

4. When only small quantities of sugar are present, and the precipitate of suboxide of copper is not decided, the fermentation test should be resorted to.

Upon treating different specimens of diabetic urine with the tartrate or with Trommer's test, it has been often noticed that in one case the precipitate is produced as soon as the mixture reaches the boiling point, or even before; while in other instances it is necessary to keep it in active ebullition for some minutes before any precipitate is produced. This circumstance receives explanation from the facts above detailed, with reference to the presence of ammoniacal salts; and other anomalous results which must have occurred to many in the habit of employing this test, become explained.

In those cases, however, in which the suboxide is reduced after boiling for not longer than a minute (or after being allowed to stand for some hours without the application of heat), and falls as a reddish or reddish-yellow precipitate, we have, we believe, as positive evidence of the presence of sugar as can be obtained. As a test for diabetic or grape sugar, if proper cautions be observed, much greater reliance can be placed upon Trommer's or the tartrate of copper and potash tests, than upon simply boiling with liquor potassæ; and in many cases it has advantages over the yeast test, which, however, need not be discussed here. Specimens of urine in which sugar is suspected to be present, and no decided precipitate of suboxide (which must be distinguished from phosphates*) occurs,

* The precipitate of suboxide of copper may be distinguished from the precipitate of phosphates, by its solubility in muriate of ammonia.

should always be carefully fermented with yeast before any conclusion is arrived at.

The facts now pointed out show what extreme care is necessary before the absence of sugar from urine can be determined with any certainty. Equal care is required before the presence of traces of sugar can be affirmed from the application of the tests in present use. We shall, however, if possible, return to the question in our July number, and endeavour, with the aid of additional facts, to form as accurate a judgment on this point as the present state of science will permit.

Lionel Beale.

REVIEW IX.

A Practical Treatise on Diseases of the Skin. By J. MOORE NELIGAN, M.D., M.R.I.A.—*Dublin*, 1852. Small 8vo, pp. 439.

FEW Dublin physicians are more favourably known to the profession than Dr. Neligan. The author of an excellent work on the Materia Medica, he has also been intimately associated with Dr. Graves in the preparation of the second edition of his valuable lectures on Clinical Medicine. Nor is the present the first occasion on which Dr. Neligan brings forward observations on diseases of the skin; already, his little treatise on the treatment of Eruptions of the Scalp, has earned for him, in this department, a well-deserved reputation. The evident aim of the author in the work now before us—an aim in which he has undoubtedly succeeded—has been to furnish the student and practitioner of medicine with a thoroughly practical guide to the knowledge of those important diseases of which it treats. Indeed, the greatest value of Dr. Neligan's treatise consists in the plain and thoroughly practical exposition he has given of a class of maladies, the opportunities for the study of which are unfortunately too limited in this country. The magnificent and complete hospitals for the relief of those affected with diseases of the skin, which several of the cities of the Continent, and particularly Paris, afford, contrast most unfavourably for us with the small advantages this country yields. And chiefly on that account the study of these diseases, and the knowledge of their pathology and treatment, have obtained a much more general diffusion among the profession abroad than at home. We are disposed to think that the causes which have led to this are not now likely long to exist; for without raising the question as to the propriety or non-propriety of the establishment of distinct hospitals for particular classes of disease, we think we may safely say that the study of skin-diseases is becoming every day more and more common; and we further think, that in the many excellent works which we now possess in our language on this subject, we have of the truth of this remark a ready confirmation.

Dr. Neligan's volume commences with a chapter on the classification of skin-diseases, a subject of confessedly great importance. His arrangement includes ten groups. 1. Exanthemata; 2. Vesiculæ; 3. Pustulæ; 4. Papulæ; 5. Squamæ; 6. Hypertrophiæ; 7. Hæmorrhagiæ; 8. Maculæ; 9. Cancroides; 10. Dermatophytæ; together with two supplementary groups, Syphilides, and diseases of the cuticular appendages, the Hair and Nails. These various divisions are considered in detail: and in now endeavouring to present our readers with a very short summary of what Dr. Neligan has written, we shall, for the sake of convenience, follow him in his arrangement.

1. Exanthemata.—From this group Dr. Neligan, contrary to usual practice, excludes rubeola and scarlatina, retaining erysipelas, and classing it along with erythema, urticaria, and roseola, as the components of the division. The reason for thus including erysipelas is not apparent, seeing that its febrile nature, which is allowed by all, and its contagious property, which is admitted by Dr. Neligan, naturally ally it with the two excluded diseases.

Our experience of the erythema nodosum differs in several particulars from that of Dr. Neligan. Unlike his observation, we have repeatedly found this form of erythema in males, chiefly boys. Lately we had an opportunity of seeing a case in the Royal Infirmary of Edinburgh, under the care of Dr. Keiller, in which the disease presented itself in a man of upwards of forty years of age. Dr. Neligan has rarely seen the nodose erythema dependent upon disorder of the menstrual functions; in this particular our own observation leads us to differ from him. No allusion is made to the connexion of this affection with the rheumatic diathesis, yet, if carefully looked for, we are satisfied this connexion will in the majority of cases be detected. A remedy which in our hands has proved most useful in this disease, and even in cases where quinine and other medicines had failed, is colchicum, whose anti-rheumatic virtues are well known and appreciated.

In the treatment of erysipelas the author remarks that his own experience is decidedly in favour of the tonic and stimulant plan, though, very properly, the opinion is modified to a certain extent by the statement that the experience referred to was chiefly acquired in a large and crowded city. Notice is also made of the mode of treatment lately proposed by Mr. Hamilton Bell, and his brother, Dr. Charles Bell, of Edinburgh—namely, the exhibition of the tincture of the muriate of iron in repeated doses. Since the publication of the paper of the Messrs. Bell, various corroborative testimonies have been offered, particularly by Dr. Begbie;* and from what we have ourselves seen of its efficacy in the worst cases of the disease, we entertain no doubt of its high value as a remedy, notwithstanding what has been said to the contrary by Professor Bennett and others, who, let it be observed, have never given it a fair trial.

2. Vesiculæ, including eczema, herpes, pemphigus, rupia, and scabies.—Dismissing Willan's group of bullæ altogether, Dr. Neligan has placed rupia among the vesicular diseases, and so doing has, we think, committed an error. If the bullæ are not to exist, which we cannot approve, then much rather had rupia be at once included among the pustulæ: for, although in many instances of rupia the bullar exceeds the pustular character, still in none can that disease be correctly classed with such truly vesicular ones as eczema or herpes.

The huile de cade, of the employment of which in the treatment of chronic eczema Dr. Neligan speaks, we have lately used in a large number of cases, both of eczema and of impetigo, affecting all parts of the body and scalp, and in almost every case with marked success. In a few cases the

* See Monthly Journal of Medical Science for September, 1852.

application of the oil has increased the uneasiness and pain in the affected part, and its use has in such been suspended. We cannot agree with the author in recommending inunctions with the huile de cade to be made twice daily; on the contrary, we feel certain that, so employed, few cures will result; twice or thrice a week is abundantly sufficient; and M. Devergie, whose experience of its use has been the largest, expressly states that more frequent application than that is very apt to magnify the disease. We are entirely at one with Dr. Neligan in his unqualified commendation of alkaline lotions in the treatment of chronic eczema. A word in regard to the employment of arsenic in the treatment of eczema. Dr. Neligan does not speak very sanguinely on this point. Our own experience of its use leads us to do so. Lately, at the suggestion of Dr. Begbie, we have seen most enviable results follow its exhibition to nursing mothers, whose infants had become affected with eczema, in some cases of the face and scalp, and in others of the whole body. In these cases the eruption in the child yielded exactly at the time the physiological effects of the remedy were discernible in the mother. In such cases we strongly recommend the above mode of treatment to our readers, feeling certain that it will not disappoint them.

3. Pustulæ, including acne, impetigo, erythema.

4. Papulæ, including lichen, prurigo.

On these two groups, the various members of which are well described, we have no remarks to make. The observations made by Dr. Neligan on the different methods of cure proposed for these affections are deserving of special attention.

5. Squamæ, including psoriasis, pityriasis.

We approve of Dr. Neligan's regarding lepra as a mere form of the scaly eruption of which psoriasis is the type. We object, however, to the removal of ichthyosis from this class, in which Willan placed it, and to which undoubtedly it belongs. The forms of psoriasis Dr. Neligan recognises. are three—guttata, aggregata, and lepræ-formis. We are glad to find Dr. Neligan speaking strongly in favour of arsenic in the treatment of psoriasis; it is certainly the most powerful remedy we possess.

Psoriasis is frequently rendered a complex disorder, its recognition difficult, and its treatment unsatisfactory, by being associated with other forms of skin disease—for example, with eczema and herpes; indeed, almost all of the simple eruptions, are liable to be thus complicated—a circumstance which ought to be borne in mind, both as regards diagnosis and treatment, yet one which, in systematic works on diseases of the skin, is not sufficiently dwelt upon, and to which Dr. Neligan too seldom alludes. There is no great difficulty in recognising a case of eczema or a case of psoriasis, and no great mystery in directing the treatment of either; but to establish easily and quickly, and that from a close observation of its characters, a case of eczema impetiginodes, or one of psoriasis herpetiformis, is a more difficult task, yet not one less likely to occur, for all varieties of the complex eruptions are sufficiently common.

The following are the remaining five groups of Dr. Neligan's arrangement:

6. Hypertrophiæ, including ichthyosis molluscum, stearrhoæ, elephantiasis verruca, clavus, nævus.

7. Hæmorrhagiæ, purpura.
8. Maculæ, including vitiligo, and ephelis.
9. Cancroides, lupus, and keloës.
10. Dermatophytæ, porrigo, sycosis.

We must not omit to mention that the concluding chapter of the volume is devoted to some interesting remarks on the therapeutics of diseases of the skin. Dr. Neligan's work we can recommend both to student and practitioner, as a useful guide to the knowledge and treatment of cutaneous diseases.

J. Warburton Begbie.

Review X.

1. *Die Eigenthümlichkeiten des Kindlichen Organismus im gesunden und kranken Zustande. Eine Propädentik der speciellen Kinderheilkunde.* Von Dr. Daniel Gottlob Moritz Schreber, Vorsteher der Orthopädischen Heilanstalt zu Leipzig.

The Peculiarities of the Organism of the Child, in the Conditions of Health and of Disease. A Propædentic to Special Pædiatrics. By Dr. D. G. Moritz Schreber, Director of the Orthopædic Institution at Leipsic. —*Leipsic*, 1852. 8vo, pp. 118.

2. *Ueber den Durchfall der Kinder.* Von Dr. C. F. Eichstedt, Assistenz-arzte bei der geburtshülflichen Klinik zu Greifswald, &c. &c.

On the Diarrhœa of Children. By Dr. C. F. Eichstedt, Assistant-physician at the Maternity Charity of Greifswald.—*Greifswald*, 1852. 8vo, pp. 131.

3. *Die Krankheiten der Neugebornen und Säuglinge vom klinischen und pathologisch-anatomischen Standpunkte bearbeitet.* Von Alois Bednar, Dr. der Medecin, Vorm. prov. Primar-artze des K.K. Findelhauses in Wien, &c. &c. Dritter Theil. Krankheiten der Respirations-organe, &c. &c.

The Diseases of New-born Children, and of Children at the Breast, Viewed in Relation to Clinical Medicine and Pathologic Anatomy. By Alois Bednar, M.D., &c. Part III. Diseases of the Organs of Respiration, &c.—*Vienna*, 1852. 8vo, pp. 208.

4. *Journal für Kinderkrankheiten.* Herausgegeben von Drn. Behrend und Hildebrand.—Band XVIII., Heft 3 u. 4.

Journal for the Diseases of Children. Edited by Drs. Behrend and Hildebrand.—Vol. XVIII., Numb. 3 and 4.—*Erlangen*, 1852. 8vo, pp. 156.

We had intended, if sufficient space had been at our command, to have made some general remarks upon those circumstances which appear to us to have operated in causing the more eminent writers on the diseases &c. of children, to limit their systematic treatises to the consideration of the individual maladies of infancy and childhood, and to withhold from us any distinct or substantive work upon the general physiologic and pathologic peculiarities which distinguish the youthful from the adult frame.

Re infectâ, however, we may just observe that although, in connexion with the latter subject, the reader may refer with much profit to the opening chapters of the new edition of Bouchut, and to those appended to the well-known and elaborate work of MM. Rilliet and Barthez, as also to the introductory remarks in a few other systematic treatises; yet it cannot be denied that the want of such a separate work as we have alluded to from one well known in our present *spécialité*, has been a chief cause why the subject has fallen into the hands of both *extra* and *intra*-professional charlatanism, and we have become overridden with so many 'Mother's Guides,' 'Lectures on the Management of Infancy,' &c. &c., which deal either with the very vaguest and most puerile generalities, or with such topics and recommendations as prove their authors to have had alone in view the recommendation of Iago, "Put money in thy purse." Be this as it may, however, Dr. Schreber, participating in some of our opinions, and apparently having no faith in the value of any semi-popular and professional 'Guides to the Nurse and Nursery, (advertising media, we know from experience, his own country, like this one, patronizes,) has thought it advisable to offer a scientific sketch that might be of service to the student as an introduction to the study of special pædiatrics.

"The scope of this work extends only to that point where the office of the latter study commences, and with which of course it stands in immediate connexion. It is intended to assist in establishing that foreknowledge which the practitioner must possess if he would apply himself with a hope of success to the study and practice of medicine as connected with children. The system of the child in its whole essence offers so much to the practitioner, and so many peculiarities and specialities as relates to practice, that the attempt to establish a 'propædentic to special pædiatrics' may be well justified." (Pref. iv.)

Dr. Schreber divides his treatise into five sections, the titles of one or two of which do not certainly very clearly indicate what is contained under them. The first section is entitled "Physiologico-practical Fundamental Propositions;" the second, "Dietetics of Childhood in their general Indications;" the third, "General Propositions and Rules relative to the Management of the Diseases of Children;" the fourth, "General Observations on the Eruption and Treatment of such Affections which, although common to the Adult as well as to the Child, offer, in the latter, marked peculiarities;" the fifth and last treats of Dentition, and some allied subjects. Upon the first section, we shall offer no comment—not that it admits of none, but in regard to our limits. The second refers to the general hygiène of the child, the author introducing the subject by some remarks relative to that of the mother, as the only medium by which the child can be influenced during its intra-uterine existence.

"By such prophylactic precaution alone is it in the power of the physician to affect the fœtus. All attempts and recommendations for operating curatively upon the diseases of the fœtus have hitherto failed, and evidently upon the double ground, first, because the diagnosis of its affections is not yet established upon a sound basis; and secondly, because experience hitherto has shown that we possess no sure and previously-to-be-reckoned-upon method by which we can exert our therapeutics or employ our drugs. Nevertheless, many persons have been misled by an appearance of the possibility of such, and have erroneously imagined that particular remedial measures (as the antiphlogistic, especially loss of blood, alteratives, &c.,) must, through the maternal organism, operate upon the fœtal one just in the same way as they do upon the first." (p. 27.)

M. Devilliers seems to be of this opinion, since in the abstract given of his communication to the Academy in Paris upon the syphilis of pregnant women, in the 'Journal für Kinderkrankeiten,' he is reported to have said that

"In every month of pregnancy the antisyphilitic treatment of the mother and child seems to be the better borne the more complicated and serious the syphilitic symptoms. The primary, as also the secondary ones, which show themselves during the latter weeks of gestation, require not only a local treatment (especially when having their seat on the genitals of the mother), but a general one, from which it at all events results, that the child at birth, if not healed, comes into the world with milder symptoms, and more readily supports the continued mercurialization, if under such circumstances it should appear necessary." (p. 312.)

Dr. Schreber's remarks upon diet, clothing, sleep, education, &c., are all very good so far as they go, but offer nothing novel or sufficiently important to detain us. We find place, however, for the following note concluding the section:

"The distinctive characters of a prematurely born child (viability being considered possible from the thirtieth week of pregnancy) are the following: whilst the body is generally smaller and more delicate, the head is unproportionably large, the skin of a deep red colour and still covered with soft hair, the power of generating animal warmth low, the nails of the fingers and toes soft and imperfectly developed, the movements weak, and the power of respiring and sucking below par. The latter at least is not continuous, without cessation or pause, the cry is weak or whimpering, and the child is almost always asleep. According to the numerous measurements of Outreponts ('Gemeins. deutsche Zeitschr. f. Geburtsk., B. iv. H. 4, S. 558) the position of the umbilicus affords a sure criterion. In children born at the full time it is placed exactly midway between the vertex and sole of the foot; its position away and lower down from this middle point is in a ratio with the prematureness of the child." (p. 44.)

When we direct our attention to this point in a cursory examination, the small size of the little finger, and of its nail especially, and the tendency of the child to curl up and cross its legs in a fœtal-like manner, are the signs to which we generally refer.

The third section of Dr. Schreber's work is, in our opinion, the best, and will well repay perusal, particularly the subdivisions on "Methodics" and "Semeiotics." While perusing our author's "Therapeutics," we could not help recalling to mind the able little work of Dr. Beck, noticed by us in a former number, and which made us wish that Dr. Schreber had not kept himself within such a limited scope as relates to this subject. From the "Methodics" we extract the following, well knowing the value of the caution urged in it:

"In consequence of the greater mutability of the child's system, by which one disorder easily passes into or becomes complicated with another, and from the, in general, more deceptive symptoms of its disorders, the *prognosis* should be given with the greatest circumspection; and whenever it appears necessary, it is advisable to inform the relatives of the patient (at least the more prudent of them) at the outset, of the possibility of the supervention of the usual modifications and complications of the disease. Inattention to this rule will soon destroy the reputation of any desirous of being thought conversant with the diseases of children. Relative to it one of the most difficult points of our 'methodics' is seen, for the physician of a sick child has particularly to be aware that he is the source of alone the most soothing influence in everything connected with his little patient." (p. 48.)

Those conversant with the maladies we are discussing, cannot fail to have been struck with the very rapid and extreme emaciation which frequently accompanies many of them, particularly remittent fever complicated with diarrhœa, occurring in the summer months. What with the loss of appetite preventing nourishment being taken, the sickness, the discharge from the bowels, and the systemic erethism, the marasmus becomes the point of most anxiety to the parents, and consequently the one most urged by them on the notice of the practitioner. On the part of the latter, it is requisite he possess the full confidence of the relatives ere they will believe the justice of his assertion, that such symptom is not of chief importance in his mind. The recollection of the bilious remittent (with its tendency to jaundice) of the past summer, forcibly reminds us of the truth of the above. In his "Semeiotics," Dr. Schreber remarks:

"In all diseases associated with diminished nutrition the child emaciates far more quickly than the adult, but, on the other hand, the former regains its condition much sooner than the latter. Such emaciation—if the disease is not very dangerous—is of no great import so long as the countenance does not exhibit marked change; but if it does, if it assumes an unnatural, more aged, thin, wrinkled appearance, we may always reckon upon a deep and danger-threatening diminution of reproduction and of the whole vital power." (p. 55.)

It is stated (p. 62), that great dilatation of the pupils, with bluish rings beneath the eyelids, is a common symptom of the presence of intestinal worms. That largely dilated pupils with bluish irides are frequent in the xanthous variety of scrofulous children, and that in them intestinal parasites are very frequent, are two facts of which we are well aware. But that the former statement, applied generally and purely in reference to verminous disorders, is true, we are loath to admit. That general bloodletting has a different relative effect, *cæteris paribus*, upon the young child, to what it has upon the adult, is universally admitted. When carried to any extent, the nervous system is more powerfully affected; and if syncope should happen to supervene, convulsions and coma may terminate the matter. A repetition of general bloodletting is also in general detrimental, although the first venæsection may have produced no ill effect. Under any circumstances, too, the recovery from the effects of loss of blood, though the latter may have been of avail in arresting the malady, is slower and more difficult than in the adult. These, and other circumstances we need not now allude to, with the exception of the fact that local depletion has relatively more influence upon the child than upon older persons, must incline one to agree with the doctrine taught by our author (p. 78), that, except under peculiar and very urgent circumstances, local bleeding is to be had recourse to in young children in preference to general; the more particularly as the former, under proper management, is in *most* cases a weapon of such force that no stronger should be employed. Whilst we have no hesitation in maintaining this doctrine, we would express our belief that such *local* depletion may be often substituted with great advantage for the antimonial and mercurial &c. depressants by which alone, in many cases, particular affections are therapeutically met. The disease is more quickly, and we think effectually arrested, a relapse less probable, and the patient and the relatives less troubled in the long run, than by the constant administration of the above remedies, every two or three hours *in* the day. Amongst the lower classes of society, too, very little dependence is often to be placed upon their giving properly what has been ordered; though at the same time we admit that serious results often arise from their mismanagement of leeches. Upon this point we have before touched when reviewing the work of Dr. Churchill.* If general bloodletting is of limited application in the young child, blistering by cantharides plaster is not less so. This statement we intend more particularly to apply to the treatment of thoracic inflammation generally, and with especial force to it when occurring in the course of the exanthemata. Blisters may be applied behind the ears, even in very young children, with—as a rule—impunity, but on the sternum or between the scapulæ they prove generally as bad as the disease (and often much worse), for which they have been employed. Too often have we seen the child sink from the want of sleep, local and general irritation, thus given rise to; sloughing of the sore is by no means uncommon, and altogether the practice should be, as far as possible, abrogated. Even in good hands, employed by those who are aware that they produce their effects here in a shorter time than in the adult—that the local inflammation is greater, the constitutional erethism they give rise to of a higher grade, and their primary action far more likely to be followed by the secondary consequences of ulceration and gangrene, and thus only allowed to remain on the skin but a very short time—we have known them produce the greatest anxiety in the mind of the practitioner when he has witnessed the result of the application of the emplastrum cantharidis for one hour and a half. We are of full agreement, therefore, with the general tenour of Dr. Schreber's advice (p. 80), but not so with the particular dislike he appears to have to the use of sinapisms. We prefer mustard as a counter-irritant in the young child, to any softened or modified preparation of the cantharis vesicatoria. Our author concludes his "Therapeutics" by some remarks upon the value of the "cold and warm water cure," as applied to children. The fourth section of his work is very well, so far as it goes; but the observations are of so general a character, as to offer no reason for our detaining our readers by any comments on them. The subject of syphilis receives more notice. The fifth and last section is mainly occupied with the question, "Whether and how far is the process of dentition to be considered of pathogenetic account?" In this country at least it has been long since answered in the affirmative; and with Dr. Schreber we entirely differ from those who deny that dentition may become of "pathogenetic account." Such persons—and we may take Dr. Brefeld† as their fitting representative—assert, that, "like all other universal developmental and formative epochs, dentition is a normal physiologic process, necessarily involved in the idea of organic life." This proposition may be freely admitted. But not so a conclusion which involves the admission of another (the suppressed) proposition—viz., that no normal physiologic process &c. can become of pathologic import, and therefore (the conclusion) that dentition cannot; or, as Dr. Schreber puts it—

"Is not the ordinary regeneration or moulting-process (including the decadency of the hair, skin, shell, &c.) of animals quite in a natural condition, well known to

* Vol. vi. p. 145, *et seq.*

† Dentitio difficilis—das gefährlichste aller medicinischen Vorurtheile. Hamm. 1840.

be often accompanied by more or less pathologic phenomena? In the human race, is not the appearance of puberty, are not pregnancy, parturition, the puerperal state, decrepitude, clearly pathologic processes, and yet admitted by all practitioners as capable of becoming of pathogenetic moment?" (p. 113.)

Upon what ground, then, is it to be maintained that dentition cannot? For further remarks we must refer to Dr. Schreber. In parting from him we would say, that although his work is of too slight and sketchy a character to fill up the void we have alluded to and he has aimed at closing, yet it is good in its intentions, able so far as it extends, and a fitting, though a scanty, introduction to the wide field of special pædiatrics.

If we stated that our present branch of medicine had now attained a development equal to any other, we should intend, of course, the assertion to be of general application only, well knowing that in respect to certain points of detail, it is yet behind in that precision which it is necessary to look for. Were a student, after having spent a few mornings in the month of August at a dispensary for children, and witnessed the numerous cases of a disease which would then prevail—viz. diarrhœa—to return home and seek to renew his acquaintance with it as afforded by books, we are sure he would very soon be sorely puzzled. In one work he would be told of the importance of diarrhœa as a special substantive disease, and all sorts of modifications or varieties of it explained to him. In another he would not discover the word at all employed as the sign of a special disorder; he would be told that there was no such disease *per se*, but that it was simply the name of a single symptom of several allied maladies, and which latter he must study, and which he must treat. He would find it by one asserted that instead of diarrhœa, he must think of general or local inflammation of the intestinal mucous membrane, or of ulceration of its follicles; or be directed to accept the doctrine that it is one of the aggregate of symptoms attendant upon some " phlegmasie ou ramollissement" of the linings of the digestive tube. On the other hand, it would be hinted to him by another, that whilst in many cases the above statement would hold good, it would not in all; and that it is not always easy to say during life which would be correct. It would be shown that the same lesion is common to many forms of diarrhœa; or that the lesion generally met with in a particular variety may be wanting after death, though during life we might have fairly inferred its existence. Nor would such views embrace all that he would meet with; he might still be assured that the best view of the matter is to accept only two chief forms of the disorder—viz., a functional or catarrhal diarrhœa, or a diarrhœa of irritation, and an enteritic or inflammatory one, a diarrhœa of organic lesion. Whilst another would produce proof that from the examination of no less than 336 cases, he could only come to the conclusion that " hyperhæmia of the brain and its membranes, anæmia of the lungs and of the liver, and the viscid exudations of the serous membranes, constitute the anatomical results found as proper to diarrhœa;"* that there is no such thing to be demonstrated in infants, either microscopically or anatomically, as catarrh of the intestinal mucous membrane, and that its chronic forms, or "muco-enteritis chronica,"

* Bednar, Part I, reviewed in vol. vii. p. 112.

must be described as a symptom of "tabes;" that he has never seen "dysentery" in children at the breast; and that in all probability the chief cause of their diarrhœa is to be sought in "a kind of fermentation," "a primary abnormal process of decomposition of the contents of the stomach and intestines."* Lastly, our new friend, Dr. Eichstedt, would impress upon our noviciate in pædiatrics, that in diarrhœa not of a chronic character, "dissections have afforded very various results; that no change was constant, except viscidity of the blood in obstinate cases" (p. 31); and that "the massing together by Bednar of the diarrhœas of children as one disease, only variable in degree, can only have, so far as treatment is concerned, the most prejudicial consequences" (p. 66). Now, considering the high authorities who have treated this subject, and, moreover, knowing the difficulties by which the exact elucidation of this apparently simple malady, diarrhœa, is surrounded, we would wish it to be understood that we speak with all deference of intention, if not of terms. It is evident that much of the discrepancy of opinion which exists has its origin, like many other things, in a defective analysis, and consequently too limited a generalization, on the one hand, and in a too sweeping synthesis and too universal a generalization, on the other. If Rosèn von Rosenstein, with his fourteen kinds of diarrhœa, one from overloading the stomach, another from acridity of bile, one from the retrocession of cutaneous eruptions, &c. &c., may represent an instance of the former; Rilliet and Barthez, in describing the results of their observations on affections of the digestive tube, under the terms of gastro-intestinal inflammation and typhoid fever, afford an example of the latter. In the present state of our knowledge, and as sufficient for all practical purposes, we may ask the following questions:—1st. Is not the disorder usually understood as diarrhœa frequently presented to us in such a form and under such circumstances as to justify us fully in viewing and treating it as a substantive affection, and this without directing our attention to any undeniable specific or constant anatomic lesion of the digestive mucous lining or its appendages, as its cause? 2ndly. Are there not other forms, often wrongly considered as representing a purely functional yet substantive disease, which should rather be regarded as a severe and important symptom of a malady having as one of its lesional characteristics variable grades of inflammation, and its different results, of the intestinal mucous membrane and its appendages? We would answer both in the affirmative. In respect to the first, it may be argued against us that, in viewing certain forms of diarrhœa as a purely functional as well as substantive malady, we are disregarding the apophthegm of Legendre,† and which has so fully received the sanction of the precise pathology of the day. We would reply, it must be so then, seeing, 1st, That the nature of the causes of these forms of the affection, the course and symptoms of the latter, their temporary character, their amenability to particular forms of treatment, forbid us to believe we have to combat with inflammatory action, with its results, or with any definable anatomic lesion. 2ndly. That post-mortem investigations, where a patient has had diarrhœa, but has died from another malady, absolutely show that no such intestinal organic lesions exist, and that even in some severe forms of the disorder,

* Bednar, op. cit.

† "C'est par l'altération des organes que l'on doit caractériser et spécifier les maladies."

where life seems to have succumbed to the violence of the diarrhœa, the intestinal changes bear no sort of proportion to the symptoms previously existing. And, 3rdly, As we believe that a refined analysis will inevitably lead us to the doctrine that all affections, not what may be termed traumatic in their character, are primarily functional, that is to say, originally dependent on some modification, alteration, or disturbance of *power*, we find ourselves under no such absolute necessity of discovering, as the essential nature of every malady, some material, visible, structural change; the more particularly as we too frequently see such change considered as *the* disease, or at least giving a name to it, when after all it is only the effect or result of a disease, and which latter is something else altogether different in its nature. So far, then, as the above propositions hold good, we feel justified in maintaining an affirmative to the first question; so far as they do not, and for the following reasons, we yield it to the second:—1st. Because the causes, extrinsic and intrinsic, of the forms of the disorder there referred to, the intensity, complications, progress, and duration of the latter, the form of treatment to which they are most amenable, or by which they are best palliated, lead us to believe that with the functional disturbance has co-existed from the first a distinct organic change,—that this disturbance becomes the more severe the greater this change, and that treatment is generally the more successful the more it is directed in reference to such change. 2ndly. Because post-mortem investigations evince the existence of morbid lesions, which we may fairly associate with the previous diarrhœa. Now that this affection must be regarded in this twofold light, we have quite sufficient personal experience for believing; but that it is an easy matter always to affirm of every diarrhœa under which category it is to be reckoned, we are far from maintaining. But this is not uncommon in other things: we know the general law, and are able to express it with a fair universality of application, but yet feel puzzled in reducing certain apparent cases within it, although we feel satisfied that it might be done, notwithstanding. As applied to disease, we infer from certain symptoms, &c., according to a general law, the want of or the existence of certain organic changes after death. In a particular case our inference is proved wrong. Where is the error? Have we interpreted the symptoms wrongly, and therefore the inference is not right; or did we seize them in all their bearings, give them their usual meaning, and yet they failed to speak the language they generally utter? Be the error where it may, we admit, as we before remarked, it is liable to be made when reasoning upon certain cases of diarrhœa, and therefore may be taken as another reason for the discrepancy in opinion which prevails respecting the latter. Thus Dr. West remarks, in proposing to distinguish between simple and inflammatory forms of it:

"I yet was forced to acknowledge that the distinction was one rather of degree than of kind, or perhaps it would be more correct to say, that our observation has not hitherto been minute enough to enable us to draw the line of demarcation strictly between the two affections. (Lect. p. 432.)

And Rilliet and Barthez, that—

"Not only are the lesions answering to the same symptoms sufficiently different, but, also, there is not always to be found a proportionate intensity between the one and the other; for whilst on the one hand we may have entero-colitis severe or slight in its symptoms during life, and its lesions equivalently so after death, we find other cases very intense, symptomatically, and but slight, anatomically considered; and likewise the converse.*"

Nor should the remarks of Bouchut† or of Bednar‡ and others, be forgotten. Had our limits permitted, we would have dilated somewhat upon the symptomatic characters of the two great divisions under which we have viewed this affection, and the anatomic alterations characterizing one of them. As it is, we must now allow Dr. Eichstedt to come under observation, premising that upon the morbid anatomy of diarrhœa he offers nothing that is new; on the contrary, it seems to have lain beyond his province. In his preface he informs us—

"Having had no opportunity for making numerous dissections, and being anxious to substantiate the more prominent effects of particular agents upon special portions of the intestinal canal, I instituted experiments upon rabbits." (p. ii.)

That our author participates in some of our own views is apparent from his commencement:

"By diarrhœa is understood the frequent evacuation of watery excrementitial matter from the anus. In recent times, instead of being regarded as a special disease, it has been looked upon as the symptom of various morbid conditions, and treated accordingly. Though I may not be disposed in the least to withstand this opinion, yet it appears to me, practical advantage will be found in a closer consideration and comparison of those circumstances in which diarrhœa appears as the chief symptom of the disease, the more particularly as in infancy it is often the only appreciable morbid phenomenon. It would be out of place here to enter more fully upon the subject, in respect to such cases where an important malady like pneumonia, or typhus, or tuberculosis, is present, and where the diarrhœa appears simply as a subordinate circumstance. In the adult the different forms of the affection are by no means of the same import, nor do they offer such peculiarities as in infancy, to which period, therefore, I shall confine my attention. Every practitioner will certainly agree with me in admitting the high importance of this abnormal condition in children, partly from the frequency of its occurrence, partly from the great danger arising to the little patient, particularly when it is prolonged, and the treatment at the beginning not exactly what it should have been.

"Before passing to the special investigation of it, it may be proper to allude to the anatomic relations of the digestive canal, so far as they are connected with our present subject." (p. 1.)

The observations of Dr. Eichstedt upon the latter we shall pass by, simply remarking that the structural peculiarities of the intestinal tube, the physiological incidents of digestion, the properties of the fæcal evacuations, &c., are all ably discussed. A general description of the symptoms and course of the acuter form of diarrhœa next follows; that of the chronic is afterwards given. Speaking of the amelioration of simple functional diarrhœa, it is observed—

"Unfortunately this favourable issue does not always follow; on the contrary, danger may arise in a twofold way, either by the supervention of brain affection, very easily induced as exhaustion comes on, or by the transition into inflammation of the intestines, most usually of the larger."

"If the disorder is not soon relieved the signs of exhaustion are quickly evinced, as seen in the sunken condition of the eyes, encircled by bluish rings, and in the

* Maladies des Enfans, tom. i. p. 510. † Malad. des Nouveaux-Nés, p. 524.
‡ Krankh. d. Neugeb, tom. i. s. 6, 46.

bluish colour of the lips. In children in the state of exhaustion, blueness of the skin soon comes on, as also coldness of it, particularly at the extremities. The fontanels sink and become elevated only when the child cries. Convulsions are now superadded, as likewise tendency to sopor, and the child dies, exhibiting the phenomena of acute hydrocephalus. Sometimes convulsions with their untoward termination supervene earlier, even at a time when the child is quite lively and the disorder considered as of no great importance." (p. 28.)

For the symptoms indicating the transition into inflammation we must refer to the author's pages. Following these we have a very short account of the post-mortem appearances, but a long one of Dr. Eichstedt's experiments upon his rabbits. The *résumé* of these we shall of course give, as our author appears to pride himself somewhat on them. Fourteen rabbits were employed, and were dosed with camboge, colocynth, hellebore, aloes, croton oil, rhubarb, and jalap.

"The result of these investigations is the deduction that all purgatives do not operate equally strong upon all portions of the intestines. Camboge and aloes had evidently exerted their chief influence upon the follicles of the larger bowel, croton oil influenced particularly the duodenum and jejunum, whilst colocynth, as it appeared to me, operated upon the nervous system without affecting in a high degree the mucous membrane of the canal. Rhubarb seemed in particular to excite the small intestine to increased action, as did also jalap, although in both cases a stronger flow of blood to the brain was likewise observed. Calomel appears to be converted into the sublimate in the stomach of the rabbit, and thus to acquire a corrosive power. That the loss of vitality to the mucous membrane arose from such local causticity, and that the ulceration was owing to the detachment of the membrane, there can be no doubt." (p. 39.)

In the tenth case the rabbit was dosed with ʒiss. of rhubarb; no pultaceous or frothy evacuation had taken place by the next day; it was then soporified by æther, and its abdominal cavity opened. The peristaltic movement soon became very strong, and intussusception followed. The process of its formation was ocularly observed by Dr. Eichstedt, who regards it as one of the most interesting observations he had the opportunity of making.

"A portion of the small intestine contracted itself quite closely, and continued some time in this state; the upper part of the bowel then commenced strong peristaltic action, and soon afterwards the part beneath and contiguous to the contracted portion assumed an antiperistaltic movement. As the peristaltic was stronger than the antiperistaltic motion, the contracted portion became pushed into the part moving antiperistaltically. I witnessed five such intussusceptions, in all the process being the same; after some time they became loosened. In one the contracted portion was pushed a full inch deep into the under bowel." (p. 38.)

It is well known to those who have examined the bodies of many children, that invagination or intussusception is not unfrequently met with in them; but, from the diseases of which such children have died being very variable, often no abdominal disturbance to speak of having been present during life, and from the intestines betraying no signs of inflammatory action, although as many as a dozen invaginations are said to have been found, it has been fairly presumed that they have arisen in the agony or moment of death. But this lesion is known to occur, and we have met with it, under two other circumstances—in the course of inflammatory diarrhœa, and as a primary disease—which, in the case of the latter variety occurring to us, was followed by peritonitis. Louis is reported* to have seen 300 cases of invagination among infants at the Salpêtrière dying during the travail of dentition or "*d'accidents vermineux.*" But as in these cases no appreciable symptoms of such a lesion were present during life, they evidently belong to the form of the affection first alluded to. Instances of the other varieties are by no means common, as may be judged of from the statement of Dr. West, that it has never happened to him to meet with a fatal case, and but only once to have observed an instance in which the symptoms of intus-susception having existed in a marked degree, at length spontaneously ceased, and were followed by the restoration of the infant to perfect health. (Lect. p. 424.) Still scattered amongst serials will be found the detail of numerous cases, nine being alluded to alone in Mr. Gorham's 'Observations,' published in the 'Guy's Hosp. Reports' in 1838. Indeed to this gentleman is due the credit of a very able analysis of the symptoms &c. of the disease in question; and it was chiefly by attention to the diagnostic sign laid down in his work that we were able to arrive at a correct diagnosis in the two cases we attended. This sign is "the passing of blood per anum in various degrees of purity, never indeed contaminated with fæculent matter, but chiefly with mucus," and to which should be added, constipation, vomiting, and constant straining as if a motion was to pass, nothing however but blood being seen, from a few drops in some cases to truly hæmorrhage in others. Of the two cases coming before our own notice, one was in a male child of eleven months, suffering under entero-colitis. No food was taken for several days before death; there was constipation, much vomiting, and frequent straining, with the passing of a little blood. The other was also a boy, æt. four months. He was brought to us two days before death, and described as having had no fæcal evacuation for five days, but vomiting and straining, the latter frequently accompanied by small bloody and slimy discharges. The abdomen became tympanitic and tender, and the child fell rapidly into a state of collapse. Intus-susception was diagnosed here, as in the other case. In both, the necroscopsies substantiated the diagnosis, as well as that of peritonitis in the latter. In both of these cases, and in another, that of a young lady, in whom it was assumed, from the symptoms only, that invagination had occurred, the plan of injecting large quantities of fluid (in the latter up a long tube) was tried: in neither was it of any avail, but, on the contrary, seemed to be a source of much trouble, if not pain, to the patient. We shall probably recur to this subject at a future period, as there is much to say upon it.

The subject of Ætiology is next and well discussed. The effects of local irritation on the alimentary canal, as from bad maternal milk, faulty diet, too hot and fermenting aliments and drinks, substances producing increased secretion of bile, agents having a direct purgative action of their own, and circumstances operating physically, are severally considered. The influence of dentition, cold, &c., applied to the surface, epidemic causes, and contagion, then follow. In this portion of his book, Dr. Eichstedt introduces, unnecessarily, as we think, a long account of one of his *hobbies*—viz., the structure and functions of the secreting organs of the skin. Of the correctness of his views upon these points we will offer no opinion, but leave

* By Hevin, in his Memoir on Gastrotomy, and referred to by Bouchut. Mal. des Nouveaux-Nés, p. 585.

him to the care of the anatomists and physiologists; but we cannot avoid remarking, that if the following be the chief practical result our friend has arrived at, we must place him by the side of that famous historian, who discovered, after much research, that "the Dutch had taken Holland."

"The whole external skin, viewed particularly as a sweat-secreting organ, stands in close antagonistic relationship to the intestinal canal; so that when suppression of the perspiration ensues, increased secretion from the intestinal mucous membrane very readily supervenes, and on a sudden hindrance to the increased secretion from the bowels the perspiration is augmented. Such alternating circumstances are frequently observed during the course of a malady such as phthisis. In this way 'taking cold' operates, the cutaneous exhalation is depressed, and in lieu of it the intestinal secretion is augmented." (p. 54)

The consideration of the symptoms and treatment of the different forms of diarrhœa established by the author, next occupies attention. For systematizing the latter, Dr. Eichstedt has evidently some leaning towards a division, depending upon the character of the stools. A sketch of such a classification is given, and a description of its various forms—viz., diarrhœa fæculenta, biliosa, serosa, mucosa, puriformis, chylosa, sanguinea, and lienterica (p. 60). Now, if we are to have a classification of this kind, we should prefer Trousseau's, with the addition of one form, the D. serosa. However, Dr. Eichstedt himself, scarcely satisfied with this principle of division, and believing that the best of all for practical purposes is that which most prominently brings forward "the *periods* when we have to do with their treatment" (p. 67), finally adopts the following arrangement:

1. Diarrhœa previous to *dentition*. { *a.* Simple diarrhœa. *b.* Diarrhœa tending to inflammation of the mucous membrane.
2. *Diarrhœa of dentition.*
3. *Diarrhœa of weaning.*
4. Diarrhœa caused by epidemic *influences*. { *a.* Epidemic. *b.* Dysenteric. *c.* Choloric.
5. *Chronic diarrhœa.*

Whatever opinion we may hold upon some minor points of our author's views, we can strongly recommend this portion of his treatise, as containing much valuable practical information; as an example, we select the following:

"If the diarrhœa has been caused by improper food it will often be necessary to remove such matters, indigestible by the child, by gentle purgatives. For this purpose, a combination of magnesia with rhubarb answers very well. If a decided tendency to the formation of acid has already shown itself, it will be necessary to neutralize the superfluous acid, or remove it from the stomach, and destroy such tendency. For its neutralization, magnesia usta, or the carbonate, is most frequently used, in union with rhubarb, to carry it off. Bressler regards magnesia as lying too heavy upon the stomach, and as not always the best remedy against acidity. He prefers equal parts of aq. calcis, and some aromatic water, in teaspoonful doses, or the liq. kali carb., very cautiously administered in small quantities. As antacids, the hydrate of alumina, and wood charcoal are to be particularly recommended. The former, first advised by Percival, then approved of by Ficinus, has been strongly advocated by Weese and others. If much acid be present, mere neutralization will not suffice; it will be proper that an emetic be first given, and then magnesia, with rhubarb, administered. The use of emetics in young children has found many advocates, especially in former times. They were chiefly recommended in the diarrhœa of children by Armstrong and Schœfer, the former employing tartar emetic every six to eight hours, until the evacuations improved. Schœfer agrees with this, and remarks: 'Very many, indeed I might almost say most, of the diseases of new-born and young children require repeated evacuations, and particularly the employment of emetics.' In modern times ipecacuanha has been preferred, as by Trousseau, Verson, and others. It is often necessary to lessen the increased sensibility of the intestinal mucous lining, caused by the irritant action of the acid; for this saffron best suffices, and which can be fitly conjoined with rhubarb and magnesia. Nux vomica, in small doses, has often been found useful in these cases, but opium should never be resorted to After the evacuation of the acid contents, the tendency to the formation of new acid must be obviated; the diet must therefore be changed; milk must be entirely avoided for a few days. The more useful remedial agents will be found to be the chloride of calcium and the carbonate of ammonia, in small doses; by these the disposition to the generation of acid is often quite removed in a short time. If such be effected, no gastric impurities being now present, then the diarrhœa, if continuing, may be met by astringent remedies, amongst which, calumba, the extract of cascarilla, and lime-water, with milk, have proved the best." (p. 75.)

The treatment thought most highly of by the author, where inflammatory symptoms follow the simpler forms of the malady, consists of leeches to the abdomen, and calomel frequently repeated in small doses. The epidemic varieties of diarrhœa are illustrated by an account chiefly of an outbreak of the disorder which occurred at Greifswald, in the summer of 1842. The few experiments made by Dr. Eichstedt in feeding rabbits and a hen with matter from the evacuations of cholera patients, should be compared with our abstract of M. Renault's, in our last July number, page 252. The former thinks he may draw at least the deduction that an agent was present in the evacuated material, which operated particularly energetic on the nervous system of such animals as he administered it to. The work terminates with the consideration of chronic diarrhœa. It is one of considerable practical value, and contains much valuable information in a comparatively small compass. Upon some litigated points of morbid anatomy, it gives us, it is true, no new information; indeed, this subject is the weaker point of the book; but it contains physiological information of much interest in connexion with the disorder it treats of; and the ætiology, symptomatology, and theraupeutics, of the latter are well worked out. We could have wished to have dealt more fully with Dr. Eichstedt.

In our review of Dr. Weber's work* we drew attention to some of the more interesting points in reference to *cephalhœmatoma.* Opinions being various still as regards one or two circumstances connected with the subject, we would further direct our readers to an able paper† by Professor Levy, of Copenhagen, in the 'Journal für Kinderkrankheiten.' It is based upon the personal observation of fourteen cases, aided by reference to the experience of other observers. We shall give a short analytic *résumé* of the more important conclusions arrived at by the author. These are, 1st, that too much stress is not to be laid on the assertion of different writers, that the affection is chiefly met with in primiparæ, since out of fourteen cases, four were not such; 2ndly, that there is even less reason for believing its frequency of occurrence in boys to much exceed that in girls, as the ratio was only 8—6; 3rdly, that the views of Valleix, as to its

* Beiträge zur Pathologischen Anatomie der Neugebornen. Rev., vol. x. p. 6.

† Beobachtungen über des Kephalämatom, &c. (p. 161 of Journal.)

usual origin or ordinary mode of formation are probably incorrect, the disease being rather due to a strong resisting force applied to a particular portion of the head, by some part of the bones of the pelvis, varying according to the point of greatest frictional pressure during labour. The assumed cause of Valleix—viz., circular pressure by the neck of the uterus, only produces subpericranial ecchymosis, between which and cephalhæmatoma, the difference, although in one respect scarcely more than in degree, is symptomatically very great. The former is only known to exist by post-mortem investigation, and is of importance practically to the medical jurist only. 4thly. In respect to diagnosis, there are two points demanding attentive consideration. The first is, that the base of the tumour never approaches nearer to the edges of the bones than from one to one line and a half, and never intrudes upon a suture or a fontanelle. The second, that the base of the well-defined swelling is encircled by a hard ring. By attention to the former, as also to the facts that the tumour is non-pulsating, does not move during deep respiration, crying, or coughing, cannot be lessened by pressure, and that the want of bone is an illusory sensation, cephalhæmatoma is to be distinguished from hernia cerebri. The presence of the "ring" is constant in *mature* cephalhæmatoma. Sometimes it is formed by the first day after birth, in other cases it is not complete in its whole circumference until several days after.* It begins to be formed so soon as the periosteum ceases to be separated from the bone, in consequence of the increase of the circumferential swelling of the tumour being arrested. 5thly. Valleix is probably correct in the main as regards the nature of this "bony pad, or cushion." It is due to ossification taking place in plasma-exudation, poured out by the periosteum in a state of irritation from the constant distension and pressure it suffers where it is separated from the bone by the extravasated fluid of the tumour. 6thly. As regards treatment, it may be stated that the latter may heal spontaneously, or inflammation or suppuration may ensue; or even caries of the underlying bone. Guided by the first fact, cold evaporating lotions may be applied to hasten the resorption of the extravasated blood. If in six or seven days no very marked diminution of it ensues, the hair is to be shaved from the whole surface of the swelling, and widely around its base: a puncture a quarter of an inch long is to be made by a lancet at a depending point of it, equable pressure to be applied by the fingers to expel as much of the contents as possible, and afterwards general pressure by a compress, the strapping and bandages to be maintained undisturbed for about six days. During this time attention must be directed to the patient and the tumour, so that the effects of the pressure, or the supervention of inflammation, &c., may be known.

In the same journal, Dr. Riecke details three cases of abscess occurring in the mastoid process, from the effects of exposure to cold during stormy weather in the early part of the same year. He throws out rather the whimsical hint, as it appears to us, that as the atmosphere at certain times is often loaded with "ozon," operating very excitingly upon the

* In the new edition of Bouchut's work (Maladies des Nouveaux-Nés), which we have just received, we find the following statement:—"M. Morel exhibited to the Society of Biology a case of cephalhæmatoma, in which the 'bourrelet osseux' existed only upon one side of the tumour." In default of further information, we assume that this is explainable by Professor Levy's views regarding *maturity*.

respiratory passages, so a forcible draught of air rushing into the ear, charged with an irritating agent, may produce peculiar effects, such indeed as the production of the three cases he brings forward. Dr. Riecke, amongst other points he touches on, recommends the bichloride of mercury in acute hydrocephalus, and the application of the actual cautery in obstinate cases of umbilical hæmorrhage in the new born-child. We meet also with some observations on pertussis by Trousseau, by whom the affection is defined as a "specific and infectious bronchitis," and its treatment by emetics of the sulphate of copper, and afterwards by the powdered root of belladonna, recommended. The clinical experience of Mauthner von Mauthstein is so extensive, and his remarks always so practical in their character, that our readers may be glad to be informed that several pages of the journal are indebted to his pen. They are occupied by the subjects of laryngismus, eczema, and the effects of blood-letting. The dependence of the former upon abnormal states of the thymus gland is held to be negatived by experience, the disease being caused by a variety of circumstances, including epidemic influences, the form of the affection prevailing when Dr. Mauthner wrote, being symptomatic of a croupose catarrhal malady of the air-passages. In by far the generality of cases, the prognosis is favourable. In the less complicated forms, counter-irritation by croton oil, with the use of the tincture of rhubarb and the carbonate of soda, or the tincture of the malate of iron or cod-liver oil, with particular regard to the general hygiène of the child, is the most recommendable treatment. The author, although admitting his ordinary aversion to general bloodletting, notices those forms of pneumonia in which it is not only advisable to enforce it, but often absolutely necessary to do so to save the child.

We can do no more on this occasion than simply record the publication of the third, and, we assume, the concluding, portion of Dr. Bednar's treatise. The former two divisions of it (reviewed in vol. vii. p. 110, and vol. x. p. 1) led us to augur highly of its character when completed; and now we can safely say that it constitutes one of the best expositions of the morbid anatomy of children extant. It is rather more a book for reference than for continuous reading; but as the former, it is quite indispensable to any desirous of becoming acquainted with the detail of the important subject to which it relates.

W. H. Willshire.

Review XI.

Military Surgery; or, Experience of Field Practice in India during the years 1848 *and* 1849. By J. J. Cole, M.R.C.S.E., H.E.I.C.S., late Surgeon to the Auxiliary Forces during the war in the Punjaub.—*London*, 1852. 8vo, pp. 223.

The practice of the army-surgeon on the line of march or upon the field of battle consists in the adaptation of the established principles of surgery to the character of surrounding circumstances, and not in any material deviation from the laws laid down for the treatment of injuries in civil life. Consequently we find, without surprise, that a young surgeon carrying to India no other advantages than those afforded by the education at so excellent a school of instruction as Guy's Hospital, is enabled to perform military duties of a peculiarly onerous and responsible nature with efficiency, judgment, and courage. He has not yet become a surgical martinet; he sees things with an eye uninfluenced by military routine, and he does not conceal ignorance by sheltering himself in the incomprehensibilities of "malingering."

Mr. Cole found himself, seven months after his arrival in India (November, 1847), in medical charge of Lieutenant Edwardes' army (18,000 strong), then encamped before Moultan, into which it had driven the rebel Moolraj and his troops. The wounded in the battles of Kuneyreh and Suddozam, in various skirmishes in the sieges of Moultan, during six days' cannonade from the Moolraj's guns, in the battle of Soorajkoond, in the siege of Luckie, passed through his hands. His health, which suffered from the hardships of the campaign, prevented his recording the cases which fell under his care so fully as he might otherwise have done; but no one can rise from the perusal of the volume without the conviction that it contains a faithful account of the scenes witnessed, and that the author exhibited no small amount of determination.

The style is somewhat dogmatical, and the language occasionally hard; a list of prescriptions might, with propriety, have been omitted; and the unqualified disapproval of the use of chloroform must be condemned. Mr. Cole has no right to "draw the attention of subordinate medical officers" (p. 89) to anæsthetics as highly pernicious agents, when they are now employed by most surgeons in civilized countries, after machinery or railway accidents quite as severe as those inflicted by missiles discharged by the explosion of gunpowder. No man, it is true, would administer chloroform to a patient in a state of extreme collapse; but that is no reason why the bottle should be "*left with the medical storekeeper, or be placed on a high shelf in his warehouse with the stopper from it to keep it cool.*" (p. 89.) Every medical man in the public services should be provided with chloroform as fresh and as pure as possible, for there is many a case in which an operation performed without pain will save the life of a patient unable to sustain the shock of more suffering without dying.

We could have wished a full account of the construction of hospitals, and of the different means adopted to keep this singular force under Lieutenant Edwardes' command in health; but the work is purely surgical, and treats only of injuries to the soldier upon the field of battle. Speaking of the effects of a cannon-ball, he says:

"For the first five or six hundred yards it grinds to powder, and destroys everything. If the wounded person be a European, the skin looks more or less livid; if an Indian, blacker than natural. The tissues are torn through, and hang down in shreds; small portions of the muscles, veins, arteries, and other structures, come away when you pinch them with the finger and thumb, and the exposed surface of the truncated bone is almost ground to powder. . . . . These wounds are not, however, as fatal as the injury the same shot is able to produce when it has travelled four or five hundred yards further, or when, perhaps, it has nearly reached its goal; then it is that it occasions the most severe lesions which can befal the extremities of man. It tears its way, apparently with some little difficulty, through the structures, and hangs in the wound long enough to impart its destructive influence to the adjacent tissues. The integuments are extensively lacerated above and below; the muscles are dreadfully torn, and separated from their attachments, and their interspaces are filled with coagulated blood. Although it is true," continues the author, "that many gun-shot wounds do not bleed at all, yet it is better to view all wounds of this nature as hæmorrhagic. The surgeon sees the patient with the body cold, and the pulse scarcely perceptible; but let him go to the spot where the round shot strikes him, and the bleeding is found sufficient to call forth every skill." (pp. 6—9.)

Upon this point public attention has been especially directed by Mr. Guthrie. Wounds of large veins are deemed more dangerous than those of arteries.

We trust that for the future it will not be necessary even to refute the nonsense about the injurious effects of air put in motion by the cannon-ball.

Mr. Cole is an advocate for immediate amputation, but he does not indicate the precise period for performing the operation so clearly as does Mr. Guthrie in his work on Gun-shot Wounds, nor does he point out well the danger attending immediate collapse. "During the course of the Peninsular war, the success of amputations," says Mr. Guthrie, "performed on the field of battle became so notorious, even among the soldiery, that the anxiety expressed by them to have these operations executed with as little delay as possible, has frequently become prejudicial, the sufferer not having had time to recover from the shock of the injury and to approach even in some degree the state of health."—(*On Gun-shot Wounds.*)

Mr. Cole recommends that gun-shot wounds of the chest should not be closed, but rather left open for the escape of fluids, in opposition to the course of treatment suggested by both Larrey and Hennen. He also thinks it right to remove a small portion of the rib with a Hey's saw, to secure an intercostal artery, when the hæmorrhage cannot be restrained by the usual means. The surgeon who would undertake this operation must be a bold man; and it is worthy of remark, that such a measure is never thought of in ordinary cases of fractured ribs, when the vessels here spoken of must often be torn. Fatal hæmorrhage into the cavity of the chest would in all probability be from some other source; either from the vena azygos, or from the large vessels about the heart, of which accident there are cases upon record. Hennen observes, that in some irritable habits, where the bony covering is removed, the hæmorrhage from the intercostal arteries is much more profuse than could be supposed from their size; yet he adds, "I have never met with a case requiring the tenaculum." Plenck, it is true, carried a needle round the rib of an injured intercostal artery, and putting a tent under it, tightened the ligature so as to compress the vessel; Theden, the Prussian surgeon-general, found the plan, upon trial, followed by fatal consequences, as any reasonable man might have anticipated.

We are glad to see that no undue stress is laid on putting the patient upon the injured side; a piece of advice often impracticable and useless.

An European who surveyed, for the first time, the small-handled, apparently ill-balanced, but sharp-edged sword of the Sikh or the Belooch, would scarcely fancy it capable of inflicting such severe wounds in the hands of the experienced warrior as those recorded by all observers. The British soldier soon ceased to regard with ridicule the small dark-coloured, yet active native, who, under the combined influence of religious enthusiasm and of opium, danced along the line alone, muttering some chaunt, wounding those whom he could reach, and prepared to die upon the field. The weapon is not used for striking, and but rarely for thrusting; it is drawn across the object with such address, that it cuts down to the bone even in such a well-covered limb as the thigh. Mr. Cole relates the case of a soldier who was most effectually scalped, "a portion of the integument, three inches by two and a half, being clean cut off." (Case lxxx.) In other instances the bone was divided. In case lxxxii., the sword in its course "cut off the ala of the right nostril, shaved the malar bone, severed the lobe of the right ear, grazed the ramus of the jaw exposing the teeth, turned into the neck, and terminated in front at the sternal end of the clavicle, behind at the spinous processes of the cervical vertebræ, laying three of them bare." In the treatment of such wounds all morbid fear respecting the use of sutures must be set aside—the edges must be effectually approximated till lymph is effused.

We think some of Mr. Cole's methods of treatment severe. In wounds of the tongue attended by bleeding, he recommends—

"Pinch the tongue above the wound with a flat pair of forceps until the bleeding ceases; quickly sponge out the wound, dry it, and apply nitrate of silver freely over its whole surface: now allow the tongue to return into the mouth, to be again drawn out and treated as before, if hæmorrhage returns. A ligature will occasionally suffice, especially when the wound is near the lip. Often, however, neither ligature, nitrate of silver, nor any other styptic, will succeed; nothing but the actual cautery—the red-hot iron itself—will do!" (p. 160.)

The tongue goes no further back than the os hyoides, which can be easily drawn forwards to the front of the mouth; we have witnessed the tying of cut arteries of the tongue without difficulty, after the extirpation of one-half of the organ. Dangerous hæmorrhage from such wounds is rare.

Again, in speaking of opening the temporal artery in the treatment of iritis, why should *caustic* be applied to the wound? Such a proceeding is both unnecessary and painful.

The author approves of the application of a ligature to the femoral artery after wound to the popliteal (p. 168); and he has in his possession, no doubt, some successful cases in support of the plan here approved. He does not, however, clearly state whether the treatment has been positively satisfactory in his hand—a point of much practical interest now that great importance is attached to the *fiat* that surgeons should in all cases cut down to the wounded artery at the spot injured, and put a ligature above and below the aperture. That such, when readily within our power, is the proper course of treatment, no one can for a moment doubt; but it might be a question whether amputation would not be preferable to the operation of putting a ligature upon the popliteal in a limb much bruised by the passage of a bullet, with torn muscles, with parts displaced and disfigured by extravasated blood, and when the immediate seat and absolute extent of the injury were doubtful. A well-authenticated case, to show that under such circumstances ligature of the femoral may be successful, would be, at the present moment, valuable and acceptable.

The work shows talent, and is both truthful and original. Mr. Cole evidently had the welfare of the soldier at heart. The reader will find in it much that is interesting and useful, and the blemishes can be easily rectified in a second edition.

Holmes Coote.

Review XII.

Mémoires de l'Académie Nationale de Médecine. Tome XVI. pp. 818.—*Paris*, 1852.

Of the great part which Academical Transactions have played in the advancement of medical knowledge, there can be but one opinion, and the beneficial tendency of their operation is as great now as at any period of their history. For, if at an early stage of this they formed the principal mode of intercommunication of learned Europe, at the present time they rescue many a meritorious production from the whirlpool of periodical medical literature, which, while it swallows up all within its reach, by its very multiplicity renders the escape from notice easy, and the process of search difficult. In the Transactions of Academies and Societies, the author's production appears with the dignity of an *imprimatur* (sometimes too easily granted, it must be admitted), unsurrounded by the heterogeneous mass of matter which encumbers most of our modern periodicals, and in a form that is comparatively easy of reference, and certain of preservation.

Among these Transactions, those of the French Academy of Medicine scarcely, in our opinion, holds so high a place as might have been expected from the official publication of so renowned a body; and certainly, whether viewed in relation to their practical utility, scientific value, or the important improvements they have heralded to the world, are considerably inferior to those of our Medico-Chirurgical Society. It is to be observed, however, that besides its Transactions, the Academy publishes an official *Bulletin* of its weekly meetings, in which detailed and corrected reports of all papers read, and of the reports and discussions these give rise to, are contained. The reports, indeed, usually confided to men of great eminence in the branch of knowledge to which the communication relates, are often of high value and interest; and it has not unfrequently happened that papers of very poor pretensions have given rise to most admirable reports and animated discussions, in which the leading men have taken their full share. This is a practice we think might be beneficially imitated in this country. At present we have at the Royal Society reports without discussions, and at the Medico-Chirurgical Society discussions without reports. The consequence is, that while in the former body the grounds of the report are insufficiently sifted, in the latter the discussion is often of the most trivial and conversational character, if indeed it can be called discussion at all. What else, indeed, could be expected from *extempore* remarks on a paper read in an abbreviated form, and often only imperfectly heard. If, however, it were confided to the leisurely consideration of a well qualified reporter, its merits and demerits might be thoroughly examined in relation to the present state of knowledge upon the subject. The conclusions the reporter came to would form a subject of profitable discussion, in which the most eminent members of the Society would probably join, especially if their consideration were adjourned, as is often the case at Paris, to the meeting after that at which they had been stated. The inducement to join in such discussion, and the utility derivable from it, would be much enhanced if the Society published its *Bulletin*, like the French Academy.

The present volume of the Memoirs is a good average one, and we proceed, without further preamble, to place an account of its contents before our readers. The introductory portion, consisting of some historical particulars concerning the celebrated Paris Academy of Surgery, and its no less celebrated secretary, Anthony Louis, and an *éloge* upon Richerand, from the pen of M. Dubois (d'Amiens), the secretary, need not detain us farther than to remark that if the mode in which the French signalize the departure of their great men is somewhat too theatrical, we, for our parts, allow ours to go from amongst us with too much indifference.

1. The first paper is a report, by M. Gaultier de Claubry, upon the "*Epidemics which prevailed in France during* 1848 *and* 1849." In noticing M. Gaultier's former reports upon this subject (vol. ix. p. 51), we described an admirable provision made in France by the appointment of certain medical officers, called "Physicians for Epidemics," whose duty consisted in watching, and reporting upon, the different epidemic visitations that may occur. Like many other institutions, however, this seems to look better on paper than to work in reality; for from these medical officers, scattered all over France, only thirteen reports, relating to nine departments, were received in 1848, and but twenty-two in 1849. The political circumstances of the country in the former year may perhaps account for the paucity; and in the latter year M. Gaultier was precluded from reporting on the cholera, for the consideration of all facts relating to which a special commission, with M. Guerin as reporter, has been appointed. There is little to require our notice in either of the two reports. *Typhoid fever*, as is always the case in France, constituted by far the most prevalent disease, and predisposing causes of the most opposite kinds are assigned by the physician-reporters. M. Poggioli, an army surgeon, gives an interesting account of an epidemic of the so-called *cerebro-spinal meningitis* which prevailed in the garrison of St. Etienne (Loire) from June to October, 1848. The garrison consisted of two squadrons of dragoons, all old soldiers, of 1100 men of the 13th regiment of light infantry, almost all being old Algerian soldiers, and of a similar number of the 22nd regiment, composed chiefly of new conscripts. This last regiment was well lodged and fed, and under very mild discipline. The barracks of the other two regiments were in far less favourable hygienic conditions. Nevertheless the 22nd had 107 men seized, of whom 32 died; and when no new cases appeared, then came the turn of the 13th regiment, in which, however, five only were attacked, of whom two died. Among the dragoons only one case occurred, which proved fatal. Between the 11th June and 3rd October, of 25 patients, 23 died. M. Poggioli was now called in, and instituted free cupping at the nucha, rubbing in over the scarifications muriate of morphia, with extract of belladonna and mercurial ointment. Employed at the commencement, this treatment seemed to cut short the disease; but if the case had made some progress, it failed to do so, and active antiphlogistic and revulsive treat-

ment was resorted to. Tincture of cantharides was administered internally. After this treatment was put into force the alarming mortality much diminished. The etiology of the disease, on this occasion, was just as obscure as on so many others. Epidemics of *dysentery* were remarkably prevalent in 1849, occurring from August to October, after the excessive heats of a burning summer; hygienic conditions seeming to have exerted no appreciable influence on the production, duration, or extension of the epidemics.

2. *M. Bouchardat on the Nature and Treatment of Diabetes Mellitus, or Glucosuria.*—This paper gives a connected account of M. Bouchardat's researches upon diabetes, which have now extended over twenty years, and have been reported from time to time in his 'Annuaire de Thérapeutique.' With a tenacity not unusual in science, he holds firmly to his own opinions, and appears scarcely able fully to recognise the facts noted by others. In spite of the numerous observations which are opposed to his view, and which, to say the least, prove it to be incomplete and partial, Bouchardat arranges his opinions with as much confidence as if their accuracy were not almost universally denied. However, we will allow him to speak for himself, and to present in one connected view his matured creed.

Bouchardat still believes that the normal digestion of starchy substances is the starting point in this discussion. Diabetic patients do not digest these as persons in health do, and they suffer from thirst in proportion to the amount of farinaceous matter they consume. The quantity of fluid requisite to assuage thirst is exactly equal to that required to aid the action of *diastase* on starch. In the stomach of a healthy man, two or three hours after a meal, very little glucose will be found; in the stomach of a diabetic patient at the same time, a large quantity is present. In persons in health, aliments for the most part pass into the small intestines before they undergo solution, and even there this process is far less rapid than in diabetic patients. In the latter, diastase is secreted in the stomach, and feculent aliments are, through the intermedium of the water which their thirst compels them to drink, by it converted into glucose, which is absorbed by the numerous venous ramifications, and transported into the blood, to be excreted in the urine. This rapid absorption causes a feeling of emptiness, which induces the patient to take more food, and with no effect, as regards his nutrition, if it still consists of farinaceous substances. M. Bouchardat does not consider that the discovery of sugar in the liver by Bernard, at all militates against these views; believing, in spite of the careful observations of Lehmann on the blood of the vena portæ and hepatic vein, that the liver does not generate the sugar, but retains it in its substance, whencesoever derived, to pour it into the blood. When the food employed can only furnish very little sugar, as in the case of meat (inosite), only the small quantity retained by the liver is to be found, it being no longer detectable in the blood of even diabetic patients. When, on the other hand, feculent food is given in glucosuria, the moderating power of the liver no longer suffices, and large masses of sugar are poured into the circulation, to be eliminated by the kidneys—experiment having shown that such elimination always takes place when a larger quantity than 30 grains are present in the blood.

The diminution of temperature by 1° or 2° Cent., always observed in glucosuria, and probably due to the refrigerating effects of cold drinks, and to the waste of caloric in converting starch into glucose, doubtless is one of the causes which render the destruction of the glucose in the blood slow and incomplete. A diminution of the alkalescence of the blood would have a similar operation; and a knowledge of the fact has led in certain cases of the disease to the beneficial administration of alkalies. The theory of M. Mialhe, however, which explains glucosuria exclusively by a vicious assimilation, due to the diminished alkalescence of the economy, is completely at fault. M. Bouchardat has frequently noted remarkable aberrations of the nervous system in diabetic patients; but he is by no means disposed to conclude, from the induction of sugar in the urine by Bernard's experiment of irritating the floor of the fourth ventricle, that this system exerts a primary influence in the genesis of glucosuria. The presence of sugar may, in fact, be determined by various influences, and it is possible that this irritation of the fourth ventricle operates through the diminution of temperature it gives rise to. The diabetic patient, deprived of sugar or feculents, no longer produces glucose; while the urine of an animal, similarly deprived, in whom this experiment is performed, exhibits it. Moreover, the amaurosis met with in diabetic patients, disappears under suitable hygienic measures, without any special action upon the nervous system being resorted to. Still it is to be remembered that in bad cases of glucosuria a decided affection of the *medulla spinalis*, attended with paraplegia, exists; but although, in some cases, this possibly may be a primary cause of the disease, in others it is inadmissible. Indeed, in M. Bouchardat's opinion, multiple causes may give rise to glucose, and any theory founded on an exclusive one is defective. His own statement that feculent substances are differently digested by the diabetic, and that the thirst and amount of glucose found in the urine are proportionate to the quantity of these consumed, is merely, he says, the result of observations that every one may verify; and he considers that it is an injudicious step to call upon him to enter upon the difficult question of why diabetic patients digest differently to persons in health. Nevertheless, he offers the following explanation.

"The most active ferment for the digestion of feculent aliments is the diastase found in the pancreatic juice; but almost all the other digestive fluids contain an albuminoid principle, which, in lesser degree, possesses a similar solvent action, that under the influence of different causes may become augmented in power. Thus, while in the dog and the pigeon the pancreatic juice is almost exclusively charged with the solution of feculent bodies, if we tie the duct of the pancreas in the one animal, and extirpate the gland in the other, the albuminoid matters of the gastric and intestinal juices gradually assume the same solvent powers as the diastase of the pancreatic juice possessed. If, in the case of diabetes, we assume atrophy or organic disease of the pancreas, the albuminoid matter of the gastric juice would gradually assume the solvent power, and the digestion of feculents be achieved in the stomach in place of the intestines." In answer to this hypothesis, it may be stated, that while in some cases obvious changes in the pancreas and ducts have been observed, these have not been seen in the great majority. Again, in some patients there may exist a narrowness of the pyloric orifice, and a diminished activity in the contractile fibres of the stomach, which permits the dilatation of its cavity.

and M. Bouchardat has always found the stomach much more developed in glucosuria than in health. Moreover, such persons almost always have an excessive liking for bread and other feculent aliments. The stomach, thus habituated to this long delay of so much feculent aliment, acquires in the modifications of its juices the power of dissolving it. This hypothesis explains the intense thirst, inasmuch as the feculent aliments require for their digestion from seven to ten times their weight of water, which the gastric juice is quite unable to furnish; as it also explains why, during the suspension of digestion in the dying, or in those suffering from fever, the sugar may disappear from the urine of the diabetic.

Treatment of Diabetes.—By the plan which M. Bouchardat now recommends to our notice, in its full detail, he declares that he can cure the majority of cases of diabetes—his test of cure being not only present removal of the sugar from the urine, but the ability of the patient to employ fæculent aliments without its reproduction. He. however, requires the intelligent co-operation of his patient, and, above all, the frequent testing of the urine, by the patient himself,* as a means of ascertaining progress and guarding against relapse. The means chiefly to be relied upon are those of a hygienic character; and, at all events, the power of these should be exclusively ascertained at first, before resorting to any medicinal agents.

1. *Diet.* As long as the urine exhibits sugar, all feculent and saccharine aliments must be entirely excluded; but the patient need not be confined to what is called an exclusively flesh diet, although this, when not repugnant to him, is the best. Every description of meat, dressed with the usual sauces and seasonings, (to the exclusion of flour, however,) may be employed; and for those who can get over the prejudice against it, the flesh of *carnivorous* animals, M. Bouchardat says, is best. By proper management (and what cannot a French cook do?) that of the cat or fox becomes a highly relishing viand. Several poor patients, who otherwise would have been unable to procure a flesh diet, have resorted to this means with advantage. Fish, in all its numerous varieties, forms a valuable resource for both rich and poor, and may be eaten with abundance of oil and a moderate quantity of vinegar. Eggs, again, so susceptible of varied modes of preparation, are excellent; and although milk is forbidden, good fresh cream and all kinds of cheese are allowed. Except in extreme cases, green vegetables and salads, although they contain some sugar, starch, or gum, may be taken in moderate quantities; but abundance of oil, or the yolk of eggs, should be conjoined. For such patients who cannot well overcome their liking for bread and other feculents, M. Bouchardat has, during the last ten years, had prepared a bread of flour containing 70 per cent. of gluten.

As the prohibited feculent and saccharine bodies belong to the respiratory group of alimentary substances, we have to choose others from the same group; and those best calculated to supply their places are fatty bodies and alcoholic drinks. Among the latter, Bordeaux wine occupies a prominent place, as much as from one to two *litres* (from two to four pints) being admissible *per diem*, which, at 10 per cent. of alcohol, would supply about 150 *grammes* ($2\frac{3}{4}$ oz.) of this substance in the 24 hours. Fatty bodies

* For indicating the presence of sugar in the urine, he employs lime water, and believes it to be a safer test than the copper solution; for the quantitative determination he employs exclusively Biot's polarizing apparatus.

must not be given too exclusively, lest they excite disgust, but mingled with other aliments, from 150 to 200 *grammes* being required, in addition to the alcohol. Beer is objectionable, from containing dextrine. Coffee, drunk without milk or sugar, and to which a little rum, cream, or brandy may be added, is a good drink. To relieve thirst, Seltzer, Spa, Vichy, or soda water may be taken; but acid drinks, so keenly desired by the patients, are very objectionable. The patient should always eat and drink in moderate quantities, slowly masticating his food. This practice tends to the relief of the attendant dyspepsia, and to assist the distended stomach to return to its normal dimensions. A flannel bandage applied around the epigastrium contributes to the same end.

2. *Clothing.*—As chills operate very injuriously on these patients, warm flannel clothing forms a valuable protective agent, and beneficially excites the languishing functions of the skin. Indeed, some medicinal agents are of no avail unless aided by complete flannel clothing, which maintains diaphoresis. General frictions are very useful, and a moist warmth of the feet should be maintained.

3. *Exercise.*—To recommend this indiscriminately would be injudicious, for many patients are too feeble to undertake it. But when their strength has become somewhat recruited by regimen, walking, gymnastics, agricultural labour, &c., much expedite the cure, and are found, as recovery is approaching, to enable the feculent aliments to become utilized by the system.

4. *Pharmaceutical Agents.*—M. Bouchardat entertains a high opinion of the utility of *carbonate of ammonia* (from 5 to 15 *grammes*—77 to 230 grains—in the 24 hours), providing flannel clothing be worn. Other *alkalies* suffice for slighter cases, when the urine contains uric acid as well as glucose. Employed consentaneously with out-of-door exercise, they seem to exert great influence in preventing the reappearance of sugar in the urine, when feculent aliments are resumed. *Opiates*, if given alone, are mere palliatives; but when conjoined with other remedies, and in moderate doses, so as to act on the skin, they are very valuable. M. Bouchardat sometimes prescribes Dover's powder, but prefers the old theriaca before all other preparations, without defending the absurd complexity of its composition.

In severe cases of glucosuria, then, diet, exercise, and flannel clothing constitute the basis of treatment, carbonate of ammonia and opiates best aiding their action. Other remedies have their occasional uses, such as iron, tonics, chloride of sodium, and antiscorbutic plants. M. Bouchardat often employs emetics at the commencement, and endeavours to modify the disturbed functions of the liver by aperients, of which, ox-gall with rhubarb are the best.

Circumstances influencing the effects of Treatment.—Foremost among the *favourable* indications in a case is the *rapid return of the urine to a normal state*, which may take place in from 24 to 48 hours after the feculents have been excluded. The *recent date of the affection* is another highly favourable circumstance; and because it is so, M. Bouchardat urges testing the urine whenever the slightest suspicion can be held, and for the detection of relapses, which are frequent and insidious. Other favourable circumstances are the retention of considerable *embonpoint*, the easy circumstances of the patient, and his being in possession of great perseverance.

The *unfavourable* circumstances are the reverse of the above; but negligence is still worse than poverty, as the poor man has some resources. The treatment of the case is usually ill managed in *hospitals*, owing to the vitiated air, the absence of exercise, the sameness of diet, and the insufficiency of surveillance. The existence of a great *appetite* is a common and not unfavourable circumstance, requiring only moderation in its gratification, at meals not too far separated. *Want of appetite* is a far more unfavourable sign, which should be actively combated. M. Bouchardat has found small doses of rhubarb, and exercise in the open air, of advantage. *Obstinate constipation*, resisting the most varied purgatives, is a bad complication, indicating disease of long duration, which has produced important modifications in the condition of the alimentary canal. Fatty substances, combined with matters which leave residue, as spinach and gluten-bread with bran, are here indicated. *Cold and damp air* is unfavourable to diabetic patients; but M. Bouchardat has had patients from Algeria, and has not derived advantage from sending others to Italy. M. Bouchardat agrees with Dr. Prout in considering the appearance of *albumen* in the urine, which is often met with, as an unfavourable occurrence. The prognosis of saccharine albuminuria is not so serious as is that of simple chronic albuminuria. The frequency of the occurrence of *phthisis* in cases of glucosuria is familiarly known. In all the autopsies the author has made, when the patient has not been cut off by an intercurrent affection, tubercles have been found in the lungs; and he feels convinced that many cases of phthisis have had their origin in a glucosuria that has been overlooked, and which might have easily been removed. In severe and old cases of glucosuria, *vision* is always found more or less enfeebled; but in most cases, when not of old date, as the condition of the patient has improved under appropriate regimen, this amaurosis has subsided. When, indeed, this is not the case, the prognosis of the glucosuria is serious; and it will often be found complicated with albuminuria. *Impotence*, more or less decided, is another effect of glucosuria; but in young subjects the generative functions resume their power when the original disease is rationally treated. Glucosuria may occur at any *age*, from infancy to senility; M. Bouchardat having met with most cases between forty and fifty. He has met with none between eighteen and twenty-five. Old age does not constitute an obstacle to cure; but so difficult is it to watch over children, that the author is not aware of a sustained cure prior to fifteen years of age. He has met with more male than female patients.

The two next memoirs do not call for notice. One of them consists in the minute relation of a successful case of "Amputation at the Hip-joint" for exostosis of the femur, by M. Henot; and the other is an essay on "Cervical Adenitis," as observed in the French military hospitals, by M. H. Larrey, a summary of which we furnished (vol. vi. 260), when the paper was reported upon to the Academy by M. Grisolle.

The fifth memoir is from the able pen of M. Gosselin, and is entitled, "*Researches on the Synovial Cysts of the Hand and Wrist.*" In it he relates the results of his anatomical researches, made in the attempt at clearing up some of the confusion and contradictions that have prevailed in the description of these tumours. Confounded formerly under the general name of *ganglion*, these swellings have been, of late, distinguished by most writers accordingly as they have occupied the track of the flexor tendons of the palm, or the extensors at the back of the wrists. The distinction is, indeed, an essential one, for in the former case (*dropsical cysts*), an entire synovial bursa is involved, while in the latter (*partial or ganglionary cysts*), a very circumscribed portion of the synovial membrane is concerned. The object of this paper is to determine the anatomical limits and connexions of the first of these, and the mode of formation of the second.

1. *Dropsical Cysts of the Palm.*—Authors have varied much in the descriptions they have given of the direction and number of the synovial membranes accompanying the flexor tendons; and the dissection of more than sixty hands, in both children and adults, enables M. Gosselin to account for this discrepancy by the varieties of distribution which actually prevail. Amidst these there is, however, a regular disposition observed in the majority of cases, and met with in the fœtus, children, and women not accustomed to manual labour. There are two synovial membranes, which, taking their origin a little above the annular ligament, are continued, the one to the second phalanx of the thumb, and the other to the second phalanx of the little finger. The first of these is reflected from the level of the carpus around the tendon of the flexor proprius of the thumb. The internal one is reflected on to the tendons of the flexors of the little and ring fingers, extending to a much less length along the latter. The tendons of the index and medius fingers are unprovided with these bursæ, and may be separated without opening either the outer or inner synovial cavity. Normally, these two cavities do not communicate, but an occasional variety is met with in which they do. A more frequent variety consists in the presence of a supernumerary sac, placed between the two others, above the annular ligament, and not unfrequently communicating with one of them, especially the internal. In the course of the prolongation of the internal cavity it normally undergoes a narrowing at about midway of its course, and a not unfrequent variety is found in its becoming obliterated at this point. As a fourth variety may be noticed the presence of small, isolated, synovial sheaths, distinct from the others, formed from the cellular tissue of the superficial tendons. Thus, normally, there are two synovial cavities in this locality; and when authors have described more, they have been alluding to some of these varieties. We see also why, as a result of chronic inflammation, the little and ring fingers are those especially liable to become contracted.

2. *Ganglionary or Partial Synovial Cysts.*—M. Gosselin has nothing new to communicate respecting the symptoms or treatment of these ganglions, so often met with at the back of the wrist, his object being merely to draw attention to their mode of origin. After adverting to the different explanations of this, hazarded by prior writers, he states that he has been led to offer a new one, by having observed, in a great number of radio-carpal joints he has examined, immediately beneath the synovial membrane, behind the semilunar and scaphoid bones, certain whitish or greyish corpuscles, varying in size from that of a millet-seed to that of a pea. Some of these slightly project into the articulation, but, in most cases, concealed in the sub-synovial cellular tissue, they are only brought into view by the careful removal of the synovial membrane, to which they somewhat firmly adhere. Usually they do not communicate with the joints, no orifice being discernible; and their contents, which resemble

those of ganglions, only issue out upon incision. These *sub-synovial* bodies are also met with in the medio-carpal joints, but much more rarely. Whether viewed as to their *locale* or their contents, these bodies must be regarded as exhibiting the earliest stage of ganglionary formations; and the frequency of their existence, and regularity of their position, lead to the inference that they are modifications of a natural disposition. Additional researches have brought M. Gosselin to the conclusion, that all the articular synovial membranes (and especially those of the wrist) are provided with prolongations, or *culs de sac*, which may be called *synoviparous crypts, or follicles*, and that the obliteration of the orifice of these crypts gives rise to the accumulation of synovia. The crypts are, indeed, but one of the means employed for extending the synovial secreting surface; and in some instances they communicate with the general cavity by broad, and in others by very minute, openings, the latter being liable to this obstruction. Velpeau and the Webers have, indeed, adverted to these synovial prolongations, but they have not generalized their descriptions, or made the pathological application. These crypts are to be found in all the principal articulations; and in the horse these follicles and their communicating orifices are found on a much larger scale. Taking this general view of them, the conclusion becomes irresistible, that these depressions of the synovial membranes are means for the enlargement of surface analogous to the single follicles of the skin and mucous membranes. The reason why the orifices of these follicles of the wrist-joint should so especially become obliterated, and give rise to ganglions, is not to be ascertained any more than that of the greater frequency of sebaceous cysts in certain regions of the skin. The sub-synovial bodies thus produced may remain stationary, their outward progress being opposed by the aponeuroses which surround them; or they may increase towards the cavity of the joint, into which they may at last burst. If the aponeurotic resistance is less decided, or in part destroyed, the bodies make their way towards the surface under the form of *ganglions*. M. Gosselin has been unable to observe any of the synoviparous follicles in the sheaths and bursæ of the tendons; and it is therefore a highly important fact, in a practical point of view, that the ganglions are in communication, not with these sheaths, but with the joint itself, into which, indeed, they sometimes, at an advanced period, discharge themselves.

The next essay is an important practical paper by M. Hutin, chief surgeon to the Invalides, upon the "*Necessity of extracting foreign bodies and splinters, in the treatment of Gun-shot Wounds.*" In a celebrated discussion which took place at the Academy after the events of June, 1848 (Brit. & For. Med.-Chir. Rev., ii. 261, 546), great discrepancy of opinion prevailed among distinguished surgeons as to the rule of practice to be followed in this emergency. The present paper, based upon its author's fourteen years' experience in Algerian warfare, and five years' observation of the results of treatment among more than 4000 inhabitants of the Invalides, is an energetic protest against the temporizing practice advocated by some surgeons. It scarcely calls for or admits of analysis; and we may content ourselves with saying, that the advocates for the prompt removal of foreign bodies will find in M. Hutin a most able coadjutor, speaking with great authority, in consequence of the opportunity he has had at the Invalides, of contemplating the results of the most opposite lines of practice. In relation to this subject we cannot refrain from noticing the fact, that while by the events of 1848 surgical science received illustration on several important points, those of 1851 have not been allowed to contribute to this end. No cases, statistical results, or clinical lectures, have appeared in any of the medical journals; and a reader relying upon them alone for information, would remain in absolute ignorance that occurrences attended with so much suffering and slaughter had ever taken place. Repression has invaded even the realms of science.

The last memoir in the present volume is a prize essay by M. Broca, Prosector of the Faculty of Medicine, having for its subject the "*Pathological Anatomy of Cancer*," and occupying nearly 400 pages. Of so lengthened a production, a minute analysis with the space we have at command is out of the question; and we will only attempt to furnish an outline of the conclusions the researches of M. Broca have led him to arrive at; and especially of such of them as differ somewhat from those of other observers. While entertaining a very high opinion of the value of the revelations of the microscope, he believes that real practical utility can only be attained by conjoining laborious clinical research with its employment; and to the neglect of such association he attributes the errors and reveries of certain of the German microscopists.

M. Broca considers that the *lactescent juice* which every cancerous tumour yields to pressure or scraping, sufficiently characterizes the disease, in the great bulk of cases, even to the naked eye. Any tumour yielding no juice at all, or only a *serous* juice, may be at once set down as non-cancerous. (?) It is true, lactescency may result from other causes, as purulent, lacteal, and tubercular infiltration, &c., and in certain of these cases may require the intervention of the microscope to distinguish the nuclei and nucleated cells, which constitute the essential feature of cancer. The microscopic appearances are given in full detail; but with these the writings of Professor Bennett (whose researches receive from M. Broca well-deserved acknowledgment) and others, have rendered our readers familiar; and we will only quote one of the concluding passages of the description.

"It is thus seen how varied are the forms and mode of existence of cells in cancer. We find in cancer, and in it alone, every disposition which the elementary vesicles are capable of assuming in organized beings. Every imaginable type, every possible mode of production and multiplication—isolated development, endogenous generation, fissiparity, are found combinedly or separately in this tissue." (p. 482.)

M. Broca first considers cancer as a tumour in the abstract, and then as a disease affecting various parts of the economy. Its principal forms are next described, the paper terminating with an account of the characteristics of *pseudo-cancer*. In treating of the *local evolution* of *cancer*, he asserts that Vogel's statement that fibres are first organized in the blastema in which cancer-cells are afterwards deposited, must have arisen from his having mistaken for cancerous deposits the lymph which is effused in the immediate vicinity of the tumour, as a consequence of the irritation it excites. In early cancer M. Broca has usually found a large proportion of free nuclei, while the nucleated cells that are present are very much smaller than they are at a more advanced stage of the disease. He totally denies the possibility of healthy tissue undergoing *degeneration* or *transformation* into cancerous, *substitution* supervening upon atrophy (as in the so-called fatty degeneration of muscle) being really the change which does take place. In like manner he cannot admit that a homœomorphous or innocent growth is ever converted into a heteromorphous or malignant one. He points out the utter absence of evidence of such change, that can bear the scrutiny of modern means of investigation. A supposed homœomorphous growth must never be removed on the pretext that it may degenerate. It has the same, but no more, risk of becoming a seat of the deposit of cancer, as any of the normal tissues. Cancer invariably *increases;* and cases, in which a *partial* diminution of the size of the tumour takes place, must be regarded as examples of resorption of homœomorphous elements, which have been deposited amidst the heteromorphous elements. As to the *entire disappearance* of a true cancerous growth, not only can no clinical fact of sufficient certainty be adduced in its favour; but the results of histological investigation do not encourage the admission of the possibility. M. Broca bears testimony to the faithfulness of Bennett's description of the fatty and granular alteration sometimes observed in the cells; but he cannot admit that these should be regarded as abortive or retrograde cells, or as tending to show the possibility of the spontaneous cure of cancer.

The increased deposit of cancer-cells is attended with the *propagation* of the tumour at its periphery. Its progress is for a while delayed by a greyish or yellowish infiltration of lymph, which surrounds the tumour; but this is invaded by the increasing deposit, which gradually insinuates itself into the interstices of surrounding tissues, compressing and causing the atrophy of these, and eventually occupying their places—not by transformation, but substitution. The looser and the more cellular the tissue, the more easily does the new deposit gain admission, while where but few interstices prevail, the slower is the progress of the cancer in that direction. As the cancer advances, too, its substance undergoes *ramollissement*, which M. Broca looks upon as a true phasis of its evolution, and sign of the increased activity of the specific element of the disease, and not as an internal gangrene with Hodgkin and Carswell, or a process of decay with Walshe. This real *ramollissement*, which affects a tumour at first hard, is to be distinguished from the *apparent* ramollissement due to the deposit of certain portions of cancer which are soft or diffluent from the beginning.

The *vascularity* of cancer is usually proportioned to its softness, being slight in schirrus and considerable in encephaloid; proportioned, in fact, to the amount of the specific elements of the disease, the nuclei and cells. The circulation presents great analogy to the cerebral circulation, arteries, and veins, ramifying upon the surface, while capillaries penetrate the substance. The middle coat of *arteries* may long resist the progress of cancer, the compression caused by which may induce partial obliteration of their calibre. Where this is not the case, extensive external hæmorrhage may follow the successful invasion of the disease, or an apoplectic effusion may take place into the substance of the tumour—the effused blood undergoing changes very similar to those observed when it is effused in normal structures. Sometimes such effusions are multiple and intercommunicate, the blood not coagulating, and receiving an impulse from adjoining vessels. This is often the case in encephaloid disease of bone, and has been mistaken for *erectile tumours* of that tissue. As far as M. Broca's experience has gone, all such so-called erectile tumours of bone are really examples of this *hæmatode cancer*. The thin and unresisting structure of the walls of *veins* soon yield to the invasion of cancer, but their contents are not effused into the substance of the cancer. Veins of small size may become quite obliterated, but those of larger calibre are either opened into or plugged up with a cancerous deposit, more or less of which may become detached, and gain admission into the circulation. *Inflammation* of cancer is of far seldomer occurrence than from its richness in capillaries might have been expected; and inflammation occurring around the cancer has often been confounded with that of its substance. It is an error to consider *ramollissement* of cancer as one of its results. Several of the alleged cases of *suppuration* of cancer will not bear examination, that quoted by Walshe from Levêque-Lasource among them. But rare as such cases are, M. Broca has met with an indubitable one occurring in cancer of the lungs. He has also had three cases of partial *gangrene* of cancer, and has met with about thirty others recorded, the great bulk being examples of encephaloid.

Treating of cancer as a disease, M. Broca states he has only met with three cases in which the *primary tumour* was not a *solitary* one, and M. Lebert has communicated another to him. Other examples are recorded, but the cancerous nature of the disease was not ascertained by the microscope. In all cases, however, these simultaneous tumours have existed in the *same* organ. The progress of primary encephaloid cancer is sometimes considerably delayed by its becoming surrounded by a kind of cyst, produced by the hypertrophy of the surrounding cellular tissue. M. Broca only admits the propagation of cancer by direct continuity, and is quite unaware of the grounds upon which Walshe assigns simple proximity as a mode of propagation. The cancers which become successively developed in the lymphatic glands are due to the transport of cancerous matter, which has obtained admission by the destruction of the walls of the lymphatics.

Proceeding from the local manifestations of cancer, M. Broca passes on to the consideration of the state of the economy in which *cancerous infection* manifests itself, and before examining into the nature of this, he details the changes in the fluids and solids by which it is characterized. We will pass over this portion as containing nothing novel. The author denies altogether the alleged antagonism of tubercle and cancer. Under the influence of cancerous infection, multiple cancers, termed by M. Broca *cancers of infection*, are produced in various organs, most of them being at more or less distance from the primary centre, and unconnected with it by direct communication. These act just as so many primary centres, propagating themselves around, and increasing the intensity of the infection to which they are due. Most of these cancers assume the encephaloid form.

M. Broca distinguishes the two very different conditions which precede and follow the appearance of the primary cancer, applying to the first the appellation *cancerous diathesis*, and to the other, *cancerous infection.* The diathesis does not directly produce the infection, but it gives rise to the primary tumour, which at a later period poisons the economy. The tumour is, so to say, the bond of union between the diathesis and the general infection. Of what constitutes this modification of the economy which he terms the cancerous diathesis, M. Broca professes his complete ignorance; but he strongly opposes the hypothesis of Carswell, that the blood is primarily affected, as unfounded on any fact whatever. Even the relation which the primary tumour bears to the cancerous infection is at present obscure; but M. Broca considers the balance of evidence is in favour of the view which regards the infection as resulting from the transport of cancerous matter into the circulation, by means of the opened veins, and in exceptional cases, the lymphatics. Rejecting the metastatic theory, he contents himself at present with noting the facts connected with the appearance of the secondary or infection cancers, and acknowledges that he is as ignorant of the mechanism of their production as of that of the production of the pustules of variola, or the eruption of typhoid fever. M. Broca adds some illustrations to the melancholy chapter of *relapse* of *cancer;* which he regards as of nearly constant occurrence. Among the patients operated upon by Blandin in 1847 and 1848, there were 69 who furnished tumours, most of which, prior to the employment of the microscope, would have been regarded as cancerous. On examination, 2 proved to be fibro-colloid, 5 fibro-plastic, 15 epithelial, and 6 partial mammary hypertrophy. Of 39 really cancerous patients, 11 died from the consequences of the operation, and 28 survived these. Of these last, 19 were kept in view, and every one of them had relapse, 16 within the first year, 2 in the course of the second, and the last at the end of the twenty-fifth month. By the beginning of 1850, 17 were dead, and the other two were expected soon to follow them. In spite of so discouraging a result, M. Broca believes the *operation* should always be resorted to, whether for primary or relapsed cancer, providing general infection of the economy has not taken place. Sources of this infection are thus removed, and its production delayed. By those who confound the diathesis with the infection, the operation should, of course, be always interdicted.

John Chatto.

Review XIII.

On Rheumatism, Rheumatic Gout, and Sciatica: their Pathology, Symptoms, and Treatment. By Henry William Fuller, M.D. Cantab., Assistant-Physician to St. George's Hospital.—*London*, 1852. 8vo, pp. 403.

When we consider how large is the number of its victims, how intense and protracted the sufferings it occasions during its continuance, and what subsequent evils it entails from the cardiac mischief which on its subsidence it so often leaves behind, we cannot but feel convinced that the disease described as rheumatism is one of the most formidable affections to which the human body is liable. When we consider, too, that all this goes on from month to month and year to year, at one season almost as much as at another, and apparently but little influenced by the epidemics and vicissitudes by which most of the other prevalent forms of disease are characterized, we can well understand why so many zealous inquirers have entered the field against this formidable enemy, with the view of discovering its nature, disarming it of its power, and reducing the number of its victims. And yet what can we really be said to know of the true pathology of this disease at the present time, after all the efforts which have been directed towards its elucidation. We certainly know that it occurs at all ages, but chiefly in the young; that it attacks the strong and robust scarcely less often than the weakly and debilitated; that it manifests itself by great febrile excitement attended with severe inflammation of a peculiar kind in the parts about the joints, and very often with serious cardiac disease; and that these phenomena are accompanied by profuse acid secretion from the skin, by the separation of large quantities of uric acid through the kidneys, and by a highly fibrinous condition of the blood. But as to the essential cause of these and other manifestations of the disease, we really know nothing with certainty, and the only approximation to truth we can be said to have attained, is, that these various phenomena are probably indicative of or dependent upon some, as yet undiscovered, morbid condition of the blood.

Among the more recent labourers in this wide and important field of investigation must now be ranked Dr. Fuller, the author of the work at the head of the present article. We purpose taking a general survey of the principal contents of this work, briefly alluding to such points as seem to present anything particularly novel or instructive, and entering somewhat more fully into one or two of the more important topics discussed in it.

An introductory chapter of twenty-eight pages is occupied with a consideration of the probable cause and nature of rheumatism. The author offers sufficient reasons against the opinion that mere exposure to cold and damp is of itself enough to induce an attack of the disease: yet, we think, he scarcely ascribes as much weight to the agency of this supposed cause as the history of many cases seems fairly to justify. For although it be true that the mere exposure to low temperature combined with moisture is not the essential, yet in a large majority of cases it seems clearly to be the exciting, cause, acting on a system already predisposed to the disease. The real predisposing cause is ascribed by Dr. Fuller, in common with many other modern pathologists, to some morbid condition of the blood. In support of this view he observes, very justly, that—

> "If certain substances are introduced into the circulation, fever is set up, rigors often occur, and inflammatory symptoms very shortly supervene in various parts of the body: symptoms which vary in intensity and locality, according to the amount and character of the poison. And if the blood be altered in character, it is practically the same, whether it contain matters foreign to the system, and altogether morbid in kind, or whether it contain an excess only of some material, a certain amount of which is compatible with health. In either case it is unhealthy and poisonous in its nature: in either case it contains a *materies morbi*, which may not only produce fever, or the symptoms of general derangement, but if irritating in its nature, may give rise to local inflammatory symptoms."

It is by no means clear, as yet, on what the morbid condition of the blood in rheumatism really depends. Dr. Fuller believes it to be due to the existence of some poisonous principle probably "generated in the system as a product of mal-assimilation," and seeming to bear a "close identity with some natural excretion of the skin." Accordingly he entertains the opinion held by Dr. Prout, Dr. Todd, and others, that this poisonous principle, the actual *materies morbi*, is lactic acid, retained in the blood instead of being eliminated by the skin. No facts, however, are given in direct support of this view of the real nature of the morbid principle in the blood, and we must be considered as still in the dark respecting a question which bears so importantly on the pathology of rheumatism. All, however, will doubtless agree with Dr. Fuller, that whatever may be the morbid material or materials, on the existence of which in the blood the phenomena of rheumatism probably depend, they are generated within the system, and not absorbed from without; that they constitute a peculiar or specific poison, which is of the same general nature in every case; and that the development of this poison may be called into play by anything which exercises a prolonged depressing influence upon the system, especially if at the same time there be an hereditary tendency to the disease. When the system is thus deranged, and the

"Rheumatic poison is present in it, any disturbing circumstance, even of temporary duration, such as over-fatigue, anxiety, grief, or anger, by rendering the system more susceptible of its influence, may prove the accidental or exciting cause of the disease, and exposure to cold or to atmospheric vicissitudes is almost certain to induce an attack."

Chapter II. is occupied chiefly with a consideration of the hereditary character of the disease, the author stating that he had distinctly traced the hereditary taint in 71 out of 246 cases of rheumatism admitted into St. George's Hospital: (nearly 29 per cent.) With regard to the influence of age and sex on the development of the disease, there is nothing strikingly new in the author's remarks. Most pathologists will concur in his observation, that

"Those persons are naturally the chief sufferers who, through want and privations, irregularity of life, and neglect of their general health, are rendered most liable to that state of mal-assimilation whereby the *materies morbi* is produced; and who, again, from the nature of their occupations, are most exposed to atmospheric vicissitudes, and to other exciting causes of the disease."

Chapter III. consists of a brief inquiry into the localities and textures of the body which are the seat of pain and inflammation in rheumatism. Although the fibrous and fibro-serous structures are those on which the stress of the disease mainly falls, yet since the disease is one of the blood, which circulates to all parts, so, the author thinks, "all must be more or less liable to suffer."

From a good general account of the symptoms of acute rheumatism in Chapter IV. we quote the following, as an important passage:

"One of the most remarkable and suggestive facts in regard to rheumatism is, that the fever and constitutional distress are not [always] commensurate with the extent and intensity of the local symptoms. Not only is rheumatic inflammation of the joints very frequently *preceded* by febrile disturbance, but sometimes the fever runs so high before any local symptoms have been established, as to cause even cautious and intelligent practitioners to mistake the nature of the impending attack. Moreover, when febrile symptoms do thus precede the establishment of local inflammation, they are not only not increased by its occurrence, but, as was remarked by the sagacious and observant Sydenham, they are very generally relieved, the pulse becoming calmer, the countenance less anxious, and the patient altogether easier."

Chapter V., on the treatment of acute rheumatism, is one of the best in the book. After sketching in outline the several plans recommended by writers on the subject, and drawing the comparative merits of the different remedies usually employed, the author describes the course which he himself has been led to adopt. His observations on this subject are prefaced by remarks so appropriate in regard to the assumed efficacy of particular remedies in this disease, that we are induced to quote them. After remarking that the advocates of each remedy have founded their opinion of its efficacy on the rapidity with which the symptoms disappear after its employment, he continues, with justice,—

"But as, when uncomplicated by cardiac affection, the disease usually terminates, sooner or later, in recovery, and sometimes subsides with marvellous rapidity under every variety of remedy, it is obvious, that no sound inference can be drawn as to the success of any particular method of treatment, unless such treatment has been largely adopted, and has been attended with tolerably uniform results. And I am sure I may say, without fear of contradiction, that each and every plan of treatment which has been hitherto proposed is regarded by the profession as unsatisfactory. If in one person's hands any particular remedial course has proved efficient, it has signally failed in those of another: if at one time a remedy has proved efficacious, it has been found inert or injurious at another, under different circumstances of age, sex, constitution, and the like. Nor does this appear strange to those who consider the true nature of the disorder, and the variety of circumstances under which the physician may be called upon to minister to his patient's relief. The bleeding, which in the young, plethoric, and robust, may be necessary to allay excessive vascular action and cause free secretion, may, in the weakly, induce irritability of the heart, and a consequent attack of cardiac inflammation. The opium, which in one person may prove of the greatest service in promoting free perspiration, and in allaying the general irritability of the system, may, in another, check the biliary and other secretions, and thus prevent the elimination of the rheumatic poison. The continued use of calomel, and the constant purging, which may be beneficial to one patient, by removing large quantities of unhealthy secretions, may unnecessarily exhaust the strength of another, and tend very greatly to impede recovery. And so in regard to every remedy which has been proposed: what is useful at one time, proves useless or positively injurious at another."

The author then passes in review the principal measures hitherto recommended for the cure of rheumatism. Against the general abstraction of blood as a part of ordinary practice in this disease, he is strongly opposed, believing, with most physicians in this metropolis, that although venesection may be called for in some cases in which young robust persons are seized with a first and severe attack, yet even then it should be cautiously, and in other cases very rarely, employed, since even if the abstraction of blood does not tend to engender cardiac mischief, "still, convalescence is retarded, and the patient weakened and rendered liable to frequent relapses.' Although he speaks highly of the efficacy of freely clearing out the alimentary canal, especially as it is often loaded with dark unhealthy secretions, yet, he expresses "dissent from the practice of repeated active purging;" and this for three reasons:

"First, because it is not necessary to the cure of the patient, and, like bleeding, tends greatly to reduce his strength, and protract recovery. Secondly, because, from the nature of the complaint, the patient is quite incapable of moving, and his sufferings are aggravated, his irritability is increased, and his heart's action accelerated, by the repeated shifting of his position, which is rendered necessary by the calls of nature. And thirdly, because it necessarily gives rise to more or less exposure, which must be prejudicial to a person bathed in perspiration."

To keep up a free action of the bowels without undue purgation, he recommends calomel, with a full dose of opium, at night, and followed in the morning, if necessary, with a draught of rhubarb or senna with colchicum and potassio-tartrate of soda. The other main remedies in common employment are fully considered, including opium, mercury, antimony, colchicum, nitre, lemon-juice, and alkalies. But as there is nothing particularly novel in his views respecting the effects of these several remedies, we will pass over his remarks, merely observing that his experience does not enable him to speak highly either of nitrate of potash or lemon-juice, when employed alone; though of the value of alkalies and their salts generally, he speaks in the warmest terms. To this favourable opinion of the value of alkaline remedies, the profession generally is beginning to yield assent, especially as the opinion gains ground, that rheumatism is dependent on a morbid, probably acidulous, state of the blood; and that, in consequence, there is a preternatural tendency to the deposition of fibrine, which is counteracted by the free introduction of alkalies, by means of which the solubility of the fibrine is increased.

The general plan of treatment which Dr. Fuller advocates does not materially differ from that commonly practised, consisting principally of the free use of alkalies, combined with colchicum, calomel, and opium. Besides these internal measures, however, he employs, apparently with very marked advantage, the application of warm alkaline and opiate fomentations to alleviate the pain of articular inflammation. The solution he commonly uses for this purpose is composed of an ounce of carbonate of potash dissolved in a pint either of decoction of poppies, or of rose-water to which six drachms of Battley's solution is added; and he speaks most highly of its efficacy. "In every instance in which it has been employed, the relief obtained has been almost immediate, and the pain and inflammation have subsided rapidly."

The treatment of rheumatic disease of the heart is so closely mixed up with that of the articular inflammation, that they can scarcely be well considered apart. Therefore, although the author has dedicated a separate and distant chapter (chap. viii.) to this subject, we will take the liberty of appending to the remarks just made, the few observations suggested to us by the perusal of this chapter. Dr. Fuller strongly deprecates the free employment of mercury in acute rheumatism, preparatory to the onset of cardiac symptoms, and with the view of warding them off; for he observes, that when this remedy is given "so as to affect the constitution before the commencement of cardiac inflammation, it not only has no influence in preventing the disease, but by the irritability and general depression which it occasions, appears to modify its course in a manner by no means conducive to recovery." When the cardiac mischief, however, has fairly set in, he advocates, with others, the advantage of free mercurial action, for the purpose of arresting the morbid process, and absorbing the materials deposited by it. Although, therefore, it may be fully conceded, that to push the influence of mercury to the verge of salivation previous to the development of cardiac symptoms would be objectionable, yet the cautious employment of this drug, especially in such cases where the occurrence of cardiac complication may justly be feared, with the view of gaining a certain hold upon the system, and so of saving time when the internal mischief fairly manifests itself, must, we think, be regarded as a salutary and judicious procedure. Combined with the use of mercury, the author advocates the employment of local depletion, effected by leeches rather than by cupping, as a less painful and distressing method of abstracting blood from the neighbourhood of the affected part. In other respects, the author's views on the mode of treatment of rheumatic inflammation of the heart, do not materially differ from those generally entertained.

Chapter VI. is dedicated to a consideration of the causes of rheumatic affection of the heart. The author makes it quite plain, that the cardiac affection cannot be due to a mere metastasis of the disease from the joints to the heart, but must be regarded as another mode of manifestation of the same morbid condition of the blood; for it not unfrequently happens that the heart is affected before the joints, while the disease sometimes appears to be limited almost entirely to this organ. He is of opinion that the occurrence of cardiac mischief is indicative of a severe form of the disease, rarely accompanying the sub-acute variety; and that it is most prone to arise in those cases in which the heart is either naturally irritable, or rendered so by previous active treatment or general debility.

In this chapter the author has almost necessarily had to notice that very important subject, the probable mode of origin of the fibrinous concretions, or warty vegetations, so frequently found adhering to the valves of the heart in fatal cases of rheumatic fever. Until recently, these warty growths have been almost universally ascribed to inflammatory exudation from the tissue of the affected valve; but latterly the opinion has been gaining ground, that they consist principally, if not exclusively, of fibrine deposited directly from the blood. This view, which is strongly maintained by Dr. Fuller, was brought very prominently before the profession, in 1850, by Mr. Simon, in his very excellent series of lectures on general pathology. Among the reasons which Mr. Simon there offers in support of this view are, first, the improbability of the lining membrane of the circulating apparatus becoming inflamed at all, since it contains no bloodvessels, and probably derives its nutriment from the stream of blood directly in contact with its inner surface. Yet, as observed by Dr. Ormerod, "this objection is more specious than real; for inflammation may be carried on by the same organic means as nutrition. And if, in any part, nutrition by unusual means effect an ordinary end, surely inflammation in the same part may do so likewise."* And it may be further urged, that even although the opinion, that the lining membrane of the circulating system depends, both for its ordinary nutrition and its morbid changes, on the blood in contact with its interior, and not on that in the vasa vasorum, be admitted to apply to the membrane lining the bloodvessels and general cavities of the heart, which seldom present evidences of disease, it can scarcely be supposed to hold good in

* Gulstonian Lectures, 1851.

the case of the valves; for both artificial injection and the vascularity induced by disease equally demonstrate the existence of a very free interstitial supply of blood to the textures both of the semilunar and auriculo-ventricular valves. And since it is on these parts that fibrinous deposits almost exclusively occur, and on which various unmistakable evidences of inflammatory action, such as general vascularity, swelling, and occasionally ulceration, are observed, we should be cautious ere we entirely discard the opinion, that fibrinous vegetations are, to some extent at least, dependent on inflammatory exudation, though probably increased by subsequent deposition from the blood. Against this inflammatory origin of these vegetations there are, however, other very important objections, which Mr. Simon, in common with others, strongly urges: for example, the far greater frequency with which the left valves in comparison with the right are affected, and the very striking and almost constant fact, that the fibrinous deposits are observed only on that side of the valve in contact with the moving blood. If inflammation were the cause of the warty growths, the right valves might naturally be expected to be as much affected as the left, since both are supplied by the coronary arteries; and both sides of an affected valve should present the exudations in equal amount, or if there was any difference, the side next the moving blood ought perhaps to show a smaller amount, owing to the liability of the stream to wash the deposit from the surface as rapidly as it is formed. By referring the fibrinous growths to direct deposition from the blood, both of these facts are explained; the left valves, being in contact with arterial and more highly fibrinized blood than the right, are more likely to become coated over with fibrine, while the surface directly opposed to the sanguineous stream is more likely to receive the deposit than the opposite one. The greater tendency of arterial than of venous blood to deposit fibrine when in motion, was repeatedly made the subject of direct experiment by Mr. Simon, who passed a thread through a main artery and contiguous main vein, leaving it for some hours to cut the stream: on removal, the portion of thread in the artery was invariably found coated with warty vegetations of fibrine similar to those on the valves of the heart, while that in the vein was never thus affected.

Against this weight of evidence in favour of the opinion, that the vegetations so frequently found on the valves of the heart in connexion with articular rheumatism are the result rather of direct deposition of fibrine from the blood than of inflammatory exudation from the tissue of the valve, must, however, be still opposed the fact already mentioned, that certain changes are sometimes observed, which can only be ascribed to an inflammatory process; as, for example, the soft, swollen, and vascular state of the tissue of the valve, but most especially to occasional ulceration. And since each of these, though sometimes observed to be unaccompanied by attendant fibrinous growths, are yet often found combined with them, the safest and perhaps the most correct view we ought at present to take of the vegetations in question is, that they may, in some cases at least, have a double source, being partly and primarily derived from inflammatory exudation, and subsequently enlarged by the deposition of fresh particles of fibrine from the blood flowing over the thus roughened surface, the tendency to such deposition being at the same time highly increased in consequence of the blood being unduly charged with fibrine, and, perhaps from the existence of some acid within it, less able than usual to retain the fibrine in solution. Whether the opinion be correct, that in some, and if so in what proportion, of the cases, the fibrinous vegetations on the valves result exclusively from direct deposition of the fibrine of the blood, independently of any previous disease of the tissue of the valve, are questions which we are scarcely yet in a position to pronounce upon with certainty. Whatever view, however, he really held concerning the origin of the growths in question, there can be no doubt of the propriety of employing in the treatment of rheumatism those remedies which, such as the alkalies and neutral salts, especially those of potash, seem to possess the property of preventing the deposition of fibrine from the blood by increasing its solubility, and thus perhaps guard against any great increase of the vegetations, however they may have been formed in the first instance. Dr. Fuller briefly alludes to the possibility of the fibrinous deposits being occasionally detached from the valves; but he does not enter into the details which the admission of such a possibility almost necessarily entails. As this subject has been fully discussed by the writer of the present article, in the last volume of the 'Medico-Chirurgical Transactions,' it seems unnecessary to dwell upon it here.

Chapter VII. is a very important one, containing as it does an account of "Rheumatic Inflammation of the Heart, with its Pathological Effects, its Symptoms, Progress, and Terminations." The author treats, first, of inflammation of the pericardium, and after briefly alluding to the several products which may be effused from the surface of this inflamed membrane, and the mode in which they are subsequently disposed of, he says—"It has been suggested that the lymph effused in pericarditis is often thoroughly reabsorbed, so that the pericardium is restored to the condition of health, and perfect recovery takes place. This I do not believe to be the case."

Although thus doubting the possibility of complete removal of the effused lymph, yet in the next line he expresses the opinion, that "doubtless it may be reabsorbed to a very great extent:" one is therefore naturally tempted to ask—If absorption of lymph can go on to a certain extent, why, under favourable circumstances, should it not be supposed to continue until not merely a given quantity, but the whole of the effused lymph is taken up? Admitting the possibility of absorption at all, there is nothing to justify our assigning any limits to the extent to which it may go. Dr. Fuller does not enter at any length into the subject, adding little more than the expression of his opinion, that when a large quantity of lymph is poured out into the pericardium, "its complete absorption is next to impossible;" and that, therefore, as maintained also by others, "adhesion, more or less general, between the two layers of the pericardium is the most favourable issue we are justified in expecting, when pericarditis has been extensively diffused over the membrane." We think a more extended examination of this question would have been serviceable, especially since the opinion is now somewhat gaining ground, that general inflammation of the pericardium does not terminate in adhesion so frequently as it was thought to do, and that such a termination is not the most "favourable" one that nature could accomplish under the circumstances. In a paper on this subject, which the writer of the present notice published about three years ago,* he endeavoured to show

* Medical Gazette, 1850.

that pericardial adhesion was a rare event in comparison with the frequency of pericarditis; and subsequent observation has tended to strengthen the opinion then expressed. Many difficulties lie in the way of demonstrating the correctness of this opinion; and although much may be done by mere reasoning, yet it is only by repeated and careful observation that its truth can be established. Cases, however, have been observed in which death has occurred at a comparatively remote period after a pericardial friction-sound has been heard, and yet no adhesion of the pericardium has been found. This seems to prove that adhesion is not an invariable result of the effusion of inflammatory lymph into the pericardial sac. The favourableness of such a termination is rendered questionable by the cardiac symptoms noticed during life, and the enlargement of the heart found after death in some of the cases of adherent pericardium; while the absence of these effects in other cases cannot justly be considered to prove that the heart tolerates without resentment this crippling and hampering of its free movement; for in the majority of these latter cases, there may usually be found enough to explain the absence of evident signs of cardiac disease.

It may, indeed, be stated, as a general rule, to which we think there will be found but few, if any, real exceptions—that permanent adhesion of the pericardium, whether universal, or involving only a comparatively limited portion of the serous membrane, will, by impeding and embarrassing the action of the heart—which it cannot, in reason, be supposed not to do—naturally tend to induce the same hypertrophied condition of the muscular tissue of this organ that any other impediment to its free action, whether resulting from valvular imperfection, arterial disease, or such a morbid condition of the blood as exists in Bright's disease, is universally admitted to be capable of producing. In any one of the cases, the amount of hypertrophy observed may be determined in some measure by the comparative demand made upon the action of the heart. If, from favourable circumstances, this organ be kept comparatively quiet, the degree of hypertrophy induced may reasonably be expected to be less than when this tranquil state has not been maintained. So, too, the amount of hypertrophy discovered after death, may naturally be supposed to bear some relation to the state of general nutrition of the muscular and other tissues in the rest of the body: for if the body generally is wasted and emaciated, either from simple inanition, or from any disease materially interfering with nutrition, such as cancer of the stomach or tubercular ulceration of the intestines, or the like, one cannot feel surprised if the heart, in common with other parts, should give evidence of imperfect nutrition of its muscular walls, especially when it is remembered that the enfeebled state of the body existing in such cases would furnish the least favourable circumstances for the development of hypertrophy, notwithstanding the existence of a formidable mechanical obstruction to the action of the heart. Therefore, it is scarcely consistent with common reasoning to conclude, that because in a given case of adherent pericardium the heart is not enlarged, therefore pericardial adhesion does not tend to produce impediment to the heart's action, and consequent hypertrophy of its walls to obviate and counteract this impediment: for, to make the conclusion at all exact, it ought to be shown in such case, that the patient had not lived under conditions of rest and comfort, in which all undue action of the heart had been scrupulously guarded against, or had not died under circumstances attended with general wasting of the muscular and other soft tissues of the body. And we venture to express the firm belief, that where conditions such as these have not existed, the heart which is trammelled with an adherent pericardium will, in almost every instance, be found associated with enlargement of the heart, and this, whether there be valvular disease or not.

In regard to this part of our subject, we have re-perused with much interest a paper recently written by Dr. W. T. Gairdner,* on what he terms the "favourable termination of pericarditis." Although the first and principal conclusion at which the author arrives is to the effect, that "a *considerable* majority of the cases of adherent pericardium is unattended by enlargement of the heart," yet a careful examination of the ten cases on which this conclusion is principally founded, convinces us that his fourth inference or conclusion, "that, in some circumstances at least, adhesion of the pericardium, originally uncomplicated, may lead to very great hypertrophy and dilatation of the heart," more nearly expresses the real truth than the first. For, by his own showing, all the ten cases in which an adherent pericardium was found unaccompanied by enlargement of the heart, proved fatal under conditions of more or less extreme general wasting and cachexia, which, according to the opinion we have just stated, would serve to explain the absence of the hypertrophy.

The remainder of this chapter is occupied with a lengthened account of the appearances presented, and subsequently undergone, by the fibrinous vegetations on the valves of the heart; and of the general and physical signs of rheumatic pericarditis and endocarditis, in which, however, we do not notice anything particularly new.

In Chapter IX. there are some useful statistics of "heart-disease in connexion with rheumatism," deduced principally from the analyses of 379 mixed cases of acute and subacute rheumatism treated in St. George's Hospital. Among the general results of this analysis, we find that the author agrees with the observations of Dr. Latham, M. Bouilland, and others, that some cardiac affection arises in about one-half of all cases of *acute* rheumatism; while of *subacute* cases, as noticed by the author, not more than about one-third are attacked with cardiac mischief. From an analysis of the recorded experience of Dr. Latham, Dr. Taylor, Dr. Macleod, and others, as well as from the author's own observations, he deduces that the proportion of cases in which pericarditis ensues in the course of acute rheumatism is as 1 in every 5·97; the proportion of endocarditis (or at least of endocardial murmur) is 1 in 2·25; while the proportion which recent pericardial bears to recent endocardial affection is as 1 to 2·9. In many of the cases of supposed endocarditis, however, he believes that the murmur on which the supposition of inflammation is founded, results either from purely functional causes, or from "temporary imperfect closure of the mitral orifice, consequent on irregular contraction of the structures connected with the valves, or by the presence of fibrine deposited on the valves, without the concurrence of endocardial inflammation;" and he is of opinion that

"Somewhat less than one-third of all recent cardiac murmurs met with among patients suffering from acute rheumatism, will be found to result from pericarditis,

* Edinburgh Monthly Journal, 1851.

and somewhat more than one-third from endocarditis, while the remainder will be referable to one of the three causes above specified, as contributing to the production of valvular murmur."

In Chapter X., which is occupied with "Affection of the Brain, Inflammation of the Lungs and Pleuræ, and Disorganization of the Joints," occasionally attendant upon rheumatism, the author develops in full the very probable opinion, that the singular cerebral and spinal symptoms which sometimes arise in this disease, are dependent, not upon organic affection of the nervous centres, but upon a morbid condition of the circulating blood producing irritation in the brain and spinal cord, previously disordered, or rendered unduly susceptible to morbid impressions.

The remaining three chapters of the book are devoted to Rheumatic Gout, Chronic Rheumatism, and Sciatica and other forms of Neuralgic Rheumatism; but into these we do not purpose entering.

Having given an analysis of some of the principal contents of this work, it only remains to state the impression which a careful perusal of its pages has left upon our mind—namely, that although there is nothing strikingly novel in the manner in which the author has dealt with his subject, or in the opinions set forward by him, yet the volume contains a large amount of valuable and instructive information, clearly and connectedly put together; and while it may be said to represent very fairly the extent and kind of our present knowledge concerning the symptoms and pathological nature of the important disease of which it treats, it furnishes us with sound and judicious views in regard to the most appropriate mode of dealing both with the rheumatism itself and the complications so apt to arise in its progress.

W. Senhouse Kirkes.

Review XIV.

Des Epidémies;—Thèse soutenue au Coucours pour une Chaire d'Hygiène, Fevr. 23, 1852. Par M. Marchal (de Calvi).

On Epidemics. By M. Marchal (de Calvi).—*Paris*, 1852. 4to, pp. 235.

There is probably no department of medicine which occupies a larger share of attention at the present time, with the general public as well as with the profession, than that which relates to the spread of epidemic diseases; and there is no inquiry which seems to us more likely to yield results of the highest and most extensive advantage. It is on this account, that although we are opposed on principle to the multiplication of associations for special objects, we have welcomed the establishment of the Epidemiological Society; as likely to direct into one channel a large amount of information, which is at present dispersed over a wide range, and to bring to bear upon it an intelligent scrutiny that shall select the points most worthy of consideration, and a power of philosophical combination that may draw forth some general principles, the attainment of which shall give a positive direction to further investigation, as well as afford a comprehensive expression of the facts already determined. We feel convinced that this is a subject on which mere *empirical* generalization will do very little. A vast mass of facts may be collected and tabulated—

the numerical method may be applied to these in every conceivable mode —and yet, so long as no *connecting idea* is discerned among them, they remain utterly incapable of serving for the establishment of those general principles, which enable true Science to predict with certainty what *will* or what *will not* occur in any given contingency, and which therefore furnish the only satisfactory basis for the rules of Art as to what *should* or *should not* be done. "Un fait qui n'emporte pas une idée, duquel on ne peut s'élever pour voir plus loin," says M. Marchal (quoting from Montesquieu), "est un caillou qui ne vaut pas la peine d'être ramassé, et qu'il faut, au contraire, repousser du pied pour en débarrasser la route !"

It is in this spirit that M. Marchal professes to have investigated the subject of epidemics. He has been desirous, he tells us in his preface, of establishing a doctrine, and of bringing all the elements of the discussion into convergence upon one single idea, which he believes to be novel, and on which he thinks that the practical rules of prophylaxis may be most securely based. From such a preliminary flourish of trumpets, we naturally expected something of greater value than the conclusions at which our author has arrived; which are nothing more than that plague, yellow-fever, cholera, and intermittent fevers of all types, are diseases of paludal or malarious origin; that typhus and epidemic typhoid fevers, dysentery, &c., are engendered by animal miasmata; and that mixed forms of disease may be produced by the combined action of these different influences. Our readers will recollect that a doctrine of this kind was long since put forth, and sustained with great ingenuity, in our own pages (vol. iii. p. 74, et seq.).

It is not our present purpose, however, to discuss the *origin* of the poisons, whose introduction into the system gives rise to the various forms of epidemic disease. We shall not enter upon the question of the relation of these diseases to each other, nor inquire why, in the very same localities, and under the identical circumstances, so far as can be traced, different forms of epidemic disease prevail at successive periods, typhoid fever giving place to typhus, typhus to cholera, cholera to scarlatina, and so on. The complete extermination of such diseases, by the annihilation of the causes from which they originally spring, may or may not be within the reach of medicine; but it will need a far larger amount of evidence than we at present possess, or are likely soon to attain, to enable us to determine with anything like certainty what these causes are, and what is man's power over them. There is, however, a far more ready method of greatly mitigating the severity of these epidemic visitations, if not of absolutely keeping them at bay; and this is, by bringing the mass of our population under conditions which shall destroy their liability to become the subjects of such diseases. This subject has been several times dwelt on in our pages; but it is one of such vast importance, that it can scarcely be pressed too often upon the attention both of the medical and general public; and we revert to it now, because we believe that we can bring forward a connecting idea, which is a legitimate generalization of a vast assemblage of facts, and which, if admitted will be found very fruitful both in practical and in scientific applications.

In every Etiological inquiry, it behoves us to set out with a definite idea of the meaning of the term *cause;* and to a vague and imperfect apprehension of the legitimate sense of this term, we trace much of the unsatisfactory character which appertains to medical inquiries generally. The true object and bearing of our inquiry is altogether lost sight of, in fact, unless we regard causation as consisting in *unconditional sequence*—that is, unless we recognise the *concurrence of a certain set of conditions as being essential to the certainty of the result, which invariably occurs when those conditions are fulfilled, and never occurs save when they are in operation.* The doctrine of causation in medicine cannot be different from that which holds good in every other science. It is only in appearance that the causes of disease, the action of remedies, &c., are uncertain. If we knew *all* the antecedent conditions that are present in each case, we should, doubtless, find the consequents as invariable as they are elsewhere. And it is only by the recognition of this principle, that we are led to the search for those more recondite conditions, which determine a marked difference of result, when, from the similarity of those which are more ostensible, we have been led to expect a repetition of that which we have before observed. It is a great mistake, as Mr. Mill has most clearly demonstrated,* to single out any one antecedent, and designate *it* as *the cause*, assigning to the rest a subordinate place as mere *conditions*. All are alike necessary to the result, if this invariably follows them when they are all present, and is no less invariably wanting when any one of them is withdrawn. Considered under this point of view, the "predisposing causes" are just as essential to the production of the resultant disease, as are those "exciting causes" on which our attention is usually more strongly fixed. The cholera-poison conveyed to our shores, whatever may be its mode of transport, owes its potentiality to the condition in which it finds the subjects of its invasion. If an average hundred of the inhabitants of our island were to receive the most powerful dose of cholera-poison that has ever been concentrated within it, probably not more than one in ten would be affected by the disease in its full intensity; the remaining nine-tenths either escaping altogether, or suffering only from diarrhœa of greater or less severity. In the ordinary phraseology of medicine, the individuals who suffer most are said to be *predisposed* to the disease; and collective observation has enabled the pathologist to specify, with considerable certainty, those antecedent circumstances, which, when present, act as predisposing causes, and the absence of which, on the other hand, effectually prevents the production of the disease.

It is remarkable, however, that *any one* of these predisposing causes, joined to the exciting cause (i. e. the presence of the cholera-poison), seems able to determine the result. Putrescent food, foul water, offensive effluvia, imperfect ventilation, &c., may operate singly and separately; or they may act in combination; and their separate action, if sufficiently intense, seems fully as capable as their combination in lower degrees, of giving potency to the specific poison. Hence it would at first sight appear difficult to apply to this subject the doctrine of invariable and unconditional antecedence; finding, as we do, that so many antecedents, apparently differing greatly in character, may each produce the result, when acting concurrently with one other condition. But if we look a little deeper than the surface, the question is naturally suggested to our minds, whether the really inva-

* Elements of Logic, book iii. chapter v.

riable antecedent be not *some condition of the human body,* which they all in common tend to produce, and which is that which the cholera-poison requires for its potential action. It is obvious that if any such condition can be distinctly specified, we come to possess a more satisfactory *rationale* of the *modus operandi* of the known predisposing causes of epidemic diseases, than we have yet obtained; we shall be able to predict their operation with greater certainty in new combinations; and we shall be guided in our search for others which are as yet hidden from our observation.

But further,—experience has shown that what is true of cholera, is true also of a large number of other diseases of the same class; the causes which engender a special liability to one, being equally efficacious in producing a similar liability to another: so that we have a strong presumption that the *cause* of the invasion of epidemic disease in each individual case, may be expressed by this simple formula,—a certain general condition of the human body, *plus* the specific poison of the particular disease. When these two antecedents are concurrently present, the disease is invariably produced; when either of them is wanting, the disease cannot be developed. And we may thus prevent its invasion no less effectually by preventing the occurrence of the predisposition, than by preventing the development of the poison itself. Now as the conditions of the former are for the most part both certainly known and readily within our reach, our first attention ought surely to be given to them; the other question, however, being not disregarded, although less capable of an immediate solution. Towards this solution, however, we shall have made an important step, if we can clearly determine that condition of the system which is essential to the operation of the morbid poisons; since we thereby gain a clue to the nature of that operation, which may be of great importance in leading us to a precise knowledge of it.

Now we believe that we shall be able to show, that all the recognised predisposing causes of epidemic disease, tend to produce, in the blood of the individuals exposed to them, an excess of those decomposing organic compounds, which, as physiology teaches us, are always present in the circulating current, in minute proportion; being conveyed by it, from the spots in which they are introduced, or in which they are generated, to the organs through which they are to be eliminated. Such an excess is manifestly producible, either by the direct introduction of these matters from without, in the food or drink consumed, or in the air respired; or by the production of them within the body, at a rate beyond that at which they are normally eliminated; or by some obstacle to their elimination, which prevents the amount ordinarily generated from escaping at its normal rate through the usual outlets. And as we shall find that to these three categories all such causes may be referred, we have very strong ground for the belief, that the condition of the blood to which we have alluded, is that which, in concurrence with the specific poison, affords the invariable antecedent of which we are in search.

Assuming, for the moment, the truth of this conclusion,—which we think we shall be able to substantiate to the satisfaction of our readers, by a subsequent examination of the *modus operandi* of each of the predisposing causes upon the living body,—let us stop to notice its bearing upon our notion of the *modus operandi* of the specific poison. All the epidemic diseases with which we are acquainted, belong to that class, which the term *Zymotic* (first suggested by Mr. W. Farr, in the Reports of the Registrar-General) has now come, by a sort of general consent, to be applied in this country. By some, it is true, this term has only been employed as a convenient designation for a group of diseases presenting a strong family resemblance one to another. But those who use the designation in the sense in which it was originally proposed, mean by it to imply, that the specific poisons of these diseases act on the blood in the manner of *ferments;* a doctrine which, vaguely recognised in our older pathology, has acquired precision within a comparatively recent period, from the researches of Liebig and others on the essential nature of ferments, and on the variety of modes in which they may operate in the animal body. A *ferment*, it is now clearly understood, is an azotized substance in a state of putrefactive alteration; the changes which are taking place among its components having the power of exciting changes of analogous character in certain other substances brought into contact with it, whereby new compounds are formed, into the composition of which the elements of the ferment do not enter. The influence of the ferment may be regarded, therefore, as not *material* but *dynamical;* consisting in the *propagation of force*, not in the introduction or substitution of components; and hence it is that ferments derive their extraordinary potency, extremely minute quantities of them being capable of effecting most important and extensive changes in fermentible substances brought within the sphere of their activity. The nature of the product depends upon (1) the nature of the ferment, (2) the nature of the fermentible substance, and (3) the stage of decomposition of the ferment; very minute differences in any one of these particulars being competent to determine most important variations in the result.

The substances on which ferments have a special action, may be ranged under two heads, the azotized, and the non-azotized. In the former, they tend to induce a change which enables them, in their turn, to act as ferments; whilst in the latter, they simply occasion that re-arrangement of their components in which fermentation consists, these substances having no power, even whilst themselves in the act of change, of inducing a similar change in others. Hence the potency of a ferment will depend, not merely upon the activity of the change which it is itself undergoing; but also upon the presence of some azotized substance in which it can excite the same change, and through which it can act upon other fermentible matter. Thus if a small quantity of yeast be introduced into a solution of pure sugar, the amount of fermentation which it will induce is very limited, and no reproduction of the ferment takes place; for the dynamical action of the yeast does not extend itself to the whole mass of the sugar, and completely ceases as soon as the yeast has itself gone through its entire transformation. But, on the other hand, if the yeast be introduced into a solution of malt, which contains not only sugar, but the albuminous constituent of the corn-grain, its main action is upon the latter, exciting in it an analogous change, whereby not only the whole sugar is made to undergo fermentation, but a reproduction of the ferment is effected, to many times the amount originally introduced.

Now this fact has been already applied by the advocates of the zymotic hypothesis, to the exanthemata and other zymotic diseases (such as hooping-cough) which usually occur only once in life. It has been supposed that the specific poisons of these diseases cannot act upon pure healthy blood, but that the presence of some special fermentible element is necessary to enable them to be developed within the system; and that when this has been changed by the agency of the ferment, and has been eliminated from the blood, that particular form of zymotic poison can no longer take effect. In this case it is further necessary to suppose, that the renewal of the liability, which occasionally manifests itself in a second attack of some one of these diseases, is consequent upon a reproduction of the fermentible matter. By some, this hypothesis has been rejected as altogether improbable, and as being entirely incapable of proof. By others, it has been provisionally adopted; the argument from analogy being considered sufficiently valid to justify its acceptance as a *possible* explanation of the facts of the case, and therefore as entitling it to hold its ground until displaced by a better one. We think that the cumulative evidence we shall adduce respecting the conditions under which zymotic diseases in general tend to develop themselves, adds great weight to this hypothesis, and affords to the zymotic theory generally a confirmation that seems, whilst stopping short of actual demonstration, adequate to substantiate its essential truth; since it is scarcely possible that the large number of coincidences which we shall presently point out, can be accounted for on any more satisfactory principle.

Our general proposition, then, when stated (so to speak) in the terms of the zymotic theory, amounts to this:—that all the recognised predisposing causes of zymotic disease tend to produce in the blood an undue accumulation of azotized matter, already in a state of *retrograde metamorphosis*, and therefore precisely in the condition in which it is most readily acted on by ferments; that the presence of such matter is absolutely requisite, in the great majority of cases, for the morbific action of the zymotic poison, which has no direct action upon healthy blood, all whose components are in a state of progressive metamorphosis; and that the liability of each individual, among a number who may be concurrently exposed to the same poison, will mainly depend upon the degree in which his blood may be charged with the matters in question.*

I. The first indication which we shall adduce, of the actual existence of this hypothetical entity, is that which first suggested the doctrine to our own minds; namely, that peculiar liability of the *puerperal* female to become the subject of zymotic disease, which affords the plainest evidence (if the zymotic theory have any truth whatever) of her peculiar susceptibility not merely for the reception, but for the further development, of poisons

* There are certain zymotic poisons, whose action is so powerful as to be able to exert itself on blood in a perfectly healthy state; such, for example, as the venom of serpents, the pus of pustule maligne, the sanies of glanders, the cadaveric matter of the subjects of particular diseases, the syphilitic poison, &c. But with regard to all save the first of these, there appears strong reason to believe that the potency of their effects is, in great measure, dependent on the previous condition of the system; the worst results being generally seen in those who have been subjected to the influence of some one or more of the predisposing causes to be presently enumerated. Thus, it is notorious that the dissecting-room poison is more virulent in its effects towards the *end* of the winter session, when the students exposed to it have been living for some months amidst putrescent effluvia, as well as (too frequently, at least,) in an atmosphere otherwise vitiated by want of ventilation, and have been impairing their health by over-study or by other irregularities which tend to impart to the degenerative processes an undue excess; than it is when the same set of individuals come fresh from the country, with their blood purified by active exercise and exposure to fresh air, and with their systems invigorated by wholesome refreshment and mental repose.

of the zymotic kind. Upon this point we apprehend that not the slightest question can exist, in the minds of any who have attended to the accumulation of evidence which now exists, with regard to the communicability of the puerperal infection, and with respect to the production of the various forms of puerperal fever by the introduction of some other *materies morbi* than that which might be considered peculiar to it. Thus, it is well known to be a fact of no unfrequent occurrence, that if a medical practitioner or a nurse go from a case of puerperal fever to attend other puerperal females, the chances are that these, too, will be attacked with the disease, although no epidemic of puerperal fever may exist at the time; and not only so, but even if such communicating individuals should abandon obstetric practice for weeks or even months, there is a great risk of the occurrence of this fearful disease in the very first puerperal patient with whom they may come into intimate relation. But further, if the antecedents to the *first* case of puerperal fever be strictly inquired into, it will very probably be found that the practitioner or nurse had come to the patient from attendance on a case of scarlatina, typhus, erysipelas, or small-pox; there being strong evidence that the specific poisons of the first three of these diseases, if not of the last, develop themselves in the form of puerperal fever, if introduced into the system of a parturient female. We cannot refer to a more "pregnant instance" of the peculiar receptivity of her system for zymotic poisons of even a non-specific description, than that which has been afforded by the experience of the Vienna Lying-In Hospital; in which, as our readers will remember,* a mortality of from 400 to 500, in an average of 3000 deliveries per annum, appears clearly traceable to the introduction of cadaveric matters, through the uncleanliness of the attending students; these matters being especially potent, when derived from the bodies of those who have died from the adynamic forms of zymotic disease. We learn from a communication made to Professor Murphy by Dr. Routh,† that the occurrence of puerperal fever in one of the Parisian lying-in hospitals, is traceable with equal certainty to the putrescent effluvia of a neighbouring abattoir; for, whilst the wind blows in such a direction as to waft these effluvia *from* the hospital, there is no disposition to puerperal fever; but the reversal of its direction, and a diffusion of the putrescent emanations through the hospital, is followed by an outbreak of that disease. There can be no doubt whatever, that a foul state of the atmosphere of the lying-in room, consequent upon deficiency in its ventilation, greatly favours the operation of the poison, if it be not actually capable of generating it; and we believe that there is adequate evidence, that the poison may be actually produced by the putrescent miasmata arising from deficient sewerage of a lying-in hospital.

We need not dwell upon the destructive ravages of this terrible disease, so fatal to those whom it attacks, and so pertinacious in its preference for the wards of a particular hospital, or the patients of a particular accoucheur. Our business is to inquire, what there is in the constitution of the puerperal female, which gives to the poison its special potency; and the answer to this question we find in one of the causes recently assigned by Dr. West‡

* British and Foreign Medico-Chirurgical Review, vol. v. pp. 273, 468.
† Medical Gazette, June 28, 1850, p. 1113.
‡ Medico-Chirurgical Transactions, vol. xxxiv. p. 75.

for the ill success of the Cæsarean section;—namely, the fact that the uterine tissue must be in a state of rapid disintegration; the evidence of which has been obtained by the observations of Virchow, Kilian, and Kölliker, as to the state of fatty degeneration of its muscular substance, and the presence of abundant fat globules in the lochial discharge; and (we may add) by the observations of Dr. Retzius (brother of the distinguished professor of anatomy at Stockholm) upon the abundance of fat globules in the urine of the puerperal female. We look upon this degeneration as only a peculiar case of that which is constantly taking place in muscular tissue as a consequence of its functional activity; for although we may not yet be in a condition to specify the precise nature of the chemical changes which occur in it, there is adequate evidence that these changes involve the ultimate production of two classes of *complementary* compounds; on the one hand, a *non-azotized* series, including sugar, fat, and lactic acid, which are destined to be eliminated by the respiratory process; and on the other, a *highly-azotized* series, of which creatine and urea are among those that are first traceable, and to which the kidney affords the appropriate channel of exit. After the mighty effort which the uterus has made in the expulsion of its contents, rapid disintegration of its substance occurs: and there can be no doubt that, as the *complement* of the fatty compounds which are generated more abundantly than they can be carried off by the respiratory process, there must also be produced an equivalent amount of highly-azotized compounds; such as, until they have assumed the comparatively permanent forms of urea and creatine, must be, like the azotized matter of sweet-wort, peculiarly susceptible of having their constitution entirely altered by the action of a ferment.

II. Now if this be true of the puerperal state, it ought to be true also of any other condition of the system, in which a rapid disintegration of tissue is going on; and we are naturally led to inquire, whether any peculiar receptivity for poisons of the kind we have alluded to, exists in those who are suffering under severe injuries, and whether in such individuals they develop themselves in a form of peculiar malignancy. We need not, we think, enter into any laboured argument to prove, that such a class of phenomena *is* presented by the peculiar liability of this class of subjects to a certain form of adynamic fever, which has hence received the special designation of "Surgical Fever," as well as to erysipelas. And our view of the case is most strikingly confirmed by the results of the inquiries of Professor Simpson; who has been led to conclude (1) that Surgical Fever is as much communicable from patient to patient by the hand of the surgeon, as Puerperal Fever is by that of the accoucheur; and (2) that a reciprocal relation exists between these two diseases, each being able to generate the other, so that they are to be regarded as the manifestations of one and the same *materies morbi*, their differences being dependent upon the peculiarities in the condition of the two classes of subjects.*

III. From the consideration of the cases in which the predisposition to

* See Professor Simpson's essay "On the Analogy between Puerperal and Surgical Fevers," in the Edinburgh Monthly Journal, vol. xi. p. 414; and the communications of Professor Simpson and Dr. Moir, to the Medico-Chirurgical Society of Edinburgh, in the Edinburgh Monthly Journal, vol. xiii. p. 72.

zymotic disease appears to be given by the existence of some peculiar source of degenerating, and therefore fermentible matter, *within* the living body, we pass on to those in which matter of this kind can be shown to have been introduced *ab extra*. Here, again, the evidence which experience affords in support of our position, is both cogent and abundant; for all who have watched the progress of epidemic diseases, are well aware that the habitual or even occasional ingestion of putrescent *food*, or of *water* charged with decomposing matter, or the inhalation of *air* loaded with miasmatic emanations, are among the most powerful of those "predisposing causes," by which the attack is determined in any individual case. Particular examples of this kind were abundantly furnished during the last visitation of cholera; and some of them are chronicled in the Report of the General Board of Health (pp. 63, 64). One instance, however, which has not yet found its way into print, we may especially notice. Our readers may recollect that just at the time when the decline of the epidemic in the Metropolis, and generally throughout the country, occasioned a "Thanksgiving Day" to be ordered by our Government, there was a remarkable and (so far as it extended) a very fatal outbreak of cholera at Bridgwater and Taunton. Whether the zymotic agency was or was not present in these two places in any peculiar intensity, we have scarcely sufficient means of determining; but this much is certain, that a considerable part of the severity of the attack was clearly attributable to the condition of the subjects of it; this condition having been induced, in a portion of them, by the ingestion of decomposing food, and, in another portion, by the respiration of an atmosphere deteriorated by over-crowding. The circumstances of the Bridgwater outbreak, as related to us by Dr. Brittan (who was sent down by the Board of Health to inquire into them), are as follows:—A cargo of oysters, spoiled by long detention in the Bristol Channel, had been brought to the town; and the sale of them having been prohibited by the authorities, on account of their putrescent condition, they were given away to any who would receive them; and several of the children in a neighbouring school partook of them plentifully. In the course of the following night, *all* the children (so far as Dr. Brittan could ascertain) who had partaken of the oysters, were attacked with cholera and choleraic diarrhœa; and eleven of them died the next day. Now, had the same occurrence happened, when no special zymotic poison was present, we apprehend that the mischievous effects of this unfortunate feast would have been confined to a simple diarrhœa, by which the noxious matter would have been carried out of the system. But a zymotic poison being present, this found in the decomposing matter the condition most favourable to its development; and it therefore fastened itself on the systems of those, in whose blood such matter was circulating.

The special cause which determined the severity of the Taunton outbreak, will come to be considered under another head; and we shall now only advert to another case, in which a peculiar liability to the attacks of zymotic disease seems to be engendered in an entire population, in consequence of their habitual ingestion of putrescent food. Referring our readers to our seventh volume (p. 419), for an analysis of Dr. Panum's 'Observations on an Epidemic of Measles in the Färoe Islands,' we may remind them that these islanders live during a large part of the year upon meat in a state of incipient decomposition; and acquire such a liking for the highest possible goût, that *rast*, or half decayed maggotty flesh, fowl, or fish, is commonly introduced as a special relish at the end of a meal. The result of such a diet (as might be anticipated) is a continual disorder of the digestive organs, manifesting itself especially by diarrhœa; and this is a symptom of annual occurrence on the bird islands, and is also invariably observed after a large "take" of whales, when much of the flesh of these animals necessarily becomes *rast* before it is consumed. Now this diarrhœa complicates the course of other diseases, and may even become, from its obstinacy and exhausting character, their most serious occurrence; as was particularly observed by Dr. Panum, in regard to the epidemic of measles, which he specially investigated. Notwithstanding this, the ordinary rate of mortality among these people is very low (only 1 in $64\frac{2}{3}$ annually), and a large proportion of the population live to a good old age; a circumstance which we attribute to their active, hardy lives, and their habitual exposure to a low external temperature; since both of these causes will tend to favour the elimination of the noxious matter as fast as it may be introduced, by promoting the activity of the respiration, which causes its speedy oxidation. If a population subsisting on the same diet, were transferred from the open air of the Färoe islands, to close ill-ventilated dwellings in some tropical country, we cannot doubt that the evil would be most fearfully aggravated; and even in the Färoe islands, although the general rate of mortality is so low, yet there is such an extraordinary liability to the spread of epidemic diseases, that when they *are* introduced (which is fortunately at rare intervals), they spread like wild-fire through the entire population. Thus, the epidemic of measles investigated by Dr. Panum, attacked in the course of six months scarcely less than 6000 out of a population of 7782; no age being spared, and very few individuals escaping, save such as had suffered from the malady in the epidemic which had occurred sixty-five years previously, and such as maintained a very rigorous isolation.

IV. That the habitual ingestion of decomposing matters in the water used as drink, is capable of inducing a similar liability to zymotic diseases, scarcely admits of a question. We extract the following from the Cholera Report of the Board of Health (p. 62):

> "In Manchester a sudden and violent outbreak of cholera took place in Hope-street, Salford. The inhabitants used water from a particular pump-well. This well had been repaired, and a sewer which passes within nine inches of the edge of it became accidentally stopped up, and leaked into the well. The inhabitants of 30 houses used the water from this well; among them there occurred 19 cases of diarrhœa, 26 cases of cholera, and 25 deaths. The inhabitants of 60 houses, in the same immediate neighbourhood, used other water; among these there occurred 11 cases of diarrhœa, but not a single case of cholera, nor one death."

In this instance, it seems impossible to avoid the conclusion, that the impregnation of the system with the putrescent matter thus insidiously introduced, determined not merely the selection of individuals by the cholera poison, but the terrible fatality of the disease in those attacked by it, *only one out of twenty-six having escaped.*—We could cite, from the same Report and its Appendices, many other cogent examples of the same kind, several of them extending the evidence to zymotic diseases of different forms; but such of our readers as may not be satisfied upon this point, can easily refer to these sources of information; and we shall confine ourselves to the mention of a case, which occurred within our own knowledge. In a certain terrace, in the most aristocratic suburb of a large provincial town, consisting of houses of a superior class, and very favourably situated as regards free access of pure air, an epidemic of gastric fever broke out, a few years ago, much to the astonishment and dismay of the residents, no such malady having been known to prevail in the neighbourhood within the memory of "the oldest inhabitant." It was soon observed, however, that this epidemic was limited to particular houses; and, further, that whilst in some houses whole families were more or less affected by it, in others only the servants were attacked. Now the water-supply of this terrace was in part derived from a neighbouring well, and in part from a deep spring at a distance; the latter, however, being paid for, was by no means universally employed, although much preferred as a purer water; and thus, whilst some of the houses only used the spring-water, others only used the well-water, and in others, again, the spring-water was employed in the parlour, the well-water by the servants in the kitchen. For some little time before this outbreak of fever, a disagreeable taste had been observed in the well-water; and this was subsequently traced to the bursting of a sewer, which had discharged part of its contents into the well. The houses *first* attacked by the fever, were those in which only the well-water was employed; and it was where the use of well-water was limited to the servants of the family, that they alone were at first affected by it. There appeared reason to think that some of those who had not used the well-water became the subjects of the malady by subsequent communication with those first affected; but there seems no reasonable doubt, that the contamination of the water was the cause which originally determined the development of the zymotic poison in this locality, which has never again suffered from any such invasion, the faulty sewer having, of course, been repaired.

V. That the habitual introduction of miasmatic effluvia into the system, by *pulmonary* absorption, has a like effect in determining the active development of zymotic poisons in the bodies of those who are subjected to their influence, appears to be no less conclusively proved by the evidence now so abundantly accumulated in regard to the "seats of election" of epidemics. Our cholera reports teem with examples of this kind. The fearful outbreak at Albion Terrace, Wandsworth—in which, without any other ostensible cause than abominably bad sewerage, and, possibly, contaminated water, with a special accumulation of filth in the house first attacked, no fewer than 42 persons were attacked with cholera, and 30 died, out of a population of 120, inhabiting seventeen roomy, comfortable dwellings—will be in the recollection of most of our readers.* We may also remind them of the case of Witham, a suburb of Hull,† in which there was an accumulation of night-soil and other offensive rubbish in a triangular space of about three acres, which had been represented to the local authorities as almost certain to induce a severe outbreak of cholera; the disregarded prediction was most fearfully verified by the occurrence of no fewer than 91 deaths

* Vol. vii. p. 22. † Vol. vii. p. 17.

in the immediate neighbourhood,—a greater number than was observed in any other open area within so limited a circuit. That the invasion, not only of cholera, but of other zymotic diseases, is determined by the habitual respiration of an atmosphere charged with miasmatic particles, is sufficiently obvious from the fact, that most of the localities in which cholera raged with peculiar intensity, had previously acquired an unenviable notoriety as "fever-nests;" and that they were remarkable, too, for the severity of exanthematous diseases, and for the prevalence of such affections as cancrum oris, laryngismus stridulus, and others which are now coming to be traced in great degree to similar predisposing causes. The case of the Whitechapel Workhouse, formerly cited by us* from the Report of the Board of Health on Cholera, is a very striking example of this general fact; and the evidence afforded by the Potteries at Kensington most strikingly accords with this; the locality being distinguished above almost every other by the intensity of its "piggishness" (to use the most expressive term we can think of), and also enjoying the unenviable notoriety of being more unfavourable to human life, and especially to infantile life, than probably any other locality in this country.†

VI. It might not seem at first sight apparent, in what way the condition of persons who are suffering under the simple *privation of food*, is allied to that of individuals who are subjected to the introduction of decomposing matters into their blood *ab extra*, or who possess *within themselves* some unusual source for the generation of like substances. Yet nothing is more certain in the whole range of etiology, than that famine is one of the most powerful among the conditions predisposing to pestilence, and that it is also most constant in its operation; so that, whenever we hear of a famine, we look for fever or some other epidemic as its almost necessary sequel. Now it has been commonly supposed that the lowering of all the vital forces by deficiency of food, constitutes the particular condition which renders a starved population so peculiarly open to the invasion of zymotic diseases; but we think that there is something far more potent than this. It is one of the most curious phenomena of starvation, yet one of which it is not easy to give a satisfactory explanation, that a state of general putrescence supervenes even during life; as if the want of material for the generation of new tissue, were an obstacle to the deportation of that which has become effete. Upon this point all observers are agreed. Thus, Dr. Donovan, in his account of the Irish famine of 1847,‡ speaks of the fœtid odour exhaled from the skin, which is itself covered with a brownish, dirty-looking, and offensive secretion. The general tendency to decomposition is further evidenced by the rapidity of post-mortem decay, an absolute putrescence being often apparent before the extinction of life. And the colliquative diarrhœa, which is so frequently the immediate cause of the fatal termination, by its weakening effect, not merely in cases of inanition, but also in the subjects of exhausting diseases, may probably be considered as a manifestation of the general disintegration of the system. When these facts, then, are taken into the account, does it not seem next to certain, that the presence of a large amount of decomposing matter in the system, which is not adequately eliminated by the excretory processes,

* Vol. vii. p. 16. † Vol. vii. pp. 18, 19; and vol. ix. pp. 146, 147.
‡ Dublin Medical Press, Feb. 1848.

here also affords the condition most needed for the development of the zymotic poison?

VII. An accumulation of disintegrating matter in the system may be due, not merely to its excessive production, but to any obstacle which interferes with its due elimination; and this will be especially the case, when the respiratory process is imperfectly performed. All physiological and pathological evidence tends to indicate the paramount importance of this process; not merely as regards the direct elimination, through the lungs, of a large amount of matter which is undergoing change; but also as furnishing the conditions, by which the matters properly to be excreted by the kidneys, are brought into the normal condition for being thus eliminated. For any prolonged deficiency of respiration necessarily lowers the general oxidating process throughout the body; and thus it happens that an undue amount of carbonaceous matter is thrown upon the kidneys for excretion, and that the highly-azotized compounds are not so completely brought, as they should be, into the condition of urea. Now, that overcrowding, and consequently deficient aëration of the blood, is one of the most frequent causes of the severity of epidemics, is a fact so universally recognised by all who have attended to the subject, that we need scarcely do more than advert to it. The cases of Kurrachee,* Bellary,† Taunton workhouse, Tooting, and Millbank prison,‡ formerly referred to by us, are most pregnant proofs of the influence of a limited supply of air in producing a liability to the invasion of cholera; and what is true of cholera is true also, there is every reason to believe, of other zymotic diseases. Valuable evidence to this effect is furnished by the experience of some of our Indian military stations; for it may be generally predicated of these, that whenever they are distinguished by an unusually high rate of mortality through a long series of years, and this excess is not attributable to any local causes of endemic disease, it is occasioned by insufficient supply of air. On this point we would refer our readers to the facts stated in the article on "Tropical Hygiène," contained in our fifth volume; but would here remind them, that whilst the average annual mortality of European troops under favourable circumstances does not much exceed 30 per 1000, this may be increased to 75 or even 100 per 1000 by insufficient barrack-accommodation; and that the average mortality in the gaols under British control in India is actually not less than *one in ten*, rising in some instances to *one in four*, the average allowance of air in these miserable dens being no more than 300 cubic feet (from 800 to 1000 cubic feet being provided in all well-constructed gaols in this country), whilst in some instances it is no more than 70 cubic feet. It is a fact not a little confirmatory of the doctrine here advocated, that of the twenty-three individuals who survived that fearful night's imprisonment in the Black Hole of Calcutta, which proved immediately fatal to 123 persons, several died very shortly afterwards of "putrid fever." We might refer, moreover, to the sad experience of our emigrant ships, as demonstrating the terrible influence of overcrowding and insufficient ventilation upon the propagation of fever; but it can scarcely be necessary to enter into details on this point. We would simply make a remark, however, on the well-known fact, that the sailors who navigate these vessels suffer far less severely than the passengers; not-

* Vol. ii. pp. 81—89. † Vol. iii. p. 26. ‡ Vol. vii. pp. 7—10.

withstanding that the state of their berths in the forecastle, as regards ventilation and cleanliness, is probably no better than that of the "between-decks." This seems to us fairly attributable to the fact, that the sailors never remain below for more than four hours at a time; and that, although their respiration may be partially obstructed during that period, it is so much promoted by the exposure and activity which their duties involve whilst they are on deck, that the deleterious effects of the habitual respiration of a foul atmosphere is thus averted. The emigrant passengers, on the other hand, are crowded below during the whole night, and for much longer periods during foul weather, and have no active employment when on deck; so that their respiration during the whole voyage is below the normal standard.

The evil results of an insufficient supply of air are not exerted merely through the imperfect oxidation and elimination of the substances which are undergoing decomposition within the system; for the same cause will operate to confine the putrescent effluvia that are given off *as such* from the lungs and skin, which will produce the same effect upon the individuals habitually exposed to them, as if these were generated from some external source. It was ascertained by the experiments of Collard de Martigny, that the fluid exhaled from the lungs is by no means pure water, but contains as much as three parts in 1000 of organic matter. If this fluid be kept in a closed vessel, and be exposed to an elevated temperature, a very evident putrid odour is exhaled from it; and from the recent experiments of Mr. R. A. Smith,* it appears that its putrescence depends on the decomposition of an albuminoid substance. There is every reason to believe that the fluid exhaled from the skin is charged with a very similar substance; its presence being indicated by the foul odour of garments that have been too long worn. And thus imperfect ventilation becomes the means, not only of preventing the due elimination of decomposing matter from the body, but actually of re-introducing its poisonous products into the blood, by the very process which was designed for the purification of the vital fluid. It seems, moreover, quite legitimate to conclude, that when the normal oxidation of the decomposing matter is prevented, the amount of that matter excreted in a state of imperfect oxidation will be increased; just as a lamp or fire smokes, when there is not enough air to support a perfect combustion. This view derives strong confirmation from the experiments of Professor Liebig, who has shown that putrescent compounds which give the peculiar character to fæcal discharges, may be artificially generated by the imperfect oxidation of albuminous substances; whilst Städeler has recently obtained from the extractive of the urine of the cow, a set of products bearing a remarkable analogy to those produced by the imperfect oxidation of organic compounds in the process of destructive distillation, one of them being actually identical with the carbolic acid of soot.

Of the tendency of the habitual respiration of an imperfectly-renewed atmosphere, already despoiled of part of its oxygen and charged with carbonic acid, and also loaded with the miasmatic emanations of the bodies which it supports, to produce a peculiar liability to zymotic dieases, we have a pregnant instance in the condition of the Icelandic population,

* Philosophical Magazine, vol. xxx. p. 478.

formerly noticed by us (vol. v. p. 456), whose susceptibility to epidemics is scarcely less extraordinary than that of the Färoe islanders; the state of the system, in our apprehension, being essentially the same in both cases, although induced, in the one, by the retention within the body of the products of its own decomposition; and, in the other, by the introduction of similar products as ordinary constituents of the food.

VIII. Among the predisposing causes which promote the spread of epidemics, *intemperance* is admitted to hold a high rank. Its efficacy has been generally attributed to the general disorder of the nutrient process, and to the weakening of the vital powers, which it tends to induce; but to us it appears that it possesses a more direct and special action. One of the best-established among the consequences of the introduction of alcohol into the blood, is its rapid oxidation, whereby it is itself eliminated from the circulating current: but, in thus greedily appropriating to itself the oxygen which the respiratory process supplies, it *prevents* the oxidation of other substances, of which it is one of the special objects of that process to get rid; thus tending to induce the same condition of the blood, as that which is consequent upon obstructed respiration. And the peculiar potency of this cause in hot climates, where the oxidating process, as measured by the production of carbonic acid, does not take place at above half the rate at which it is carried on in a colder atmosphere, is a strong confirmation of this view. The experience of the whole medical service of India is in accordance upon the fact, that fever, cholera, dysentery, &c., are peculiarly prone to attack the intemperate, and are peculiarly severe and fatal among the individuals attacked; and the evil is greatly aggravated, when the activity of the respiratory process is further diminished by overcrowding or insufficient ventilation. Thus, the 63rd regiment, which had not been remarkable for sobriety, lost 73 men during a nine-months' sojourn at Secunderabad, or at the rate of nearly 79 per 1000 for the whole year; the mortality of all the other stations in the Madras command being at the rate of 30·2 per 1000 for the same year. But when this regiment was replaced at Secunderabad by the 84th, a large proportion of which consisted of total abstainers, whilst nearly the whole remainder were habitually temperate, the mortality of the station was reduced to 34·2 per 1000, which *for it* was unprecedentedly low, being less than half the average of that station for the fifteen years preceding, and absolutely below that of the remainder of the Presidency for the same year. Now the mortality of the 84th regiment during the preceding year, whilst quartered in Fort St. George, had been but 12·1 per 1000; so that as we have no reason to believe any other causes than those now assigned, to have been in operation in either case, we may regard the increase of mortality in that regiment from 12·1 to 34·2 per 1000, as due to the imperfect barrack-accommodation at Secunderabad, whilst the difference of 47 per 1000 between the temperate 84th and the intemperate 63rd seems fairly attributable to the effects of alcoholic excess, aggravated by imperfect respiration.

IX. The fatigue resulting from excessive muscular exertion, moreover, is commonly accounted one of the predisposing causes of zymotic disease; and this too is usually supposed to operate merely in occasioning a general depression of the vital powers. But here too we trace a more definite and direct connexion between the cause and its effect. All muscular exertion, it is now universally admitted, involves as its condition a disintegration of muscular tissue, the components of which normally undergo oxidation, so as to be partly eliminated by the respiratory process under the form of carbonic acid and water, and partly by the kidney under that of urea, &c. Now if the disintegration of muscle by exercise take place faster than the matter thus set free to decompose can be oxidated and eliminated, it must remain in the blood for a time, in that very state of readiness to change which renders it peculiarly fermentible; and thus its presence in the circulating current will give to the blood the same susceptibility to the action of zymotic poisons, which it will derive from any of the causes already mentioned. And that this is the true *rationale*, seems to us to be strongly indicated by the fact which lies within the experience of every one, that muscular exertion so much more speedily induces fatigue in a warm atmosphere than in a cold one; and also by the fact, which is a matter of common notoriety in India, that troops on a march are peculiarly liable to suffer from fever, cholera, dysentery, or other zymotic diseases. For in proportion as the oxidating process is reduced in activity by a high external temperature, in that proportion must the products of the muscular disintegration tend to accumulate, inducing the feeling of fatigue by the deterioration of the blood to which their presence gives rise; and in like manner occasioning an extraordinary fermentibility in the circulating fluid, which renders it peculiarly ready to undergo a morbid change when any zymotic poison is introduced into it. Now if this view be correct, we should expect to find the predisposing power of muscular exertion peculiarly augmented, either by any obstacle to free respiration, or by the ingestion of alcoholic liquors which will diminish the efficacy of that respiration. And such is undoubtedly the case. The careful analysis which we formerly made of the causes of the terrible fatality of the cholera at Kurrachee, brought into the strongest possible relief the similar potency of insufficient respiration and of excessive muscular exertion, taken separately, and their terrible increase of power when combined: the details of this analysis we need not at present recapitulate; but its general results are so strikingly brought out by a tabular arrangement of them, that we cannot refrain from presenting them anew under this form.

Designation.	Strength.	Deaths.	Deaths per 1000.	Exposure at Drill, &c.	Provision for Respiration.	Previous Exertion.
Officers' ladies	42	0	0	Nil	Good	Nil.
Officers	200	3	15	Ordinary	Mostly good	Nil or slight.
Horse Brigade	135	5	37	Ordinary	Good	Moderate.
60th Rifles	980	75	76·5	Ordinary	Bad	Nil.
Artillery	375	37	96·6	Ordinary	Good	Severe.
Bombay Fusiliers...	764	83	108·6	Ordinary	Very bad	Nil.
Soldiers' wives	159	23	144·6	Nil	Mostly very bad	Partly severe.
Do. of 86th Regt....	...	...	166·6	Nil	Very bad	Very severe.
86th Regiment......	1091	238	218	Ordinary	Very bad	Very severe.
	3746	464	124			

Thus we see that the *highest* rate of mortality presents itself where the three causes referred to—namely, the fatigue and exposure of drill, the imperfect ventilation, and the previous exertion in a long march—were in concurrent action; the *absence* of mortality where neither of them was in operation. The difference between the mortality of the Bombay Fusiliers and that of the 86th regiment, which were under precisely the same circumstances as regards exposure and ventilation, shows the marked influence of previous exertion; but that this would not itself account for the high rate of mortality in the 86th, is shown by the far smaller proportion of deaths in the Artillery, three out of the four battalions of which had been subjected to the same previous fatigue, but were well accommodated at Kurrachee, so that their mortality was less than that of the Bombay Fusiliers who had not marched at all, but were overcrowded in ill-ventilated tents. The condition of the soldiers' wives again would be much the same as that of their husbands, as regards accommodation and exposure during a march; on the other hand, they are exempt from the fatigue and exposure of drill; and accordingly we observe that whilst the officers' ladies escaped altogether, the soldiers' wives were affected nearly as severely as their husbands, the loss being the greatest among the women of the unfortunate 86th.

So, again, when the influence of a march in producing an unusual amount of muscular disintegration, is added to that of alcoholic excess in retarding the elimination of the decomposing products, and the march is performed under a high external temperature, which tends to diminish the activity of the respiratory process, we should expect that the liability to zymotic disease would be peculiarly augmented; and of this we have a marked example in the marches of the 84th Regiment, from Madras to Secunderabad, and of the 63rd, from Secunderabad to Madras, performed at the same time, through a pestiferous country; for whilst the former did not suffer at all from cholera or fever, and only lost two men by dysentery (both of whom were old chronic cases taken out of hospital at Madras), the latter lost a considerable number of men on the road, and had so many sick when it met the 84th, as to be obliged to borrow its spare *dhoolies* or sick-palanquins.

Thus, then, if we bring together the results of our examination into the *modus operandi* of the generally-recognised predisposing causes of zymotic disease, we find that they are all reducible to one of three categories:—(1) Those, namely, which tend to introduce into the system decomposing matter that has been generated in some external source;—(2) Those which occasion an increased production of decomposing matter in the system itself;—and (3) Those which obstruct the elimination of the decomposing matter normally or excessively generated within the system, or abnormally introduced into it from without.

1. Under the first head will rank putrescent food, water contaminated by sewerage or other decomposing matter, and air charged with miasmatic emanations.

2. Under the second, any unusual source of degeneration of the tissues within the body, such as presents itself in the puerperal state, after severe injuries, or as a consequence of excessive muscular exertion.

3. Under the third, an insufficient supply of air, a high external temperature (which slackens the respiratory process), and the ingestion of alcohol.

We cannot assign a definite place to starvation, until we more clearly know the cause of the retention of the decomposing matter within the body.

Any one of these causes will tend to produce *an accumulation of disintegrating azotized compounds, in a state of change, in the circulating current;* and this is precisely the condition which, on the fermentation-theory, will afford the greatest readiness to the development of any zymotic poison in the system, whatever the specific nature of that poison may be. But as each of these causes will operate separately, their potency is vastly augmented when they act concurrently; as when the puerperal patient, whose special liability arises from the source of disintegrating matter within the system, is also subjected to putrescent emanations from some external source, and is pent-up in a heated atmosphere, very insufficiently renewed. Let any poison, capable of engendering puerperal fever, be applied to a female thus circumstanced, and our readers will not have much doubt about the fatality of the result. Or, again, if to the influence of the fatigue and exposure of a long march in a tropical country, be added that of overcrowding in tents or barracks, and on that be superimposed the ingestion of putrescent water, or of alcoholic liquors in excessive amount,—and if the individuals, thus primed, be exposed to the poison of cholera, dysentery, or fever,—the experience of our Indian army tells a fearful tale of the consequences of such a combination.

Looking at the question, then, in the first place, as one of experience simply, we believe that no one who duly weighs the evidence, can hesitate to admit the facts on which our superstructure is based. Looking, secondly, to the immediate consequences of each of the causes we have enumerated, we believe that no physiologist can hesitate in admitting the possibility, to say the least, of the *rationale* which we have assigned for every one. And when we find that each of these causes, without a single exception, tends to induce *one and the same condition of the blood,* the conclusion seems to us almost irresistible, that this condition of the blood,—in which it is charged with decomposing organic compounds,—is that which is the immediate source of its liability to be affected by the introduction of zymotic poisons. All this is a matter of direct *induction;* and the only hypothetical part of the doctrine, is the occurrence of anything like fermentation as a consequence of that agency. Here we consider that our doctrine affords to the fermentation-theory a support of which it stands greatly in need; for if experience demonstrate, that zymotic poisons have little morbific power over blood which is exposed to them in a state of purity, but immediately exert their malign influence when the blood is unduly charged with organic matter prone to change, the *modus operandi* of the poison acquires so remarkable an additional feature of resemblance to that of "ferments," that the essential correspondence of the two seems to us almost indubitable.

If it be inquired, what is the practical bearing of this discussion, we at once reply that, if our view be correct, it would be possible to extinguish the greater number of epidemic diseases, however intense or abundant may be the atmospheric or other agencies which constitute their potential causes, by preserving the blood of every individual in that state of unfermentibility (if we may coin such a word), which shall effectually prevent these poisons from finding the conditions of their development within the body; this end being to be attained, on the one hand, by preventing (so far as may be possible) any unusual production of fermentible matter in or out of the body; and on the other, by promoting its removal when it is inevitably generated (as in the puerperal state), through the respiratory process, which ought to be favoured as much as possible, not merely by a free supply of air, but by the reduction of that air to the lowest temperature at which the condition of the patient will allow it to be safely inhaled. We have no new measures to propose; but we do think, that by more clearly specifying than has heretofore been thought possible, the *rationale* of those of which experience has demonstrated the value, we may promote their employment, not only with greater positiveness and consistency, on the part of the profession, than has yet been manifested by the majority of them, but also to a far wider extent than that to which they have hitherto been carried out.

William B. Carpenter.

REVIEW XV.

Das Epithelioma, eine eigenthümliche Geschwulst, Die Man im Allgemeinen bisher als Krebs angesehen hat. Von ADOLPH HANNOVER. Nebst zwei Lithographirten Tafeln.

Epithelioma, a peculiar Tumour, which has been hitherto generally regarded as Cancer. By ADOLPH HANNOVER.—*Leipzig.* 1852. 8vo, pp. 149. With Two Lithographs.

As a provisional term, to be employed until, in the progress of research and with the results of more close diagnostic acumen, we may be enabled to separate from each other lesions bearing many strong points of mutual resemblance, the word "cancroid" has been and is of essential service to the pathological observer. It embraces a group of lesions which have been for a long time confounded with cancer, and forms a very convenient heading under which to place certain morbid specimens, of which we are in a position to state that they are not cancer, but of whose real pathological nature and exact nosological place we are still uncertain. With the advance of micrologic inquiry, however, still greater precision becomes requisite, and we find the term cancroid too vague to satisfy the necessities of a strict pathological anatomy.

Diseases hitherto associated by the common feature of an absence of the general or anatomical characters of cancer, require to be no less carefully distinguished from each other.

M. Hannover's memoir takes up the consideration of one very important subdivision of this class of diseases—namely, those tumours whose chief constituent element is epithelioma in a more or less modified form, and which occur on surfaces naturally covered with a layer of epithelial cells. For this class of tumours he proposes the name of epithelioma; we shall have to consider, in the course of our analysis, how far he is warranted in forming these tumours into a separate order, and how far we are justified in accepting this new addition to medical nomenclature.

The term epithelioma is applied by our author to those tumours which occur on surfaces provided with a perfect pavement epithelium, as the skin and mucous membrane; and he states that no observation has been hitherto recorded of their occurrence on mucous surfaces provided with only cylindrate or ciliate epithelium, or in the interior of organs, or in shut cavities not furnished with a complete pavement epithelium. The vast majority of epithelial tumours present themselves on the mucous membranes, but three remarkable examples are on record of their occurrence on serous surfaces. M. Lebert has observed one instance of an epithelial growth on the internal surface of the parietal layer of the arachnoid membrane, which consisted of little, soft, red tumours, composed of vessels and epithelial cells, presenting, in all respects, the characters of the epithelium of this serous membrane.* M. Robin has seen an epithelial tumour on the inside of a vein in the bone, while Rokitanski has given a case of an epithelial tumour of the liver, which was invested with a fibrous capsule.

The principal seats of epithelioma are the vicinities of the great orifices, the mouth, nose, anus, and those of the male and female sexual organs. The lips, and more especially the under lip, are particularly liable to the formation of epithelial growths; and, according to M. Hannover, the number of cases which occur in the latter situation exceeds that of all the cases which present themselves in other parts of the body. Next to the lip, in order of liability to the disease, come the tongue, the male and female sexual organs, the several parts of the head, the trunk, and limbs; it is least often found on the inner surface of the cheeks, in the jaw-bones, the larynx, the cardiac orifice of the stomach, and the anus.

The disease commences as a small induration superficially covered by the epidermis or epithelium, having its seat in the rete Malpighi, and being sometimes accompanied by a pustule or an excoriation. The chorion also becomes implicated. In proportion to the extent to which each of the constituents of the skin, the chorion, rete, and epidermis enters into the formation of the growth, will be its further development, and the appearances which it presents superficially and on section. Thus, the tumours will be found mulberry-shaped, villous, of a cock's-comb or cauliflower appearance, or they may resemble a collection of pedicled condylomata. The surface of the tumour may, in its smallest divisions, remain for a long time covered with epidermis; but in many cases the surface becomes ulcerated and excoriated at an early period, and an actual loss of substance takes place, especially towards the centre of the growth, where an ulcer forms with an irregular, grey, often bloody base, while its borders are elevated and undermined, and are generally covered with crusts of the fœtid and ichorous discharge. If cut through in its early stages, the tumour presents on the surface of section very well-defined borders between the epidermis, which is frequently several lines in thickness, and the rete, and between the latter and the chorion, which is also hypertrophied. The latter division is indicated most clearly by the undulating outline of the papillæ prolonged from the chorion. The entire substance is friable, breaks asunder readily, and can be separated from the surface of the chorion,

* See Lebert, Traité des Maladies Cancereuses, &c., p. 758.

from which the papillæ spring. When the epithelial formation has struck root more deeply into the chorion, it presents a white-and-grey sprinkled surface, in which is found a dense basement layer or stroma, wherein a variable quantity of whitish bodies may be seen, varying in size from a point to a millet-seed; this appearance, according to M. Hannover, has in many cases led to the confounding of epithelioma with reticulated cancer. The white granules can be easily removed from the grey stroma, in the form of cylindrical bodies many lines in length. The grey substance is in general firm, the white masses being soft or even semifluid; the quantity of both varies not only in different tumours, but even in different parts of the same tumour, which may thus be in one place extremely hard, and in another very soft. When the papillæ of the chorion become elongated, the pedicles of the condylomata at the same time being implicated in it, so that all the constituents of the skin are mixed up together, the tumour assumes a fibrous aspect. From the freshly-cut surface, when scraped with a knife, a milky, granular fluid is obtained, which when mixed with water does not give the semitransparent, opalescent emulsion which is produced by the mixture of the *succus* of cancer with water. On the contrary, the mass mixes with difficulty, as if it contained fat. This characteristic is forcibly dwelt on by Lebert, who considers that there is no physical property of cancer appreciable by the naked eye, of such value as that furnished by careful examination of the *succus*. As the best means of obtaining it pure, he recommends gentle compression of the cancerous tissue; this proceeding causes a number of little drops to exude, which can be then transferred, by the point of a scalpel, to a glass slide; if the drop be then covered with a thin slide, it still presents a homogeneous appearance, but acquires a certain amount of transparence by compression. On the addition of a small quantity of water the mixture becomes a little less troubled, but remains equally homogeneous. Cancer-cells thus form an emulsion with water, and this becomes an important characteristic, for if water be added to a liquid which does not form an emulsion, a grumous appearance results, leaflets or irregular masses being distributed through the liquid, which may remain almost limpid in the intervals. Thus, tuberculous matter mixed with water becomes separated into a multitude of little clots. The division into leaflets takes place principally in epidermic cancroids (epithelioma), and the distribution into more regular masses is observed in the pseudo-succus of the hypertrophy of the mammary gland, and in certain fibro-plastic tumours.* Whatever value we place on these observations, and they are certainly entitled to great weight, the diagnosis cannot be considered complete until the morbid specimen has undergone a careful microscopic examination.

Into the composition of an epithelioma enter the elementary constituents of the skin or mucous membrane, only modified in form, and in their mode of arrangement.

Thus the epidermic or epithelial cells will be found to enter, not only into the thickened superficial layers and detached crusts, but also into the interior of the wart-like bodies, and into the roots which have struck into the epidermis.

We thus encounter cells in all parts of the tumour, in the deepest as well as in the most superficial layers; and as they sometimes constitute

* Consult pp. 6, 7, 8, of Lebert, op. cit.

nearly the entire of the growth, it is evidently on the microscopic character of the cells found in any particular tumour that the question of diagnosis turns. MM. Lebert and Hannover are both strenuous advocates of the specific nature of the cancer-cell, and agree pretty closely as to its chief diagnostic features. In order to establish a standard of comparison, we will first give a brief exposition of the most recent results arrived at with regard to the microscopic characters of cancer; with these we shall then contrast the microscopic appearances of tumours most liable to be confounded with cancer, more especially the epitheliomata; and we shall then give a succinct resumé of M. Hannover's researches on this peculiar class of growths.

Under the head "Microscopic Characters of Cancer," M. Lebert* has entered with considerable detail into the description of the microscopic elements of cancer. He dwells with great force and distinctness on the cell which he calls the "specific element of cancer."

The cell in cancer is frequently found in an incomplete condition, the perfect cells bearing, in many tumours, but a very small proportion to the number of free, large nuclei, with voluminous nucleoli. When the cell is unbroken, it presents a certain fixity of dimensions, the true mean being according to M. Lebert, from $0{\cdot}02^{mm}$ to $0{\cdot}025^{mm}$; in very rare cases, this dimension may be increased to $0{\cdot}04^{mm}$ or diminished to $0{\cdot}015^{mm}$.

The type of the cancer-cell is, according to the same author, a small regular sphere, with an excentric elliptical nucleus occupying half, or even more, of the interior of the cell, and enclosing one or many large nucleoli. This type, however, says M. Lebert, is but seldom pure, and in fact a very marked feature of cancer-cells is the *multiformity* of their walls. The nucleus is an exceedingly important and constant element in the cancer-cell, its predominant type is the ovoid or elliptical form, its borders being strongly marked. Its mean size is from $0{\cdot}01^{mm}$ to $00{\cdot}15^{mm}$; it may reach $0{\cdot}02^{mm}$ or descend so low as $0{\cdot}0075^{mm}$. The nucleolus is large, dark, and much more voluminous and striking in appearance than in any other kind of cell; its mean dimensions vary between $0{\cdot}0025^{mm}$ and $0{\cdot}0033^{mm}$. There may be as many as three nucleoli, but there are seldom more.

Under rare circumstances, the nucleolus may reach a size of $0{\cdot}004^{mm}$, or $0{\cdot}005^{mm}$, and may then present one or more large molecules in its interior.

Grouping these characters together, we obtain a series of diagnostic elements not presented by cells of any other kind—viz., fixed mean dimensions, as above, multiformity of the cell-wall, a nucleus voluminous in itself as well as in relation to the cell which surrounds it, and lastly, a large dark, and very apparent nucleolus. Placed in contrast with this class of cells, those normally found on the epidermis, or any of the mucous surfaces, or developed in any of these situations as the results of diseased action, will be found to present very marked and striking differences; in fact, in not one of the above characteristics will there be found a strict accordance between cancer-cells and those of any epithelial surface.

We shall now turn to the study of the microscopic elements of the

* Op. cit., p. 15, et seq.

epitheliomata, as developed by M. Hannover. The cells found in these tumours are all more or less modified forms of those normally found on the surface which is the seat of disease. They consist of cell-membrane, contents, nucleus, and nucleolus. The cell-membrane is capable of assuming various forms; it may be oval, circular, or, in situations where the cells are pressed closely together, of an angular form; the cell-wall is very thin, so that it is frequently rolled up, or thrown into thin folds, whereby the cell assumes a striated appearance. The fine transparent, sometimes granular contents, which are best seen when the cell is made to roll over, fill the entire cell when it is round or oval; but if it be angular, they are generally collected in particular places, often in the middle of the cell, or around the nucleus. When the contents escape, the borders of the cell collapse, and the cell-wall appears as a flat irregular plate or table. Sometimes the folds of the cell-wall are so fine that the cell appears to have split into fibres; but even in this state, by the addition of weak acetic acid, the cells become softened, the whole preparation appears clearer, and the nuclei become visible.

The nucleus is small in relation to the cell (compare, *supra*, the cancer nucleus); it is round or oval, often pointed, cordiform or irregular; it is finely punctated, and may contain separate large granules; in some cases it presents a double contour, or, again, it may be surrounded by a clear ring. The nucleus may be absent in large as well as in small cells, or its place may be supplied by a mass of fine granules having the appearance of fat drops. Two or three nuclei are occasionally met with. The nucleolus is seldom clear; one large or many small ones are to be found in different cases. These characters, it will be observed, differ remarkably from those above assigned to the cancer-cell. In the case of an imbedded cell, which is occasionally met with, it will be necessary to attend to the form and appearances of both the containing and the contained cell, as otherwise they might be mistaken, on superficial examination, for a cancer-cell, with large nucleus. The occurrence of a doubtful isolated cell amidst others which present unmistakable evidence of an epithelial origin, cannot lead to any practical difficulty. The epithelial cells exhibit the property of becoming elongated at one or two sides; they thus will be found to present caudate, spindle-shaped or club-like forms.

As long as the tumour remains smooth, the disposition of the elementary parts is not notably changed; the rete Malpighi has generally the greatest proportionate thickness, its cells are clear, and especially on the addition of dilute acetic acid. The youngest and deepest have the smallest diameter, but possess larger nuclei than the completely developed cells. When the tumour has a villous surface, each little cylinder will be found to consist of an atrophied papilla of the chorion, containing a small quantity of areolar tissue, and a vascular loop; the chief mass of the papilla, however, is formed of epithelium. When the papillæ are more fully developed and wart-like, the epithelial cells become prolonged, and from their longitudinal arrangement cause the fibrous or striped appearance which is seen chiefly in the pedicles of the warts, but this may be present in more superficial parts also. When the epithelial formation has invaded the chorion, the gray stroma already spoken of will be found to consist partly of areolar tissue and elastic fibres, and partly of the cells of the rete Malpighi, which have become incorporated with the elements of the chorion. The little isolated bodies which can be pressed out of the mass consist of epithelial cells aggregated together in concentric layers; if treated with acetic acid the cells may be separated from each other, and then become apparent as such.

Besides the part which the chorion takes in the formation of the gray stroma, it also constitutes the basis from which the tumour springs, and it will consequently be found more or less hypertrophied and inflamed. Its vessels increase in number, blood frequently escapes, and the ichor secreted may be mixed with blood. As the blood generally comes from the base of the ulcer, or from the deep furrows between the warts and papillæ, it is more probably derived from the vessels of the chorion itself than from those of the papillæ. In the examination of epitheliomata we must be prepared to meet with different extraneous elements, such as the molecular masses or *débris* of muscles and bones, fat cells, oil drops, &c. Crystals (cholesterine frequently), vibriones, and minute vegetable formations, likewise occur.

If we now compare these results of the microscopical examination of an epithelial tumour with those already detailed as characteristic of cancer, we may at once perceive that the resemblances are only superficial, the differences sufficiently marked and striking to enable us to distinguish between the two forms of disease without much difficulty. M. Hannover speaks, however, of a condition of things in which it is much more difficult to draw the line of distinction; we allude to the combination of cancer with epithelioma in the same tumour. Our author is by no means an advocate of the now exploded doctrine of the transformation of tissues, but he recognises the possibility of a cancerous deposit taking place in an epithelioma already formed, which he considers to present just as favourable a locus for the development of cancer as any other indifferent situation. He speaks of having observed such a case; without denying the possibility of its occurrence, we can only say we have ourselves no experience of such a combination.

Having thus studied the microscopic characters of epithelial tumours, and having seen by contrast how they differ from those of cancer, the question arises as to how far it is expedient or coexistent with the present state of our knowledge, to separate this class of tumours from others at present included with them under the term cancroid, and how far it is necessary to admit an addition to our nomenclature for the purpose of more distinctly specifying the order of growths under consideration.

In wholly rejecting the word cancroid, which, as we have already observed, forms such a convenient heading under which to range many nondescript tumours frequently confounded with cancer, but capable of being separated from it by accurate and now established principles of diagnosis, we should experience much inconvenience in dealing with these formations, which, though differing much from each other, have the one feature in common of being certainly not cancerous, though liable to be confounded with cancer. It may be well, therefore, to retain the term until to each of the several affections now grouped together under the name of cancroid be assigned a distinct and proper place in the category of tumours, and be appropriately named. That such a classification and nomenclature is required by the strict rules of pathological anatomy as now cultivated must be admitted; and as an advance in this direction we are decidedly of opinion, that the term epithelioma should be received and adopted. The group of lesions which it comprises have all special characters which separate them from other non-cancerous diseases. Thus they differ widely from the fibro-plastic, the fibrous, and the proper glandular, non-cancerous tumours. Amongst themselves they present a uniformity of structure and of pathological relations. They are therefore entitled to be withdrawn from the vague group of cancroid diseases; and we agree with M. Hannover in considering that they deserve to be ranked as a separate family, and to receive a distinctive name. Perhaps until some other observer shall have effected as much for the precise diagnosis and pathological history of the other non-cancerous affections, it will be well to retain the term cancroid to group them together, it being always understood that this term is retained and used provisionally.

The special history of epithelioma, as it occurs in different parts of the body, occupies a very considerable portion of M. Hannover's memoir. The several sections contain much interesting critical and historical matter as well as original observation, and will be read with interest. We may observe, in conclusion, that this class of tumours has received much elucidation from the labours of M. Hannover; and that our knowledge of them has been rendered more precise and definite by the introduction of an appropriate term under which to group them.

Robert D. Lyons.

Review XVI.

1. *A Treatise on Tuberculosis, the Constitutional Origin of Consumption and Scrofula.* By Henry Ancell, late Surgeon to the Western General Dispensary, &c. &c.—*London,* 1852. 8vo, pp. 779.

2. *The Nature, Symptoms, and Treatment of Consumption; being the Essay to which was awarded the Fothergillian Medal of the London Medical Society.* By Richard Payne Cotton, M.D., Member of the Royal College of Physicians, and Assistant-Physician to the Hospital for Consumption and Diseases of the Chest.—*London,* 1852. 8vo, pp. 286.

3. *Tuberkulose und ihre Beziehung zur Entzündung, Scrophulosis, Typhus.* Von Virchow, Phys.-Med.—*Würzburg,* 1850. *Band* I. pp. 82.

   Zur Geschichte der Tuberculose. Von Virchow, Phys.-Med.—*Würzburg,* 1851. Band II. pp. 70.

   Ueber die Verschiedenheit von Phthisie und Tuberkulose. Von Virchow. *The non-identity of Phthisis and Tuberculosis.* By Virchow.—*Wurzburg,* 1852. Band III. pp. 98.

4. *Handbuch der rationellen Pathologie.* Von Henle. *Manual of rational Pathology.* By Henle.—1850. Band II. pp. 784.

5. *Ueber die Uebereinstimmung der Tuberkelablagerungen mit den Entzündungsprodukten. Annalen der Charité-Krankenhauses zu Berlin.* *The Identity of Tuberculous Deposits with the Products of Inflammation.* By B. REINHARDT.—*Berlin.* Band I. pp. 362.

6. *Benno Reinhardt's pathologisch-anatomische Untersuchungen, nach seinem Tode zusammengestellt und herausgegeben* von RUD. LEUBUSCHER.

   B. Reinhardt's Pathologic-Anatomical Researches. Collected after his death, and Edited by RUD. LEUBUSCHER.—*Berlin,* 1852. 8vo, pp. 144.

WHAT is tubercle? What is its anatomical constitution? What is the pathological process by which it is formed? How is tubercle to be defined, so that all may signify the same body by the same name? What is phthisis pulmonalis? Do all writers using that term intend by it the same disease—i. e., the same anatomically and pathologically? Are many diseases confounded as one under the name phthisis pulmonalis? Curious it is that we should have to ask these questions at the present day. Strange that pathologists, renowned for the accuracy of their observations and for the soundness of their reasoning, whose earnest desire is to see what is, and whose anxious wish is to interpret aright what they see, should give different answers to these questions. Strange that after the labour bestowed on the investigation of these subjects by the most eminent pathologists, from Morton and Bayle to Rokitansky and Lebert, we should have still to ask what is tubercle?—what is phthisis?

The opinions now held in regard of tubercle may be divided broadly into two classes; the first is, that tubercle is an exudation essentially pathological in character. "It is beyond doubt," says Rokitansky, "that tubercle is an exudation."* The second, that tubercle is merely a retrograde metamorphosis of pre-existing structures. This latter notion is strongly advocated by Virchow, in the papers before us.

The opinions referred to, however, readily admit of more minute division; and for the purpose of enabling us, in a subsequent article, to estimate what amount of the truth they respectively contain, we shall here briefly describe them under five heads.

1st. Tubercle is a specific exudation poured out under the influence of a special general pathological state; in other words, it is the local anatomical expression of a definite constitutional affection. Or, as Mr. Ancell says: "As healthy blood supplies a blastema or succus nutritivus for healthy nutrition, tuberculous blood supplied a tuberculous liquor from which tubercle is formed."

Lebert's statement, that he had discovered in tubercle a peculiar and distinctive microscopic element—a tubercle-corpuscle—appeared to give force to this view; and coinciding as it did with opinions previously entertained, was received in this country as strong evidence in favour of the favourite creed. If this opinion be correct, tubercle ranks pathologically and anatomically in the same order as cancer, there being in both a specific constitutional disease, a specific exudation, and a specific or distinctive cell.

The truth or falsehood of this view will come hereafter to be examined.

* Handbuch der Allgemeinen pathologischen Anatomie, p. 413.

2. Tubercle is a degraded condition of the nutritive material. Some pathologists, as Dr. C. J. B. Williams, refer tubercle to a "degraded condition of the nutritive material from which new textures are formed," and hold that "tubercle differs from fibrine or coagulable lymph not in kind, but in degree of vitality and capacity for organization."* Examined microscopically, tubercle contains, according to Dr. Williams, a few irregularly-shaped, shrivelled cells, with imperfect nuclei, the main substance being composed of granular or amorphous matter. "No fibres are," he says, "perceptible."

3. Tubercle is composed of the products of inflammation. Reinhardt is at once the most recent and able advocate of this opinion, and the high reputation as a microscopical observer he enjoyed among those most intimately acquainted with him, recommend his statements to our attentive consideration. Reinhardt sees in tubercle only the products of chronic and repeated inflammations. In some cases of chronic pneumonia, Reinhardt found a gelatinous fluid in the cells and interstitial tissue, containing epithelium and pus. At a later period the epithelium was in a state of fatty degeneration; the fluid was diminished in quantity; the interstitial tissue contracted; the cells lessened in volume; and, finally, a kind of cicatrix was formed. In various stages these states have been termed, respectively, gelatinous infiltration, gray tubercle, and tubercular cicatrix. In other cases of so-called yellow tubercle, Reinhardt found pus in the air-cells; the pus became thickened, dried up, and the nuclei disappeared. Shrivelled pus-cells, and not nuclei which have become free, form the so-called tubercle-corpuscles. Although Reinhardt considers that in some instances the tuberculous process arises from local causes—viz., hyperhæmia and recurrent inflammation; yet he admits that in many cases these indicate a state of dyskrasia.

4. Tubercle is composed of dead-tissue elements: such is Henle's opinion. In the lungs, he says, tubercles are bloodless, dead (nekrotesche) lobules, gorged with the dried-up elements of the epithelium or with pus, heaps of granules and granular cells, and these dead lobules continue in connexion with the sound pulmonary tissue, as a withered limb may with the trunk. "The corpuscles," he says, "which are found most frequently and in the greatest number in miliary and crude soft tubercle, and which have generally been described as specific, are the corpuscles named by me 'elementary corpuscles,' and they belong to that variety of these which is rendered pale and dissolved by acetic acid. I have proved," he continues, "that such forms arise out of cytoid corpuscles long exposed to the air." And, further on—"The microscopic analysis renders it probable that the nucleated cells arise out of the epithelium of the air-cells; it offers no explanation, as to whether the cytoid corpuscles, the products of the development of which we find in the air-cells, arise out of bronchial mucus, or from the pus of a circumscribed inflammation, or from extravasated blood."†

Tubercle corpuscles have already been stated by Gulliver to be "effete and shrunken primary cells"—a definition which might be adopted by Henle.

These views of Henle agree in the main with those propounded, in 1843, by Dr. William Addison:‡ "A tubercle," says Dr. W. Addison, "involves

* Principles of Medicine, p. 386.

† Handbuch der rationellen Pathologie, p. 788.

‡ Transactions of the Provincial Medical and Surgical Association, vol. xi.

or includes in its substance the vesicular structure of the lungs: minute bloodvessels, lobular passages, and air-cells, are all capable of demonstration on the dissection of tubercle under a Coddington lens; the bloodvessels are no longer permeable, but their presence may be demonstrated." Tubercles themselves are composed of abnormal epithelial cells. Henle maintains that gray granulations are imperfectly-coagulated fibrine, and if they sometimes pass into yellow tubercles, cannot be considered as their first stage. He discards the idea of a specific exudation, and advocates the opinion that the first change, as far as the lungs are concerned, is coagulation of blood in, and obliteration of the vessels consequent on, defective capillary circulation, arising from imperfection of the respiratory movements.

5. Tubercles are composed of metamorphosed organized elements—a metamorphosis co-ordinate with the fatty and the waxy degenerations. This is the opinion of Virchow. His views are developed at some length in the papers placed at the head of this article; and as they contain much that is peculiar and novel, we shall enter into them somewhat fully.

To do justice to the opinions of Virchow we shall first describe what we understand him to mean, and then give his own summary of his opinions in the words he has himself used in one of the papers above mentioned.

A tubercle is composed essentially of dead tissues, the death of the part being occasioned by the accumulation of cells amid its vessels, and consequent compression of those vessels and cessation of the circulation through them. The cells which thus play so important a part in the formation of tubercle may have their origin,—

1. In the physiological cells of a structure or organ. The mode in which the increase in these cells takes place may, he says, be exquisitely perceived in the lungs. The first step in the tuberculous metamorphosis in these organs is an increase in the epithelium of the air-cells by endogenous formation. "I have seen," Virchow says, "cells with five large, oval, granulated nucleolated nuclei." Subsequently the "cells fall to pieces, a granular detritus is left, in which the nuclei remain for some time as shrivelled irregular opaque bodies, finally these also crumble, and an entirely amorphous finely granular mass remains behind." It is these nuclei, shrivelled, irregular, and opaque, which, in Virchow's opinion, constitute the tubercle-corpuscles described by Gluge and Lebert. "*They are not,*" he says, "*exudation-corpuscles.*" "The peculiarity of the local process lies in the tendency of the organization, and by no means in a peculiar exudation." In *lymphatic glands* affected with so-called scrofulosis, there is hypertrophy of the elements of the part through endogenous nuclei formation. The cells enlarge to five or six times their normal size, and as many as twelve pairs of nuclei may be seen in the same cell. The nuclei probably increase in number by cleavage into pairs. What share an exudation takes in this change, Virchow says, he "cannot decide." Still he maintains that tubercle is not developed exudation, but merely metamorphosed pre-existing tissue-elements—elements to which, in their primary state, the name of tubercle could not be applied; and that, consequently, the tuberculous metamorphosis is not the mark of a specific process, of a particular constitution.

2. The cells by the accumulation of which the vessels are compressed and death of the part produced may have their origin in the endogenous

development, or in atrophy of the cells of cancer, pus, or typhous matter, but not in their simple desiccation.

3. These cells may be developed in the fibrine poured out in what is termed tuberculous inflammation. Is the tubercle here formed directly of inflammatory exudation-matter? Virchow says, No: the whole mass of fibrine passes on to organization; but while "one part develops itself into uniting tissue and vessels, another forms nucleated and cellular formations, which rapidly increase by endogenous growth, so that their number at some points is very great, and the amount of the endogenous nuclei is occasionally even colossal." The subsequent steps of the process—i. e., death of the part, disruption, atrophy, shrivelling, and desiccation of the cells, are the same in all three cases.

But although all pathological and all physiological cell-growths may thus tubercularize, yet there is a local process which leads to the exudation of a material, the cells resulting from the development of which, whether they be physiological or pathological, so constantly tubercularize and lead to local death, that this may be said to be the ordinary termination of the process. This process, in the phraseology of Virchow, is tuberculosis; while scrofulosis is used by him to signify the constitutional state in which tuberculosis occurs.

To pass from the general state to the particular local lesion.

Scrofulosis is that constitutional affection which commonly leads to tuberculosis.

Tuberculosis is that local process in the ordinary progress of which there occurs an exudation of a material, nutritive or pathological, which develops into cells that tubercularize or undergo the tuberculous metamorphosis.

Tubercularization is the local process by which the metamorphosis of the elements of a part into tubercle is effected—i. e., endogenous development, atrophy, shrivelling, and desiccation of the cells.

A tubercle is formed of the detritus of the metamorphosed and atrophied cells, with the remains of the vessels &c. of the part in which they were seated.

It requires some little attention to grasp fully Virchow's meaning; and to those who have been accustomed to use the word "tuberculosis" to denote a special constitutional affection, the employment of the term scrofulosis to express this state, and the restriction of the word tuberculosis to the local changes going on in a particular part, may be confusing; but a little consideration will prevent any misconception.

With this preamble we shall allow Virchow to state his own views, in his own words:

"1. Tuberculization, the indubitably local process by which the body described by the name tubercle is formed, is not a peculiar specific exudation, but a peculiar transformation of tissue elements, such as in 1847 I described in regard of cancer under the name of tuberculous metamorphosis.

"2. The tuberculous metamorphosis is therefore co-ordinate with the fatty and the waxy metamorphosis, calcification, and atheromatous degeneration, but in no way co-ordinate with inflammation or serous effusion, and even less so with suppuration or with cancerous formation.

"3. The tuberculous metamorphosis sometimes affects newly-formed pathological tissues, sometimes the primitive, the so-called physiological tissues, and finally, sometimes it affects both old and new simultaneously, and this last is its ordinary and peculiar characteristic. The tuberculous metamorphosis attacks cellular and transitory as well as fibrous and permanent elements.

"4. The tuberculous metamorphosis consists in a cessation of the nutritive and formative processes, in a mortification, death of the elements of the tissue, with subsequent peripheric absorption of the fluid constituents, and drying-up of the parts lying beyond the sphere of nutrition; the death itself of the elements of the tissue is caused by the accumulation of cell-elements, and is immediately determined by compression of the vessels of the part.

"5. These cells may arise from an absolutely new formation, or from an increased formation of the normal elements (epithelia, enchymkömer, &c.), or, finally, from an endogenous formation (ans einer endogenen Bildung).

"6. All these processes presuppose definite disturbances of the local nutrition, especially an altered exudation, and point back, accordingly, either to inflammation itself or to an analogous affection, no matter whether they owe their origin to an irritation produced by local mischief, or to an excitation consecutive to constitutional causes, primary changes of the blood, &c.

"7. There is therefore an inflammatory, cancerous, typhous, glanderous, sarcomatous, &c. tuberculization, which are altogether the same in reference to the essence of the local process, so far as this depends on tissue metamorphosis, but which are more or less different in reference to the essence of the whole process, as well so far as the latter is local (disturbance of nutrition, exudation, &c.), as also when it is due to general constitutional causes.

"8. Tuberculosis is the whole process of the affection, comprising the conditions of the local disturbances of the process of nutrition with the changes appertaining to it in the exudation, both in regard of the cell-formation and transformation, and finding in tuberculization its constant regular expression. Every tuberculization (tuberculous metamorphosis) does not have its origin in tuberculosis; tuberculosis can be present, as far as its early stages (exudation, cell-formation) are concerned, and yet there may be no tubercle. We shall therefore call that diseased process tuberculosis, which in its ordinary course always leads to tuberculization; while we shall ascribe cancer and sarcoma, which accidentally tubercularize, to an altogether different process, and shall never give the name of tubercle to a thickened abscess, pus become cheesy, pus concret.

"9. Scrofulosis is the constitutional affection which, after glanders and typhus, the most frequently produces tuberculosis—i. e., the local disease with the regular termination in tuberculization. But all its products are not tuberculous; tuberculosis is rather co-ordinate with a succession of other local processes.

"10. As tubercle is everywhere formed by the accumulation in the tissues of cells of the most varied kinds, these cells in the majority of cases breaking up, it has no peculiar characteristic elements. The shrivelled nuclei arising from the remains of the cells exhibit the greatest degree of constancy in their outward characters, and therefore we can retain for them the name of tubercle-corpuscles." (band ii. p. 72—74.)

What is Phthisis? A considerable portion of Virchow's papers is occupied by a discussion concerning the meaning that is and that ought to be attached to the term phthisis pulmonalis. He argues that there are various kinds of pulmonary phthisis, and that the practice of using the terms phthisis pulmonalis and tubercle as synonyms is founded in error.

Whenever cheesy-looking matter has been found in the lung, it has been considered, says Virchow, to be tubercular; while in reality, pus, cancer, &c., are equally susceptible of metamorphosis into this cheese-like substance. And whenever ulceration of the lungs has been found in conjunction with the presence of cheese-like matter, the case has been regarded as one of tubercular phthisis, when it may have been merely ulcerative bronchiectasis, the cheesy-looking matter being only thickened pus. Virchow would have us adopt Morton's definition of phthisis.

"Phthisis pulmonalis est consumptio totius corporis cum febre, a mala affectione et ab ulceratione pulmonum tandem originem ducens. Quæ quidem est phthisis maxima famosa et καὶ ἐξοχὴν dicta, de qua autores tractare solent, tanquam nulla esset alia phthiseos species. Hæcce phthisis pulmonaris est vel originaria, quæ a mala diathesi et ulceratione pulmonum primo instante dependet, vel secundaria et symptomatica, quoties scilicet pulmones a morbis præcedentibus jam altius afficiuntur."

He would have us distinguish pulmonary tuberculosis from pulmonary phthisis, of which latter there are various kinds. Reinhardt has, Virchow considers, proved that a great part of the so-called tuberculous destruction of the lungs arises out of chronic suppurative pneumonic infiltration, and he adds, " the statements of Carswell, which are illustrated by such beautiful drawings, have experienced the more positive confirmation that Reinhardt worked without any knowledge of them." At the same time, Virchow maintains that the cheesy matter found by Carswell and Reinhardt in the bronchi and alveoli of the lungs did not deserve the name of tubercle.

The origin of cavities in the lungs is traced by Reinhardt, in many cases, to disease of the walls of the bronchi—viz., ulceration and abscess, leading to abscess in the pulmonary tissue, and subsequent gangrene of the cavity so formed. In some cases, the apparent cavities are dilated bronchi, the walls of which may become the seat of ulceration or of gangrene.

The propriety of distinguishing ulceration of the lungs, the result of the softening of tubercle, from destruction of the same organs from other pathological changes, is more generally admitted in this country than the statements of Virchow would lead us to suppose it to be in Germany. In illustration of our statement, we may refer to a series of valuable papers published some years since in the 'Guy's Hospital Reports,'* by Dr. T. Addison, in which he dwelt on the fact, that a large number of pulmonic excavations commonly considered to be the result of the softening of tubercle, are in reality due to pneumonia; and proposed to establish three varieties of phthisis—viz., Pneumonic Phthisis, Tuberculo-pneumonic Phthisis, and Tubercular Phthisis.

With reference to the first variety, he writes:

"This pneumonic phthisis may be acute; the deposits and inflamed tissues softening down and disorganizing at once, without any attempt whatever being made at induration or repair, thereby constituting one form of acute or galloping consumption.

"It may be *acuto-chronic;* of which I would distinguish three [two?] varieties:

"1. The inflammation, though more or less acute, is slower and more invidious in its course, and manifests some attempts at repair, as indicated by various stages and degrees of induration. The induration, nevertheless, is not complete; the pulmonary tissue continues to be friable; and sooner or later—that is to say, in a few weeks or months—softens down, and gives rise to excavations; most frequently by a slow ulcerative process; more rarely by an actual slough, of greater or less portions, of the indurated but still friable pulmonary tissue.

"2. Inflammation may supervene upon or around ancient induration, leading to disorganization either of the newly-inflamed tissue, of the old induration itself, or of both at the same time.

"Lastly, pneumonic phthisis may be chronic; of which I would also distinguish two varieties:

* Guy's Hospital Reports, 1837, 1843, 1845.

"1. That in which old indurations undergo a slow process of disintegration, giving rise to vomicæ.

"2. That very rare form of the disease, in which an insidious inflammation proceeds very slowly to convert a considerable portion of pulmonary tissue into gray induration without any excavation whatever."

In that form of phthisis which he terms tuberculo-pneumonic, Dr. Addison says, that "although tubercles are present, the really efficient cause of the phthisical mischief is pulmonic inflammation."

Some of the cases referred to by Dr. T. Addison were probably examples of bronchial abscess, so well described by Dr. W. Gairdner, in his able papers on the Pathological Anatomy of Bronchitis.*

Speaking of the lungs of a female, aged 30, who died of dysentery, Dr. Gairdner writes, they

"Presented great variations in density; the anterior edges were partially emphysematous, but between the portions thus affected could be felt numerous condensed parts, which, when superficial, presented a somewhat sunk, collapsed appearance, and a deep purple colour. At the posterior part of the lung were considerable masses similarly condensed. On cutting into the pulmonary tissue, there were seen throughout the condensed portions, numerous small yellow points, resembling softened tubercles, but more irregular in outline; these when scraped with the knife were found to be bronchial tubes, or small cavities, filled with and surrounded by pus. Except at these points, the condensed tissue yielded to the knife a little sero-sanguinolent fluid, which, when examined under the microscope, contained mostly blood-corpsucles, with a few-epithelium scales and pus-corpuscles."

Dr. Gairdner, in commenting on this case, observes, that it is the same affection of the lung as that figured by Dr. Addison in his third plate.

In the cases of pulmonary collapse of which Dr. Gairdner is speaking, the evacuation of the pus from the bronchial tubes is, as he remarks, prevented by the absence of *vis à tergo*. Now, when common exudation-matter, the result of chronic pneumonia, has been poured out into the substance of the lung, and obliteration of the air-cells has followed, there must be the same absence of *vis à tergo;* and consequently, inflammation of the bronchi in the consolidated tissue will be followed by the accumulation in them of their secretion, and ulceration of their walls; and finally by more or less rapid destruction, by ulceration and gangrene, of the consolidated pulmonary tissue.

While, then, we are inclined to regard some of Dr. Addison's cases as examples of bronchial abscess in collapsed tissue, we believe that in others the primary lesion was, as he himself thinks, chronic pneumonia. In Dr. Gairdner's cases, the order of the succession of the lesions was bronchitis, pulmonary collapse, bronchial abscess; in many of Dr. Addison's cases, at least exudation of lymph, obliteration of the air-cells, collection of purulent-looking fluid in one or more bronchial tubes, abscess.

Virchow very properly dwells on the importance of distinguishing the various kinds of phthisis from each other, when estimating the influence of hereditary predisposition, antagonisms, &c., on its occurrence.

In our next review we shall enter on the general consideration of Mr. Ancell's and Dr. Cotton's works.

W. Jenner.

* Edinburgh Monthly Journal of Medical Science, 1850.

PART SECOND.

Bibliographical Record.

ART. I.—*The Physical Diagnosis of Diseases of the Abdomen.* By EDWARD BALLARD, M.D. Lond., late Medical Tutor in University College, London, &c.—*London*, 1852. pp. 276.

ALTHOUGH in many systematic treatises, and in medical cyclopædias, chapters or articles are devoted to the physical methods of examining the abdominal organs, we are not aware that any special work has hitherto been published on this subject. This is not a little singular, since the physical signs of abdominal diseases may be said to have been studied at an earlier period, than those even of pulmonary and cardiac affections. They have not, however, attracted an equal degree of attention, and perhaps have not been cultivated with all the diligence they deserve. Dr. Ballard's work has undoubtedly met a want which all have more or less felt; and we have no hesitation in saying, that the manner in which the want has been supplied is deserving of high commendation.

The work is divided into three Parts: the First treats of the methods of examining the abdomen and its organs, by inspection, mensuration, palpation, percussion, and auscultation. On this part we have little to say. It is well done, and, as far as we can see, no important fact has been omitted. The various methods are detailed in clear language, and the subdivisions of each chapter have been evidently arranged with no little care and attention.

In the Second Part, the diseases of the abdomen are considered *seriatim*, and the physical signs of each are enumerated. Two or three extracts will serve to give an idea of the manner in which this division of the work has been executed.

"The physical signs of *simple congestion of the liver* are those of enlargement (86 *et seq.*) in a degree commensurate with its amount. The liver may thus appear to have undergone very little increase in bulk, or, on the other hand, it may be greatly enlarged, especially in cases of cardiac disease, where it is often conjoined with some textural alteration and hypertrophy (95); it may, in the latter case, reach as low as the level of the spine of the ilium. In the pure form of congestion, it is rarely that *palpation* can discover the margin of the organ so as to ascertain its characters, partly on account of the fulness of the colon with gas and accumulated fæcal matter, and partly because there is no laxity of the abdominal wall, while the tenderness may in some cases occasion an involuntary muscular resistance to the pressure of the hand. All that palpation can mostly discover, is deficient yielding to pressure beneath the margin of the ribs on the right side, when compared with the left, to the extent of a few fingers' breadth. When the edge of the liver can be felt, and the character of the enlarged organ ascertained, the former will be perceived to be even, and only slightly thickened, and the latter to be smooth. *Percussion* is more useful in determining the dimensions of the organ. The increase of dulness is generally in all directions, but it may be perceived to occur principally in the downward direction, or it may occur principally upwards, so as to encroach upon the pulmonary resonance, while but little is perceived below the margin of the ribs. The dulness does not shade off into the resonance of the intestines quite so perfectly as it does in health. An important and interesting character in enlargement from congestion, lies in the rapidity with which its indications sometimes subside within the course of a few hours." (p. 71.)

As another example of this part of the work, we select the following account of the physical signs of cancer of the pylorus.

"Cancer of the pylorus is, for the most part, not indicated by any *visible* external sign, but when involving neighbouring tissues, as the pancreas and mesocolon, it may produce visible elevation and prominence over the epigastric or upper umbilical regions, which may extend even to the pubic symphysis, and be accompanied by visible pulsation. Palpation commonly discovers superficial or deep-seated tumour; but in the early stage of the disease it requires a careful search to be made for it at different times of the day, and after recent and full evacuation of the bowels; there may, however, be considerable tumour present, and yet it will escape the hand, from being overlapped by the liver, or obscured by various conjoined tumours within the abdomen. Its usual seat is a little to the right of the median line, and from one to three inches below the margin of the ribs. As the disease advances, and the tumour increases in size and weight, it gravitates more or less towards the lower regions of the abdomen, and may be felt in situations where it might be little expected to be found, such as the umbilical or right iliac regions, the right flank, below the cartilages of the left false ribs, or over the pubes. Its size varies from simple palpable thickening to that of an egg or larger, and its surface may be either smooth or nodulated. Its consistence is mostly hard, but sometimes it presents a certain amount of elasticity. It is commonly moveable, being found to alter its position a little, according as the stomach is full or empty, descending towards the navel if the patient sits up or stands, and during the acts of inspiration; and moving a little towards either hypochondrium when he turns upon the corresponding side. As in the case of any other tumour, however, there may be such adhesions to the parietes and surrounding organs as to render it completely immovable; and when the parts behind are involved in the disease, the tumour may be firmly fixed to the spine. It sometimes pulsates, and the pulsation may be most remarkable when the stomach is full. In any case of cancer of the stomach, the palpable signs of tumour may be obscured by muscular resistance to the pressure of the hand. *Percussion* over the tumour elicits a modified dulness. Occasionally there has been *heard* over the tumour a murmur transmitted from the aorta behind it." (p. 111.)

These extracts are sufficient to show the kind of information contained in this Second Part, and the manner in which it is given. It appears to us both full and accurate.—The Third Part is the most original portion of the volume, and must have cost its author no little labour. The various physical signs are enumerated, and under the head of each, all the pathological conditions which may cause it are arranged. Thus, if a physical sign is once recognised, we have, at a glance, all its known causes, and can thus judge from other circumstances which cause may be present in the particular case under consideration. We cannot conceive anything more useful for a student than a thorough training in this method, and even an experienced practitioner would find it very useful to refresh his memory occasionally with a glance at this part of the work. The following extract will exemplify our meaning. Under the head of *inspection* are arrayed, of course, all the enlargements of the abdomen, or of any part of it. Enlargement in the right iliac region may be caused by no less than twenty-three different conditions—viz.

1. Flatulent distension of cæcum (152, 153).
2. Fæcal accumulation (154).
3. Intestinal concretions (155).
4. Intestinal obstruction, above cæcum (156).
5. Typhlitis (159).
6. Cancer of cæcum—extensive (163).
7. Circumscribed peritoneal abscess (175).
8. Cancer of peritoneum (180).
9. Displacement of bladder upwards and to right.
10. Pelvic inflammation and abscess (187).
11. Pregnancy with lateral obliquity (203).
12. Extra-uterine pregnancy (209-213).
13. Retention of menses (214).
14. Fibrous tumour of uterus (224-220).
15. Inflammation and abscess of ovary (230).
16. Encysted disease of ovary (231).
17. Solid ovarian tumour (247).
18. Aneurism of aorta (249).
19. Aneurism of iliac artery (256).
20. Phlegmon and abscess in wall (260).
21. Œdema of wall (5).
22. Tumour in wall (261, 262).
23. Psoas abscess (263).

The numbers at the end of each line refer to the paragraphs in which the other physical signs of the particular condition are given, so that by reference to these paragraphs, we may, by the aid of the other signs, determine which of the twenty-three pathological states is present.

A very copious index is given at the end of the work.

In concluding this short notice of Dr. Ballard's new work, we have only to say that it is a production rather to be studied than to be reviewed; and we are convinced that the more it is studied, the more its numerous facts will be appreciated.

ART. II.—*Proceedings of the Pathological Society of London. Sixth Session.* 1851-52.—*London,* 8vo, pp. 490.

THE London Pathological Society pursues its investigations with zeal, and, as the present volume proves, with success. We think it, indeed, the best volume which has yet been issued. On comparing the descriptions of the preparations given in this and in former volumes, it is impossible not to perceive that the majority of them are better done than heretofore. The enumeration of the coarser physical characters is given with greater care, and the microscopic appearances are detailed with infinitely more fulness and correctness. The Society has in fact educated itself, and as it is still in the vigour of youth and growth, we do not doubt that its improvement will continue.

In reading over this and the former volumes of Proceedings, the question has forcibly presented itself to us, whether the Society might not now attempt something more ambitious. Are these numerous facts to bear no further fruit than what may be drawn from them here and there by some hard-working student? Could they not be brought to bear on each other, be compared and analyzed, so as to allow the deduction of some general expression? Medicine at the present day is like a heap of stones; every one brings his pebble;—but where is the master hand to build them up in order?

Again, we would say, could not the Society push farther its principle of combination? In the present volume, they have carried to some length the practice of appointing one or more of their members to make microscopic examinations of specimens exhibited by other members. The results are most interesting, and prove what power combined working has in this case as in all others. But could not the Society have a more efficient combination than this, which, after all, is the rudest form of it? Could not some special class of diseases be investigated, or some particularly obscure points in pathology be systematically worked out? Some two or three hundred able and practical men, all labouring for one object, would surely achieve some great results.

In order, however, to do this, the Pathological Society must work upon a system, and the first labour is to settle what that system is to be. And here we will make our last suggestion, and at the same time entreat the Society to believe that we are actuated by the most friendly motives in doing so. Many of the cases in their Proceedings are most ably drawn up, but others are not so. It is evident that either in the original they have been imperfect, or that in the abstract they have suffered damage. Condensation may be carried too far, and it is a great mistake to save space at the expense of completeness. Sometimes a good description is given of a single valve in the heart, all the other valves, and the condition of the cavities, being left unnoticed. Attention is concentrated upon a single point, and all coincident conditions are disregarded. This is surely not the way in which pathology is to be cultivated. No doubt if a man takes a heart from one body, and a lung from another, he may arrive at curious results, but he will never develop the utmost consequences from his facts. In every case, were it possible, we ought to extend our researches beyond the narrow sphere of a single organ. Could the Society not devise a method of observation to be used by all its members, which might aim at some more regular and complete record of pathological states? The members of the Pathological Society have learning, industry, and zeal. If they would only work together more systematically, their exertions might create a new era in medicine.

Art. III.—*A Complete Treatise on Midwifery, &c. &c.* By Alf. A. L. M. Velpeau, M.D. Translated by Professor Meigs, M.D., Philadelphia. Fourth American Edition. Edited from the last French Edition by W. B. Page, M.D.—*Philadelphia*, pp. 652.

It is much to the credit of our American brethren that they have preceded us in the translation of many standard French works on midwifery. To Professor Meigs we are indebted for an excellent translation of Velpeau, which has reached a fourth edition. One of the admirable points in Velpeau's original work is the extensive research which is rendered available to us by his minute and upon the whole accurate references. We are sorry to find that in the American translation these latter have been omitted. Their insertion would, doubtless, have increased the expense, but the advantage, we conceive, would have been more than a counterbalance. It is scarcely necessary to say, that the translation is well and faithfully executed; the names of Dr. Meigs and Dr. Page are a sufficient guarantee for that. The work is, however, little more than a translation; there are a few foot-notes, on practical matters, by the editors, but not so many as to give a compound character to the work. One of these notes we shall extract, as expressing Dr. Meigs' opinion upon two points in which he differs from M. Velpeau. The latter has been enumerating the sources of danger to the child in pelvic presentations—such as pressure upon the abdomen and chest impeding the circulation, and determining blood to the head; upon which Dr. Meigs remarks, very justly—

"The view taken here of the causes of death which so frequently operate effectually on the fœtus in pelvic presentations, omits one of the most considerable, and which appears to me to be readily conceived of in the following manner. When the vertex descends first, in any woman, the child begins to breathe as soon as the mouth and nostrils are exposed to the air, and it generally cries before the shoulders are born; but when it is enabled to reach the air, it becomes instantly a matter of indifference, as to its security, whether the afterbirth be detached or not. Now it most generally happens, that the afterbirth is wholly or partially detached by the contraction of the womb, long before the hips and legs of the child are expelled; for the womb is by this time grown so small that the placental superficies of it can no longer hold the placenta. This, as I have said above, is a matter of indifference to the child as soon as it can communicate with the atmosphere. In a pelvic presentation, on the contrary, it is a matter of the greatest consequence to the child's safety, that the detachment of the placenta should not take place so early; for although the feet or the breech are born, the child's head having no access to the air, it perishes with real, I might say with double, asphyxia—to wit, the placenta is separated from the mother, and its lungs receive no air. I am far from asserting that the placenta is detached in all cases at so early a stage of labour, as that which I have indicated, though I am free to utter my opinion, that, in the vast majority of cases, the placenta is separated by the time the head is fairly born, in ordinary vertex cases."

Dr. Meigs says, that the results in his own practice are more favourable than those given by M. Velpeau, which he attributes to his custom of sending for his forceps so as to have them at hand whenever he detects a pelvic presentation. "I make very slight traction on the shoulders, in order to facilitate the expulsion of the head; and as soon as I find that the head is not likely to come down, I grasp it in the forceps and deliver it at once. I have safely delivered a number of children which I think would have been born dead but for such a precaution."

It is right to state, that this translation is virtually from the last French edition, which, as our readers may be aware, has been very much enlarged, and in many respects rearranged by the author. The merits and defects of M. Velpeau's work are probably as well known to our readers as to ourselves, and we are quite sure that they will cordially welcome his appearance in his English dress.

ART. IV.—*Quarterly Journal of Microscopical Science, including the Transactions of the Microscopical Society of London.* Edited by EDWIN LANKASTER, M.D., F.R.S., and GEORGE BUSK, F.R.C.S.E., F.R.S. No. I. Second Edition.—*London*, 8vo, pp. 64.

WE congratulate all lovers of natural history on the issue of this journal. We are happy to observe that the first edition has been entirely sold off, and we have little doubt that its future issues will be equally successful. The journal is divided into two parts; the first being the Transactions of the Microscopical Society, and containing papers on Lacinularia Socialis, by Mr. Huxley; on the Raphides of a Cactus, by Mr. Quekett; on a cyst upon an olfactory nerve of a horse, by Mr. Simonds; and on the development of Tubularia Indivisa, by Mr. Mummery. The second part, which is separately paged, is the journal proper, and is occupied by papers on the anatomy of Melicerta ringer, by Mr. Williamson; on the contractile tissue of the Iris, by Mr. Lister (*vide* Chronicle of Medical Science); hints for collecting objects for microscopical examination, by Mr. Shadbolt; and on the cellulose in the tunic of Ascidians, by Mr. Huxley. Then follow translations of Kölliker's paper on Actinophrys Sol, and Schacht's observations on the mantle of some Ascidians. Reviews and news fill up the remaining space.

We have given the list of the papers, to show our readers what kind of fare they will find in this journal.

The getting-up is extremely good, and both editors and publisher seem to have done their utmost to do justice to their theme.

ART. V.—*The Fever at Boa Vista in* 1845-46, *unconnected with the visit of the 'Eclair' to that Island.* By GILBERT KING, M.D., R.N., Inspector of Hospitals and Fleets.—*London*, 1852. pp. 110.

DR. KING was sent out to Boa Vista in 1846, after Dr. M'William's return thence, and he presented a Report on the fever which had been prevalent there, which was reviewed in this journal.* This report was replied to by Dr. M'William, and was severely criticised by Sir William Pym; and the present publication is a reply to the observations of these two gentlemen. We have lately gone into this controversy at such length, that we must be excused from again considering it. We have looked through Dr. King's Report, but without finding that he has brought forward any new facts. He has made a minute criticism of Dr. M'William's evidence, which any one is competent to do for himself. We may remark, however, that there is one point in which Dr. King is a little disingenuous. Speaking of the evidence contained in Dr. M' William's Report, he writes—

"In the discussion of this question, the arguments in support of contagion are founded solely on the assumption, that Anna Gallinha died in Port Sal Rey on the 16th of October, but of that fact we have no adequate proof, and in default of any authentic particulars of the outbreak and progress of the epidemic, we are required to substitute the oral testimony of ignorant and illiterate persons in the very lowest grade of civilized society, upon whose carefulness in observing, accuracy in remembering, and truthfulness in narrating, the exact dates, and the precise order in which the events are said to have happened, the whole case rests." (p. 24.)

* Vol. x. p. 285.

No one will dispute the justice of these opinions; but it so happens that the evidence concerning the death of Anna Gallinha was given, not by any of these ignorant, illiterate, and uncivilized persons, but by an Englishman, John Jamieson, the consul's storekeeper.

Dr. King himself afterwards says "The only authority for the dates of the attack and death of Anna Gallinha is the oral testimony of John Jamieson." (p. 30.)

Having thus ingeniously argued that the date of this woman's illness *cannot* be known, because the testimony is supposed to be of a particular kind, it would be just as fair to affirm that the date *can* be known, because the testimony is proved not to be that which has been condemned as inaccurate. We must say, this single point lessens our faith in Dr. King's impartiality.

ART. VI.—1. *A Manual of Elementary Chemistry.* By GEORGE FOWNES, F.R.S. Fourth Edition.—*London.* 8vo, pp. 681.

2. *A Practical Handbook of Medical Chemistry.* By JOHN E. BOWMAN, F.C.S.—*London.* 8vo, pp. 261.

THESE are two standard works, the re-issue of which we need scarcely do more than announce. Mr. Fownes' Manual preserves its reputation of much knowledge in little space; and Mr. Bowman's second edition possesses the same exact and careful division of subjects as his first. We could certainly have wished that the portion devoted to animal chemistry in Mr. Fownes' work had been longer. Thirty-three pages out of 681 is surely an inadequate amount; and at a time when the unexampled work of Lehmann has appeared, the standard by which the matter of this part must be judged, is high. There are several points in Mr. Bowman's generally very accurate work to which we must also take some exception. He still inserts a table for calculating the solids of the urine from the specific gravity, although nothing is more fallacious than this method, which is not even accurate enough for the roughest calculation. The use of the urinometer is only to detect extremes. Again, in determining the solids of the urine by evaporation, 1000 grains are directed to be taken, a quantity far too large to be thoroughly dried except with immense trouble. We observe, also, that the common sediment of the amorphous urate is said to consist chiefly of urate of ammonia. We had thought it now admitted that the so-called urate of ammonia is for the most part urate of soda, with some mixture of urate of lime and of ammonia.

The usual precision of the book makes us more anxious to direct attention to these few inaccuracies, which do not in the least diminish its general value.

ART. VII.—*The Dictionary of Domestic Medicine and Household Surgery.* By SPENCER THOMSON, M.D.—*London*, 1852. Small 8vo, pp. 572.

NEARLY 600 pages of close print are here published for the sum of six shillings. When we add that the paper is good, the type legible, and the matter of the work useful and practical, we shall have said enough to convince every one that this is one of the cheapest publications we know.

The subjects are arranged alphabetically, and frequent reference is made from one topic to others, so that without much trouble any one may acquire a good deal of information on any medical or hygienic subject. We are bound to say that Dr. Thomson has executed a difficult and delicate task with success, and has succeeded in producing a book which will give the *laity* sound information on many points, which it is for the interest both of themselves and of the profession to know. No rule is more pernicious than that of "every man his own doctor;" but nothing is more useful to a medical man than to have an intelligent patient. Dr. Thomson's book will have the effect of making its readers not only know more of medical science, but trust more in medical men. Ignorance is the parent of quackery—that is, of faith misplaced; and there is no better antidote to quackery than a correct knowledge, even though this may be limited. In addition to this, Dr. Thomson's work will undoubtedly be found useful both in the sick room and in families remote from towns, and medical men; and it will probably supersede the old treatises on domestic medicine, which, bad in the beginning, are now a disgrace to science.

ART. VIII.—*Demonstrations of Anatomy.* By G. VINER ELLIS, Professor of Anatomy in University College.—*London*, 1852. pp. 822.

WE need not do more than announce the appearance of the third edition of this excellent text-book and dissecting guide. Some few alterations have been made, but the bulk of the volume remains the same as in the second edition.

ART. IX.—*Handbuch der Geweblehre der Menschen, für Aetze und Studirende.* Von A. KÖLLIKER.—*Leipzig*, 1852. pp. 637.

THIS is an abstract of Kölliker's larger work on 'Microscopic Anatomy.' In many places the text is altogether the same, and after a glance through it, we do not perceive that it contains any novelties. It will no doubt prove to be very useful for students and practitioners, as in a comparatively short space it gives a summary of our knowledge up to the present time.

ART. X.—*Specielle Pathologie und Therapie.* Von DR. E. A. LUDWIG HÜBENER.

Special Pathology and Therapeutics. By Dr. HÜBENER.—*Erlangen.* pp. 538.

THIS is the second and final volume of a work, the first part of which was issued in 1851. Although only intended to be a kind of abstract of Canstatt's and Wunderlich's great works, it is itself of no inconsiderable length. It is a useful and sufficiently exact, though not a first-class work. The most remarkable feature in the present volume is the space devoted to the subject of poisons. The chronic diseases produced by mercury, zinc, silver, copper, lead, phosphorus, aconite, strychnine, and in fact by some eighty mineral or vegetable substances, when introduced into the body, accidentally or medicinally, in too large doses, are detailed at length. This is a subject which in English works has been much neglected, and has been considered to be too strictly in the province of the toxicologist.

ART. XI.—1. *A Letter to Dr. Lyon Playfair on the recent Analysis of the Buxton Tepid Water.* By W. H. ROBERTSON, M.D.—*London*, 1852. pp. 19.

2. *A Guide to the Use of the Buxton Water.* By W. H. ROBERTSON, M.D. Seventh Edition.—*London*, 1852. Small 8vo, pp. 40.

The following analysis of the Buxton tepid water has been lately made by Dr. Lyon Playfair.

IN ONE IMPERIAL GALLON.

	Grains.
Silica	0·666
Oxide of iron and alumina	0·240
Carbonate of lime	7·773
Sulphate of lime	2·323
Carbonate of magnesia	4·543
Chloride of sodium	2·420
Chloride of potassium	2·500
Fluorine (or fluoride of calcium)	a trace
Phosphoric acid (or phosphate of lime)	a trace
	20·579

In addition, the water contained some carbonic acid, and an extraordinary amount of free *nitrogen*, amounting to no less than 206 cubic inches in an imperial gallon.

Dr. Robertson believes that the discovery of this large proportion of nitrogen will explain the undoubted good effects of the Buxton water. Its first effect is strongly stimulating, and its second is depressing. It therefore requires some caution in its use. It is chiefly useful in chronic rheumatism, gout, sciatica, dyspepsia, general feebleness, and some cases of paralysis. It is also often very useful in catamenial irregularity.

The chalybeate Buxton spring contains, according to Dr. Playfair's analysis, about one grain of proto-carbonate of iron in the imperial gallon.

ART. XII.—*Principles of Human Physiology.* BY WILLIAM B. CARPENTER, M.D., F.R.S., &c. Fourth Edition.—*London*, 1853. pp. 1140.

THIS new edition of a justly esteemed work has been somewhat enlarged and greatly improved. Many parts have been entirely re-written, and the latest discoveries of physiology and of physiological chemistry have been carefully included. Among the new portions which have most struck us, is the second chapter, "On the chemical components of the body, and the changes which they undergo within it." The chief facts detailed in Lehmann's great work are included, and are narrated in Dr. Carpenter's clear and easy style. Physiology is advancing in this department with extraordinary rapidity, and it is evident that it must soon react upon practical medicine. The chapter on the blood is also of great interest. In the anatomical part of the work, the most recent researches of Kölliker and other authorities are given; and in the subsequent chapters on food and digestion, sanguification, circulation, and nutrition, every recent observation has been incorporated. The chapters on the nervous system have, however, undergone the greatest alteration, and this large portion of the work may be said to be almost altogether new. It is in this direction, we think, that Dr. Carpenter's chief strength lies, and we know no one who is equally able to deal with this vast and almost illimitable subject. In dealing with it, he appears more in the character of an original thinker, than in any other portion of the work, although everywhere his powers of combination and acuteness in deduction, enable him to educe original conclusions from the facts collected by others, or gathered by himself.

It would be useless to review this work by itself, but its publication will afford us an opportunity of considering the various topics of which it treats, and of applying, as far as can be done, the discoveries of physiology to practical medicine. In the mean time we commend it to our readers, as the most complete exposition of physiology which any language can at present give them.

We must not omit to mention that many of the woodcuts and plates appear to be either new or re-cast. The whole work is copiously illustrated.

ART. XIII.—*Pulmonary Consumption and its Treatment.* By W. M. BURSLEM, M.D.—*London*, 1852. pp. 160.

THIS work would have been improved, if the whole of the first part had been omitted. It contains some useful facts on the subject of the treatment of Consumption, but the opening chapters on the anatomy and symptoms of Phthisis are little more than a meagre statement of the doctrines of some of the better-known writers. The reader has consequently to wade through some fifty pages of unnecessary matter before he arrives at the really useful part of the treatise. Dr. Burslem has been led to form a high opinion of the use of emetics in phthisis, especially in the early stages; he has given them an extensive trial, and has also collected together the not inconsiderable evidence in their favour which may be found in the annals of medicine. He prefers the essence of ipecacuanha, (prepared for him by Twinberrow, of Edward-street, Portman-square) as it operates in ten or fifteen minutes, and leaves no depression behind it. He gives it every third or fourth day, or every week, and usually in the morning. He states, that it dislodges the sputa, and diminishes the dyspnœa and cough. With emetics, Dr. Burslem combines cod-liver oil and nourishing diet, and sulphuric or gallic acid, when there is any tendency to hæmoptysis. Twenty cases are detailed as proof of the utility of emetics, but the treatment appears to have been usually so complicated that it is difficult to draw any very decided conclusions. Looking, however, to the experience of other observers, and to some of the most marked instances recorded by Dr. Burslem, we cannot but believe that emetics in the early stage of phthisis have been occasionally very beneficial, and that their employment has been too much neglected. Dr. Burslem has done good service by again calling attention to them.

PART THIRD.

Original Communications.

Art. I.

On the Employment of Iodide of Potassium as a Remedy for the Affections caused by Lead and Mercury. By M. Melsens. Translated from the paper in the 'Annales de Chimie et de Physique,' June, 1849, by William Budd, M.D., Physician to the Bristol Royal Infirmary.

PREFACE.

The readers of the 'British and Foreign Medico-Chirurgical Review' will probably require no apology from us for presenting them with the following translation of M. Melsens' Memoir on the Treatment of Metallic Poisoning by Iodide of Potassium.

The great practical importance of the facts it sets forth, and the highly interesting character of the evidence by which they are authenticated, give it a double claim to the serious attention of all who practise the healing art.

If, as M. Melsens alleges, and as his numerous experiments seem conclusively to show, iodide of potassium is not only a safe, certain, and radical cure for the common forms of saturnine and mercurial poisoning, but an equally sure preventive of the injurious effects so frequently produced by emanations from lead and mercury, the fact is one which cannot be made too widely known.

When we consider the great number of working men who, from the nature of their occupation, are constantly exposed to the poisonous influence of these deadly metals, such a result would of itself deserve to rank as one of the most important discoveries of the day. But M. Melsens' researches have a yet wider scope.

The relation which he seems to have established as occurring between iodide of potassium and mercury when present together in the living body, is not limited to the case of mercurial poisoning, but has an important bearing on the therapeutic use of these agents, more especially in the numerous cases in which, according to present practice, they are given concurrently, or in near succession. This can only be fully understood by those who have read the Memoir itself. Some notion of it may, however, be formed by considering for a moment the mode in which M. Melsens supposes the iodide of potassium to exert its curative power.

In all cases of mercurial and saturnine poisoning, he assumes, and no doubt rightly, that the metallic substance is in actual union with the affected part or parts, and is retained there in the form of some *insoluble* compound.*

According to his view, the iodide of potassium, after its absorption into the blood, combines with the metallic poison, and forms with it a new and *soluble* salt—liberates the poison from its union with the injured part—dissolves it out, so to speak, from the damaged fibre, and sets it once more afloat in the circulation.

The new compound thus set at liberty (under the form, it is presumed, of a double iodide of mercury and potassium) he supposes to be eliminated through the kidney almost as soon as formed, in combination with any excess of iodide of potassium that may happen to be present. So that, both poison and remedy being cast out together, the cure may be said in a peculiar sense to be radical and complete.

Surely this is a mode of action which, quite independently of the practical importance of its application in the present instance, is one of the very highest order of interest; and not the less, because there is much reason to believe that it may be the type of what occurs in many other cases.

I do not know whether M. Melsens' researches have been verified or not by other observers. It would certainly have been more satisfactory if results so novel and striking as are here laid before us had been confirmed by other and competent hands. It is right, at the same time, to say that the evidence on which he rests his case, although not in some points so complete as might be desired, is on the whole sufficiently conclusive. With regard to the case of mercurial poisoning it is especially so. Many instances are given of the rapid cure of this terrible affection by iodide of potassium, including one in which there was chemical proof of the elimination of mercury through the kidney during the treatment. The documents showing the progressive and daily improvement, in the handwriting of the same patient, while taking the iodide, may also be referred to as possessing a peculiar interest as a piece of medical evidence.

Although I have had no opportunity of putting M. Melsens' doctrines to the test, *chemically*, since his Memoir first fell into my hands, it may not be out of place to mention, that in regard to mercury many facts had already come under my observation, which, so far as clinical evidence goes, seem to offer a very striking confirmation of them; so striking, indeed, that long before seeing M. Melsens' paper, I had been led by the force of evidence to anticipate the author's theory, in the interpretation of the particular cases to which I refer.

The first case was that of a man who was admitted to the Bristol Royal Infirmary about three years ago, for the cure of secondary syphilis, affecting his throat, joints, and bones. He was also the subject of partial paralysis of the lower extremities, which had come on concurrently with the other symptoms. He had had chancre some months (as well as I remember, about five months) before, for which, from what I could learn, he took mercury largely. From that time, however, up to the day of admission to the Infirmary, he had not taken any mercurial compound. The day

* Conclusive evidence might be given of the truth of this assumption, if it were needed. I may be pardoned, perhaps, for referring to some considerations bearing upon it, which were advanced by myself in 1841, in a paper on the Symmetry of Disease, published in the 25th vol. of the Medico-Chirurgical Transactions, although these considerations by no means exhaust all that might be said on the subject to the same effect.

after he came in he was put under treatment by iodide of potassium in pretty free doses. My surprise was great on coming to the hospital a few days afterwards to find my patient profusely salivated. The swollen face, the peculiar state of the tongue, the loosened teeth, the ulcerated mouth, and the characteristic fœtor, were all present. For many years I had not seen a severer case of mercurial ptyalism. And yet this man, as I specially ascertained—for I at first thought there might have been some mistake—had not taken a grain of mercury since his admission.

Under these circumstances, I came to the conclusion that a portion of the mercury administered some months before had become fixed in the body, and that the liberation of this mercury under the solvent influence of the iodide of potassium was the cause of the severe salivation under which the patient was now labouring. No other theory of the case, in fact, seemed admissible. It may be interesting to mention, that under a continuance of the iodide, not only the syphilitic affection soon got better, but the paralysis also was almost entirely removed.

Since the occurrence of this case, I have seen many others of exactly the same kind, in which mercurial ptyalism came on during the use of iodide of potassium, in persons who had taken mercury some weeks or months before, but none within a more recent period. On several occasions, indeed, the facts have been so striking, that I have been led to make them the subject of clinical remark. A very interesting example of it is under my eye at the present time, in which the same result, in a milder degree, has appeared to be produced by the free inunction of compound iodine ointment. The lady who is the subject of the ptyalism, took a mild course of mercury about ten weeks ago, but was not then sensibly affected by it.

On the subject of lead poisoning I have at present but little to add. A very interesting case of lead palsy is now under my care, in which I have adopted M. Melsens' plan, and apparently with great success. As the case is, however, still in progress, and many important details in it have yet to be filled up, it would be premature to speak of it more at length.

In conclusion, it is hoped that M. Melsens' Memoir, by the wider publicity now given to it among English practitioners, may, by the interest and importance of the results it sets forth, stimulate others to undertake similar inquiries in other branches of therapeutics.

Most of us must have felt, at one time or another, that the state of our knowledge respecting the action of medicines is the least satisfactory part of our art. Much of this depends, no doubt, on the great difficulties which beset the subject, but not a little arises also from the want of searching investigations bearing directly upon it. The position we hold in relation to the whole subject is, in many respects, a reproach to us. Seeking at every hour to modify the phenomena of life by the introduction of common chemical agents—often in large amount—into the living economy, nothing can appear more strange to a philosophic inquirer than the almost entire indifference we show as to what becomes of these agents after their absorption. Whether they abide in the living body, or merely pass rapidly through it; whether, in exerting their power, they are resolved into their elements, or retain their original form; in what manner, and in what company, or through what outlets, they are finally cast forth, with many other questions of equal interest, are secrets, into which, for the most part, we care not to inquire. Yet there can be no doubt, that not one of these questions can be indifferent to a right comprehension of the wonderful and complex part these agents play in the cure of disease. If we were never beset by doubt or difficulty in their use, the case would be different. The fact that, on the whole, we have learned to use them with success, and even, perhaps, with most success where our knowledge of their power is most empirical, is no valid excuse for not endeavouring to throw light on their action by all the means we have.

These researches of M. Melsens, and others which could be referred to, show what invaluable results may be obtained by subjecting such questions as those just now cited to a well-devised system of chemical research. There are many problems in therapeutics, which, it is scarcely necessary to say, cannot be solved by any other means. How many of the obscurities and uncertainties which beset the action of mercurial compounds, for instance, and which so often baffle the physician in their employment, can only be cleared up by a well-directed series of chemical investigations.

To ascertain, if nothing more, the exact distribution of the drug in the several structures of the body, in fatal cases following its employment, and, in particular, the degree in which the diseased part or parts have shared or not in its appropriation, could not fail to furnish results of the highest interest. Subjects of similar inquiry invite us on every hand. And yet, with the exception of some recent researches by Dr. Bence Jones (which may be quoted as models of their kind), this Memoir by M. Melsens, some contributions by Magendie, Liebig, and others, and some important results incidentally obtained by the toxicologists, little or nothing of a serious kind has yet been attempted in this line of investigation.

It should be always remembered, that the subjects with which we are dealing are not subjects of mere scientific curiosity. They are not merely abstract speculations, but problems which we ourselves are daily working out to their practical ends, in the terrible issues of life and death.

To place our knowledge of the action of medicines, as far as it is susceptible of being so placed, on the sure footing of scientific truth, is clearly our most pressing want. Not to speak of the infinitely higher interests concerned, much of the discomfort that too often attends our relations with the public may be ultimately traced to the comparative uncertainty which besets the use of a large number of remedial agents. To reduce this uncertainty to its lowest possible point, no effort should be spared, no mode of inquiry should be left untried. Our duty to the sick, our own conscience, loyalty to our profession, alike require this of us. Doubtless, in this matter, as in most others, or rather, one would say, more especially in this matter than in most others, the elements of truth are not to be found in any single order of facts.

But in appealing to chemistry to help in their detection, we are not only encouraged by fruits already gathered in the same field, but following a path plainly indicated by the course of discovery in physiology itself. For to chemistry we certainly owe the discovery of the highest truth yet realized in this branch of human knowledge. What would be the condition of physiology now, what should we know of the phenomena of life, and of the great cardinal relations of living forces, if Lavoisier had never been, and if the nature of respiration were still unknown to us? Even the use of drugs in the treatment of disease would have remained without its highest sanction. In the incessant dependence of all the phenomena of life, including the most transcendant, on the chemical action of a substance external to us (oxygen), we have not only a warrant coeval with the creation of animal life itself, for seeking to modify these phenomena by the employment of common chemical agents, but a living and enduring proof that even in their most subtle dynamic effects, these agents are inexorably governed by quantitative relations as strict as those which regulate their grosser actions—relations of which weight and measure are the only exponents.*

William Budd.

MEMOIR.

Medical men will, no doubt, pardon me for having, in this memoir, treated the question of the cure of the disorders produced by metallic compounds in a purely chemical point of view. I leave it to them to discuss the effects of iodide of potassium, considered with reference to the various cases and symptoms which these disorders present, as also to determine the influence of the secondary methods of treatment, by which, without doubt, this mode of cure may be rendered more rapid and energetic.

From the very outset of my researches, I have always looked at the subject in its simplest aspect. I have never, in fact, kept in view more than two definite things: *the disease from the presence of poison in the system, and the cure by the expulsion of this poison out of the system.*

It is difficult to form an idea of the inherent obstacles which stand in the way of investigations of this kind; obstacles which, indeed, present themselves in every attempt to connect together, in the same individual case, medical observations and chemical or physiological researches. It is my intention to recur at some future period to the anatomical changes, and especially to the physiological effects of metallic poisoning, which I have not, as yet, been able to clear up in a satisfactory manner. Thus, when I have endeavoured to trace the road which the different iodides follow in the body, all that I have been able to establish is, that they must differ strikingly in this respect; but the experiments I have hitherto made have not enabled me finally to solve such a delicate question. It appears to result from many experiments, that the iodide of potassium does not tend indifferently to every part of the body, but that marked differences occur in the quantity found in different organs. Thus, blood from the heart and blood from the liver do not appear to contain similar quantities of the iodide; differences appreciable to the eye are discernible not only between the heart and the liver, but also between the lungs, the spleen, the kidneys, the brain, the eye, the tongue; iodides are found in the liver, when they are absolutely wanting in the liquid of the gall-bladder; the mucus (serum ?) which bathes the intestinal canal, the pleura,

* It is proper to add, that in the following translation I have, for the sake of brevity, omitted the first page of the Memoir, which contains no information on the subject of which it treats, and consists merely of prefatory matter. It appears that these researches were begun by M. Melsens, in conjunction with M. Guillot, in 1843, in the private laboratory, and under the auspices, of M. Dumas. MM. Melsens and Guillot continued to work together for some time, but were at length separated by circumstances. Some more decisive experiments made by M. Melsens, since he has been parted from his friend, have led to the publication of the whole series of researches under his name. I have also omitted the details of some experiments in §§ 10, 11, 12, 13, and 18, and also the last paragraph of the Memoir.

&c., contains compounds of iodine, but the matters found in the intestine itself contain ordinarily none beyond the first half of the gut.

Iodide of potassium is found in the intestinal canal much below the *ductus choledochus*, when the bile contains no trace of iodine; but if, according to the notions generally entertained on the localization of poisons, the poisonous double compounds of iodine pass in part through the gall-bladder, in order to be cast out into the intestine, then they must of necessity be taken up again into the system, since in the neighbourhood of the *anus* no iodides are ever found. This well-established fact would explain why, independently of other circumstances, the cure is slow, and the intervention of secondary modes of treatment might possibly be useful. Suppose, for example, a dog poisoned by lead compounds to be subjected to the treatment by iodide of potassium, and the resulting double iodide passing by the liver and the gall-bladder to be just poured into the intestine, in order to be returned from thence into the system,—we may conceive at once the utility of purgatives, and of the sulphate of magnesia, especially, associated with the iodide of potassium. I should not have put forth this view, if facts had not warranted me in believing that in certain cases the phenomena may take place in the order above stated, and if it were not of advantage to practical medicine to clear up this question.

§ 1. *Principle of the treatment by iodide of potassium. To render soluble any metallic compounds which have become fixed in the living body, and to facilitate their elimination by uniting them with a substance most readily cast out of the system.*

Such is the principle of the treatment by iodide of potassium of the disorders due to mercurial and saturnine poisoning. In a chemical point of view, it differs essentially from any plan of treatment hitherto proposed. . . . . It has been generally supposed that mercury and lead are present in the body in these cases in the form of soluble salts (a supposition which, for lead especially, is very doubtful); and many remedies founded on chemical views had for their object to form insoluble compounds with the poisonous substance, such compounds being thought to have no action on the living economy. I shall, however, prove, farther on, that the sulphate of lead, for example, is so far from being without action on the living body, that its presence inevitably causes death within a limited period—a fact which leads to a distinction between slow and rapid poisoning.

According to the old chemical view, therefore, the object was to render insoluble a poison which was supposed to be previously soluble. The object of the treatment by iodide of potassium, on the contrary, is to render soluble metallic compounds that have become fixed in the body, to the end that, being again in a state to be taken up by the blood, they may be cast out of the system.

§ 2. *Harmlessness of iodide of potassium in the dose of from* 30 *to* 92 *grains (two to six grammes*) a day for an adult.*

I took in the course of two months about 2315 grains (150 grammes)

* One French gramme is equal to 15·438 English grains. In converting the gramme into grains, in this and other instances, I have ventured to drop the decimal figures occurring in the English scale, any quantity less than a grain, where such considerable weights are dealt with, being plainly immaterial as affecting the dose.—TRANSLATOR.

of iodide of potassium. I began with 30 grains (two grammes) a day, and went on increasing the dose, with an occasional interruption of a day or two, up to 92 grains (six grammes), or rather more than a drachm and a half a day.*

This treatment caused neither pain nor disorder of any kind. I had coryza for the first few days; some pimples on the skin; a peculiar taste in the mouth, without metallic after-taste: my appetite increased.

§ 3. *The kidneys are the principal outlet of the iodide of potassium. It is even with extreme difficulty that this salt can be made to pass through the bowels into the stools.*

The presence of iodine was very readily detected in the expectoration, in the perspiration, in the saliva, and in the tears. The mucus of the nose contained it, but in very small quantity. My surprise was great when, on examining the fæcal matter of two patients who were taking iodide of potassium in the dose of 77 grains (five grammes) a day, I could not detect the slightest trace of iodine. Their urine contained it in large quantity.

I macerated two pounds of fæcal matter in water; the water was examined by the ordinary tests—starch, an acid, and chlorine,—but without result. As the proper reaction might possibly be hindered by organic matters, I calcined two pounds of fæcal matter procured from another patient, and rendered alkaline by the addition of potash, but could discover no trace of iodine.

I examined my own fæcal matter four days in succession, while I was taking 92 grains (six grammes) of iodide of potassium daily; operating on about 3087 grains (200 grammes) of moist fæces, which were carefully calcined after having been made alkaline by potash. In the first trial I found no trace of iodine; in the second and third slight traces were discovered; in the fourth, again, there were none.

I wished to ascertain next in what manner the iodide would be eliminated when associated with an active purgative. After using an enema, I took 926 grains (60 grammes) of sulphate of soda, and 61 grains (four grammes) of iodide of potassium. The saline residue of three copious stools thus produced contained but a very small quantity of iodine, whereas the urine was much loaded with it. A fourth stool, passed twelve hours afterwards, contained not a single trace of iodine. The principal outlet of the iodide of potassium is, therefore, the kidney.

* This will probably seem to the British practitioner a very large dose. I have often seen troublesome disorders occasioned by a much smaller quantity. I believe, however, that the dose mentioned by the author may be safely given in most cases, provided two precautions are observed. One is, to take the remedy fasting; the other, to take it largely diluted. It is difficult to say which of the two precepts is the more important. A painter is now under my care who has taken two drachms a dav of the iodide for the last month, with little other sensible effect beyond increase of appetite and a progressive gain of flesh and strength. On the whole, there can be no doubt that the doses generally given in this country are too small to admit of the full virtues of the drug being brought into play in every case. For this reason, I believe that these virtues are not even yet estimated as highly as they deserve.

In the treatment of rupia and of syphilitic ulcer of the throat, of phagedenic character, I have often seen this strikingly exemplified. One or two cases in particular, of rapidly destructive ulceration of the throat, occur to me, in which, under the employment of the iodide in the dose of fifteen grains a day, the ulceration continued to make frightful havoc, but began to *heal rapidly* when the dose was raised, first to half a drachm, and then to two scruples, and then to a drachm, in the 24 hours.—TRANSLATOR.

§ 4. *Every mercurial compound which can possibly occur in the living economy, even metallic mercury itself, is soluble in iodide of potassium; the presence of the organic substances of the body does not hinder these reactions.*

It is easy to understand the motives which led me to propose iodide of potassium for the treatment of the disorders produced by lead and mercurial poisoning, especially when these disorders occur in a chronic form. Let us first consider the case of mercury.

It is well known that persons who have undergone mercurial treatment have observed, even after an interval of many years, that gold placed in contact with their perspiration has become coated with mercury, especially when excessive perspirations have been caused by the use of the vapour bath. If this fact be true, it proves that the system may absorb and retain mercury for a long time under forms which I will not try to define, but which very probably result from the insoluble compounds which the salts of mercury form, either with the organic or inorganic materials of the body, or with both conjoined. Perhaps the mercury may even occur in the metallic state, as some have admitted; at any rate, it is present in the body in such form as to be retained there. The principal combinations which might thus occur may be reduced to the following:—

1. Combinations of corrosive sublimate, whether in its simple state, or as modified by the animal substances of the economy—namely,
   a. With albumen.
   b. Albumen, and the materials of the brain.
   c. Gelatine.
   d. The nitrogenous extractive matters of the blood, of muscle, of the urine, &c.
   e. Albumen, fibrin, muscular fibre, gelatine, whether in the natural state or modified by digestion.
   f. Matters of the bile.
2. Mercurial soaps.
3. Phosphates of mercury.
4. Mercury in the metallic state. (?)

All these compounds are soluble in alkaline or neutral pure iodide of potassium dissolved in one of the liquids of the body. I have made experiments with each of the compounds here enumerated, and have always succeeded in dissolving them under whatever circumstances. If the iodide of potassium be associated with a dilute acid which has no energetic action either on the solution of the salt or on the principles which occur in the body, the solution of the fixed mercurial compounds is still effected perfectly. In obtaining this last result I operated with lactic acid. After fixing corrosive sublimate on nervous filaments, muscular fibre, or on tendons, it is only necessary to wash them for a short time in a solution of iodide of potassium, whether acid, alkaline, or neutral, in order to remove entirely the mercurial salt. It is especially with a solution of albumen and sublimate that these properties admit of being perfectly demonstrated; indeed, the experiment has been for many years a class-experiment in M. Dumas' course at the School of Medicine. It is only necessary to pour a solution of iodide of potassium on the precipitate formed by albumen and sublimate, in order to see the liquid become instantly limpid. The iodide of mercury possesses a property which I must not omit to point out. This compound, as is well known, is soluble in caustic potash. Now, although caustic potash is not found in the living body, the alkalinity of the greater number of the fluids which are found there is worthy of being borne in mind, and acquires a certain degree of interest when viewed in relation to the following experiment. Mercury coarsely divided was placed under a layer of water holding in solution caustic potash and iodide of potassium. After some weeks' contact a considerable quantity of mercury was found dissolved in the liquid.

It is well known that metallic mercury in contact with alkaline chlorides in solution, itself passes in part to the state of chloride. It remained for me to prove that this reaction might occur in perfectly neutral or even in alkaline fluids. It was necessary in this experiment to guard against the intervention of the carbonic acid of the air, or of the acids sometimes diffused in the air of chemical laboratories. It appeared to me quite necessary to make this experiment, especially as it was possible that mercury might exist in the body in the metallic state, and yet act as a poison; in which case, the experiment I have just cited would still permit the hope that the poison might become dissolved by the action of the iodide of potassium, rendered alkaline by the fluids of the living body.

When metallic mercury is shaken in a solution of iodide of potassium, whether neutral or slightly acidulated with hydrochloric acid, the solution soon acquires an alkaline reaction—an incontestable proof that the oxygen of the air has laid hold of the potassium of the iodide, which, in its turn, has yielded its iodine to the mercury, the iodide of mercury thus formed having entered into combination with the iodide of potassium remaining in excess. It suffices to shake vigorously a neutral or slightly acidulated solution of iodide of potassium with an excess of mercury, for the space of a minute, in order to see the reaction occur. This might be easily done as a class-experiment, to show the tendency of the alkaline haloid salts to form double salts with the corresponding haloid salts of the metals properly so called. An analogous phenomenon may be observed, although less readily, with common salt, and, indeed, simple potash excites the oxidation of mercury, and dissolves small quantities of the oxide. When these experiments are made in closed vessels, it is easy to demonstrate the disappearance of oxygen, by analyzing the air which has been operated on.

§ 5. *Rapidity with which the iodide of potassium traverses the system.*

If these properties of the iodide of potassium seem remarkable when standing alone, they acquire a new interest when they are confronted with the phenomena which this salt offers to us in its passage through the body. In fact, if the alkaline iodides have a very great tendency to unite with the metallic iodides, whether associated or not with organic matters, the former possess the property of passing off in the urine with extreme rapidity, and, when they are pure, the body rids itself of them in a very short time. Is it not probable that the elimination of the alkaline iodide is accompanied by the elimination of the double or triple compounds which it is so easy to produce in the laboratory?

I attach some importance to the demonstration of the rapidity with which the iodide of potassium passes from the stomach into the urine, and of the very short time which is required in order that the system may be entirely rid of it. The following experiments prove it :

A person, after emptying his bladder, took 77 grains (five grammes) of iodide of potassium. A few minutes afterwards iodine was detected in the urine. This experiment has been repeated, and iodine always detected in the urine passed on the first occasion of a call to make water.

The following experiment shows the rapidity with which the economy gets rid of the greater part of the iodide taken in. I took 679 grains (44 grammes) of iodide of potassium in eight days, 77 grains (five grammes) daily in four days, and 92 grains (six grammes) in other four days. After having ceased to take the salt, I tested my urine, every time I voided it, with starch, an acid, and chlorine, and on the second day after that on which I ceased to take the iodide, I could no longer detect its presence, although the most minute portion of iodide added to the urine was made manifest at once.

Are the double iodides eliminated as easily, and in the same manner? There is every reason to believe so. However that may be, I have thought this experiment worthy of attention.

§ 6. *It is not possible to give directly the same accumulated proof of the solution of lead, as of that of mercurial compounds.*

All that has been said of the solubility of mercurial compounds in iodide of potassium becomes less clear and more difficult to prove when we have to deal with the compounds of lead. I shall content myself with observing, therefore, that the iodide of lead is soluble in alkaline liquids, and that it has a marked tendency to combine with alkaline iodides. These facts have appeared to me to constitute adequate motives to induce medical men to employ the treatment by iodide of potassium for complaints which, however much they may be relieved, are, according to our best physicians, rarely, if ever, radically cured.

I have proved that metallic lead becomes dissolved in a solution of iodide of potassium, rendered alkaline by potash. It is well known how rapidly certain metals become oxidized when exposed to moist air, in the presence of even feeble acids, like the carbonic acid of the atmosphere. This is the case with iron, and indeed with lead. The intervention of an alkali sometimes suffices to prevent oxidation. Such is the case with iron. Lead, copper, and zinc become oxidized, on the contrary, more rapidly in contact with an alkaline liquor. I have proved that granules of perfectly metallic lead, bathed by a solution of iodide of potassium, rendered alkaline by potash, become, after a time, partly dissolved in this mixture.

§ 7. *Lead colic comes on more especially after the process of washing in the "second water," ("lavage à l'eau seconde.")*

The property which metallic lead possesses of being easily attacked by alkaline liquids exposed to the air has appeared to me the more worthy of remark, because it seems still to be generally admitted that lead occurs in the form of a salt in the living body, whereas there is every reason to believe that it exists there in a masked condition (état dissimulé), or rather, in the form of plumbate of soda. I have several times been struck with a fact which, to a certain degree, renders this last assertion probable. Painters, on being carefully questioned, with the view of fixing accurately the date of their attacks, and the circumstances under which they occurred, very often answer, that they were seized with their pains after washing old paint. Now it is well known that this process is always done with alkaline solutions.

§ 8. *Succedanea of the iodide of potassium for diseases caused by metallic poisons.*

I do not think it necessary to refer to other alkaline iodides, earthy or metallic, as substitutes for iodide of potassium. The action of the other iodides, although capable, possibly, of varying the energy of the treatment, must be at bottom the same.

The property which many salts of an alkaline base possess of combining with the corresponding salts of a metallic base, warrants the supposition that the chlorides, bromides, &c., might be employed with effect in the treatment of these terrible maladies. An excess of common salt in the ordinary food seems already to be useful as a prophylactic. Thus, in seeking information as to the habits of life of those workmen who have escaped the scourge of these disorders, or on whom it has fallen lightly, it has often occurred to me to ask if they were fond of salted food. Those among them who told me they were fond of salt, had either suffered less, or not at all. I had occasion to make this observation in the case of ten workers in mercury. It is one which well deserves to be tested by the medical practitioner. In this case, does salt act as an excitant, or does it act, as one might suppose, in rendering the mercurial compounds at once more stable and more soluble, and thus favouring their expulsion?

The double cyanide of potassium and iron, which may be taken with impunity in a large dose, at least, during several days (as I have again shown in the case of dogs), might also be useful, especially in the disorders due to mercury.

§ 9. *Cases of lead poisoning.*

Let us examine now the facts on the strength of which I would induce medical men to employ iodide of potassium, in the chronic disorders produced by poisonous metallic compounds.

1. M. Boucher, house-painter. Saturnine pains in the spine. (Rachialgie). Incomplete paralysis of the arms. Lead colic. Treated without success at the Charité, under two different physicians; also, at the Hôpital Cochin.

From the 10th December to the 13th March, 1844, he took 3087 grains (200 grammes) of iodide of potassium. At the last-named date he was perfectly cured.

This patient was treated at his own house. As soon as he could take nourishment he was allowed to follow his usual mode of living. He had at his command a graduated solution of iodide of potassium. He began with a small dose; he stopped now and then for some days, increasing or diminishing his dose according to his pains and sensations. On the 27th of May he had already gone to work, and continued well in spite of the weakness of his forearms. Frictions, with an ointment containing iodide of potassium and a little carbonate of soda, were now ordered. He derived benefit from them, and if the paralysis did not entirely disappear, he at least recovered strength in his hands.

At the moment when the metallic compounds fixed in the body become dissolved or transformed, phenomena of acute poisoning may occur, caused by their liberation. These phenomena were, at the outset, so intense in the case of Boucher, that the treatment might have been supposed to be hurtful rather than beneficial. But the occurrence of these phenomena affords incontestable proof of the energy of the treatment: the dose of iodide which excited them was only 46 grains (three grammes) a day.

At the commencement of his treatment the patient's urine was of a deep brown, and had been so for some time, but after the first few days this colour disappeared, and the urine acquired the tint of common urine. I have made the same observation in other cases.

2. A typefounder complained of *tormina*, and of weakness of the legs. 2778 grains of iodide of potassium entirely restored him.

3. A workman, of forty or fifty years of age, who had worked in a white lead manufactory, and also in an establishment for making visiting cards, had been in hospital more than six weeks. When first subjected to the iodide of potassium treatment he was weak and quite broken up.

It is almost impossible to form an idea of the rapidity of the amendment which ensued in this man. He grew better, so to speak, as you stood by. At the end of three weeks he left the hospital completely cured. The dose of the iodide had been raised pretty rapidly: when he left the hospital he was taking 77 grains (5 grammes) a day.

4. A man about fifty years old had been subjected to different modes of treatment, without benefit. When he began his treatment he could scarcely hold himself up; all his limbs were more or less palsied; he was pale and emaciated. After five or six weeks' treatment he was perfectly well, and left the hospital at his own request. When his treatment began his urine was of a deep brown; a fortnight afterwards it had the colour of common urine.

5. Ordinarily, dogs and cats die within a very short time in the greater number of establishments where lead and its compounds are worked: rats and mice do not harbour there. All animals exhibit symptoms analogous to those which are observed in man. A bitch of middle size kept guard on a terrace covered with lead. In the month of September, 1840, she poisoned herself by eating a bladder, in which painters' colours had been kept. She was seized with colic, vomiting, &c. She was cured, but remained thin. The year following, at about the same time, she again had colic, vomiting, &c.; and although she recovered she became from that time subject to similar seizures. These phenomena recurred four or five times a year, always with the same symptoms, which varied only in severity. On the 25th of October, 1843, while in one of these attacks, she was subjected to treatment by iodide of potassium, which was mixed with her food. In forty-nine days she took 3087 grains (200 grammes), distributed in the manner following:

24 days,	31 grains	(2 grammes)	per day.
10 „	61 „	(4 „)	„
7 „	92 „	(6 „)	„
8 „	123 „	(8 „)	„
49			

The administration of iodide of potassium may, then, be carried to a high dose, for the weight of the bitch was probably only about 20 lbs. (10 kilog.) While the animal was taking the highest dose her sight was enfeebled, but it had completely recovered a few days subsequently. After having undergone this treatment, the bitch had not only entirely recovered, but was better than she had ever been. During the year which preceded her treatment she was very thin: immediately after her last attack her emaciation was extreme. After having taken 3087 grains (200 grammes) of iodide, she had become positively fat; pending the treatment her appetite was voracious. The 30th March, 1844, she was seized with a fresh attack, but much less severe than the former ones. She howled less, and did not get so thin. The 3rd of April she again began the treatment by iodide of potassium, in the following manner:

10 days,	31 grains	(2 grammes a day,	20 grammes,)	310 grains.
10 „	61 „	(4 „	40 „)	610 „
10 „	92 „	(6 „	60 „)	920 „
30			120	1840 grains.

She was again cured.

There was every reason to believe that this malady, which recurred periodically, was due to contact with the lead of the terrace. I have not been able, through circumstances, to put this conjecture to the test; nevertheless, on removing her from the influence of the lead, she lived six months without exhibiting any symptom of the disorder.

§ 10. *Neither sulphuric acid, nor sulphates, can serve as antidotes to slow poisoning by the salts or compounds of lead, the sulphate of lead being itself a slow but sure poison capable of killing vigorous dogs in* 20 *or* 30 *days.*

[In support of this assertion, M. Melsens remarks that the evidence commonly adduced is not sufficient to prove sulphuric acid to be an antidote to slow lead poisoning, although sulphate of magnesia may be properly given in cases of poisoning by a soluble salt of lead to act on the portion yet unabsorbed. Sulphate of lead, however, may be, and probably is, less readily absorbed than other compounds; but that sulphuric acid will reach lead already deposited in the system is quite unproved. Moreover, sulphate of lead is itself a poison, as can be shown by experiments on dogs, some of which are detailed. One dog was paralyzed on the 11th day after 108 grains of the pure sulphate; he then refused food, and died epileptiform, emaciated, and in a state approaching scurvy on the 22nd. Another dog took 293 grains, and died on the 28th day, with similar symptoms.—*Translator.*]

§ 11. *When a great excess of sulphate of lead is administered, the phenomena of lead poisoning are not in ratio to the quantity of poison administered.*

[Experiments on dogs are cited to prove this. It appears that only a certain portion can be absorbed—the rest passes with the excrement.—*Trans.*]

§ 12. *The sulphate of lead obtained by double decomposition washed and ground, is as poisonous as that obtained by precipitating from a soluble salt of lead with sulphuric acid.*

[The only difference observed was, that the quasi-scorbutic symptoms appeared more slowly, and ulceration of the cornea occurred.—*Trans.*]

§ 13. *If to a dog that has been for some time under the poisonous influence of sulphate of lead, iodide of potassium be administered suddenly in pretty large doses, death will ensue. If, on the contrary, the two drugs be given concurrently, the dog will suffer no harm. Iodide of potassium may be employed therefore as a prophylactic.*

[Experiments on dogs very carefully made prove this assertion, and show "that the minute quantity of lead which traverses the body disappears without causing much harm if it meets with a sufficient quantity of iodide of potassium to favour its expulsion. When, on the contrary, lead compounds accumulate in the body of the animal, death is inevitable."—*Trans.*]

§ 14. *Conditions required for the cure of a saturnine affection by the iodide of potassium.*

A little bitch, about a year old, was subjected to poisoning by the sulphate of lead in the dose of 15 grains (one gramme) a day. At the end of eight days, she had lost much flesh; the scorbutic affection was strongly marked; the muzzle stank; there was distinct paralysis of the limbs. She only weighed 3 kil. 915. The sulphate was continued a few days longer, so as to aggravate the disorder without killing the animal. After eight or ten days the administration of the poison was interrupted, and the bitch was let alone, but she still continued to waste for a fortnight longer, in spite of every care, although her appetite was tempted with milk and flesh. She would, however, scarcely take any food.

On the 9th of October death seemed impending. The emaciation was extreme—the muzzle fœtid to the last degree—the paralysis almost entirely prevented walking—the spinal column was curved so as to represent about the third of the arc of a circle: the animal was cold. In this state she weighed 3 kil. 025: so that, without counting the first loss, of which no record was kept, the animal had lost in a month about one-fourth of her weight.

While in this state she was put under treatment by iodide of potassium, commencing with rather more than 7 grains (half a gramme) a day. The dose was gradually increased as the animal seemed able to bear it; until at the end of 27 days, from 61 to 77 grains (four or five grammes) of iodide were taken daily. During the whole treatment *1003* grains (65 grammes) of iodide of potassium were given. From the very first day or two there was a change in the disorder. The appetite returned, the emaciation and paralysis disappeared, and on the 27th day the animal might be regarded as perfectly cured. A few days more, and there remained no trace of her poisoning. Nothing could be more remarkable than the alternate loss and recovery of weight, succeeding one another in so short a time, and indicating with so much fidelity the progress of the poisoning and the salutary effect of the antidote.

The following are the weights of the bitch pending her cure, taken in the mornings when she was fasting:

	Kilog.
9th of November	3·025
14th "	3·240
19th "	3·404
22nd "	3·850
25th "	4·168
1st of December	4·682
6th "	5·188

From the 6th to the 13th of December she remained of the weight last quoted. Here, then, is an instance of an animal becoming much emaciated in ten days, under the influence of a salt of lead: losing, again, one-fourth of its weight during the following month, under the same influence; but, on the contrary, doubling its weight, or nearly so, after 27 days' treatment by iodide of potassium.

In medical practice physicians have to treat painters and other workmen, who generally have been a long time under the influence of the poison. They will not always see the complaints, and the paralysis especially, disappear so easily, rapidly, and completely, as in the case I have just reported.

M. Magendie has also proved that fibrin of new formation is more easily attacked by re-agents than fibrin of old formation. We need not be astonished, therefore, if in workmen who have been a long while affected, the compounds of lead, fixed in the system from a remote period, should resist the action of solvents more strongly than combinations of recent formation.

§ 15. *Aggravation of the morbid phenomena after the administration of iodide of potassium in dogs that are under the influence of lead compounds:—harmlessness of the same doses of iodide of potassium in healthy dogs.*

If compounds of lead are administered to animals, and, after the resulting disorder has reached a sufficiently advanced stage, the mischief becomes aggravated by the employment of iodide of potassium, *this aggravation is to my mind a sure token of cure, for it proves that the remedy is acting.*

It was necessary to show that the same thing occurred with other salts of lead than the sulphate. In order to prove this I had recourse to white lead, the substance which gives the disease to painters, although this same aggravation of the disease had been perfectly observed in the first patient mentioned in this memoir. (Case of Boucher.)

A dog of middle height took 123 grains (eight grammes) of the impure white lead of commerce in eleven days. He exhibited much the same morbid phenomena, but occurring here rather more rapidly than those produced by the sulphate of lead. On the fifth day the appetite was gone, and there was vomiting—a symptom which had not been observed in dogs treated by the sulphate. On the eleventh day he was powerfully affected, and would take absolutely nothing until the fifteenth day, when he again swallowed rather more than 7 grains (0·50 grammes) of the poison. On that day 46 grains (three grammes) of iodide of potassium were given to him. A short time afterwards the morbid phenomena were aggravated in an extraordinary way.

Some hours afterwards the dog was sad, his eyes were half closed; there was extreme prostration and trembling of the whole muscular system. He seemed to be in a state of drunkenness; the sight was dim; the head was carried on one side as from vertigo: there were epileptiform convulsions—he staggered and fell, to rise again with a more tranquil look; but the convulsions soon after returned, to disappear again.

On the morrow, although the phenomena were much the same as the evening before, 92 grains (six grammes) of iodide were again given. The dog's appetite seemed excited: he seized and ate the food offered to him. On the day after the morrow, after the administration of 123 grains (eight grammes) of iodide, the hope of recovery seemed so small that on the fourth day the dose was only carried to 61 grains (four grammes). The dog, nevertheless, died on the next day, at five in the morning.

The dangerous phenomena which supervene on the administration of iodide of potassium, in cases of lead poisoning, cannot be too strongly insisted on, as showing the necessity of great caution in the employment of this remedy in man, for the first few days.

I took a dog of about the same stature and strength, as the preceding one, and I gave him the same quantity of the same iodide of potassium, administered at the same hours. These 324 grains (21 grammes) of iodide of potassium administered in four days to the second dog, which weighed 5 kil. 500, produced no injurious effect; there was only a slight vomiting on the second day.

Another dog of the same size took in twenty hours 77 grains (five grammes) of iodide of potassium, and appeared to be in no way inconvenienced by it.

§ 16. *Action of iodide of lead on the living economy.*

In what way does the iodide of lead, itself, act on the living economy? 115 grains (7·50 grammes) of this salt, administered in the dose of one gramme at a time in the course of seventeen days, caused the death of a dog. Most of the resulting morbid phenomena resembled those produced by the sulphate and carbonate of lead, except in the rapidity of their appearance. This animal had convulsions and fits from the sixth day, having then taken about 77 grains (five grammes) of the salt. Iodide of lead is, therefore, a more active poison than the sulphate, being nearly on a par in this respect with the carbonate. But this is not all we would desire to know.

Is this poison, administered as in the preceding case, less active when associated with the iodide of potassium?

Is it possible, when the poison and antidote are already administered in poisonous doses, to aggravate the phenomena of poisoning by pouring an excess of iodide of potassium into the system?

The following is an experiment which answers this question. To be conclusive, however, it should be varied and repeated with the drugs in other proportions.

A bitch, of the same size and strength as the preceding, took in thirty-three days, and generally in doses of 15 grains (one gramme) at a time, 216 grains (14 grammes) of iodide of lead. With every dose of iodide of lead there were given 30 grains (two grammes) of iodide of potassium. In thirty-three days she had taken 370 grains (24 grammes) of this latter salt. This animal wasted much less than the former one: the paralysis, although easily recognised, was much less marked; to judge from her melancholy appearance she was, however, very seriously affected. She died in a few minutes, after having taken 92 grains (six grammes) of iodide of potassium, which had been rendered slightly acid.

§ 17. *Cure of gilders, and of workers in quicksilver.*

The first patient subjected to the treatment was an old man at Bicêtre, whose case has been recorded with full details by M. Guillot. He had been a gilder, and had lost the use of his limbs by the exercise of his calling. His sight was gone, and he was, moreover, tormented by violent and constant pains. Iodide of potassium was administered every day, in the dose of 61 grains (four grammes). At the end of a fortnight's treatment he left the infirmary alone and without help, whereas on his admission he was obliged to be brought in a litter.

2. I saw a gilder in mercury, about twenty-five years old, treated by the iodide of potassium during three weeks. At the end of that time he could write with ease, whereas at the beginning it was perfectly impossible for him to write at all.

3. The only person on whom the iodide of potassium did not act with energy was an old man of seventy, a working gilder. He was only treated during a few days for a strong salivation; the treatment was given up.

4. Barthelemy Pichon was a working gilder. His memory was very feeble; his speech much embarrassed; and the tremors were so violent, that he could only be employed in the coarser operations of water or galvanic gilding. He belonged to the factory of M. Christofle.

In the beginning of 1844 he was six weeks at the Hôtel Dieu, but with small result. The 1st of June, 1844, I let him have 3087 grains (200 grammes) of iodide of potassium in solution, recommending him to begin by taking a portion corresponding to 15 grains (one gramme) of the salt daily, increasing the quantity as he liked; guiding himself by the severity of his pains, and diminishing the dose when these pains should become too severe. After a few days' treatment he was seized with pains in the head and stomach, and had pimples over the body,—symptoms which did not, however, prevent him from working constantly during the seventy days that his treatment lasted, and that were taken up in consuming the 3087 grains (200 grammes) of iodide of potassium. He followed his ordinary regimen, and changed his habits in nothing.

After the treatment, Pichon was completely cured. He wrote without shaking; he spoke glibly. According to the report of M. l'Eté, this workman, who before his treatment was unable to execute any but the coarsest works belonging to gilding on metal, after seventy days' treatment executed the most delicate processes. There was intense headache on one occasion during his treatment, but this was in sequel to a debauch, after which he had taken no medicine for three or four days. In order to recover the lost time, he took at one draught a dose which should have been distributed over several days. Immediately after this dose the pain of the head was so agonizing, that he thought he should have gone mad.

5. A looking-glass maker, employed in one of the Brussels manufactories, put himself under treatment by iodide of potassium. Before he became a gilder he had been a typographer. He trembled to such a degree as often to drop everything he held in his hand; his speech was slightly embarrassed; his legs supported him ill; he often made false steps; his strength failed him almost entirely. Three hundred and eight grains (20 grammes) of iodide of potassium, taken in ten days, scarcely affected him. As he belonged to a provident society of operative typographers, the medical attendant of the society desired, before continuing the iodide of potassium, to subject him to other treatment. He was of opinion that trembling of another character might complicate the mercurial trembling. A month afterwards he sent the patient back to me in exactly the same state as before. He was again placed under treatment by the iodide. Although this artizan had left his trade, the amendment following the treatment was very slow. He told me, nevertheless, that he felt his strength return daily: the tremors gradually disappeared. It was not, however, until he had taken 3087 grains (200 grammes) of iodide that the amendment was very marked. He scarcely trembled at all, his step was firm, his handwriting good; but his strength had not entirely returned. Observing that the cure in this patient was progressive, but slow, in spite of his withdrawal from his workshop, I ordered iodide of potassium to be used in frictions, under the form of iodine ointment. It was especially from this time that the recovery of strength became perceptible. It was not until towards the end of August, after three months' treatment, that he had entirely recovered.

I saw this workman again in the beginning of the month of November. He told me that his health and strength were much as they had been before he began the business of a looking-glass maker. In the month of April, 1849, he was perfectly well, and nothing remained of his old complaint.

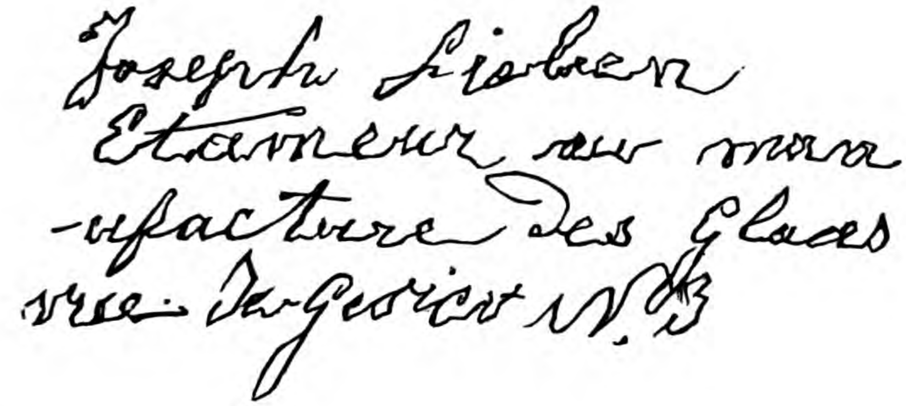
This man only remained proof against the pernicious influence of the trade twenty-one months. Having entered the manufactory in March, 1846 the trembling came on in January, 1848. In questioning him in the presence of many of his comrades I made this observation, which appears to me important:—Those workmen who are fond of salt resist the contamination longer than those who are not.

6. An operative in the same factory was affected with a trembling, which embarrassed him much in his work. His limbs were exceedingly weak; the slightest effort fatigued him. Sometimes objects dropped from his hands: his gait was insecure. His tremors were already of old standing: a year before, he was ill enough to keep his bed, and although relieved then, he remained weak after the treatment which was adopted.

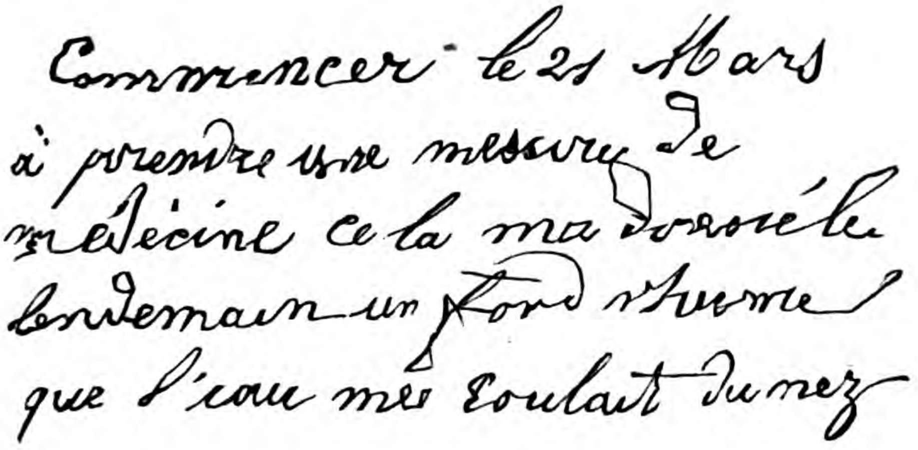
From the 21st of March, 1848, to the 7th of May, he took 771 grains (50 grammes) of iodide of potassium. During the whole of this time he continued to work at silvering, as before, and, what is more important, his work in contact with mercury had almost doubled in the number of hours. Nevertheless, on the 7th of May the tremors were almost reduced to nothing. He took afterwards, up to the 22nd of June, about 1543 grains (100 grammes) of iodide of potassium. He often interrupted the use of the salt for a few days, taking it according to his own judgment, the severity of his pains being his guide. The 4th of September he was perfectly well; trembled little or not at all, and continued his calling without any recourse to medicine. During the whole of this time he never ceased to work, and was always in contact with the poison. His cure was accomplished without preventing him from gaining his livelihood and maintaining his family, and without any change in his regimen or habits.

Nothing is more curious than to examine a portfolio in which the patient wrote to me every day a few words. A fac-simile of some lines taken at hazard will give an idea of this:

1. Writing of the 1st of March previous to the treatment.


Joseph Lieben
Etameur au manu
-ufacture des Glaces
rue de Gericot N. 3

2. After three or four days' treatment.


Commencer le 21 Mars
à prendre une mesure de
médecine cela ma donné le
lendemain un fond rhume
que l'eau me coulait du nez

3. Writing of the 5th of April.

J'ai eu un gout dans ma
bouche tous long du jour
Comme ci je manger de
Cuivre.

4. Writing of the 7th of April, after having taken 262 grains (17 grammes) of iodide of potassium in seventeen days.

Après avoir pris 34 mésures
De la médecine sur 17 jours
et Souffert un peu des Douleurs
dans mon corps, Tous mon
tremblement étois passé je
me trouvé beaucoup mieux
la mine changer et la force
plus fort dans les Membres

Joseph Lieber

5. Writing of the 5th of June, after having taken 1543 grains (100 grammes) of iodide of potassium.

Pendant mon intérruption je m'ai
toujour bien porter mais les
dernier jour ~~jai~~ je m'ai senti
trembler un peu plus fort que
l'habitude =

6. Writing of the 12th of April, 1849. He has continued to work at silvering since the month of March last.*

Bruxelles les 12 Avril 1849
Depuis le mois Juillet 1848 j'ai
été guéri et je n'ai plus rien
aperçu du tremblement jusqu'à
ces jours.
J Lieben

At the beginning of the treatment, the urine of this patient exhibited twice a somewhat uncommon phenomenon. It was limpid, of a bright yellow, but alkaline to such a degree as to effervesce with acids. After a fortnight's treatment, this phenomenon did not recur.

I examined, on eight occasions, four pints of the urine of this patient, in order to see if, under the influence of the treatment by iodide of potassium, the mercury would be found again in his urine. The first

* In the original, two other specimens are inserted of the writing, the 5th of May and 7th of August, but the steps of improvement are sufficiently shown by the above.

specimen contained mercury in perfectly recognisable quantity; the second also contained mercury, but sensibly less.

In following the process employed for the first two specimens, I found no mercury in the third. The fourth specimen, examined by another process, gave scarcely an appreciable trace; but on distilling, in a tube drawn out into a point, one of the slips of gold which terminated the pile I had used, I obtained a small yellow sublimate, which on the morrow had become red. In dispersing this by the flame of a spirit lamp, it became again sublimed with a yellow colour, passing afterwards into red. This is one of the characteristic properties of iodide of mercury.

Is it not remarkable to see a man under the influence of slow poisoning by metallic mercury, void, under the influence of iodide of potassium, iodide of mercury in his urine?

The patient now and then interrupted his treatment. I examined the urine a fifth time during one of these interruptions. I failed to discover mercury in it, but at this time the patient might be considered as cured.

I examined three other specimens of urine, collected when he was taking the strongest doses of iodide of potassium; but whether it was that the urine contained none, or contained it in quantity too small to be detected by the means employed, I could no longer detect any mercury. However that may be, this operative, who continued to work in quicksilver, and who, since the end of June, had taken no more medicine, continued perfectly well. At the beginning of the month of November, all his functions were performed well; his strength and vigour had returned. The organs of generation, previously for a long time passive, had recovered their former energy after some weeks' treatment, and had maintained it since.

This cure proves in the clearest manner, that when mercury is only absorbed in small daily portions, as is the case with those who work at the cold silvering process, iodide of potassium not only possesses the property of curing the patient, but acts also as a powerful prophylactic.

§ 18. *Experiments tending to prove that the iodide of potassium protects against or retards the phenomena of poisoning when the system is subjected to the action of metallic mercury.*

[Experiments are cited to show that, given with mercurial ointment, an excess of iodide of potassium retards, if it does not prevent, fatal results. M. Melsens remarks, however, that although iodide of potassium may act as a prophylactic to metallic mercury, in other cases the iodide of potassium may, instead of relieving, be a dangerous remedy.—*Trans.*]

§ 19. *Experiments showing that the iodide of potassium renders medical treatment or poisoning by certain salts of mercury more active, and may occasion serious accidents.*

For a long time past (from the year 1838-39 at least) M. Dumas has enjoined, in his lectures, the importance of abstaining from common salt when calomel is given, and is desired to remain in the state of calomel in the digestive tube. He has recommended, on the contrary, the addition of sal-ammoniac to corrosive sublimate, in order to render it more stable and less easy to be decomposed when in contact with the fluids of the system. The work of M. Mialhe is sufficiently well-known for me to content myself with a simple reference to it. If the same doses of calomel are given to two dogs, and to one iodide of potassium is given at the same time, the dog which takes the iodide will be destroyed first if the doses of the two salts are at all large. In a comparative experiment made on two dogs, chosen of the same age and strength, as near as may be, I obtained the following results. The first took seven grains (0·500 gramme) of calomel in all, and about one drachm (four grammes) of iodide of potassium administered in the following manner:

First Day.—Rather more than three grains (0.200 gramme) of calomel and 30 grains (two grammes) of iodide of potassium.

Second Day.—Nothing.

Third Day.—Rather more than four grains of calomel (0·300 gramme) and 30 grains (two grammes) of iodide of potassium.

He died in the night of the fifth day. The same doses of calomel without the iodide scarcely affected the second dog at all. On the day of the death of the former he still took about one and a half grain (0·100 gramme) of calomel. He was then let alone for seven days. Nearly two and a half grains (0·150 gramme) of calomel did not prevent him from living five days longer. He had taken, therefore, twice as much calomel as the first dog, and had lived twelve days longer. He suffered, however, a good deal. I gave him at one dose as much calomel as was taken by the first in three days—namely, nearly eight grains (0·500 gramme), and on the morrow 30 grains (two grammes) of iodide of potassium. The symptoms grew worse; he died forty-eight hours afterwards, without any fresh administration of either calomel or iodide.

§ 20. *The action of iodide of potassium on a dog treated by corrosive sublimate may be so energetic, that even eight days after he has taken the sublimate, a pretty large dose of iodide of potassium will prove fatal to him.*

I desired to ascertain whether, in subjecting a dog to treatment by the sublimate, and then interrupting the treatment during some days, it would be still possible to recognise the energetic action of the iodide of potassium in the aggravation of the phenomena of poisoning. Five grains and four-tenths (0·350 gramme) of corrosive sublimate were given to a little dog in the following manner:

First day (0·150 gramme), $2\frac{3}{10}$ths grains.
Two following days, no poison.
Fourth day (0·050 gramme), rather more than $\frac{7}{10}$ths of a grain.
Four successive days, no poison.
Ninth day (0·050 gramme), rather more than $\frac{7}{10}$ths of a grain.
Tenth day, no poison.
Eleventh day (0·100 gramme), one and a half grain.

After this the dog was let alone. He continued to live, although very ill; he might, perhaps, have recovered. On the 19th day, that is to say, eight days after his last dose of sublimate, 61 grains (four grammes) of iodide of potassium destroyed life in twenty-four hours. We have already seen that this dose of iodide of potassium may be given with impunity to healthy dogs. Its highly energetic action in the present instance cannot be understood, unless we admit that the sublimate had become fixed in the body, and that the iodide of potassium, by giving it a new form, by rendering it soluble, and thus allowing it to be carried through the system, had developed an agent capable of exerting a poisonous effect on the organism.

I have not been able to pursue farther these experiments with the compounds of mercury. They deserve, nevertheless, to be kept in view, in connexion with the cure of patients affected with mercurial trembling.

§ 21. *Observations on the treatment of secondary syphilis.*

The last experiment reported finds a parallel in the treatment of the secondary and tertiary disorders of syphilis. The administration of iodide of potassium often causes intense suffering in patients who have been treated by mercurials. To what are these phenomena to be ascribed? According to the opinions put forth in this essay, it is at once seen that when, in consecutive disorders of this class, iodide of potassium is given to individuals who have been treated by mercurial compounds, two distinct effects are produced by a single agent; first, the compounds of mercury fixed in the body are rendered soluble and active; and secondly, a form is given to them which allows their (rapid) elimination. But by the very fact, the patient is subjected anew to a mercurial treatment by the compounds of mercury already present in the body.

If in the treatment of secondary syphilis iodide of potassium acts on its own account—a fact which I do not dispute—we must not, nevertheless, leave out of view the properties to which I have here desired to draw attention, for they must, without doubt, play an important part in the case. There ought to be a marked difference between the action of iodide of potassium on an individual free from mercurial compounds, and on one who holds mercurial compounds in his tissues. It results from the facts established in this memoir, taken as a whole,—whether they concern protection from poisoning, the aggravation of it even unto death, or, finally, the cure of those already poisoned,—that with the treatment by iodide of potassium the cure is never obtained except by a preliminary poisoning—poisoning which the physician has completely the power to regulate according to the strength of the patient. It suffices for this to begin by administering the iodide in a small dose, as M. Guillot and I proposed as long as six years ago. For a man, it is well to begin with fifteen grains (one gramme) a day, increasing the dose if the patient bear it well. It seems to me that inferences of some importance to medicine may be drawn from the facts contained in this memoir. Indeed, the constant aggravation of the symptoms of poisoning upon the administration of iodide of potassium in excess, and the elimination of the poison in a state of combination with one of the elements of the iodide, authorize us in saying that certain medicines act in the first instance on their own account; but may act at the same time by the agency of the materials they meet with in the living body. It is the duty of the physician, therefore, to inquire into the prior history of the patient, even more perhaps than physicians are in the habit of doing, when he is desirous of administering remedies having an action analogous to that of iodide of potassium to individuals formerly subjected to the influence of saturnine or mercurial compounds.

Art. II.

On certain Elastic Structures connected with the deep flexor tendons of the Fingers and Toes. By John Marshall, F.R.C.S.E., Assistant Surgeon to University College Hospital.

Whilst engaged some time since in a careful dissection of the human hand and foot, I found, in connexion with the tendons of the flexor profundus muscle, both of the fingers and toes, certain slender bands of yellow elastic tissue, of which I do not remember to have met with any description. Having, by a sufficient number of independent observations, satisfied myself of the constant presence, in the parts above mentioned, of these small elastic ligaments, I have endeavoured to ascertain in what way they are modified by the several conditions of age, sex, occupation, and disease; and, moreover, so far as my limited opportunities have permitted, I have studied the varieties they present in the limbs of different vertebrate animals. The object of the present communication is to put on record the facts thus accumulated, and to advance some general considerations on the use and import of these somewhat interesting, though, in man at least, apparently not very important structures.

A. *Human Anatomy.*—It is well known to the human anatomist, that the tendons both of the superficial and deep flexor muscles of the fingers and toes, are provided, where they lie in their common sheaths along the phalanges, with certain delicate cords or folds, which pass off from their deep surface, and connect them loosely with the phalanges beneath. These little ligamentous folds did not escape the notice of some of the earlier anatomists, but it appears that they were first more especially examined, delineated and named by Weitbrecht, whose account of them has been either adopted or confirmed in the descriptions of all subsequent anatomical writers.

a. *In the Hand.*—By Weitbrecht (Syndesmologia, 1742), those present in the hand are classed with other similar bands at the wrist and on the backs of the fingers, under the title, *Vincula tendinum accessoria;* those opposite to the phalanges being separately described as situated *circa phalanges digitorum* (op. cit., sect. ii. p. 51). Two sets are pointed out in the hand, as connected with each of the tendons of the flexor sublimis and flexor profundus—viz., the *ligamenta vel vincula brevia et longa.*

The *long* ligaments (*tenacula superiora seu gracilia ;* Auctor.) consist of one, *ligamentum longum perforati,* which passes forward from the first phalanx to the tendon of the flexor sublimis vel perforatus; and of *two* others, *ligamentula longa perforantis,* which commence, one from a membrane joining the split portions of the perforated tendon, and the other from the first phalanx, and are attached to a slight median longitudinal ridge (*crenula*), found on the deep surface of the tendon of the flexor profundus. These ligamentula longa, says Weitbrecht, are not constant, but vary in number and arrangement. They are very slender, and of a filamentous nature, and are generally so long, that even in the utmost extension of the finger, they do not seem able to be stretched. Nor, from their extreme fineness, can we suppose any traction to be exercised through them. Hence he adds, " I dare not determine their use. But, however slender they may be, all of them are seen after injection to be covered with elegant arteries and veins."

The *short* ligaments (*tenacula inferiora seu lata;* Auctor.) are two in number, one for each of the two associated flexor tendons. The *ligamentum breve perforati,* according to Weitbrecht, arises from the *palmar* surface of the first phalanx, opposite the depression found just above the anterior articular extremity; it has a rhomboidal figure, and is attached to the tendon as far as its insertion. The *ligamentum breve perforantis,** which is seen when the deep flexor tendon is slightly raised, "has the form of a triangle (the *triangular frænum;* Theile), the apex of which extends to the insertion of the tendon, whilst the opposite border or base is free and concave. Both of the ligamenta brevia consist of a doubled membrane, and often contain fat within them."

Now, on reviewing this description, which is, for the most part, literally translated from Weitbrecht, it is necessary to remark, in reference to the long ligaments (see fig. 1, *a*, *b b*,) that besides acting as vincula, they undoubtedly serve to convey vessels, and probably nerves, to or from their respective tendons. In regard to the short ligaments, it is likewise certain that they help to keep the tendons in place, and support nutrient vessels; but something more has to be said concerning them. In the first place, the ligamentum breve of the flexor sublimis or perforatus (fig. 1, *c*), is connected not merely with the palmar surface of the first phalanx, but with the palmar or glenoid ligament of the articulation between the first and second phalanges, and also with the synovial capsule of that joint. Moreover, so far as I have observed, it does not extend to the insertion of the perforated tendon, but its lower border is free as well as the upper.† Lastly, this vinculum is short, and non-elastic; and its direction, which is slightly downwards and forwards during the extension of the finger, is changed to upwards and forwards, whenever the finger is strongly flexed. From all which, it appears to me that this ligament performs another mechanical, though probably subsidiary, office, during flexion of the finger—viz., that of drawing upward, and, therefore, away from the action of the joint, the glenoid ligament and especially the loose synovial capsule situated just above it. So, too, the ligamentum breve of the flexor profundus vel perforans (fig. 1, *d*) is connected, by many fibres, with the synovial capsule and glenoid ligament of the articulation between the second and last phalanges, as well as with the palmar surface of the second phalanx, and hence it exerts a like action on these parts, during flexion of the finger, to that just named in regard to the other short ligament.

Occasionally, but only as an exception, I have observed that the apex of the ligamentum breve perforantis does not quite reach forward to the insertion of the deep flexor tendon, but leaves an opening there beneath the tendon. Generally, this condition was met with in the forefinger only, but once it existed in the fore and middle fingers of both hands. Very frequently, I have found a Pacinian body, or two, seated deeply in the apex of one of these ligaments. But the fact to which I wish particularly to direct attention concerning them, and which distinguishes them from all the other vincula accessoria, is, that they contain within their duplicature

* Weitbrecht, Tab. v., figs. 17 & 18, *k*.

† Upper and lower mean proximal and distal; anterior and posterior mean palmar and dorsal.

the *small elastic ligaments*, to which allusion has been made at the commencement of this paper, and which, as they seem to have hitherto escaped notice, it is my special purpose here to describe.

In a characteristic specimen, let us suppose in the middle finger (fig. 1), on gently pulling the deep flexor tendon upward and forward from the bones, the elastic ligament is recognisable without any dissection, embedded

Fig. 1.

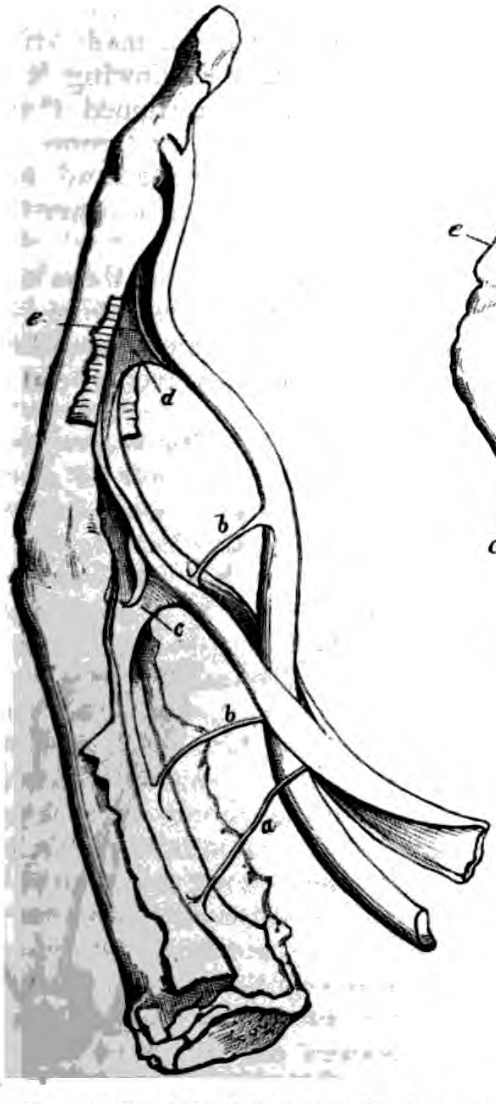
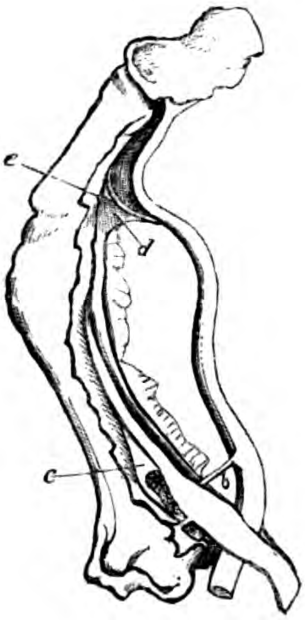


Fig. 2.

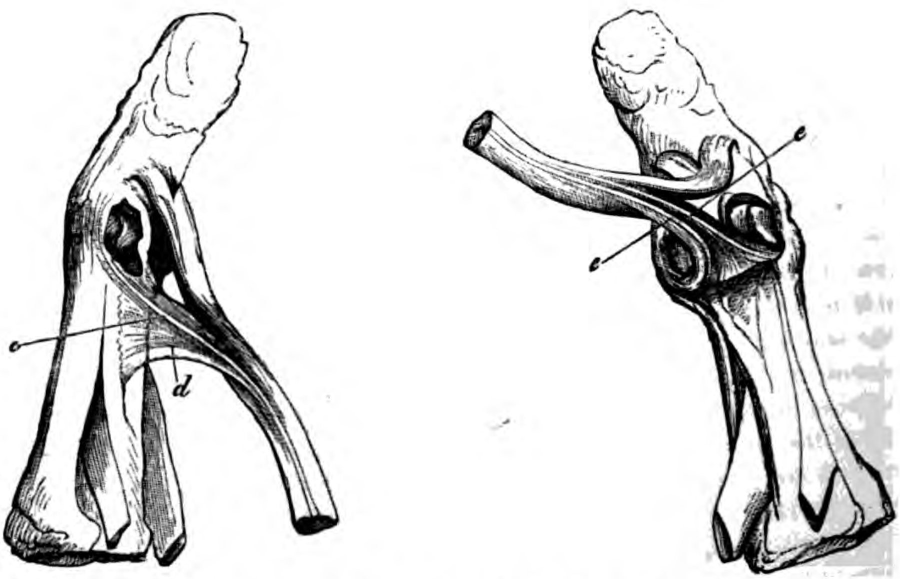


Figs. 1 & 2. Lateral view of middle finger, and of second toe, with their two tendons, and respective vincula accessoria. *a*. Ligamentum longum perforati. *b b*. Lig. longa perforantis. *c*. Lig. breve perforati. *d*. Lig. breve perforantis. *e*. Vinculum subflavum.

in the triangular vinculum (*d*), and appearing on either side as a fine yellowish streak (*e*), running obliquely from the under side of the tendon downwards and backwards in the direction of the last joint of the finger. If, after the tendon has been thus drawn away from the bones, it be now let go again, the yellow streak will be seen to retract or shorten itself, and at the same time to form a very slight ridge on the sides of the triangular vinculum. From its peculiar colour, its distinctness of outline, its special direction, and its remarkable elasticity, this yellow band is easily distinguishable from other whitish and less defined streaks, lying also within the triangular vinculum, which pass directly backwards, or even slightly upwards from the back of the tendon to the second phalanx, and which consist of bands of white filamentous tissue, or of blood-vessels. It is also quite distinct from the interposed bundles of fat. If the flexor tendon be now carefully severed near its insertion into the last phalanx, without injury to the triangular vinculum, or its contained parts, the fact of some elastic structure existing within the vinculum may be made still more decidedly evident, by alternately elongating it, and allowing it to retract. At the same time, it may be thus more readily ascertained that the elasticity resides in the yellow band above alluded to; and, moreover, it may now be seen, that this band itself is attached on the one hand, to the flexor tendon, and on the other, to the second phalanx, in the manner to be immediately described.

The *vincula subflava,* as I would venture to name these small elastic ligaments, may be dissected either from the side, by removing one layer of the duplicature of the synovial membrane of which the triangular vinculum is composed (as shown in fig. 3, *e*), or from their superficial aspect, as

Fig. 3. Fig. 4.



Figs. 3 & 4. Lateral and under or deep view of vinculum subflavum (*e e*), as dissected, to show its attachments and relation to the synovial membrane and articular surface of the second phalanx. In all the figures the vincula are shown as if much stretched.

when the flexor tendon is cut across close to its insertion, or from their deep surface (as shown in fig. 4, *e e*). In this way, each of them, when existing in the most characteristic form, is found to consist of a thin flat sheet of yellow elastic tissue, placed with its surfaces directed forwards and backwards, one towards the deep surface of the flexor tendon, the other towards the second phalanx, and having its borders or margins projecting slightly, as it were, on the lateral surfaces of the triangular vinculum in which it lies. This sheet or plane is narrow or pointed above where it is fixed to the tendon, and broader below, at its attachment to the bone; and it must be observed, that along the middle line, especially at its fore part, it is so thin, that only a few scattered bundles of elastic tissue are recognisable under a good magnifying glass, whilst, on the contrary, this tissue is accumulated in greater quantity along either border. Hence, each vinculum subflavum, in the human subject, appears rather to consist of two small rounded bundles of elastic tissue placed side by side in the same plane, and slightly diverging from each other as they pass downwards from the flexor tendon to the bone. This form will again be referred to, under the comparative anatomy of the part.

The lower, broader, and more fixed extremity of the elastic vinculum is attached to the front of the second phalanx, along a line, closely corresponding to the anterior margin of its lower articular surface, just outside and above the synovial capsule of the last finger-joint. The stouter lateral bundles take origin especially from the neighbourhood of the little bony eminences found at each angle of this articular border. The upper pointed and more moveable extremity of the vinculum subflavum reaches the deep surface of the flexor tendon, just opposite to the lower third of the second phalanx, and is attached along each side of the slight median ridge seen upon the under surface of the tendon. At each extremity, the lateral bundles are slightly spread out, so as to be less compact and defined than in the middle of their course. They are accompanied by many pale and loose bundles of filamentous tissue, which pass from the tendon to the glenoid ligament and synovial pouch of the last joint of the finger, or to the body of the second phalanx.

When completely retracted, the vinculum subflavum of the middle finger, in the adult, is nearly three-eighths of an inch in length; but when elongated as far as the ordinary action of the deep flexor tendon would occasion, it measures about three-fourths of an inch. Its widest part is about a quarter of an inch, and its thickness at the border not so much as one-thirtieth of an inch.

Examined under the microscope, both before and after the addition of acetic acid, the substance of the vincula subflava is found to consist of coalesced plates and fibres, possessing, in a marked degree, all the characters of the yellow elastic tissue.

Such being the condition of the vincula subflava, when most highly developed in the human finger, it is necessary to remark that one continually finds them in various stages of degradation; though, in almost all cases which I have hitherto examined, excepting in disease, I can truly say that a distinct representative of them exists, and in the few instances of the extremest grade of deficiency, scattered elastic bundles are still found, in their usual place. It may be well here, however, to particularize some of the varieties met with, and the conditions under which they occur.

In the first place, the dimensions of the vinculum subflavum correspond generally with the size of the finger: thus, as a rule, it is largest and thickest in the middle finger, next in size in the ring finger, then in the forefinger, and smallest in the little finger. It is generally less easily seen in the forefinger than in the ring or even in the little finger; but this depends partly on its being more obscured by the surrounding filamentous tissue, which is proportionally denser in the triangular vinculum of the forefinger. When this triangular vinculum, in any finger, extends for an unusual distance up the second phalanx and the flexor tendon, I think I have observed that the elastic ligament within it is, for the most part, smaller than usual. On the contrary, when the former has a less extensive attachment, and thus admits of a greater degree of movement, and especially when it is perforated at its apex, then the elastic vinculum is very well marked.

Nearly always one edge or border of each vinculum subflavum is thicker and more prominent than another. In the middle, ring, and little fingers, it is generally the radial border which is more developed and apparent; whereas in the forefinger it is more frequently the ulnar border.

In the *thumb*, the triangular vinculum, as noticed by Weitbrecht, is delicate and long; and I am inclined to pronounce that it possesses no true vinculum subflavum resembling those of the fingers. For all that I have been ever able to find, and that only seldom, is a single, long, slender, semi-transparent cord, passing from the long flexor tendon to the middle of the first phalanx, and not to the neighbourhood of its lower articular extremity. In the section on the comparative anatomy this will again be noticed.

Between the *two hands* I have observed no material difference, except that the vincula subflava, though probably not larger, are, from the greater delicacy of the triangular vinculum, more evident in the *left* than in the *right* hand.

In the *female* hand they are certainly quite as marked, and, in relation to the smaller dimensions of the hand, quite as large, as in the *male*.

In *youth* they are even more easily seen than at subsequent periods of life; but they exist in aged subjects. The influence of peculiar manual occupations, whether involving dexterity or force, I am not in a position to determine from sufficiently numerous observations; but, so far as I can judge, it certainly appears to me that hard labour by no means develops these elastic vincula, which I have usually found more distinct in more delicately formed hands. On the whole, from this circumstance, and from their being, if anything, relatively larger in youth, and having an inverse ratio to the extent of the triangular vinculum, it would seem that freedom of movement in each finger, and not combined exertion, is the condition most favourable to their development.

They are, of course, more distinct, and are certainly more compact, in thin than in fat persons. In deaths from chronic disease, as phthisis, they are as evident as in those from sudden causes. I have had no opportunity of examining them, either in paralysis or in congenital deformity of the hand.

b. *In the Foot.*—The vincula accessoria of the flexor tendons of the toes, which are also well described by Weitbrecht (sect. v., p. 199), are essentially the same as those of the fingers; but they are necessarily smaller and shorter, in proportion to the smaller size of all the parts. The ligamentum breve perforantis, or triangular vinculum (fig. 2, *d*), extends up the phalanx a considerable distance, and contains within its layers the elastic ligament, which appears, when well marked, as a yellowish streak (*e*), on one or both sides. The vincula subflava of the foot are absolutely smaller and shorter than those of the hand, but in proportion to the length and size of the toes they are quite as long and as wide. Their microscopic characters are quite as distinct; they present, however, greater occasional marks of degradation. They are larger in the second toe, and gradually diminish down to the little toe, in which there is often only a small quantity of representative elastic tissue. The tibial border of each vinculum subflavum is the one most usually developed beyond the other. In the great toe, as in the thumb, I have occasionally seen a very long and feeble elastic cord, which passed from the flexor tendon only to the middle of the first phalanx. As in the hand, neither age nor chronic disease produce much effect on the vincula subflava.

In a case of unremedied talipes varus, in a subject certainly seventy years old, the elastic vincula are present, though small. So, also, in a case of talipes equinus, in a body about thirty years of age, they are perfectly distinct—perhaps, even somewhat thicker than usual.

c. *Development.*—In a fœtus at about the third month, it was quite possible to distinguish the superficial and deep flexor tendons of the fingers or toes. At about the fifth month this can be done easily, though they are slightly adherent to each other, and to their common sheath. In a fœtus, born prematurely at the eighth month, the seat of the vincula subflava in the fingers and toes is occupied by a wide, dense mass of tissue, arranged in a linear manner, between the deep tendon and the second phalanx; but it does not possess much elasticity. This tissue resists the action of acetic acid, and appears under the microscope as if composed of clear-looking nuclear fibres. At birth, the yellow colour, defined outline, and special elasticity of the vincula subflava are quite evident; indeed, they seem proportionally thicker, and form a more continuous sheet, or plane, than in after life.

B. *Comparative Anatomy.*—The observations which I have hitherto made on the vincula subflava in the vertebrate animals, impart to the anatomy of these structures an interest which it would not possess, had the inquiry been limited to the human body. But the details of this comparative research, however novel and interesting, being too numerous for record here, I must restrict myself to little more than general statements.

The "vincula tendinum accessoria" of the fore and hind limb, in the vertebrata generally, remind one, in their arrangement, of those in the hand and foot of man; the exceptions being in such cases, for example, as the turtle, where the flexor tendons are blended opposite the phalanges with the surrounding parts; or as the mole, bat, dolphin, or reptilia, in which these tendons are altered in arrangement, small, or wanting. In all cases where the deep flexor tendons run independently to the last phalanges, there is a distinct synovial fold, or vinculum, situated beneath and near its termination, corresponding with the ligamentum breve perforantis in the human digit; but this is always less evident in the thumb and great toe, where they exist, than in the four outer toes or fingers.

It is within these terminal vincula *only*, excepting in the feet of birds, as will be presently explained, that the *vincula subflava* are to be found. They exist *in all vertebrate animals*, so far as I have yet examined, *which possess distinct perforating tendons* for the last phalanges of the fingers and toes. As a rule, they are not met with either in the thumb or great toe, when those parts are present; one exception occurs in monkeys, where a small roundish cord exists in both those digits; but it must be observed, that in the quadrumana the flexors of the thumb and great toe are parts of the flexor perforans. In birds an elastic vinculum is present in the hallux or so-called great toe; but this, it may be remembered, is more probably the analogue of the second toe of mammalia.

In nearly all cases the vincula subflava are larger, in proportion to the size of the limbs, in animals than in man. In certain peculiar modifications of the limbs only, which entail a restricted movement of the individual phalanges—as in the seal, for swimming; or in the foot of the kangaroo, for leaping,—are these structures proportionally smaller than in the human body. In the mole, and in the wings of bats and birds, I do not find them. In all these cases the muscles are peculiarly modified.

They are very large and wide in the monkeys, more slender in form, but well marked, in the cat, and less distinct in the dog; they are strong in the hog, the ox, and the horse; well marked in the sheep and the hare, and weaker in the rabbit. They are very slender, or obscure, or defective, in the kangaroo and seal. In the feet of birds they are proportionally more highly developed than in any other animal, not only as regards size, but in certain respects, to be immediately mentioned. They exist in the crocodile and iguana; but not in the tortoise, turtle, salamander, or frog.

For the most part there is but one vinculum subflavum for each digit. But in many birds, perhaps in most, there is always found one in the hallux or inner toe; a very strong one, consisting of two planes, in the three succeeding toes; and, besides this, a supernumerary vinculum in the outermost, or two outer toes. This last circumstance cannot fail to be associated in the mind with the unusual number of phalanges, which are always four and five, in these toes. Quite as an exception to the rule in the vertebrata generally, there is sometimes found in the largest toe of birds an elastic vinculum, proceeding from the tendon of the perforatus, near the point of its splitting, to the second of the four phalanges of that toe. I may take this opportunity of mentioning, that Meckel* describes the deep flexor tendons of the toes in birds as ending " by slips, which are given off in succession to the ultimate and penultimate phalanges;" but the peculiarity of structure of the slip to the latter does not appear to have been noticed by him, or by any other observer; nor can I find that the vincula subflava have hitherto been described in any animal by comparative anatomists. These elastic structures having escaped notice in the human body, the key to their detection in animals was wanting.

In form, the vincula subflava, in the vertebrata generally, vary between two types—viz., that of a simple undivided sheet, and the bifurcated form. Thus, in monkeys and in certain birds, each vinculum forms a short wide continuous sheet, having thicker rounded margins and a thinner centre; the end attached to the bone being always broader than the one connected with the tendon. In most cases, however, as in the cat, dog, seal, hog, ox, sheep, horse, hare, rabbit, and kangaroo, and in some birds, they are bifid, the two slips of elastic tissue diverging from each other as they pass from the tendon. The bifid vincula are generally longer and more slender than

* System der Vergleichenden Anatomie, Band iii. p. 387.

the undivided ones; as, for example, in the cat, seal, and kangaroo, and some small birds, but not always, for they are short and thick in the hog, ox. sheep, horse, and ostrich. In mammalia, one border of the simple vinculum, or one slip of the bifid structure, is very frequently thicker than the other,—the preponderance generally being in favour of the radial and tibial sides. In the ruminants it is the median border which is the larger in each toe. In birds, I have found no difference in this respect: in these creatures, however, as already mentioned, the vinculum often consists of two planes, one broad, continuous, or bifid, the other of scattered bands of elastic tissue, placed immediately anterior to the other, connected with it closely by cellular tissue, and attached to the glenoid and capsular ligament of the last joint of the phalanges. The supernumerary vinculum of the two outer toes in birds, is long, single, and round in form, and is attached to the second and third of the four and five phalanges found in those toes.

Excepting in the instance of this supernumerary structure in birds, the vincula subflava are always attached to the penultimate phalanx, just above the synovial capsule, along the margin of the articular surface; their thicker borders or lateral slips, as the case may be, being there fixed to the two slight eminences at the inner and outer angles of the articular border; in this respect, as in so many points, reminding us of, and elucidating, the human structure. Hence, too, in extinct animals we may conceive the existence and character of the vincula subflava to be determined. In the cat, the outermost slip of the bifid vinculum is attached to the second phalanx, close to the insertion of the posterior elastic ligament, described by Sir Charles Bell, as serving to retract the claw in the feline tribe; but the one is perfectly distinct from the other.

In animals, contrary to what is found in man, but quite in harmony with the general superior development of the hinder extremity, the vincula subflava are usually more marked in that limb. In monkeys, however, there is no proportional difference between the hind and fore limbs. In the seal and kangaroo, I find them more evident in the anterior limb. No difference is observable between the right and left limbs. The effects of sex and age have not been noted; nor has the development of these vincula been traced. In the adult animal, microscopic examination invariably displays the characteristic appearances of the yellow elastic tissue.

C. *Uses.*—From the almost universal presence of these elastic vincula, and from their greater size in animals, as compared with man, it is obvious that no correct conclusions as to their peculiar office can be arrived at, without taking the widest view of their structure and arrangements.

Strictly mechanical in their office, they are probably most exactly adapted in each case to the requirements of the limb in which they are found, and are thus perhaps exempt from those general morphological laws which regulate the occasional degradation of so many of the special organs of the animal economy. Otherwise, these vincula, when small, as in man, might have been regarded as merely effete representatives or homologues of a prevailing type of structure. This view, however, can scarcely be correct, although their habitual small size, and occasional deficiency, sufficiently prove that in the human body their action must be comparatively subordinate. In the human foot, the movements of the last phalanges are limited; and even the superiority of the hand depends chiefly on the number and isolation of its muscles and tendons, and on the comparatively equable balance between all its volitional movements.

The vincula subflava, considered generally, must act either on the penultimate phalanges, or on the deep flexor tendons, or on both.

a. *Action on the Bones.*—This could only be as subsidiary flexors of the penultimate phalanges, to which the deep flexor muscles ought then to be described as being in part attached. But independently, first, of the disadvantageous direction of the vincula subflava for such a purpose, inasmuch as they are inserted only into the distal end of the bones, instead of near their proximal end, as real tendons always are,—and, secondly, of their total unfitness for the economical transmission of muscular power, owing to their invariably elastic character,—it is evident, that in the majority of cases, including that of man, they are too weak to have more than a very insignificant effect on the flexion of the penultimate phalanges. Besides, the last phalanx must always be more or less flexed before the elastic ligament is drawn upon serviceably at all, in which case the penultimate phalanx must, as is well known, also be already flexed. In monkeys, and in digitigrade animals, in which they are generally well developed, these vincula may help slightly to flex the penultimate phalanges; and in birds, especially, they may serve to render steady, and yet as it were full of spring, the hold or grasp of all the orders of phalanges, during prehension, or while resting on the feet, or in the rapid movements of cursorial progression.

b. *Action on the deep flexor tendons.*—This appears to be the special office of the vincula subflava, and will be best apprehended by reflecting that their elongation and shortening in the natural action of the parts depend on the movements of the last phalanges upon the penultimate bones, and on no other movement of the digits. Thus, in moderate or extreme extension of the last phalanges upon the penultimate bones, the vincula subflava are at rest, their two points of attachment being then nearer together than in any other position of the parts. As soon, however, as the deep flexor muscle contracts, these two points begin to be drawn apart from each other, and the elastic ligaments begin to be stretched. At extreme flexion, they are elongated to the utmost. Hence, whilst the last phalanges are extended, and the deep flexor muscle is quiet, the vincula subflava are idle; but no sooner does flexion begin, than, by that very act, their elasticity is brought out, and they are ready, on the slightest relaxation of the flexor muscle, to draw forward or straighten out its several tendons through the bifurcation of the perforated tendons, and along the sheaths, in which they glide. In this way the vincula instantaneously and smoothly restore to the position of rest that long part of the flexor tendons which is included between the muscular belly and the points of attachment of the elastic vincula themselves. But at the same time—and this is also important—that part of the tendons situated beyond the vincula, between them and the ultimate attachment of each tendon to the last phalanx, is relaxed and put loosely at ease; and, accordingly, indirect but most efficient aid is thus given to the common extensor of these phalanges; a muscle which is comparatively feeble in power, which operates, in the first moment of its action, at a great disadvantage, and which would, without the ever-ready intervention of the elastic vincula, acting as they do in a straight line, and without any expense of volition, have to draw out the long flexor tendons from their canals and sheaths, by voluntary effort, with extreme loss from friction, and with no small expenditure of misapplied power.

In the human hand, and particularly in the foot, this second mode of action of the vincula subflava must be very slight; but in most animals it is unequivocal; and in birds, in which the flexor tendons are very long and complex, and the latitude and rapidity of movement from extension and flexion very great, we find the vincula in their highest state of development, both as to number and strength. This special adaptation of the yellow elastic tissue, so distantly imitated by modern art in the vulcanized caoutchouc, for purposes which it so admirably fulfils, seems to me to be as striking and beautiful as any which have been previously pointed out in the mechanism of the animal frame, and the wide presence of the vincula in vertebrate creatures, vindicates their character for interest and importance. I am conscious, however, that many more observations are necessary to elucidate the varied degrees of power exercised by these vincula —a title to which they truly have every claim, in preference to that of tendon or ligament. Their study is, after all, but part of a larger subject —namely, the comparative and morphological anatomy of the muscles, and their accessories in the vertebrata.

Art. III.

On the Functions of the Muscles of the Tympanum in the Human Ear. By Joseph Toynbee, F.R.S., Fellow of the Royal College of Surgeons in England, Aural Surgeon to St. Mary's Hospital; Consulting Aural Surgeon to the Asylum for the Deaf and Dumb; and Consulting Surgeon to the St. George's and St. James's General Dispensary.

1. *The Articulation of the Stapes.*—The Stapes is generally described by anatomists as being connected with the margin of the fenestra ovalis by a simple membrane. Sir Anthony Carlisle, in his paper on the Physiology of the Stapes, merely speaks of "a membrane which connects it to the edges of the fenestra vestibuli."* Professors Sharpey and Quain agree with Sir Anthony Carlisle. They say, "The annular ligament of the stapes (ligamentum orbiculare vel annulare baseos stapedis) connects the base to the margins of the foramen fenestra ovalis. The fibres of the ligament are covered on the outer side by the mucous lining of the tympanum, and on the inner side by the membrane of the vestibule."† Mr. Wharton Jones describes this ligament as springing "from the margin of the vestibular fenestra, and it is inserted into the jutting margin of the base of the stapes all round."‡ Sœmmering seems to have had a different view respecting this articulation. He says, "A thin articular capsule connects the base of the stapes to the fenestra ovalis."§

If the circumference of the base of the stapes be carefully examined by

* Philosophical Transactions, p. 201. 1805. † Elementary Anatomy, p. 940. 1848.

‡ Cyclopædia of Anatomy and Physiology, vol. ii. p 548.

§ De Corporis Humani Fabrica, tomus secundus, de Ligamentis Ossium, p. 10. Huschke states that "Sœmmering was wrong in regarding this ligament as a capsule;" Huschke speaks of the *ligamentum annulare* only. The quotations from Huschke have been made since the paper was completed. See Encyclopædia Anatomique, par T. L. G. Bischoff, J. Henle, E. Huschke, &c., traduit de l'Allemand, par S. J. L. Jourdan. Tome v. p. 773. 1845.

means of a lens magnifying between three and four diameters, it will be apparent that instead of a fine margin only, it presents a distinct surface, which when *in situ* looks towards the border of the fenestra ovalis, and is separated from the inner and outer faces of the base by well-defined margins. This circumferential surface of the base of the stapes varies in breadth at different parts. The broadest part looks backwards, measures about a third of a line at its centre, and gradually narrows as it becomes continuous with the superior and inferior surfaces. This broad part, instead of looking directly backwards, is oblique, the direction of its surface being obliquely backwards and outwards. The *anterior* extremity of the circumferential surface of the base is not so broad as the posterior; and instead of being oblique, it is somewhat rounded. The upper and lower surfaces of the base of the stapes are narrower than the anterior and posterior portions; their middle part is the narrowest. (Fig. 1.) When examined in a recent ear, the circumferential surface of the base of the stapes is found to be quite smooth, and covered by a very delicate layer of cartilage, which communicates a soft sensation to the finger when it is touched by a fine probe. The cartilage is most abundant at the two extremities, from which portions of sufficient magnitude can often be removed, especially in young persons, so as to admit of their being examined by means of the microscope. It consists of oval corpuscles, very similar to those in ordinary articular cartilage, but considerably smaller.† The surface of the fenestra ovalis, to which the circumference of the base of the stapes is applied, is larger than that of the stapes. (Fig. 3.) The posterior surface does not quite correspond in its direction with that of the stapes; it looks directly forwards, instead of obliquely inwards and forwards, to face the stapes, which, it will be remembered, at this part, looks obliquely backwards and outwards. The articulating surface of the fenestra ovalis is smooth, and has a very compact appearance; no cartilage is detected upon it. It is bounded by two well-defined ridges. The circumference of the base of the stapes is attached to that of the fenestra ovalis by means of two membranes or ligaments. The inner or vestibular ligament passes from the inner margin of the fenestra ovalis to the inner margin of the circumference of the base of the stapes. The outer one passes from the outer margin of the fenestra ovalis to the corresponding margin of the base of the stapes. These two ligaments have between them a space which may be called the articular cavity; this cavity contains a sufficient quantity of fluid to lubricate the articulating surfaces of the bones. (Fig. 4.) By the action of the tensor tympani muscle, the base of the stapes is pressed inwards towards the vestibule as a piston in its cylinder; as soon as the tensor tympani muscle ceases to act, the ligaments above described being elastic, draw it outwards again.

Fig. 1.*

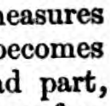


Fig. 3.

Fig. 4.‡

* The stapes from the right human ear. The circumferential surface of the base of the stapes is well seen in the subjoined drawing from the porpoise. (Fig. 2.)

Fig. 2.

† Huschke, speaking of the bones of the ear, says—"Their articular surfaces are encrusted with cartilage, which is covered by synovial membrane." He does not appear, however, to look upon the circumference of the base of the stapes as an articular surface, inasmuch as he describes a ligament as attached to it.

‡ A diagram of a horizontal section through the base of the stapes and the fenestra ovalis, showing the ligaments and the articular cavity.

2. *The Movements of the Stapes.*—The stapes is moved by two muscles, the *tensor tympani*, and the *stapedius*. Anatomists appear to agree, that the action of the *tensor tympani* is to press the stapes directly inwards towards the cavity of the vestibule; the general opinion appears to be, that the *stapedius* muscle merely assists the tensor tympani. Thus Mr. Wharton Jones says, "The first action of this muscle (the stapedius) will be to press the posterior part of the base of the stapes against the vestibular fenestra. At the same time the long branch of the incus will be drawn backwards and inwards, and the head of the malleus being, by this movement of the incus, pressed forwards and outwards, its handle will be carried inwards, and the membrana tympani thus put on the stretch. Breschet calls the muscle of the stapes a *luxator*, but I do not know on what grounds."* Professors Todd and Bowman write, "In contraction it (the stapedius muscle) would fix the stapes by pulling its neck backwards. It probably compresses the contents of the vestibule."† Ellis states that "it assists in retaining the stapes applied to the fenestra ovalis."‡ Muller writes, "The influence of the stapedius muscle in hearing is unknown . . . . The only effect which it appears to me could be ascribed to it, would be to render tense the membrane by which the base of the stapes is connected with the margin of the fenestra."§

On account of the smallness of the stapedius muscle, and the very slight degree of movement which it produces, it is difficult to determine in what way the stapedius muscle influences the contents of the vestibule. As the tendon of the stapedius muscle in its course forwards, passes slightly upwards, there is every reason to infer that it draws the neck of the stapes backwards and slightly downwards, and that it produces a slight rotation of the base. That this rotatory movement of the stapes has the effect of slightly withdrawing its base from the cavity of the vestibule is, I think, shown by the following experiment. The tympani cavity and stapedius muscle being exposed, the stapes is to be left in situ. By means of a small pair of cutting forceps a section is to be made through the cochlea, a portion of which should be allowed to remain connected with the vestibule. The scala vestibuli of this portion will be observed to be filled with fluid as far as the margin of the section, which fluid is of course continuous with the perilymph in the cavity of the vestibule. If the stapedius muscle be now pulled, or if the neck of the stapes be moved slightly backwards, the fluid in the exposed part of the scala vestibuli will be found to recede slightly into the scala vestibuli, and its surface to become concave; as soon as the stapes is allowed to return to its quiescent state, the fluid again passes into, fills the scala vestibuli, and assumes a rounded surface. Independently of this action on the contents of the vestibule, the stapedius muscle produces a slight relaxation of the membrana tympani. This is effected by the neck of stapes, in the act of rotation, passing outwards as well as backwards, whereby it presses slightly outwards the inferior extremity of the incus, while the body of the latter bone passes inwards, carrying with it the head of the malleus, thus necessarily causing the long process of the latter bone and the membrana tympani to pass outwards. It would therefore appear that the stapedius muscle is

* Cyclopædia of Anatomy, vol. ii. p. 549. † Physiological Anatomy, 1847, part iii. p. 71.
‡ Demonstrations of Anatomy, p. 286. § Elements of Physiology, by Baly, vol. ii. p. 1264. 1842.

the direct antagonist of the tensor tympani muscle, the former relaxing the labyrinthine fluid, the membrana fenestræ rotundæ, and the membrana tympani, and the latter rendering tense the labyrinthine fluid and the two membranes. This view is supported by the fact that the stapedius muscle is supplied by a branch from the portio dura nerve, and the tensor tympani by a branch from the otic ganglion.* It may therefore, I think, be fairly inferred that the function of the tensor tympani muscle is to protect the membrani tympani and the labyrinth from injury during loud sounds, while the stapedius muscle places these structures in a position to be impressed by the most delicate vibrations; and it would appear to be brought into action during the process of listening. Instances are not uncommon in which these two muscles are not able to act promptly, and the unpleasant consequences are manifest. Thus the loud noise produced by firing a cannon near to a person without any expectation of it on his part, before the tensor tympani muscle has time to contract, is often followed by the sensation of singing or buzzing in the ears, produced, most probably, by a concussion of the expansion of the auditory nerve; these sensations often endure during many years. Cases are not unfrequently met with in which the mucous membrane of the tympanum is thickened, and a considerable amount of dulness of hearing is the consequence; many patients thus affected hear sounds—the human voice, for instance—perfectly well when they are listening, but as soon as the act of volition is suspended, the same voice in the same position is not perceived. In these cases it would appear as if the action of the stapedius muscle were requisite to counteract the pressure upon the stapes by the thick mucous membrane. The friends of young persons suffering in this manner often imagine that there is no dulness of hearing, but merely a want of attention; the fact being, that the power of hearing certain sounds exists in these patients only during the exercise of an effort of the will, instead of being involuntary.

The tensor tympani muscle appears to be of use, not only to prevent the membrana tympani and labyrinth from being injured by powerful sonorous vibrations, but also to protect these organs from the forcible pressure of air, or of a foreign body. Thus the membrana tympani offers considerable resistance to the pressure of a foreign substance which is introduced into the meatus slowly; but the sudden and unexpected contact

* Since writing the above description my attention has been drawn to a monograph on the ear, by Huschke, above alluded to, in which he has arrived at similar conclusions respecting the functions of the stapedius muscle to those here advanced. As it is evident, from the quotations of writers on the ear, made above, that these views have not been entertained, I have not scrupled to publish my researches at length, although I withdrew them from a paper on the Physiology of the Tympanum and Eustachian Tube, which has been read before the Royal Society. The following are Huschke's words:—"While it (the stapedius muscle) presses the posterior extremity of the base of the stapes upon the posterior part of the border of the fenestra ovalis, it lifts the anterior extremity of this bone, and covers the fenestra. At the same time, the descending branch of the incus, with the stapes, is drawn backwards, by which the body of this bone presses the malleus forwards, and as its handle rests upon the membrana tympani it relaxes it. I have often observed this movement of the malleus when I moved the long branch of the incus in the direction of the tendon of the muscle of the stapes; I thus regard the latter as relaxing the tympanum and opening the labyrinth; that is to say, according to the view of Treviranus, it is the antagonist of the tensor tympani muscle. The two have altogether much analogy; they describe an arch looking upwards, pass over a kind of pulley, and are contained in an osseous canal; but they have also opposite functions; the stapedius muscle passes from behind forwards; the tensor tympani from before backwards; the stapedius receives its nerve from the facial, the tensor tympani from the fifth."*

* Encyclopædia Anatomique, tome v. pp. 782, 783.

of a similar body often produces extensive laceration of it. Again, a violent blow on the ear with the palm of the hand rarely produces mischief to the membrana tympani when its reception *is expected,* whereas, a comparatively gentle blow, when not expected, frequently produces not merely a concussion of the nervous labyrinth and very serious derangement of its functions, but the membrana tympani itself is not uncommonly ruptured.*

The preceding observations indicate that one function, at least, of the vesicles and muscles of the tympanum and the membrana tympani is to act as the analogue of the iris in the eye, and to regulate the amount of sonorous undulations that are to pass to the labyrinth. This view has already been to a certain extent alluded to by previous writers. M. Savart, in the course of his very interesting researches upon the functions of the membrana tympani, arrived at a somewhat similar opinion, although he did not point out the manner in which the muscles acted on the labyrinth and membrana tympani. He says, "Les osselets ont encore pour fonction de modifier l'amplitude des excursions des parties vibrantes des organes contenus dans le labyrinthe."† Mr. C. Brooke, in a lecture delivered at the Royal Institution, in the year 1843, says, "This osseous arrangement may be considered to perform an office in the ear analogous to that of the iris in regard to light,—namely, that of regulating the tension of the various structures that are thrown into a state of vibration, according to the pitch and intensity of the sound to be transmitted to the sentient nervous fibres. This was effected by the conjoined action of the tensor tympani and stapedius muscles, by which the tympanum would be rendered more tense, and a simultaneous change in the position of the stapes would alter the tension of the fluid throughout the labyrinth, and therefore also the tension of the membrane of the fenestra rotunda which intervenes between that fluid and the air in the tympanic cavity."‡ Professors Todd and Bowman state that there is "much reason to suppose that the tensor tympani muscle is analogous in its use to the iris, and destined to protect the organ from too strong impressions."§

The first effect of the destruction of the membrana tympani gives weight to the opinion here advocated. Mr. Busk lately detailed to me the particulars of a case in which, for a few days after the destruction of the membrana tympani, a patient was unable to endure the whistling of a patient in an adjoining bed; and Cheselden says, that after destroying the tympanum in both ears of a dog, "for some time it received strong sounds with great horror."||

* I may mention two cases illustrative of the above statement. The *first*, of an eminent physician in London; while playing with his children, he received a blow on one ear from the head of one of them suddenly and rapidly coming into contact with it; from that time (at least four years since) to the present, there has been a constant singing in the ear. The *second* case is of a young gentleman, now under my care, who, in play, received a box on the ear from his tutor, who came silently behind him; this was followed by a slight bleeding and some pain: on examination, I observed a lacerated orifice in the membrana tympani.

† Recherches sur les Usages de la Membrane du Tympan et de l'Oreille Externe, par M. Felix Savart. Lu à l'Académie Royale des Sciences, le 29 Avril, 1822. Journal de Physiologie, par F. Majendie. Tome iv. p. 183.

‡ Lancet, 1843, p. 380. § Physiological Anatomy, part iii. p. 91.

|| The Anatomy of the Human Body, fifth edition, 1740, p. 305.

ART. IV.

On the Specific Gravity of the Brain. By W. H. O. SANKEY, M.D. Londin., Resident Medical Officer of the London Fever Hospital.

THE observations on the specific gravity of the brain, the analysis of which forms the subject of the following pages, were commenced in December, 1846, and have been continued up to the present time.

The subjects of the observations were all patients admitted into the London Fever Hospital, under the care of the physicians, Dr. Tweedie and Dr. Southwood Smith.

All the notes used in the sequel were made at the date of the observations they record; those in the symptoms were written by the bed-side of the patient; and the morbid appearances discovered after death, and the results of the examination of the specific gravity, were committed to paper during the progress of the autopsy.

The notes of the symptoms were made by various observers; the larger number of them, by Dr. Jenner or myself; in some instances, by Dr. G. Birkett and Mr. Humphry. The descriptions of the pathological changes in the viscera have, in many instances, especial value, from the circumstance that they were made by Dr. Jenner, Professor of Pathology in University College, London, to whom also I am indebted for much important assistance afforded me through every stage of this investigation. I am also indebted to the same friends, above mentioned, for assistance in many of the autopsies. All the patients, whose cases are used, were seen repeatedly by myself, and I have taken part in all the post-mortem examinations. The observations on the specific gravity were taken by myself, and for their accuracy I am alone responsible.

The mode employed for taking the observations on the specific gravity requires a brief description. The apparatus used consisted simply of a number of tall glasses, each filled with a fluid of a different, but of a known density; the glasses being arranged in a line, and placed according to the density of their contents. The fluid made use of in the present observations was a solution of common salt. The number of glasses required varies according to the viscus which is to be examined; for taking the specific gravity of all the viscera, about twenty-four glasses are necessary; but for the brain alone fourteen or fifteen are sufficient.

The fluid in the different glasses was adjusted in my apparatus only to every alternate numeral of the same decimal; for example, for testing the brain, the fluid in the first glass was adjusted to the specific gravity of 1·050, that in the second glass to 1·048, in the third to 1·046, and so on to the specific gravity of 1·024. The most convenient method of adjusting the fluids to the density required, and maintaining them constantly at the same degree, is to keep always in each glass two of the specific gravity bubbles, which are sold by the barometer makers. Into the fluid of the density 1·050, for example, the bubbles marked 1·051 and 1·049 are placed. So long as the fluid continues at the density of 1·050, the bubble marked 1·051 will float, and that marked 1·049 will sink in it, and should any change take place in the density of the fluid, the bubbles will indicate the alteration by an alteration in their position; but as long as the bubble marked 1·051 floats, and that marked 1·049 sinks, the fluid will be sufficiently near the mean density of the two—that is, 1·050—for all practical purposes.

The mode of taking an observation with the above apparatus is as follows:—The glasses having been arranged according to the density of the fluid they contain, and the bubbles in their proper position, one being at the surface of the fluid in each glass, and the other at the bottom of the glass, to test the density of the white matter of the brain, a small piece is cut from the centre of one of the hemispheres of the brain, and entirely separated from any of the grey substance. Small portions are then dropped from a height of one or two inches above the glass into the fluid, say of the density of 1·041; should it float in this fluid, a second piece is to be dropped into the glass containing the fluid of the specific gravity of 1·039; should it float in this also, a third piece must be dropped into the fluid of the density of 1·037, and so on until the fluid is arrived at in which it will sink. If, for instance, a portion of the white matter floated in all the fluids tried, at and above the density of 1·039, but sank in the fluid of the density of 1·037, its own specific gravity would be 1·038, or of a density the mean, or sufficiently near the mean, of the two fluids.

To avoid error, the operation should be repeated two or three times, and on the white and grey substance of each hemisphere separately.

The sources of error in making an observation on specific gravity by this mode are the same as those which pertain to all other modes—viz., the adhesion of bubbles of air to the piece under examination, or to the bubbles used, &c., &c.; but besides these, there are others peculiar to the method, which are due to the use of the salt. Most of the tissues of the body will imbibe common salt, and thus after a short time will sink in the fluid in which they at first floated.

This error was avoided by noting the first effect that followed the immersion of the tissue under examination; but since the present observations were made, I have endeavoured to find a fluid which would be free from the objection that pertains to a solution of salt; and I am inclined to believe, from experiments instituted, that a solution of sugar will be found entirely free from the evil complained of.

ANALYSIS.

GREY MATTER.

§ I. The specific gravity of the grey matter of the brain was ascertained in 73 subjects; the lowest density was 1·028, the highest 1·046: the mean of all the cases 1·0346.

§ II. Of the 73 brains examined, the density of the grey matter in rather more than a quarter (or 17) was 1·032; and in rather more than one-fifth (or 15), 1·034. In nearly half it varied between 1·032 and 1·034; and in two-thirds between 1·032 and 1·036.

WHITE MATTER.

§ III. The specific gravity of the white matter was ascertained in 75 cases; its mean density was 1·0412. The highest density noted was 1·048, the lowest 1·032.

§ IV. In nearly one-fourth the specific gravity of the white matter was 1·042. In about as many it was 1·040, in rather less than an eighth, 1·038, and in the same proportion of the whole, 1·041 and 1·044. In the next largest number of cases it was 1·043.

§ V. So that in the bulk, or six sevenths of the whole, the density of the white matter varied between 1·038 and 1·044.

(A.)

Influence of Sex.

GREY MATTER.

Sp. Gr. 1·0	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	Total.
Males ...	1	...	1	...	10	3	7	2	...	...	3	2	...	1	5	1	...	...	...	36
Females	1	1	1	1	7	1	8	1	9	...	...	3	2	1	...	...	...	...	1	37
Total ...	2	1	2	1	17	4	15	3	9	...	3	5	2	2	5	1	...	...	1	73

§ VI. *Males.*—The specific gravity of the grey matter was ascertained in 36 males. The average of all the observations is 1·0353.

§ VII. *Females.*—The grey matter was tested in 37 females. The mean of the observations is 1·0349.

§ VIII. The means show, therefore, no material difference between the sexes with regard to the density of the grey matter.

§ IX. Or by another mode of comparison the specific gravity of the grey matter was 1·032 in 10 of the 36 males, and in 7 of the 37 females: it was 1·034 in 7 of the males, and 8 of the females. But the density most frequently met with among the males was 1·032, while that most often observed among the females was 1·036.

§ X. So that while the mean density of the grey matter is slightly higher in males than in females, the second mode of analysis tends rather to prove that the most frequent density in females is higher than that which is most frequent in males.

(B.)

WHITE MATTER.

1·0	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	Total.
Males ...	1	...	...	1	...	...	5	1	7	5	9	1	5	...	3	...	...	38
Females	...	...	...	...	1	...	4	1	9	3	8	4	3	2	1	...	1	37
Total ...	1	...	...	1	1	...	9	2	16	8	17	5	8	2	4	...	1	75

§ XI. The mean specific gravity of the white matter, in males, as deduced from 38 observations, is 1·0410, and in females, from 37 observations, it is 1·0414.

§ XII. A very marked correspondence, also, exists in the variations. By a reference to table B it will be seen that in the columns in which the greater number of cases are placed, as in those denoting the densities 1·038, 1·040, and 1·042, the number of cases are respectively, 4 males, 5 females; 7 males, 9 females; 9 males, 8 females.

§ XIII. It follows, therefore, so far as the number of my observations is capable of showing, that there is a great similarity in the density of the white substance in the two sexes.

(C.)

Influence of Age, and Age and Sex.

GREY MATTER.

Ages.	Males.		Females.		Both Sexes.	
	No. of Obs.	Mean Sp. Gr.	No. of Obs.	Mean Sp. Gr.	Total.	Mean Sp. Gr.
Under 15	3	1·0336	2	1·0340	5	1·0337
15 to 20	4	1·037	11	1·0353	15	1·0357
20 to 30	3	1·037	6	1·037	9	1·037
30 to 40	9	1·0355	4	1·0336	13	1·0349
40 to 50	9	1·0355	7	1·0358	16	1·0356
50 to 60	2	1·033	1	1·0290	3	1·0316
60 to 70	6	1·0365	4	1·0325	10	1·0349
70 to 80			2	1·0320	2	1·0320
Total ..	36	1·0354	37	1·0348	73	1·0346

§ XIV. Though the above table is not sufficiently extensive to give any accurate result, yet it appears to show, that the density of the grey matter is highest between the ages of 15 and 30 years in males, and 20 and 30 in females, or between 20 and 30, sex not considered.

§ XV. Or, by another method of examining the subject, it will be found that in 22 of the 36 males, the density of the grey matter was below the mean, i. e., 1·035 (§ vi.), and in 12 above it.

§ XVI. The average age of those, the grey matter of whose brain was below the mean, was 39 years. The age of those, the grey matter of whose brain was above the mean, 36 years.

§ XVII. By taking the same average for females, the actual density being 1·0349 (§ vii.), then there were 20 cases in which the density of the grey matter was below the mean, and 16 in which it was above it. The average age of the females in whom the density of the grey matter was below the mean, was 38 years, and of those in whom it was above the mean, 27 years.

§ XVIII. Again, in 5 cases, males and females, the density was below 1·031, and in 11, 1·040 and upwards. The mean age of those in which it was below 1·031, was 49 years. The mean age of those in which it was above 1·040, was 29 years. So that by this mode of analysis, also, it would appear that the density of the grey matter is greater in the early period of life than at advanced ages; and with regard to the influence of sex, it will be seen by reference to table A, that of the 11 observations in which the density of the grey matter was at or above 1·040, seven pertained to males, and four to females; and of the five in which it was below 1·031, two to males, and three to females.

(D.)—*Influence of Age, and Age and Sex combined.*

WHITE MATTER.

Ages.	Males.		Females.		Both Sexes.	
	No. of Obs.	Mean Sp. Gr.	No. of Obs.	Mean Sp. Gr.	No. of Obs.	Mean Sp. Gr.
Under 15	4	1·0380	2	1·0435	6	1·0398
15 to 20	5	1·0398	11	1·0405	16	1·0401
20 to 30	3	1·0416	6	1·0410	9	1·0412
30 to 40	9	1·0419	4	1·0400	13	1·0413
40 to 50	8	1·0418	7	1·0421	15	1·0419
50 to 60	3	1·0410	1	1·0420	4	1·0412
60 to 70	6	1·0413	4	1·041	10	1·0411
70			2	1·041	2	1·0410
Total......	38	1·041	37	1·0417	75	1·0413

§ XIX. By referring to the last column of the above table, it will be seen that the mean specific gravity of the white matter rises very slightly and regularly till the patients attained the age of 50 years, and then gradually falls. The same variations will be observed in the column devoted to specific gravity in the separate sexes; but the rise and the fall are much less regular in the column of the females than in that of the males. The variation, however, in all, is insignificant in extent.

§ XX. Or to examine the subject in the mode used with regard to the grey matter, and taking the mean of the white matter at 1·041 (§ xi.), then in 30 cases the specific gravity of the white matter was less than the mean—viz., in 15 males and 15 females, the average of the males being 31 years, of the females 30 years. In 37 the density was above the mean—viz., in 18 males and 19 females, the average age of the males being 38 years, that of the females 36 years.

§ XXI. This mode of examining the subject, therefore, leads to the same conclusion—viz., that there is very little variation in the density of the white matter at different ages, or in the different sexes.

I proceed to examine the effect on the specific gravity of the length of the period that elapsed between death and date of observation.

(E.)—*Influence of Post-Mortem Changes.*

No. of Observations.	Period of the examination. Hours.	Average hours at which the examination was made.	Mean Sp. Gr.	
			Grey.	White.
6	Under 12	8	1·0393	1·0416
26	24	23	1·0359	1·0414
9	36	29	1·0340	1·0405
19	48	44	1·0347	1·0417
9	72	68	1·0330	1·0417

Average hour after death at which all the observations were made, 34.

§ XXII. It appears from the above table, that the mean density of the grey matter is less the longer the post-mortem examination was deferred, while with the white matter no such effect is apparent.

§ XXIII. The difference between the first and second series, or between those examined at an average of eight hours, and those examined at an average of 23 hours after death, is much greater than, and bears no proportion to, the difference that is found between any two of the succeeding series. In attempting to account for this high degree of density observed in the first series, and the great difference that exists between it and the rest, the question naturally occurs, was there any peculiarity in the symptoms of these cases, that rendered an early examination of the brain advisable? Were they head cases? and was the autopsy hastened lest time should have effaced the traces of disease? This does not appear to have been the fact; for of the six patients, the post-mortem examination of whom was made thus early, two had severe cerebral symptoms, two slight delirium, and two died sensible. The actual specific gravity of the grey matter of the brain, in these six cases, was as follows:—of two it was 1·042, of one 1·041, of one 1·036, and of one 1·033; that the density of the grey matter would be found high, if the examination were made at a very early period after death, is not, therefore, universally true.

§ XXIV. In continuing the analysis of the rest of the series (and six cases are insufficient to draw conclusions from), it still appears that there is a pretty regular decrease of density, equal to about ·001 for every 24 hours that intervenes between the death and the autopsy. Is this due to changes in the brain after death? This question admits of being answered by a direct experiment. A portion of the brain of a man, killed by the bite of a cobra di capello, was examined 31 hours after death; the density of the grey matter was 1·039, of the white, 1·042. It was again examined 79 hours after death, when the grey matter had a density of 1·038, the white of 1·043. Again, seven days after death, the specific gravity of the grey matter was 1·036, of the white, 1·044. The portion of brain examined was simply wrapped in paper during the time that elapsed between the first and second examination, and it lay uncovered in the dead-house, in a plate, between the second and third experiments, the weather being cold and wet. At the time of the last examination, the outer portions of the grey matter were becoming slightly mouldy, and the white, at the thinnest parts, slightly dry and yellowish, or horny-looking.

§ XXV. The inference to be drawn from the above experiments appears to be, when the facts are closely examined, different from that which suggests itself on the first view. At first sight, these experiments appear to show that the lower densities of the grey matter in all my observations were mainly occasioned by the length of time that elapsed between death and the date of the autopsy, and that the greater densities met with were attributable to the early hour at which the post-mortem examination of the brain was made. The experiments do, indeed, show that the lapse of time, or its concomitant (i. e., probably decomposition), decreases the density of the grey matter. But they equally prove that the same influences, after acting under circumstances which might be supposed to be much more favourable to decomposition, as exposure to the air, &c., only reduced the grey matter, after the lapse of a week, to the density of 1·036.

Now, in none of the cases used in this analysis was the autopsy deferred beyond 72 hours, and the above reduced density of 1·036, is still much higher than the mean of all the cases.

§ XXVI. It will be seen that in the experiment just cited, the specific gravity, taken 79 hours after death, was just ·001, lower than that taken 31 hours post mortem; and this decrease will be found to be in exact accordance with the mean of density of the grey matter noted in all the series of table E, except that between the first and second period.

(F.)

Influence of Season.

No. of Observations.	Months in which the Observations were made.	Mean Sp. Gr. of Grey Matter.	Mean Sp. Gr. of White Matter.	Average number of hours at which the Autopsy was made.
17	June, July, and August	1·0348	1·0401	28
22	November, December, January, & February	1·0350	1·0418	37

§ XXVII. The difference exhibited in the above, between the observations made in the summer and those in the winter, is ·0002. But with respect to the grey matter, since the average time after death at which the observation was made, was less in the summer by nine hours, the density should be greater by nearly ·0004, (§ xxiv.) After making this correction, the difference in the density of the grey matter in the summer and the winter months will be ·0002; the summer being the period at which the density was greater, the difference however, is trivial.

But although the interval which elapsed after death, before the examination was made, appears to exercise some influence on the density of the grey matter, yet it is obvious that the marked increase in those cases in which the density of the grey matter was found to exceed 1·040, was not wholly due to this cause; for out of the eleven observations in which the specific gravity of the grey matter was 1·040, or more (Table A),

4	were examined	12	hours after death,
2	„	24	„
1	„	36	„
3	„	48	„
1	„	50	„

While with respect to season or temperature,

1	was examined in	January,
2	„	February,
3	„	March,
2	„	May,
2	„	June,
1	„	August.

§ XXVIII. From the above it is also evident that the first six cases in table E, in which the grey matter had an average density of 1·0393, and which were the only cases examined at so early a period after death, may have owed their greater density to other influences than that under consideration.

(G.)

Weight of the Brain in Adults, and the Specific Gravity of the White and Grey Matter.

	Sex.	Age.	Sp. Gr. of Grey Matter.	Weight of Cerebrum.	Weight of Cerebellum.	Sp. Gr. of White Matter.
1	M.	38	1·032	33 ozs.	6 ozs.	1·041
2	F.	53	1·029	33½ ,,	5½ ,,	1·042
3	F.	65	1·034	34½ ,,	5½ ,,	1·042
4	M.	60	1·033	37¼ ,,	5 ,,	1·046
5	M.	65	1·034	38½ ,,	6¾ ,,	1·040
6	F.	20	1·036	39 ,,	5½ ,,	1·044
7	M.	55	1·034	39½ ,,	6½ ,,	1·040
8	F.	22	1·032	40 ,,	6¼ ,,	1·042
9	F.	65	1·028	40½ ,,	5½ ,,	1·042
10	F.	20	1·036	41 ,,	5½ ,,	1·043
11	M.	51	1·032	44 ,,	6¾ ,,	1·044
12	M.	26	1·035	46 ,,	6½ ,,	1·042
13	M.	25	1·034	48 ,,	7 ,,	1·041
14	M.	37	1·032	51 ,,	6½ ,,	1·044

The above table is arranged according to weight of the cerebrum, the lightest being placed first.

§ XXIX. The number of cases in adults, in which the weight of the cerebrum and cerebellum was taken, does not amount to more than fourteen, but from the above table there appears to be no correspondence or relation between the weight and the specific gravity of the brain; the heaviest brain neither has the lightest specific gravity, nor the contrary; nor has the lightest brain the lowest specific gravity of either white or grey matter.

(H.)

Influence of the Duration of the Last Illness on the Specific Gravity.

Died.	No. of Observations.	Mean Specific Gravity.		Autopsy: Hours after death.
		Grey Matter.	White Matter.	
Before 7th day	5	1·0412	1·0420	17
From 7th to 14th	21	1·0350	1·0414	37
,, 14th to 21st	14	1·0351	1·0416	27
,, 21st to 28th	6	1·0345	1·0431	38
,, 28th to 35th	4	1·0355	1·0407	45
,, 35th to 60th	4	1·0327	1·0406	41
Longer periods	5	1·0308	1·0404	37
Total	59	1·0349	1·0414	

§ XXX. It will be seen that there is a wide difference between the density of the grey matter in those cases which terminated before the seventh day and those that ended fatally after that period. There also appears in the above table a general tendency in the grey matter to decrease in density as the length of the last illness increases, but the decrease proceeds much less rapidly after the seventh day than it appears to do up to that date.

§ XXXI. The great difference between the density of the grey matter in the first and second lines of the above table (H), closely resembles the difference between the first and second lines of table E, in which the influences of post-mortem changes are recorded. It becomes a question, therefore, whether in table H the great density observed in the first line is due to the period at which the autopsy was performed. On examination this does not appear to be the cause, for although the average number of hours at which the brain was examined was considerably less in the first line of table H than in the rest of the series, yet it is too great to account for the excess of density over the mean; for the density, it will be seen, is even higher in table H than in table E, though the autopsy was performed later in table E than in table H.

§ XXXII. But if persons dying before the seventh day have the grey matter of the brain of a much higher density than those dying after a longer illness, it becomes a question whether this greater density is the normal condition of the grey matter, unaltered on account of the speedy termination of the disease; or, on the other hand, is the greater density a pathological change in the brain, and the cause, or at least co-existent with the cause, of the rapid death. If the former were wholly the case, we should expect that the symptoms during life, and the cause of death, would be referable to some other viscus than the brain. Or, on the latter supposition, we should naturally infer that grave cerebral symptoms would precede death.

§ XXXIII. The causes of death and the symptoms exhibit both conditions. The causes of death were pueumonia with granular kidneys and disease of the heart in two cases: double pleurisy with effusion, and diseased liver and kidneys, in one; acute encephalitis in one; scarlatina in one.

Examined with respect to the head-symptoms exhibited during life.

The first case was noted to be "perfectly sensible and rational up to a few minutes prior to death;" the second, "utterly unconscious, with contracted pupils and convulsions;" the third, "slight delirium, sensible when roused; death by apnœa;" the fourth, "some talking while dozing; sensible when roused; the fifth, "delirium, and leaving the bed and hiding himself; answers correctly when roused."

One, therefore, was sensible, three slightly delirious, one had grave cerebral disturbance.

§ XXXIV. There remains, however, untouched the supposition that great differences in density of the grey and white substances of the brain may be found in different individuals, existing as the normal condition, and that the variations recorded are totally unconnected with any pathological influence whatever. This supposition will be more conveniently examined further on, when the analysis of the specific gravity, in connexion with the symptoms, comes to be considered. (Vide § liii.)

§ XXXV. The coincidence, however, on any supposition, is remarkable, that the grey matter of all the adults who died before the seventh day should have had the great density of 1·042, and further, that the fifth and only remaining case of those dying at the same early period should have had the density of the grey matter at 1·038, the subject of the case being a child of seven years of age, at which period of life the density of the grey matter appears to be normally somewhat lower than in adult age. (Table C, § xiv.) That this may be nothing more than a coincidence, the fewness of the observations renders highly possible; and in the next case, arranged according to the duration of the last illness, the grey matter had a density of 1·032. This case was that of a female, ætat. 63, dying sensible on the eighth day of an attack of pneumonia.

§ XXXVI. Direct experiment on the brain of an adult killed by accident, received while in perfect health, would be of much interest. I have not been able to procure such a case, but a portion of the brain of the man killed in two hours by the bite of a cobra, on October 20, was kindly furnished by the medical officers of University College Hospital; and on examination 31 hours after death, the specific gravity of the grey matter was found to be 1·039, the white 1·042. The man, it appeared, was under the influence of intoxicating liquors at the period of the accident, and could not be said to be in full possession of his mental faculties. But this observation rather favours the hypothesis, that in active health the density of the grey matter is usually high, and that length of illness is accompanied by a gradual diminution of the density of the grey matter. We shall perceive further on, that this decrease may take place and the mental faculties remain intact.

Specific Gravity of the Brain, examined in relation to the Cerebral Symptoms that existed during the Last Illness, and more especially during the Last Hours of Life.

§ XXXVII. *Classification.*—To classify the subjects of the observations according to the cerebral symptoms exhibited by them during life, is obviously a task of much difficulty; the degree of importance to be assigned to a single symptom, or to any collection of symptoms, is not sufficiently established to allow of distinct classes being readily made. In the following analysis, the cases have been separated from each other, and formed into four series, in the following manner:

In the first series are classed all those cases in which the patients were perfectly sensible up to the period of death.

In contrast to these, the fourth series is composed of all those cases in which the graver cerebral symptoms were well marked, such as convulsion, strabismus, contraction, paralysis, or utter unconsciousness, when independent of coma.

Between these extremes, a large number of cases exists, all with more or less delirium, and they have been subdivided into two, making the second and third series. In the second are placed those cases with the least severe cerebral disturbance, and which proved fatal, through some other than the nervous system, as when death took place by apnœa, exhaustion, &c. In the third series are arranged those cases in which death occurred through the nervous system, as indicated by coma, &c.

The first series is composed of 23 cases, in which death occurred from the following causes, the patients all dying sensible:—pneumonia, 6; peritonitis, 2; pericarditis, 1; bronchitis, 1; ascites and anasarca, 1; dysentery, 3; cancer, 2; sloughing of the back, 2; scrofulous disease of the hip, 1; traumatic erysipelas in a tubercular subject, 1; gangrena senilis, 1; sudden death, 2.

Of the last two, one had typhoid fever, and much frothy blood was found in the left ventricle of the heart; the other dropped down dead while apparently convalescent, and softening of the fornix was the only lesion discovered.

The second series comprises 16 cases of the following diseases:—typhus fever, 3; typhoid fever, 4; relapsing fever, 2; scarlet fever, 1; dysentery, 1; pneumonia, 2; pleurisy, 2; pyohæmia, 1.

The third series is composed of 25 cases—viz., typhus fever, 19 cases; typhoid fever, 4; dysentery, 1; tubercular pneumonia, 1.

The fourth series consists of 12 cases, of which two have been added since the other tables were made for this analysis, and one is also included in the first series.

The last case is that of a man, the notes of whose symptoms during life are less perfect than could be desired; the patient had had apparently a slight febrile attack on his first admission into the hospital, accompanied with much delirium. He had been marked convalescent for 14 days, when, while sitting by the fire, he was observed to alter in the features, and fall. He was placed on the bed, and died immediately. On examination of the body, after death, the viscera were all healthy, with the following exceptions:—A thick false membrane covered the anterior surface of the left lung, readily separable from the pleura, and the fornix was reduced to about the consistence of clotted cream. In the classification according to the symptoms, the case has been placed in the first series; but considering that death was attributable only to the pathological changes found in the brain, and that the history of the symptoms is imperfect, it has been thought just to place it in the fourth series also.

In all the cases, therefore, composing the fourth series, cerebral disease was present, as manifested either by the symptoms or by the lesions.

(I.)

The Specific Gravity of the Grey Matter examined in relation to the Symptoms immediately preceding Death.

GREY MATTER.

Series.	Mean duration of case in days.	Mean No. of hours after death of autopsy.	No. of Obs.	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	Mean.	Mean Age.
1	35	40	23	2	..	1	..	6	3	5	..	4	..	..	..	..	1	1	..	..	..	..	1·0336	36
2	17	28	16	..	..	1	1	1	..	4	2	2	..	2	..	..	1	2	..	..	..	..	1·0357	26
3	17	36	25	..	1	..	..	10	1	6	1	2	..	..	3	1	..	..	..	..	..	..	1·0352	41
4	16	27	12	..	..	..	..	..	..	..	..	1	..	1	2	1	1	2	2	1	..	1	1·041	25
		Total......	76	2	1	2	1	17	4	15	3	9	..	3	5	2	3	5	2	1	..	1		

§ XXXVIII. The above table exhibits a much higher mean specific gravity of the grey matter in the fourth series, than in the other three; and in glancing at the individual items from which the mean is deduced, it is seen that all the numbers are arranged at that end of the table denoting the higher densities.

§ XXXIX. The mean density of the grey matter of the cases composing the fourth series of table I, is higher than the mean exhibited in any other table, with the exception of table H, which gives the influences of the duration of the last illness on the specific gravity. In table H, it was shown that the mean density of the grey matter of those who died before the seventh day is 1·0412. The mean duration of the last illness, however, in the cases composing the fourth series of table I, was 16 days, and it is seen (in table H) that the mean specific gravity of all the cases dying between the fourteenth and twenty-first day, is 1·0351, so that the great density of the grey matter exhibited in the fourth series of the table under consideration, is not due to the length of the last illness.

§ XL. The next circumstance which, from the foregoing tables, would appear to have any influence in increasing the density of the grey matter, is the period at which the autopsy was performed. In table E, it appears that in the cases in which the autopsy was performed at an average of eight hours after death, the mean density of the grey matter is 1·0393, while in those examined later the mean density is lower; but the cases composing the fourth series of table I. have even a greater mean density than 1·0393, but were examined at a later period—viz., at an average of 16 hours after death. So that the greater density of series 4 is not due to the period at which the autopsy was performed.

§ XLI. With the exception of five, all the cases in which the density of the grey matter exceeded 1·040, are in the fourth series. The exceptional cases were of the following diseases:

FIRST SERIES:

1. Sudden death.—Softened fornix.
2. Heart disease, pneumonia, granular disease of kidneys, and cirrhosis.

SECOND SERIES:

3. Pneumonia and granular kidneys.
4. Pleurisy with effusion—kidneys diseased.(?)
5. Granular disease of the kidneys—Meningeal apoplexy.

So that all the cases which I have collected for this analysis, and which exhibited a density of the grey matter exceeding 1·040, have been either cases of disease of some part of the encephalon, or of disease of the kidneys. The notes of the state of the kidneys in the case of pleurisy with effusion are imperfect; but it is stated in them that the capsules of the kidneys were not readily separable.

§ XLII. It becomes a matter of interest to know whether these two diseases bear a separate relation to the increased density observed, or whether, by bearing a relation to each other, they have therefore a relation to the high density of the grey matter of the brain. If this relation be one of sequence, it is clear that the disease of the kidneys is not consequent on the disease of the brain, both from what is already known of the diseases, and from the facts exhibited in the cases themselves; for in some of the cases very little cerebral disturbance was ever manifested. If one disease be the sequel of the other, the disease of kidneys is doubtless the primary affection, and we ought to find, therefore, a diseased state of kidneys, or traces of it in all the cases. Or if, instead of one disease being secondary to the other, if a common cause acting through both occasion the high density of the grey matter of the brain, then we ought to find disease of the kidneys in all the cases of the fourth series, and some manifestation of disease of the brain in those cases of the second and third series which had a density of the grey matter above 1·040; but between which the only circumstance in common appears to have been some disease of the kidneys. On examining the twelve cases composing the fourth series, with regard to the state of the kidneys, it appears that there was found disease of the kidneys in three, in one of which the organ was distinctly granular. It would appear, therefore, that the two classes of diseases—viz., disease of the kidneys and disease of the encephalon—are separate and distinct as regards their relation to the greater density of the grey matter which was found in connexion with each of them.

§ XLIII. With regard to the relation between a diseased state of the kidneys and a high specific gravity of the grey matter, from analysis of sixty-four cases, in which the notes are sufficiently full on the state of the kidney for the purpose, there were found twelve cases in which the kidneys were granular, and the mean specific gravity of the grey matter of the brain in these was 1·0371; and in fifteen other cases in which the kidneys were otherwise diseased, the mean specific gravity was 1·0346. And in thirty-eight cases in which the kidneys are stated in the notes to have been healthy, the mean specific gravity of the grey matter was 1·0350.

It would appear, therefore, that the mean density of the grey matter of the cases in which there was marked granular disease of the kidneys, is somewhat less than the mean density of the grey matter of the head-cases.

§ XLIV. It must not be lost sight of, however, that the mere increase of specific gravity of the grey matter may represent widely different states of that tissue as regards its ultimate structure; and of course the disease consists, not in the increased density, but in the change of structure on which the higher specific gravity depends. There is nothing marvellous, I think, in the circumstance, if it should prove to be correct, that two diseases, widely different in their symptoms or seat, should similarly affect the mere density of the same tissue. And so far as the number of my cases will permit of any inference being drawn from them, they would seem to show that the increased density of the grey matter which co-exists with disease of the kidneys may be quite unconnected with cerebral symptoms, while the cases in which the grey matter of the brain is above 1·040, and which are not connected with disease of the kidney, are accompanied by severe cerebral disturbance.

§ XLV. To continue the examination of table I. It appears that the mean specific gravity of the first series of cases, or of those who died sensible, is 1·033, or slightly lower than the mean of all the cases, and this low density is not accounted for by the age of the subjects of the observations, nor by the date at which the autopsy was made, which was only one hour later than the mean of all the rest. With respect, however, to the length of the last illness, which was thirty-five days, the mean density of the series is higher than the mean of all the cases (as shown in Table H) which died at the same period of disease.

§ XLVI. With respect to the second and third series in the table, the specific gravity of the grey matter is slightly above the mean in both; but the difference is trivial; the mean density of the second series is rather less than the mean of the third series, though the cerebral symptoms were more severe in the latter than in the former; the difference is not greater, however, than can be accounted for by the difference of mean age of the cases.

§ XLVII. Lastly, with regard to the cases in which the grey matter was found much below the mean, there are two in which the density was 1·028, or taking the mean at 1·034, as much as ·006 below the average. The cases both occur in the first series, or among those in which the patients were sensible up to the time of death; both patients died after a protracted illness, one of dysentery, the other of anasarca; one a man of 40 years of age, the other a woman aged 65 years. The former was of remarkably low mental capacity, the other was a shrewd and intelligent person.

(K.)

The Specific Gravity of the White Matter of the Brain, examined in relation to the Symptoms immediately preceding Death.

WHITE MATTER.

Symptoms.	Mean duration of case in days.	Mean No. of hours of autopsy.	Mean Age.	No.	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	Mean.
Series 1	35	40	36	24	..	..	..	1	1	..	3	2	8	2	3	1	2	..	1	..	..	1·0403
„ 2	17	28	26	17	..	..	..	..	..	..	3	..	2	2	6	2	2	..	..	..	..	1·0412
„ 3	17	36	41	25	..	..	..	..	..	..	3	..	5	4	7	2	2	..	1	..	1	1·0416
„ 4	16	27	25	12	2	..	..	..	..	..	..	..	1	..	1	2	2	2	2	..	..	1·0418
		Total		78	2	..	..	1	1	..	9	2	16	8	17	7	8	2	4	..	1	1·0411

§ XLVIII. There is a great uniformity exhibited in the mean density of the white matter in the four series of table K. The mean specific gravity of the first series is, however, slightly lower than the rest, but the average length of last illness in these cases was greater, and sufficient to account for the difference. (*Vide* table H.)

§ XLIX. In examining the items, however, from which the means were calculated, there appear some few which call for especial notice. There are two cases in the fourth series remarkable on account of the lowness of their specific gravity, and which are the lowest observed in any series. These cases have a density lower by ·009 than the mean of all the cases; they form a marked contrast to the other items of the fourth series. It will be observed that all the items but the two in question are above the mean density of the white matter. If the exceptional cases were withdrawn from the series, the mean density of those remaining in the fourth series would be 1·0438. The cases with the low density occurred in children of eight and ten years of age, and were both of tubercular meningitis, and the low density is not accounted for, either by age, sex, length of illness, or any influence examined in the preceding tables.

§ L. It would therefore seem that the low density in these cases was due to a pathological change in the white matter generally; and that such an effect on the density of the white matter may take place, is rendered probable by observations made on the density of the cerebral matter in cases of softening of the fornix, in which the softened tissue was found to have a density below that of the density of the white substance of the rest of the brain.

In one of the two cases in which the specific gravity of the white substance was 1·032, there was also found a pulpy state of the fornix, and the specific gravity of the softened tissue was only 1·022.

In another case, in the fourth series, in which the density of the white substance was 1·044, the fornix was softened, and of the density of 1·032. In a third case of the same series, the white substance generally was 1·040, and the softened fornix 1·024.

The fornix, however, from my observation, is normally of somewhat lower density than the white substance of the hemispheres, in the proportion of 1·0357 to 1·0411.

§ LI. There would appear to be two states of the tissue, therefore, in the cases composing the fourth series; one, in which the density is less than the mean, and the other, in which it is slightly greater than the mean; for having deducted the two cases, as before stated, having the specific gravity of 1·032, the rest of the series have a mean of 1·0438, and this result accords with what was found to be the effect produced on the density of the grey matter in the same series, and also, with what will be hereafter shown to occur in conjunction with severe lesions met with by the ordinary mode of examination of the brain.

§ LII. To pursue the examination of table K, two cases of remarkably high density are found in the third series. Both cases were of typhus fever; one a male, the other a female. Another case of high density also occurs in the first series, or among those in which no mental disturbance was exhibited. The case was one of pneumonia and granular kidneys in a man aged 60 years. In the cases in the third series of typhus, the kidneys were healthy in both.

Lastly, there are two cases in the first series in which the density was 1·028, or taking the mean at 1·034, ·006 below the average. The cases in question were those of an Irishman, æt. 40, who died of dysentery, after a long illness, and a female, 65 years of age, who died after lengthened suffering from anasarca and diseased liver and kidneys. (*Vide* § xlvii.)

§ LIII. In conclusion, it would appear, therefore, that when the density of the grey matter is greatly above the mean, disease in some part of the cerebrum is always present; but that there may be a density of the same tissue considerably below the mean without cerebral symptoms. While, on the other hand, a decidedly high or low density of the white matter seems to be always connected with obvious disease of the brain.

Column.	—	(A) BRAIN HEALTHY. No.	Grey S.	No.	WhiteS.	(B) WHITE SUBSTANCE, NORMAL CONSISTENCE. No.	Grey.	No.	White.	(C) WHITE SUBSTANCE, FIRM. No.	Grey.	No.	White.	(D) WHITE SUBSTANCE, SOFT. No.	Grey.	No.	White.	MEANS. No.	Grey.	No	White.	Column.
	Cerebrum:																					
1	Brain Healthy	12	1·0346	13	1·0404	...	...	...	...	...	...	...	...	...	...	...	...	12	1·0346	13	1·0404	1
2	White Substance, normal	...	...	...	...	9	1·0326	10	1·0409	...	...	...	...	...	...	...	...	9	1·0326	10	1·0409	2
3	,, firm	7	1·0358	7	1·0412	...	...	...	...	25	1·0348	26	1·0418	...	...	...	...	32	1·035	33	1·0416	3
4	,, soft	...	...	...	...	...	...	...	...	...	...	...	...	8	1·0353	8	1·0406	8	1·0353	8	1·0406	4
5	Fornix, softer than normal	...	...	...	...	...	...	...	...	2	1·032	—	1·040	1	1·028	1	1·038	3	1·0306	3	1·0333	5
6	,, pulpy	...	...	...	...	0	...	1	1·032	3	1·0403	3	1·043	3	1·0433	2	1·0435	6	1·0418	6	1·0413	6
7	Septum Lucidum, soft	...	...	...	...	...	...	...	...	...	...	...	...	1	1·028	1	1·038	1	1·028	1	1·038	7
8	Corpora Striata, soft	...	...	...	...	...	...	...	...	1	1·032	1	1·040	...	...	...	...	1	1·032	1	1·040	8
9	Convolutions, packed	...	...	...	...	0	...	1	1·032	3	1·0413	3	1·0436	...	...	...	...	3	1·0413	4	1·0407	9
10	Gyri, large	1	1·028	1	1·042	...	...	...	...	2	1·033	2	1·041	...	...	...	...	3	1·0313	3	1·0413	10
11	Grey Matter, layers marked	1	1·034	1	1·040	2	1·0335	2	1·045	6	1·0356	6	1·0413	...	...	...	...	9	1·0349	9	1·0419	11
12	,, ,, not marked	...	...	...	...	0	...	1	1·032	4	1·032	4	1·042	...	...	...	...	4	1·032	5	1·036	12
13	,, soft	...	...	...	...	...	...	...	...	1	1·038	1	1·046	...	...	...	...	1	1·038	1	1·046	13
14	,, pale	...	...	...	...	...	...	...	...	3	1·032	1	1·042	...	...	...	...	3	1·032	1	1·042	14
15	,, dark	1	1·034	1	1·040	0	...	1	1·032	3	1·034	3	1·0423	1	1·043	0	...	5	1·0358	4	1·0439	15
16	,, injected	...	...	...	...	1	1·034	1	1·044	3	1·0386	3	1·045	2	1·0405	1	1·040	6	1·0366	5	1·0438	16
17	Commissura Mollis, of good consistency.	...	...	...	...	...	...	...	...	9	1·0364	9	1·0425	...	...	...	...	9	1·0364	9	1·0425	17
18	,, not discovered	...	...	...	...	...	...	...	...	2	1·032	2	1·042	1	1 038	1	1·040	3	1·034	3	1·0413	18
19	Cerebellum, firm	1	1·035	1	1·038	...	...	...	...	2	1·033	2	1·040	...	...	...	...	3	1·0336	3	1·0393	19
20	,, soft	...	...	...	...	...	...	...	...	...	...	...	...	2	1·0355	2	1·042	2	1·0355	2	1·042	20
21	,, vascular	...	...	...	...	...	...	...	...	1	1·042	1	1·042	...	...	...	...	1	1·042	1	1·042	21
22	Vascularity, generally increased	...	...	...	...	3	1·032	3	1·041	4	1·038	4	1·0432	5	1·0336	5	1·0394	12	1·0346	12	1·0402	22
23	,, ,, diminished	...	...	...	...	2	1·031	2	1·040	...	...	...	...	4	1·0372	4	1·0407	6	1·030	6	1·041	23
24	,, bloody points numerous	1	1·036	1	1·043	4	1·0345	4	1·0412	10	1 0366	11	1·042	7	1·0391	6	1·0415	22	1·0369	22	1·0417	24
25	,, ,, not numerous	...	...	...	...	...	...	...	...	4	1·0345	4	1·041	...	...	...	...	4	1·0345	4	1·041	25
26	Dura Mater, vascularity increased	1	1·033	1	1 038	...	...	...	...	6	1·0376	6	1·0435	4	1·039	3	1·0425	10	1·0381	9	1·0432	26
27	Pia Mater, vascularity increased	2	1·035	1	1·040	2	1·033	2	1 0415	11	1·0356	11	1·0414	9	1 0363	8	1·0413	24	1·0356	22	1·0413	27
28	,, ,, not increased	...	...	...	...	1	1·034	1	1·0440	4	1·036	4	1 0425	...	...	...	...	5	1·0356	5	1·0428	28
29	Membranes, Arachnoid, opaque	...	...	...	...	1	1·032	1	1·038	3	1·0373	4	1·042	7	1·0364	6	1·0425	11	1·0362	11	1·0419	29
30	,, ,, dry	...	...	...	...	0	...	1	1·032	1	1·042	1	1·042	1	1·043	0	...	2	1 0425	2	1·037	30
31	,, strip easily	2	1·0310	2	1·041	4	1·0327	4	1·0442	9	1·0334	9	1·0425	6	1·039	5	1·042	21	1·0346	20	1·0426	31
32	,, ,, with difficulty	2	1·0345	2	1·041	3	1·033	3	1·033	3	1·040	3	1·044	2	1·037	2	1·040	10	1·0362	10	1·0393	32
33	Serum under arachnoid	2	1·033	2	1·0395	4	1·033	4	1·0425	16	1·0349	16	1·0426	13	1·0369	12	1·0411	35	1·0353	34	1·0418	33
34	,, in ventricles	6	1·0343	6	1·0393	3	1·033	4	1·0375	18	1·035	19	1·0406	14	1·0361	13	1·0408	41	1·0351	42	1·0395	34
35	,, at base	2	1·036	2	1·0405	...	...	...	...	9	1·0342	9	1·0418	7	1·036	6	1·0418	18	1·0351	17	1·0416	35
36	Lymph in ventricles	...	...	...	...	...	...	...	...	1	1·042	1	1·044	...	...	...	...	1	1·042	1	1·044	36
37	,, at base	...	...	...	...	4	1·0327	4	1·041	1	1·042	1	1·044	2	1·0445	1	1·044	7	1·0374	6	1·042	37
38	Pus in ventricles	...	...	...	...	...	...	...	...	2	1·041	2	1·0445	...	...	...	...	2	1·041	2	1·0445	38
39	,, at base	...	...	...	...	...	...	...	...	1	1·042	1	1·044	...	...	...	...	1	1·042	1	1·044	39
40	Tubercle in pia mater	...	...	...	...	0	...	1	1·032	1	1·040	1	1·045	1	·032	1	1·041	2	1·036	3	1·0393	40

LIV. *Explanation.*—The foregoing table exhibits the specific gravity of the white and grey substance in relation to the morbid anatomy of the brain. It is divided into four. Under the division A are arranged all those cases in which the brain is noted healthy; under B are all the cases in which the brain was described to have the white substance of normal consistence; under C those cases in which the brain was noted to have the white substance firm; under D those in which the white substance was described to be soft; and the last column gives the averages of all the cases.

The list of morbid appearances in the first column is composed of descriptions extracted from the note-books.

The table is read in the following manner:

Line 11. Layers of the grey matter marked, of which morbid appearance one example occurred in a brain noted to be healthy, and the specific gravity of the grey matter was 1·034, of the white matter 1·040. Two also occurred in brains, the white substance of which was of firm consistence, the mean specific gravity of the grey matter was 1·0335, and of the white 1·0425. Six examples occurred in cases in which the brain was described to have the white matter firm, and the mean density of the six is, of the grey matter 1·0356, of the white 1·0413. Total number of brains found with the layers of the grey matter well marked, 9, the average density of the grey matter of all being 1·0335. Average density of the white matter in all the cases, 1·0419.

§ LV. The table exhibits a notable correspondence between high density of the grey matter and some of the most severe lesions of the brain. For example, a high mean density of the grey matter is found in connexion with the following morbid changes:

Dryness of the arachnoid; lymph in the ventricles; lymph at the base of the brain; vascularity of the cerebellum; pulpy state of the fornix; convolutions closely packed; pus found in the ventricles: increased vascularity of the dura mater; grey matter soft; lymph found at the base. All the above morbid states correspond to a density of the grey matter above 1·037. Those connected with the highest density are placed first, and the others follow in the order of their density.

On reversing the arrangement, by placing first those morbid appearances which are found co-existing with the lowest density of the grey matter.

Then the list becomes thus:

Septum lucidum, soft; vascularity of the whole encephalon generally diminished; fornix, softer than normal; gyri, large; corpus striatum, soft; layers of the grey matter, not distinct; grey matter, pale; white substance, soft. All the cases having the above morbid appearances had the mean density of the grey matter below 1·033, those lesions which were connected with the lowest mean density being placed first.

The same mode of examining the morbid changes in connexion with the density of the white substance exhibits similar results—namely, the graver morbid changes, especially those believed to depend upon inflammatory action, are found co-existing with greater density, while the changes more allied to the opposite condition of brain co-exists with a lower density of the white matter.

The following lesions are arranged in the order of the density of the white substance of the brain, found in conjunction with them, those being placed first in which the mean density is the highest.

Pus in the ventricles ; pus at the base of the brain ; lymph in the ventricles ; grey matter, dark ; grey matter, injected. In all these the mean density of the white matter is above 1·043.

In the next list, the lesion in which the white substance had the lowest density is placed first.

Fornix, softer than normal ; layers of grey matter not marked ; arachnoid, dry. In all these the mean density of the white matter is below 1·037.

Summary.—The foregoing analyses of 77 observations made upon the specific gravity of the brain, render probable the following general conclusions—viz.

That the mean specific gravity of the grey matter, in either sex, is 1·034 ; that the density of the grey matter is somewhat below the mean in the earlier and later periods of life ; that the highest density is met with between the ages of 15 and 30 years in males, and between 20 and 30 years in females ; that the density of the grey matter is, in a slight degree, lower in those persons who have died after a long illness, and greater, to a slight extent, in those subjects examined before twelve hours after death than in those examined at later periods.

That the density of the grey matter may be found in a subject after death to be ·006 below the mean, without any cerebral symptoms having been present during life; but when the specific gravity exceeds the mean by ·006, then one of the following conditions has existed during life—viz., either acute cerebral disease, attended with head symptoms of the gravest character, or chronic disease, (in all the cases analyzed of chronic disease of the kidneys,) attended either with no cerebral symptoms, or only with slight delirium.

That the mean specific gravity of the white matter after death is 1·041 ; that its density varies less than that of the grey matter in the sexes, or in the different periods of life ; that it is much less affected by post-mortem changes or length of the last illness.

That in those cases in which the gravest cerebral symptoms were present during life, the density of the white matter after death may present two opposite conditions—either it may exceed the average, or it may be much below the mean.

That high specific gravity of both grey and white matter is found in conjunction with those morbid conditions of the brain connected with hyperæmia, and that a low specific gravity exists in conjunction with the opposite condition of the brain.

That no relation appears to exist between the specific gravity and the actual weight of the brain.

Art. V.

The Action of Liquor Potassæ on the Urine, in Health. By E. A. Parkes M.D. Lond., Professor of Clinical Medicine in University College, and Physician to University College Hospital.

The following pages contain the record of some observations instituted to determine the effect of liquor potassæ on the urine of a healthy individual, preliminary to a similar inquiry into its action on the urine of diseased persons.

A series of experiments on the action of liquor potassæ on the serum of the blood, and a recognition of the strong affinity which this alkali has for sulphuric acid, led me to examine, in the first instance, the effect produced on the sulphuric acid of the urine. Before doing this, it was necessary to determine the normal amount of sulphuric acid in the urine of the person experimented upon, under different conditions, and the following table exhibits the results of this inquiry.

I.

Quantity of Water, Solids, and Sulphuric Acid in the Urine of a Healthy Man thirty-three years of age, on ordinary Mixed Diet.

Date, 1852.	Time when urine was secreted and passed.—Quantity.	Quantity per hour in round numbers.	Solids in 1000 parts.	Sulphuric acid in 1000 parts.
March 11 ...	From 12 P.M. to 8 A.M.	...	...	1·079
" 12 ...	"	...	...	1 647
" 13 ...	"	...	...	1·371
" 15 ...	"	...	39·18	1·038
" 16 ...	"	...	54·47	2·32
" 17 ...	"	...	52·51	1·371
" 18 ...	"	...	27·41	1·785
" 19 ...	"	...	24·22	1·831
" 22 ...	"	...	48·27	1·713
" 23 ...	"	...	59·39	2·196
" 24 ..	"	...	44·03	1·467
" 25 ...	"	...	31·72	1·718
" 26 ...	"	...	34·21	1·459
April 4 ...	From 9 A.M. to 11 P.M. ℥xxxiij ...	℥ij. ʒiij.		
" 4, 5 ...	From 11 P.M. to 4 A.M.	...	28·21	1·048
" 5 ...	From 4 A.M. to 8 A.M.	...	60·	2·103
" 5, 6 ...	From 8 A.M. to 4 A.M., ʒxxix.......	℥j. ʒiiiss.		
" 6 ..	From 4 A.M. to 8 A.M., ℥ix.........	℥ij. ʒij.	30·83	1·306
" 6, 7 ...	From 8 A.M. to 4 A.M., ʒxxv.	℥j. ʒij.		
" 7 ...	From 4 A.M to 8 A.M., ʒxiij.	ʒiij. ʒij.	40·7	1·533
" 13, 14 ...	From 11 P.M. to 8 A.M. ℥ix.	℥j.	60·2	1·841
" 15 ...	At 8 A.M.	...	46·35	2·262
" 15, 16 ...	From 8 A.M. to 8 A.M., ʒxxxviij....	℥j. ʒiv. ℈ij.	51·59	1·867
" 15, 16 ...	From 11 P.M. to 8 A.M.	...	44·16	2 364
" 20, 21 ...	From 12 P.M. to 8 A.M., ʒxvij. ...	℥ij. ʒj.	47·68	1·472
	From 8 A.M. to 8 A.M., ʒxxxviij...	℥j. ʒiv. ℈ij.	42·27	1·189
" 21, 22 ...	From 12 P.M. to 8 A.M., ʒxj.	℥j. ʒiij.	30·40	1·378
	From 8 A.M. to 8 A.M., ʒxl.........	℥j. ʒv.	41·14	1·248
May 13, 14 ...	From 11 P.M. to 8 A.M., ʒxvij......	℥j. ʒvij.	23·15	·866
" 14, 15 ...	From 12 P.M. to 8 A.M., ʒxvij......	℥ij. ʒj.	23·23	1·079
	From 8 A.M. to 8 A.M., ʒxlv.	℥j. ʒvij.	28·85	1·058

Quantity of Water, Solids, and Sulphuric Acid, &c.—continued.

Date, 1852.	Time when urine was secreted and passed.—Quantity.	Quantity per hour in round numbers.	Solids* in 1000 parts.	Sulphuric acid in 1000 parts
May 15, 16 ...	From 12 P.M. to 8 A.M., ℥xiij......	℥j. ʒv.	41·57	1·479
	From 8 A.M. to 8 A.M., ℥xxxvj. ...	℥iss.	50·85	1·59
„ 17, 18 ...	From 11 P.M. to 8 A.M., ℥xiij......	℥j. ʒiiiss.	47·65	1·842
	From 8 A.M. to 8 A.M., ℥xxxiij....	℥j. ʒiij.	43·03	1·271
„ 18, 19 ...	From 11 P.M. to 8 A.M., ℥xv.......	℥j. ʒv.	30·01	·894
	From 8 A.M. to 8 A.M., ℥xlvj.......	℥j. ʒvij.	32·29	·811
June 2, 3 ...	From 8 A.M. to 8 A.M., ℥xxxiij. ...	℥j. ʒiij.	34·91	1·081
„ 3 ...	From 1 A.M. to 8 A.M., ℥viij.	℥ij.	33·21	1·942
„ 3, 4 ...	From 8 A.M. to 8 A.M., ℥xxxviij....	℥i. ʒiv. ℈ij.	48·21	·991
	From 12 P.M. to 8 A.M., ℥xivss. ...	℥j. ʒviss.	25·2	1·15
„ 4, 5 ...	From 12 P.M. to 8 A.M., ℥xvj......	℥ij.	23·55	1·306
	From 8 A.M. to 8 A.M., ℥xlvj.	℥j. ʒvij.	30·53	1·012
July 12, 13 ...	From 12 P.M. to 8 A.M., ℥ix.	℥j. ʒj.	44·59	1·701
„ 20 ...	From ½ past 3 A.M., to ½ past 7 A.M., ℥viss........................	℥j. ℥v.	40·93	·822
„ 21 ...	From 1 A.M. to 4 A.M., ℥ivss.	℥j. ʒj.	26·05	1·120
„ 22 ...	At 8 A.M.	...	29·41	·555
„ 24 ...	From 1. A.M. to 8 A.M., ℥vij.	℥j.	41·64	1·972
„ 25, 26 ...	From 12 P.M. to 8 A.M. ℥xij.	℥j. ʒiv.	38·8	1·260
	From 8 A.M. to 8 A.M., ℥xxxij......	℥j. ʒij. ℈ij.	29·84	1·170
August 2, 3 ...	From 12 P.M. to 8 A.M., ℥xiij. ...	℥j. ʒv.	42·58	1·72
„ 3, 4 ...	From 12 P.M. to 8 A.M., ℥xx.	℥ij. ʒiv.	23·17	1·263
	From 8 A.M. to 8 A.M., ℥xlviij......	℥ij.	27·92	1·176
„ 4, 5 ...	From 12 P.M. to 8 A.M., ℥xix......	℥ij. ʒv.	22·13	1·187
	From 8 A.M. to 8 A.M., ℥xlviij......	℥ij.	19·62	·811
„ 5, 6 ...	From 12 P.M. to 8 A.M. ℥xiv......	℥j. ʒvj.	29·92	1·165
	From 8 A.M. to 8 A.M., ℥xliij.......	℥j. ʒvj.	31·16	1·197
„ 10, 11 ...	From 11 P.M. to 8 A.M., ℥xvj......	℥j. ʒvj.	27·2	·978
	From 8 A.M. to 8 A.M., ℥xxx......	℥j. ʒij.	34·58	1·737
„ 11, 12 ...	From 11 P.M. to 8 A.M., ℥xxiij...	℥ij. ʒivss.	27·29	·75
	From 8 A.M. to 8 A.M., ℥xlviij. ...	℥ij.	19·83	·971

AVERAGES.

1. *Urine secreted during the night, and passed in the morning.*
   Average amount per hour (mean of 155 hours) . . . = ℥j. ʒvj. ℈ij.
   Average of solids in 1000 parts (38 observations) . . = 37·27
   Average of sulphuric acid in 1000 parts (42 observations) = 1·4608
2. *Urine secreted during 24 hours.*
   Average amount in 24 hours (18 days' observations) . = ℥39. ʒvij.
   Average per hour . . . . = ℥j. ʒv. ℈j.
   Average of solids in 1000 parts (16 observations) . . = 35·41
   Average of sulphuric acid (16 observations) . . . . . = 1·199

It appears from this table that the amount of sulphuric acid bore no

* In determining the amount of the solids, a very small quantity (four to eight grains) of urine was used. The sulphuric acid was precipitated as sulphate of baryta, from an acidulated urine. It has been said, that by this method an alkaline or a carbonated ash is sometimes left after the washed sulphate has been heated to redness. In a great number of experiments, I have never found the sulphate either carbonated or alkaline. Occasionally, in a very dark urine, it carries with it a little colouring matter, but this can be avoided by dilution. A more serious inconvenience is, that some of the sulphate passes through the pores of the finest filtering paper. For this reason it is advisable not to use paper at all, but to wash the sulphate by repeated relays of water, which is afterwards drawn off with a pipette. I have only further to remark, that I am quite certain of the accuracy of the observations on the quantity of urine, as the experiments were performed on myself.

relation to the amount of solids. A very concentrated urine contained certainly more solids and more sulphuric acid than a very dilute urine; but beyond this general expression, no rule can be laid down. Sometimes the solids were in great, and the sulphuric acid in small quantity, as on the 3rd and 4th of June, when the solids were 48·21 per 1000, and the acid was only ·991. Sometimes the reverse held good, as on the 4th and 5th of June, when in the morning urine the solids were only 23·55, and the sulphuric acid was 1·306 per 1000. It is also evident that the relative quantities of water and of sulphuric acid in 1000 parts in twenty-four hours is equally varied.

From the facts recorded in the table, and from others presently to be mentioned, the influence of the following circumstances on the excretion of sulphuric acid can be more or less perfectly determined—viz., the time of year, the time of day, exercise, food, excess of liquid.*

1. *Time of year:*

In March.	Average of sulphuric acid in morning urine . . (13 observations)	= 1·684
In April.	Average of sulphuric acid in morning urine . . (9 observations)	= 1·700
,,	Average of sulphuric acid in urine of 24 hours (3 observations)	= 1·434
In July.	Average of sulphuric acid in morning urine . . (6 observations)	= 1·24
,,	Sulphuric acid in urine of 24 hours (1 observation)	= 1·170
In August.	Average of sulphuric acid in morning urine) . (6 observations)	= 1·177
,,	Average of sulphuric acid in urine of 24 hours . (5 observations)	= 1·178

These observations appear at first sight to show that the amount of sulphuric acid in the urine was greater in the cold than in the hot months. The quantity passed in 24 hours is, however, almost the same. Thus, in the three observations in April, on the urine of 24 hours, the average flow of urine was 39 oz., and the average $S\ O_3$ in 24 hours, 26·78 grs. In the five experiments in August on the urine of 24 hours, the average flow was 44½ oz., and the average, $S\ O_3$ in 24 hours, was nearly 24 grs. The higher per centage in the cold months was therefore attributable in great measure, if not entirely, to an excess of water in the hot months, from some unexplained cause.

2. *Time of day:*

During the night the average amount both of water, of solids, and of sulphuric acid, was greater than in the urine of twenty-four hours, and *à fortiori* than in the urine of the day. The excess of water and of solids was inconsiderable; that of sulphuric acid much greater. The reason of the excess may perhaps be found in the fact, that the heaviest meals in the day were taken in the evening—viz., at six, seven, and eight o'clock; and it is well known that the solids, and among them the sulphuric acid, are increased after food. To determine whether the excretion of the soluble sulphates of the food, or the rapid oxidation of the sulphur of the surplus albuminous aliment, would account for the excess of sulphuric acid in the morning urine, the principal meal was taken on eight occasions at two p.m., and very little or no food was taken afterwards. The average amount of the sulphuric acid present in the urine secreted between twelve midnight and eight on the following mornings, was 1·196 per 1000, or precisely the average of the urine of twenty-four hours. In addition to this cause, it will not appear

* The influence of food and exercise had been previously determined by Dr. Bence Jones. *Vide* various vols. of the Philosophical Transactions. I have merely enumerated my similar facts in order to complete the record of the investigation.

improbable, from what will be said of the effect of exercise, that the muscular movements taken during the day may really cause an increased oxidation of sulphur during the night.

3. *Exercise:*

The amount of sulphuric acid was increased by exercise, and it was found that the effect lasted a considerable time. Thus on one occasion, two or three hours after long-continued violent exercise, the sulphuric acid was raised to about ·6 per 1000 over the average; fifteen hours after the termination of the exercise, and thirteen or fourteen hours after food, the sulphuric acid amounted still to 1·743 or ·544 over the average.

4. *Food:*

The increase in the amount of sulphuric acid noted after food was perceptible in two hours, and continued to increase for four or five or six hours, after which the amount commenced to fall towards the figure which represented the oxidized sulphur of the tissues. The increase appeared to be more marked after meat than after bread.* The effect of food and of violent exercise together, was to cause a great excretion of sulphuric acid. On one occasion this amounted to 3·342 parts in 1000, or three times the average amount.

5. *Excess of liquid:*

The experiments on this point were not sufficiently numerous. When from twenty to thirty ounces of weak tea were taken beyond what the system demanded, the experiments showed an inconsiderable augmentation of the sulphuric acid.

Besides these conditions, there must be others affecting the oxidation of the tissues, as is proved by the extraordinary variation in the amount of sulphuric acid excreted, although the amount of exercise, and of solid and liquid food, did not apparently equally vary. The chemical agencies in the body no doubt alter continually, though the exact formula of the variations cannot of course be given. When long periods are taken, these temporary influences neutralize each other and disappear.

The above being, as far as I could determine, the variations in the state of health, the following table (II.) shows the effect of liquor potassæ on the

Water, Solids, Sulphuric Acid, and Acidity.

Date, 1852.	Meals—time and kind.	Whole amount of liquid taken during the period of experiment.	Liq. potassæ; quantity of, and time when taken.	Time when urine was secreted and passed. Quantity.	Quantity per hour.	Reaction.	Solids per 1000 parts.	Sulphuric acid per 1000 parts.
March 26	Dinner at 6—soup, meat, bread, vegetables, water.	Not noted ...	At 11 P.M., liq. potassæ (P.L.), ʒss. in ℥ij. of distilled water	From 11 to 9 A.M. (27), ℥x.	℥j.	Acid.	42·083	1·798
„ 28	Dinner at 2—meat, vegetables, water. At 7—bread, meat, and tea.	„	...	At 11 P.M., 3½ hours after food.	...	...	58·61	1·618
„ 28, 29	... ...	...	At 11 P.M., liq. potassæ ʒj., and ℥ij. of distilled water.	From 11 to 3, ℥v.	℥j. ʒij.	Acid.	55·56	2.327
„ 29	... ...	...	...	From 3 A.M., to ½ past 7 A.M., ℥iv. ʒvj.	℥j.	Acid.	57·907	2·406
„ 30	Dinner at 6—meat, vegetables, bread, water.	„						

* Dr. Bence Jones's experiments are opposed to this statement.

Water, Solids, Sulphuric Acid, and Acidity—continued.

Date, 1852.	Meals—time and kind.	Whole amount of liquid taken during the period of experiment.	Liq. potassæ; quantity of, and time when taken.	Time when urine was secreted and passed. Quantity.	Quantity per hour.	Reaction.	Solids per 1000 parts.	Sulphuric acid per 1000 parts.
Mar. 29, 30	... ...	"	...	From 11 P.M. to 9 A.M., ℥x.	℥j.	Acid.	51·67	2·475
" 30	Dinner at ½ past 5—meat, bread, vegetables, water. At 8 P.M. a glass of water.	Not noted.	...	At 11 P.M.	...	...	78·73	1·417
" 30, 31	... ...	...	At 11 P.M., liq. potassæ ʒij, in 3 or 4 ℥ of distilled water.	From 11 to 8 A.M., ℥xivss.	℥j. ʒv.	...	37·087	3·157
August 12	Breakfast at 8—tea, bread, and a small slice of meat.	Tea, ℥xiij.	...	From 8 A.M. to 12 (midday), ℥iij. ʒiv.	ʒvij.			
				From 12 to ½ past 1, ℥iss.	℥j.	...	44·39	·906
	Dinner at ½ past 1—meat, bread, potatoes, rice, fruit.	℥xij. of bitter beer.	...	From ½ past 1 to ½ past 6, ℥iv. ʒiv.	ʒvij.	...	55·59	1·992
	Tea at ¼ to 7—a little bread.	℥xij. of tea.	...	From ½ past 6 to 9, ℥iv.	℥j. ʒv.	...	56·36	2·219
				From 9 to 11, ℥iiss.	℥ij. ʒij.	...	50·17	2·096
" 12, 13	... ...	Water ℥iv., with the liq. pot.	At 11 P.M., liq. potassæ, ʒij.	From 11 P.M. to 2 A.M., ℥xviij.	ʒvj.	Not examined for 9 hours, then alkaline from ammonia.	10·18	·342
" 13	... ...	...	...	From 2 A.M. to 8 A.M., ℥vj. ʒiv.	℥j. ʒv.	Acid.	46·49	1·781
	At 8 A.M., breakfast—bread, a small slice of meat.	Tea, ℥xiij.	...	From 8 to 12, midday, ℥iv.	℥j.	...	31·28	1·238
		Water ℥iii., with liq. pot.	At 12 (midday), liq. potassæ ʒj.	From 12 to 1, ℥v. ʒiv.	℥v. ʒiv.	Faintly acid.	13·71	·749
	Dinner at ½ past 3—bread, meat, potatoes, fruit.	Bitter beer, ℥x.	...	From 1 to ½ past 3, ℥iiiss.	℥j. ʒiij.	Faintly acid.		
				From ½ past 3 to ½ past 6, ℥iv. ʒvj.	℥j. ʒiv.	Strongly acid.		
		Water, ℥iij. with liq. pot.	At ½ past 6, liq. pot. ʒj.	From ½ past 6 to ½ past 7, ℥j.	℥j.	Acid.		
	Tea at ½ past 7—a very small quantity of bread.	Tea, ℥viij.	...	From ½ past 7 to ½ past 11, ℥iv. ʒiv.	℥j. ʒj.	Acid.		2·318
" 13, 14	... ...	℥iij. water, with liq. pot.	At ½ past 11, liq. potassæ, ʒj.	From ½ past 11 to ½ past 12, ℥viiiss.	ʒviijss.	Acid.		·380
" 14	... ...	...	...	From ½ past 12 to 4 A.M., ℥viij.	℥ij. ʒiv.	Acid.		
				From 4 to 8, ℥iv.	℥j.			
" 15	Breakfast at 8 A.M.	...	...	From 8 A.M. to 10 A.M., ℥ij.	℥j.			
				From 10 to 2 P.M., ℥v.	℥j. ʒij.			
	At 2, dinner—meat, bread, vegetables.	Bitter beer, ℥x.	...	From 2 to 5, ℥ivss.	℥j. ʒiv.			
		At ½ past 5, tea ℥iv. (no solids.)	...	From 5 to 6, ℥iss.	℥j. ʒiv.	...	24·19	1·417
				From 6 to 9, ℥iv.	℥j. ʒijss.			
		At 9, tea ℥iv.	...	From 9 to 11, ℥v.	℥ij. ʒiv.			
				From 11 to ½ past 11, ʒvi.	℥j. ʒiv.			

Water, Solids, Sulphuric Acid, and Acidity—continued.

Date, 1852.	Meals—time and kind.	Whole amount of liquid taken during the period of experiment.	Liq. potassæ; quantity of, and time when taken.	Time when urine was secreted and passed. Quantity.	Quantity per hour.	Reaction.	Solids per 1000 parts.	Sulphuric acid per 1000 parts.
Aug. 15, 16	... ...	Water, ℥ij. with liq. potassæ.	At ½ past 11, liq. pot. ʒj.	From ½ past 11 to 2 A.M., ʒxiv.	℥v. ʒiv.	Acid.	5·46	·4511
„ 16	... ...	...	...	From 2 to 3, ℥ij. ʒv.	℥ij. ʒv.			
				From 3 to 8, ℥vss.	℥j. ʒj.	...	27·01	1·407
	At ½ past 8, breakfast — bread and meat.	Tea, ℥xij.	...	From 8 to 10, ℥ij.	℥j.			
				From 10 to 12, ℥ij. ʒvj.	℥j. ʒiij.			
				From 12 to ½ past 4, ℥v.				
	At 6, dinner—soup, meat, bread, fruit.	Bitter beer, ℥vj.	...	From ½ past 4 to 6, ℥iss.	℥j.			
				From 6 to 8, ℥iv.	℥ij.			
		At 9, tea ℥iv. (no solids.)	...	From 8 to 11, ℥iv.	℥j. ʒij.			
„ 16, 17	... ...	Water ℥v., with liq. potassæ.	At 11, liq. pot. ʒj.	From 11 to 1, ℥xj.	℥v. ʒiv.	Acid.		
„ 17	... ...	...	...	From 1 to ½ past 2, ℥vss.	℥iij. ʒv.			
				From ½ past 2 to 8, ℥viij.	℥j. ʒiijss.			
	At ½ past 8, breakfast—bread.	Tea, ℥xvj.	...	From 8 to 10, ℥ij.	℥j.			
				From 10 to 2, ℥xij.	℥iij.			
				From 2 to 6, ℥iv.	℥j.			
„ 25, 26	... ...	...	...	From 12 P.M., to 8 A.M., ℥vj.	ʒvj.			
„ 26	Breakfast at 8—bread, meat.	Tea, ℥xvj.	...	From 8 to 2 P.M., ℥xiij.	℥ij. ℈j.			
	Dinner at ½ past 1—bread, meat, vegetables.	Water, ℥x.	...	From 2 to 6, ℥xj.	℥ij. ʒvj.	...	32·02	·7399
	Tea at 6, a very small piece of bread.	Tea, ℥ix.	...	From 6 to 9, ℥ivss.	℥j. ʒiv.			
				From 9 to 10, ℥ij.	℥ij.			
				From 10 to 11, ℥j. ʒv.	℥j. ʒv.	...	48·25	1·578
				From 11 to ½ past 11, ʒv.	℥j. ʒij.			
„ 26, 27 „ 27	... ...	Water ℥ivss., with the liq. pot.	At ½ past 11, liq. pot. ʒij.	From ½ past 11 to ½ past 2 A.M., ℥xij.	℥iv.	Acid, in 7 hours alkaline from ammonia.	10·16	·5804
				From ½ past 2 to 4, ℥iv.	℥ij. ʒvj.	Acid.		
				From 4 to 5, ʒix.	℥j. ʒj.	Acid.		
				From 5 to 8, ℥ij. ʒv.	ʒvij.	Acid.		
				From 8 to ½ past 8, ʒiij.	ʒvj.			
	At ½ past 8—breakfast.	Tea, ℥xviij.	...	From ½ past 8 to ½ past 9, ℥iss.	℥ss.			
				From ½ past 9 to 1, ℥vss.	℥j. ʒiv.			
				From 1 to ½ past 3, ℥ij.	ʒvj.			
	At 3, dinner.	Water ℥xiv.	...	From ½ past 3 to ½ past 5, ℥iiss.	℥j. ʒij.			
				From ½ past 5 to ½ past 6, ʒx.	℥j. ʒij.			
		At 8 P.M., bitter beer ℥vj.	...	From ½ past 6 to ½ past 11, ℥viss.	℥j. ʒij.			

Water, Solids, Sulphuric Acid, and Acidity—continued.

Date, 1852.	Meals—time and kind.	Whole amount of liquid taken during the period of experiment.	Liq. potassæ; quantity of, and time when taken.	Time when urine was secreted and passed. Quantity.	Quantity per hour.	Reaction.	Solids per 1000 parts.	Sulphuric acid per 1000 parts.
Aug. 28	... ...	...	...	From ½ past 11 to 8 A.M., ℥viss.	ʒvj.			
Sept. 1	At 3, dinner—meat, vegetables, bread.	Water, ℥xx.	...	From 10 A.M. to 5 P.M., ℥x.	℥j. ʒiij.			
		Water, ℥iv., with liq. pot.	At 5, liq. pot. ʒj.	From 5 to 6, ℥j. ʒij.	℥j. ʒii.			
				From 6 to 9. ℥v.	℥j. ʒv.			
		At 9, tea, ℥x.	...	From 9 to 11, ℥vss.	℥ij. ʒij.			
				From 11 to ½ past 11, ʒvij.	℥j. ʒvj.			
„ 2	... ...	...	...	From ½ past 11 to 8, ℥xx.	℥ij. ʒij.			

On looking over this table, it will be seen that eleven experiments were made—the dose of potash being in one experiment, ʒss., in seven, ʒj., and in three, ʒij. In six of these experiments a large quantity of urine was passed within two hours after the potash was taken; in the other five experiments this effect was not produced. This apparent discrepancy is probably explained by a reference to the time when the potash was taken. When it was introduced into an empty stomach, the large flow of urine followed; when it was taken from three to five hours after food, increase in the quantity of urine only once occurred. In this latter case, in fact, the potash must have been usually neutralized by the free acids present in the stomach, and the resulting chloride of potassium, or lactate of potash, as the case might be, then exerted on the system the action proper to itself, and not that which the liquor potassæ, when absorbed into the circulation before neutralization, invariably produced.

The observations on the 13th and 14th of August may be cited as the best illustration of these facts. On the 13th dinner was taken at half-past three, and the only fluid drunk was ten ounces of bitter beer. For three hours after dinner the hourly flow of urine was ℥iss.; it then declined to ℥j. in every hour. At half-past six, ʒj. of liquor potassæ was taken without effect; eight ounces of tea being drunk at half-past seven, the flow for the next four hours was slightly raised, being ℥j. ʒj. per hour, but it showed no tendency to increase beyond this amount, and was, in fact, beginning to decline, when ʒj. of liquor potassæ in 3 ounces of water was taken. During the next hour no less than ℥viiiss. of urine were passed, a far larger quantity, in the time, than usually follows (in the individual experimented on) the imbibition of a pint of fluid.* During the next three hours the hourly flow declined to ℥ij. ʒiv., which is, however, nearly an ounce above the average. During the next four hours, the hourly flow declined to one ounce, which is considerably below the average.

* In my own case, the quantity of urine passed after an excess of fluid is taken is very much influenced by the condition of the stomach. When the stomach is full, the urinary flow is augmented much less rapidly, and to a much less amount, than when the stomach is empty. When digestion is going on, either the absorption from the stomach is less rapid, or the circulation through the liver is slower, or the water is in firmer chemical combination than when it is taken into the fasting system.

The facts noted on August 15th may be also referred to. In the morning after breakfast the average hourly flow was ℥j. ʒj. At two, dinner and ten ounces of bitter beer were taken. The hourly flow for the next three hours was raised by ʒiij. Four ounces of tea were then taken; the hourly flow declined ʒiss. during the next three hours. Four more ounces of tea being taken, the flow augmented by ʒiss. per hour, continued at this rate for two hours, and then began to fall. One drachm of liquor potassæ and two ounces of water being then taken, no less than 14 ounces were passed in the next two hours and a half, or at the rate of ℥v. ʒiv. per hour.

It is needless to recapitulate the other similar facts of the table. It is sufficient to remark, that if the eye be passed along the columns of tables I. & II., in which the hourly flow of urine is recorded, it will be observed that the highest amount of urine ever passed in one hour did not in nearly 600 hours exceed ℥iij. ʒij. when no potash was taken,* although, sometimes, a considerable quantity of fluid had been previously drunk; whereas in all the six cases in which liquor potassæ was taken on an empty stomach, the hourly flow of urine was almost double this. The following table shows this at once.

III.

Amount of Potash taken.	Quantity of Urine passed immediately afterwards.	Time in which passed.
ʒij.	℥xviij.	3 hours.
ʒj.	℥v. ʒv.	1 hour.
ʒj.	℥viij. ʒiv.	1 hour.
ʒj.	℥xiv.	2½ hours.
ʒj.	℥xj.	2 hours.
ʒij.	℥xij.	2½ hours.

Average per hour . . . . = ℥v. ʒvj.
Average per hour when no potash was taken . . . . = ℥j. ʒvj.
Highest single observation when no potash and an ordinary amount of fluid was taken (600 observations) . . . . } = ℥iij. ʒij.

The urine thus passed was pale, watery-looking; and in five out of six experiments had a faint acid reaction. In the sixth observation the urine was not tested for nine hours; it was then alkaline from ammonia. In another case, the urine, acid when passed, became alkaline from ammonia in seven hours. This rapid production of ammoniacal alkalinity, if it occur in other cases, has, perhaps, given rise to the erroneous opinion that liquor potassæ produces at once an alkaline urine.

In any given quantity of this urine the amount of solids and of sulphuric acid was, of course, greatly below the average. It becomes a question, however, whether the solids were merely diluted, or were altered otherwise, either in the way of increase or diminution. The following calculation, in which the element of time is equalized, answers this question. The quantities of water, solids, and sulphuric acid, excreted under normal

* Of course it has often exceeded this amount, when large quantities of fluid have been purposely taken.

circumstances in 24 hours, are known.* What then would be the amount excreted, supposing that the flow of urine which occurred after the liquor potassæ, had continued for 24 hours?

IV.

In twenty-four hours there were excreted—

	Water.	Solids.	Sulp. Acid.
	℥. ʒ.	Grs.	Grs.
1. In normal circumstances (average of 16 observations).	39 vij.	677·84	22·850
2. In 3 hours after ʒij. of liquor potassæ, 18 ounces were excreted; had this continued there would have been in 24 hours	144	703·64	23·598
3. In 1 hour after ʒj. of liquor potassæ, ℥v. ʒv. were excreted, or in 24 hours	132	868·86	45·456
4. In 1 hour after ʒj. of liquor potassæ, ℥viij. ʒiv. were excreted, or in 24 hours	204	...	37·209
5. In 2½ hours after ʒj. of liquor potassæ, ℥xiv. were excreted, or in 24 hours	132	345·94	28·581
6. In 2 hours after ʒj. of liquor potassæ, ℥xj. were excreted, or in 24 hours	132		
7. In 2¼ hours after ʒij. of liquor potassæ, ℥xij. were excreted, or in 24 hours	96	470·20	26·861

Averages of the preceding table, after the exhibition of liquor potassæ.

Water	140, or ℥100 ʒiij. over the normal average.
Solids	597·16, or 80·68 grains below the normal average.
Sulphuric acid . .	32·341, or 9·491 grains over the normal average.

This table sufficiently indicates the increased excretion of sulphuric acid caused by the potash. This may be shown also by comparing the quantities from another point of view. In normal urine, the mean sulphuric acid in 1000 parts was to the whole solids as 1 to 29·66. After the exhibition of liquor potassæ it was as 1 to 18·46. In other words, in 100 parts of solids of normal urine the sulphuric acid constituted 3·37 parts, while in the potash-urine it constituted 5·41 parts.

The only other experiment I have been able to make on a healthy person may be here given. A man, aged 28, dined at 1 o'clock, and took tea at 6. From 9 to 11 he passed ℥iiss. of urine (=℥j. ʒij. per hour), which contained 3·570 grains of sulphuric acid; this would give, for 24 hours, 42·84 grains. At 11, he took ʒss. of liquor potassæ, and about ℥ij. of water, but no other liquid. From 11 to ¼ to 4, A.M., he passed ℥x. ʒv. of urine (=℥ij. ʒij. per hour), which contained 9·378 grains of sulphuric acid; this would give, for 24 hours, 47·665 grains. Speaking roughly, about 1 grain more of sulphuric acid was excreted between 11 and 4, than would have been the case had no potash been taken. Thirty drops of liquor potassæ contain about 2 grains of potash, which would have required more than the excess of sulphuric acid to have neutralized it. Either some of the potash was neutralized, or, as far as a single experiment goes, this result accords

* Of course the calculation is not perfectly accurate, as no allowance is made for the increased weight given to an ounce of urine by the solids; the error, which cannot be avoided, is, however, trifling, and is sufficiently equal in all cases.

with an inference drawn from another experiment—that the potash is in union with another acid (perhaps an organic one) as well as with sulphuric acid.

The general characters of the urine after liquor potassæ being, then, that it was highly dilute, with an acid reaction, with an absolute and relative diminution of solids, and with a great relative excess of sulphuric acid, it is time to inquire more fully into its composition. For this purpose two analyses were made.

1. At 2 A.M. on the 15th of August, after ℥j. of liquor potassæ, it will be observed that 14 ounces were passed in two hours and a half. The following was the composition in 1000 parts:

Whole solids			5·460
Organic matter		2·607	
Viz.			
Urea (in undetermined quantity).			
Extractives (in large quantity).			
Uric acid (none).			
Soluble salts :			
(Determined by incineration at as low a temperature as possible)*		2·853	
Sulphuric acid	·4511		
Phosphoric acid	·1012		
Chlorine	1·139		
Potash	·6578		

Heat and nitric acid produced no precipitate, or the very merest haze; ferrocyanuret of potassium had no effect; bichloride of mercury gave a copious precipitate. Trommer's test for sugar gave a negative result.

The organic matter was peculiar. When the residue, after evaporation, was digested in alcohol, a little urea and colouring matter were taken up; a large residue was left. About two-thirds of this residue were soluble in water; the solution had a very strong acid reaction, certainly stronger than that evinced by normal urine treated in the same way; the insoluble portion was insoluble in liquor potassæ, but was nearly entirely soluble in acetic acid.

The organic matter, *after evaporation,* was, then, composed as follows:

1. Alcoholic extract, including urea.
2. Water extract.
3. Substance insoluble in water and alcohol, but soluble in acetic acid.

It is not easy to assign the exact composition of the soluble salts. The sulphuric and phosphoric acids may be supposed to have been united with potash, and would therefore absorb of it ·5309 and ·0668 respectively. This would leave ·0601 of potash, an amount quite insufficient for the quantity of chlorine, which would, indeed, have required more than double the whole amount of potash. It may be assumed, therefore, that the chlorine was united, as usual, with sodium. This would give the following hypothetical arrangement:

* The soluble salts were dissolved out, evaporated and weighed; the sulphuric and phosphoric acids were determined both before and after incineration. The earthy salts were in too small a quantity to be weighed.

Chloride of sodium . . .	1·8875
Sulphate of potash . . .	·9820
Phosphate of potash . . .	·1680
Excess of potash	·0601

Either the excess of potash existed as potassium with chlorine, or it was combined with an organic acid destroyed by heat. This last supposition is strengthened by the fact, that the watery solution of the organic matter in this and in the succeeding analysis, left undissolved by the alcohol, was strongly acid.

A careful examination was made to see whether any sulphur existed, except in the state of sulphuric acid, and this was found not to be the case. With reference especially to the amount of potash, the whole 14 ounces must have contained rather more than four grains, which is almost precisely the amount contained in 60 drops of liquor potassæ.

2. An examination was made of the urine passed at half-past 2 A.M., August 27th.

On the 26th, dinner was taken at half-past 1, and tea at 6. The urine passed from 10 to 11 P.M. had the following composition in 1000 parts:

Solids	48·25
Chlorine	5·180
Sulphuric acid	1·578

At half-past 11, liquor potassæ ʒij. and water ℥ivss. were taken. In three hours ℥xij. were passed, of the following composition:

Solids			10·16
Viz.			
Organic matter		5·371	
Salts by incineration		4·789	
Soluble salts:			
Sulphuric acid	·5804		
Phosphoric acid	·2756		
Chlorine	1·554		
Potash	1·573		

In the twelve ounces there would be contained about 8·1 of potash. Two drachms of liquor potassæ contain a little more than eight grains, so that all the potash had passed off. If the same rules are followed as in the former analysis, the hypothetical arrangement is as follows:

Chloride of sodium . . .	2·572
Sulphate of potash . . .	1·2636
Phosphate of potash . . .	·4755
Potash (in surplus) . . .	·690

The examination of the organic matter gave the same results as in the former case: there was no uric acid; urea was in small amount; there was a large amount of extractives, soluble after evaporation in alcohol, or in water, or in acetic acid.

If these two analyses are compared, the results are found to be very similar, except that the quantity of water was much greater in the first than in the second observation. In both cases, the ratio of fixed saline matters to the organic matter was extremely large, and sulphate of potash and chloride of sodium constituted the chief proportion. In both, the phosphoric acid was in moderate amount. In both, the organic matter was peculiar.

In these two analyses the proportion of chlorine to the other solids is relatively large, but it does not appear that there is an absolute increase in the excretion of the chlorine, as there is of the sulphuric acid. Thus, using round numbers, in the hour from 10 to 11 P.M. on the 25–26th, 4 grains of chlorine were passed, or at the rate of about 96 grains in the 24 hours; in the three following hours after the liquor potassæ, about 6·5 grains of chlorine were passed, or at the rate of only about 51·5 grains in 24 hours.

The mode in which liquor potassæ produces these results is easily understood. It was first observed by Chevreul, and afterwards by Scherer, that many organic substances, and among others albumen and hæmatin, when dissolved in water, are little acted upon by the oxygen of the air under ordinary conditions, but are oxidized with extraordinary rapidity when a little alkali is added.* It has become generally admitted that the same result occurs in the animal organization; and Lehmann, in his masterly work, has lately handled this subject with his accustomed power. Without entering here into this question, it is sufficient to remark, that my experiments are entirely confirmatory of this opinion, and the evident oxidation of sulphur proves satisfactorily, that in a healthy condition of the system the albuminous substances are those which are acted upon when an excess of alkali is present. The sulphur passes into the state of sulphuric acid, and is excreted by the kidneys. Although such a fact is by no means proved, it may be that the albumen or fibrine, thus deprived of an essential element, appears in the urine as the peculiar extractives formerly mentioned.†

The immense flow of urine which follows the full action of potash cannot be attributed to the sulphate of potash which is formed. It is much more likely to be owing to the altered albuminoid substance.

What protein compound is thus oxidized, albumen, fibrine, or globulin, is not certain, and it is not impossible that one or other substance may be attacked, according to circumstances.

Such is the action of liquor potassæ on the system, when it is absorbed unneutralized into the blood. Such probably is the action likewise of carbonate of potash, as it must be assumed that the liquor potassæ is at once converted into carbonate when it enters the circulation. Some of the other salts of potash do not, however, have this effect. A series of careful experiments on my own person with large doses of nitrate, and of acetate, of potash, have shown that these salts cause no increase of the sulphuric acid, nor of the solids generally.‡ The effect of iodide of potassium has not been determined.

I must now refer to some negative experiments. It has already been

* The latest notice I have been able to find of the presumed action of alkalies is by Ruete (Lehrbuch der allgemeinen Therapie. Gott. 1852), in which it is stated that unneutralized alkalies probably enter the blood in combination with the albumen of the secretion of the stomach and intestines, and with fibrine. These combinations are said to soon decompose, and the alkali emerges from the body as carbonate, lactate, and chloride. The fibrine in the blood diminishes. Nothing is said of sulphuric acid, and I do not know on what experiments Ruete bases his conclusions.

† Very lately Verdeil (Chimie Anat. vol. iii. p. 299) has shown that when albumen is boiled for three days with a very small proportion of potash or soda, an azotized acid is formed.

‡ This result, in the case of the acetate of potash, is the more remarkable, as this salt causes the appearance of a large quantity of alkaline carbonates in the urine, and renders the urine as certainly alkaline as the tartrate or the citrate.

shown by five experiments, that when liquor potassæ is taken during the process of digestion, its peculiar effects are not produced.

1. In the first (March 26), dinner was taken at 6, and ʒss. of liquor potassæ was taken at 11, i. e., between four and five hours after the conclusion of dinner. No effect whatever was produced on the water, solids, or sulphuric acid.

2. In the second experiment (March 28), dinner was taken at 7, and ʒj. of liquor potassæ at 11. The urine passed at 11 contained 1·618 of sulphuric acid; that passed at 3 A.M. contained 2·327. This increase in the sulphuric acid cannot be attributed with certainty to the potash, as the influence of food was not sufficiently excluded. No effect was produced on the water.

3. In the third experiment (March 30), dinner was taken at half-past 5, at 8 a glass of water was taken, at 11 ʒij. of liquor potassæ. The urine at 11 (urina cibi) contained 78·73 of solid, and 1·417 of sulphuric acid in 1000 parts, that passed at 8 A.M. contained only 37·087 of solid, and no less than 3·157 of sulphuric acid. In this case there can be no doubt that the sulphuric acid was augmented, and it is possible that the water may also have been increased, as it was not passed for nine hours, and the increased flow of the first two hours may have been neutralized by the diminished amount afterwards passed. This observation, therefore, although it cannot be used, corroborates the results previously arrived at.

4. In the fourth experiment (August 13), dinner was taken at half-past 3, and ʒj. of liquor potassæ at half-past 6. No effect was produced on the water.

5. In the fifth experiment (September 1), dinner was taken at 3, and ʒj. of liquor potassæ at 5. No effect was produced on the water.

The following table carries these experiments a step further. In it are recorded the effects of liquor potassæ when given in the ordinary routine way, three times daily, on the water, solids, and sulphuric acid :

V.

Effects of Liquor Potassæ, given without regard to Food.

Date, 1852.	Liq. potassæ: quantity and time.	Time when urine secreted and passed.	Quantity per hour in round numbers.	Solids per 1000.	Sulphuric acid per 1000.	Total solids in 24 hours.	Sulphuric acid in 24 hours.
March 31	During the day 120 minims in 3 doses.	From 10 P.M. to 9 A.M. (1st), ℥xj.	℥j.	54·54	2·9		
Mar. 31, April 1.	... ...	From 9 A.M. to 9 A.M., ℥xxxij.	℥j. ʒij.				
April 1	During the day 120 minims in 3 doses.	From 11 P.M. to 7 A.M., ℥viij.	℥j.	61·38	2·981		
„ 1, 2		From 9 A.M. to 9 A.M., ℥xxxij.	℥j. ʒiiss.				
„ 2	During the day 120 minims in 3 doses.	From 12 P.M. to 8 A.M., ℥vij.	ʒvij.	57·53	2·686		
„ 2, 3		From 9 A.M. to 9 A.M., ℥xxxiv.	℥j. ʒiiiss.				
„ 3, 4	None	From 11 P.M. to 9 A.M., ℥viij.	ʒviss.				
		From 9 A.M. to 9 A.M., ℥xlj.	℥j. ʒvss.				

Effects of Liquor Potassæ, given without regard to Food—continued.

Date, 1852.	Liq. Potassæ: quantity and time.	Time when urine secreted and passed.	Quantity per hour in round numbers.	Solids per 1000.	Sulphuric acid per 1000.	Total Solids in 24 hours.	Sulphuric acid in 24 hours.
June 6	At 6 and 11 P.M., 20 minims.						
„ 6, 7	... ...	From 11 P.M. to 8 A.M., ℥xss.	℥j. ʒj.	30·25	1·514		
„ 7	During the day 40 minims.						
„ 7, 8	... ...	From 12 P.M. to 8 A.M., ℥viiiss.	℥j. ʒss.	53·31	2·321		
		From 8 A.M. to 8 A.M., ℥xliij.	℥j. ʒvj.	40·53	1·189	836·53	24·540
„ 8, 9	None......................	From 12 P.M. to 8 A.M., ℥xss.	℥j. ʒiiss.	43·18	1·883		
		From 8 A.M. to 8 A.M., ℥xxxiij.	℥j. ʒiij.	53·04	1·635	840·15	25·898
July 26	During the day 40 minims.						
„ 26, 27	... ...	From 11 P.M. to 8 A.M., ℥xiiss.	℥j. ʒij.	38·16	1·513		
		From 8 A.M. to 8 A.M., ℥xxxiij.	℥j. ʒiij.	41·99	1·295	665·12	20·512
„ 27	During the day ʒj. in 3 doses.						
„ 27, 28	... ...	From 11 P.M. to 8 A.M., ℥xviij.	℥ij.	22·37	·954		
		From 8 A.M. to 8 A.M., ℥xliij.	℥j. ʒvj.	21·07	·935	434·88	19·298
„ 28	During the day ʒj. in 3 doses.						
„ 28, 29	... ...	From 11 P.M. to 8 A.M., ℥xiij.	℥j. ʒiiiss.	35·77	1·501		
		From 8 A.M. to 8 A.M., ℥xxxvj.	℥j. ʒiv.	34·26	1·118	592·01	19·319
„ 29	During the day ʒj. in 3 doses.						
„ 29, 30	... ...	From 11 P.M. to 8 A.M., ℥xvj.	℥j. ʒvj.	23·09	1·047		
		From 8 A.M. to 8 A.M., ℥xlj.	℥j. ʒviss.	27·03	1·022	531·95	20·112
„ 30, 31	None......................	From 12 P.M. to 8 A.M., ℥xx.	℥ij. ʒij.		·800		
		From 8 A.M. to 8 A.M., ℥xliv.	℥j. ʒvj.		·980		20·697

Averages.

	Morning Urine.	
	Urine of Table V.	Normal Morning Urine—Table I.
Quantity per hour	℥j. ʒij. ℈j.	℥j. ʒvj. ℈ij.
Solids per 1000 parts ...	41·95 (10 obs.)	37·27
Sulphuric acid per 1000...	1·827 (11 obs.)	1·4608

AVERAGES.

	URINE OF 24 HOURS.	
	Urine of Table V.	Normal Urine of Table I. (16 obs.)
Quantity	℥xxxviij. ʒiv. (11 obs.)	℥xxxix. ʒvij.
Quantity per hour	℥j. ʒivss.	℥j. ʒv. ℈j,
Solids per 1000	36·32 (6 obs.)	35·41
Sulphuric acid per 1000...	1·167 (7 obs.)	1·199
Solids in 24 hours	650·10	677·84
Sulphuric acid in 24 hours	21·482	22·850

The close correspondence of the two series last given, proves that the average amount of the water, solids, and sulphuric acid, already assigned to normal urine (in the individual experimented on), must be very near the truth. It also proves, that these quantities were not affected by the exhibition of liquor potassæ, given indiscriminately in small doses during the day.

The effect produced by liquor potassæ on the healthy system may be thus recapitulated. If this remedy be taken soon after meals, its action is that of an antacid. It combines with hydrochloric or with lactic acid, and then, doubtless, passes into the circulation. What appreciable effect it now produces is not indicated in the tables above given, but it does not increase either the water, solids, or sulphuric acid of the urine. If the liquor potassæ be taken into an empty stomach, it passes unneutralized into the circulation, and probably through the veins; in so doing it must produce an effect on the walls of the capillaries and small veins, but the extent of this cannot be known. As much as ʒij. have been taken with only 4oz. of water, without causing epigastric pain or uneasiness (although it produced considerable temporary scalding of the mouth and throat), and without apparently producing any local effects in the stomach. In, usually, from thirty to ninety minutes after its entrance into the circulation,* an increased flow of slightly acid urine occurs, which contains the whole of the potash, organic matter differing considerably from that of ordinary urine, and a relatively large proportion of sulphuric acid; the phosphoric acid and the chlorine are less changed. Perhaps an organic acid (not uric, and probably not hippuric) is also present. The explanation of these facts is, that an albuminous compound, either in the blood itself, or in the textures, has become oxidized; its sulphur, under the form of sulphuric acid, has united with potash, and, with possibly the changed protein-compound, is poured out from the kidneys. This oxidizing effect of the liquor potassæ is no doubt assisted by exercise, and by copious draughts of water; but in the above experiments, exercise and fluid were abstained from, in order not to complicate the results. The amount of albumen or fibrine destroyed by

* This does not sufficiently appear from the facts in the text. I subjoin some particulars of another experiment, made when the system was not quite in a state of health. At a quarter past 4, liq. potassæ ʒj, water ℥iv, was taken.

At a quarter to 5, ℥ij. ʒvj. were passed . . . = ℥v. ʒij. per hour.
At a quarter past 5, ℥iv. ʒvij. were passed . . = ℥ix. ʒvj. per hour.
At a quarter to 6, ℥j. ʒij. were passed . . . = ℥ij. ʒiv. per hour.

one drachm of liquor potassæ cannot be considerable, but if the potash were continued in large quantities, oxidation could probably be pushed to any amount. The nitrate and acetate of potash did not in a *healthy system* have the same effects.

After the increased flow of urine, the quantity passed per hour falls slightly below the standard. It appears to resume its ordinary composition, but its exact condition at this period has not been determined. Some observations on urine in disease, would lead me to infer that the uric acid will be found to be increased.

Such were the effects of liquor potassæ on the urine. The effect produced on other excretions was not obvious. The skin and the intestines appeared quite unaffected, and as all the potash was found in the urine, the reason of this is easily understood. In most of the experiments there were no subjective symptoms of any kind. On two occasions, there was rather sharp frontal headache, languor, depression, slight lumbar pain, and aching of the legs, after the large flow of urine. On the night of the 15th, when the flow of urine, which was proceeding at the rate of ℥iss. per hour, was augmented in two and a half hours by ℥xiv., and no fluid was supplied to the system, the pulse became perceptibly small (almost thready) and slow; it remained equal and regular—there was no thirst, no shivering, and no nausea; the skin was dry and warm. In six hours the pulse had quite regained its force and frequency, and the other symptoms had disappeared without any fluid having been taken.

After the experiments were concluded, the general health did not appear impaired; it was, if anything, better than usual.

The effect of liquor potassæ on the diseased system is a much more difficult problem. The chemical conditions are not the same, and the effects of the potash are necessarily influenced by them. I will not now enter into this subject, but observe that it is necessary, when its oxidizing effects are desired, to give the potash eight or ten hours after food, to drink moderate quantities of water, and if possible, to use exercise. The potash should be given pure, or with large doses of iodide of potassium, but unmixed with sugar. I may so far anticipate what will be hereafter said on this point, by stating that, administered in this way, it exerts a powerful effect on the exudations of inflammations, but appears less useful in the early stages, when an antagonistic force seems to be in action.

It remains to be seen whether the varying excretion of sulphuric acid, which is unaccounted for by diet and exercise, is occasioned by greater or less alkalinity of the blood producing variations in the amount of oxidation of the albuminous compounds.

PART FOURTH.

Chronicle of Medical Science.

ANATOMY, PHYSIOLOGY, AND ORGANIC CHEMISTRY.

On the Increased Frequency of the Contractions of the Heart by Direct Irritation. By Dr. T. Budge.

In a preliminary communication Dr. Budge states that the frequency of the contractions of the heart is constantly increased by stimulating directly the sympathetic nerve in its course below the heart. Previously to beginning the experiment, it is necessary to destroy the connexion between the medulla oblongata and the heart, either by decapitating the frog or by dividing the nervus vagus of each side. Dr. B. waited always for the whole of an hour after the decapitation, or six or eight hours after the section of the nervi vagi, ere he commenced to apply the stimulus, for which he used the apparatus of *Dubois Reymond* (which permits the experimenter gradually to augment or to diminish the degree of irritation according to convenience). The increase amounted from 8 to 12 and 24 contractions in a minute. The same effect was produced by irritating the posterior part of the lower end of the spinal marrow. Dr. B. performed this experiment by applying the poles to the denuded bone at the junction between the os coccygis and the last vertebra, without laying open the spinal marrow. He further repeatedly observed, that even in eight or nine hours after the destruction of the spinal marrow, neither by direct nor by indirect irritation could any contraction be effected in the heart, when by the same degree of irritation complete tetanus was still produced in the muscles of the extremities.—*Froriep's Tagesber.*, No. 441, 1852.

On Stagnation of Blood in the Web of the Frog's Foot. By Dr. H. Weber.

In order to test the accuracy of the opinions which ascribe a great importance to the action of the heart and the condition of the vessels in stagnation of blood produced artificially in the vessels of the web of the frog's foot, the author studies these phenomena both when the circulation is free and when it is mechanically arrested, by placing a ligature round the leg or thigh. He brings a certain portion of the web in the field, cuts the ischiatic nerve, or narcotizes the frog, or takes out the spinal cord in order to prevent movements, and then applies a ligature. When the circulation is fully arrested, which does not occur for some time, in consequence of the contractions and dilatations of the artery giving rise to oscillatory movements, he applies to the web various irritating chemical agents. He finds that these agents produce exactly the same effects when the circulation is arrested as when it is free, and when the nerve is cut as when it is entire. The phenomena noticed during the free circulation are as follow:—Dilute potash or ammonia applied to the web causes extreme contractions of the arteries; the diameters of the veins remain unchanged; soon the blood in the veins begins to move more slowly, then stops, then oscillates, then finally commences to flow backwards towards the capillaries. This backward flow in the veins lasts until all the capillaries touched with the solution are in a state of complete stagnation. That this reverse current is not owing to diminished pressure on the arterial side by reason of the contracted artery, is proved by the fact that solution of salt causes the same reverse current, although the arteries are dilated by its use and not contracted. That the reverse current in the veins is not owing to altered pressure from the heart or from arterial changes, is proved by this fact, and also by the occurrence of the phenomena when the circulation is arrested by ligature.

When the ligature is applied, the circulation in the part below is of course arrested, and after a time the blood is quite tranquil: after a still longer interval, however, (four to eight hours) the blood commences to move again, and passes directly from the arteries, and reversely from the veins into the capillaries; in this way, without the application of irritating agents, stasis occurs. This stasis is, however, incomplete, since on removing the ligature the heart's action has sufficient power to force on the stagnant blood, and thus to free the circulation. Very different is the case when reagents are employed. If to the vessels in the web of the ligatured foot, as soon as the movements of the blood have stopped, a solution of potash or ammonia, or hot water, or dilute acetic acid, or rock salt, urea, nitre, carbonate of soda, or chloride of calcium in cold saturated solution, is applied, at once the blood in both arteries and veins commences to flow towards the capillaries, in which vessels the blood-corpuscles are pressed closer together, as fresh ones are added to them: not only the capillaries but the little arteries and veins are thus filled. When this stasis is fully completed, if the ligature is removed the heart's action cannot at once force on the blood and clear the way—the blood-particles must first undergo the well-known changes; they become paler, disclose their nuclei, and finally begin to get loose from each other, to oscillate, and at last to be carried away in the torrent of the circulation. Other reagents, as syrup, or *very dilute* sulphuric, nitric, hydrochloric and nitric acids, cause a stasis similar in all respects, except that when the ligature is removed the blood-stream is sufficient at once to carry away the stagnant blood. If these reagents are tried at once on an unligatured web, they do not cause any stasis; on the contrary, they cause the superficial capillaries to become empty of blood, while in the deeper-seated a rapid circulation continues. Some other reagents, such as phosphate of soda and borax, have no effect at all on the blood-movements. The author concludes, that the heart's action and the continuance of the circulation of the blood are not necessary conditions to the stagnation, but that this occurs in consequence of certain movements in the blood itself.—*Müller's Archiv.*, 1852, Heft 3, p. 361.

On Blood-Corpuscle-Holding Cells. By M. VIRCHOW.

[MR. WHARTON JONES'S Review (p. 32) is purposely restricted to the consideration of the blood-corpuscle-holding cells of the spleen, and to the physiological doctrines which have been based upon the observations of Kölliker and Gerlach, and their respective followers. Virchow treated the question on the broader basis, and discusses the origin of these cells without especial reference to the seat in which they were first noticed.]

The author commences his paper with a discussion on the present opinions of cell-growth, and on the application of these opinions. 1. The blood-corpuscle-holding cells. Against the existence of these cells he had formerly argued that it was impossible to formularise, according to the known laws of cell-formation, the presumed envelopment of a heap of blood-cells by a cell-wall, and the subsequent conversion of a cell so formed, into an actual nucleated cell. He did not, however, deny the possibility of cells being thus formed, nor did he affirm the universal truth of the cell-formation described by Schwann, nor did he reject the possible origin of cells by cleavage of nuclei. Still, admitting these modes of formation, it is possible to frame a formula which shall include all known facts—viz., for cell-formation it is necessary to have a *cyto-blastema* of determined chemical composition, and *centra*, round which the formation occurs. As blastema we only recognise the so-called fatty-albuminous histogenetic matters, which are all amorphous: there are no facts to show that organized tissues, or entire cells, as bloodvessels or cerebral substance, can serve as cyto-blastema. In addition to the blastema, there is in all cases an external influence necessary (such as the contact and action of living tissues, &c.) which calls forth in the blastema those combinations which develop in it the power of organization.

2. After discussing these points at great length, and showing that the formation of the blood-corpuscle-holding cells cannot be brought under the same formula, the author passes on to a consideration of pigment-building. Pigment-grains were formerly supposed by Virchow to arise in two ways, either in the interior of the single or aggregated shrunken blood-particles, or from the passage of hæmatin out of blood-particles into other textures. But in addition, the author's later researches have proved that pigment-building may occur in fat, by imbibition of biliary colouring matter, &c. It is evident, therefore, how cautiously any inductions must be drawn from the existence of pigment-grains in cells; and if such pigment-grains resemble shrunken blood-particles, this may yet be a mere deception, and may not prove that the particles are first enclosed in cells, and then pass into pigment.

3. After these preliminary considerations, the author arrives at the practical examination of the subject. He formerly denied the existence of blood-corpuscle-holding cells, as he had never been able to find them. *He now, however, admits their existence: he has found, though infrequently, cells with nuclei and decided blood-corpuscles in the spleen, has isolated them, and rolled them over in the field of view.* In tumours they are much more common.

Admitting, then, the existence of these cells, how are they formed? The opinions of Kölliker have been shown to be doubtful. The bold conjecture of Rokitansky, *that these blood-corpuscles are new formations in the cells*, is not adopted, though the author does not reject it, and even seems inclined to admit its probability. As some evidence in its favour, he describes a remarkable cell discovered in a fatty liver; the greater part of the cell was occupied by a clear round body resembling the cavity described by himself in some cancer-cells. Within this body or space were more than a dozen corpuscles disposed round a hyaline bullet-shaped body; between the cavity and the outer cell-wall were two nucleoli-like bodies; the rest of the cell was in a state of fatty degeneration. Were these corpuscles new formations, or were they enclosed by this double wall laid around them?

But besides these two hypotheses, a third may also be proposed. May not the corpuscle-holding cells arise by the entrance from without of blood-corpuscles into cells already formed? As in the experiments of Œsterlen, Mensonides, and Donders, solid particles pierced the walls of vessels, and penetrated by pressure through tissues; so may not also the heavy and tough blood-corpuscles break through the tender walls of new-formed cells, and thus gain access to their interior? The hypothesis is backed by no positive facts, but as some evidence that such a thing is possible, Virchow refers to an observation of E. H. Weber, who having injected the liver, found some liver-cells filled with injection: in some of the cells the place of entrance of the injection could be seen, in others no opening could be found.—*Virchow's Archiv.*, B. iv. Heft 4.

The Nerves of the Heart. By Cloetta.

The author has examined the nerves in the hearts of men, calves, and oxen, in order to test the accuracy of the description of Dr. Robert Lee, which he in great part confirms. The nerves passing down from the great plexus between the aorta and pulmonary artery distribute themselves partly to the tissue, and partly form numerous ganglia in the auriculo-ventricular groove, as described by Lee, and as previously noted by Remak. These superficial nerves are very soft and tender, and the author has not found them so numerous as Lee did. The author doubts whether the term "fascia cordis" should be applied to the thick uniting tissue; and he states also, that the swellings formed by the nerves crossing the vessels are not ganglia, although they have the greatest external resemblance to them. They contain, however, no ganglion-cells. The author confirms Lee's statement, that the left ventricle is more richly supplied with nerves than the right, as is best seen in oxen. Whether in hypertrophy the nerves grow, he has not determined.—*Wurzburg Gesell. Verhandl.*, B. iii. Heft 1, p. 64.

On Vierordt's method of Blood-analysis. By SCHMIDT.

[THE method of determining the number of red-corpuscles proposed by Schmidt,* has been attacked by Vierordt, who has himself brought forward a new method, which is now criticised by Schmidt. Vierordt proposed to *count*, under the microscope, the number of blood-globules, as seen in a certain capillary tube of known dimensions. We need not enter into the details of this mode, nor into the strictures made upon it, but merely indicate the present paper to those who are interested in this important subject.]—*Henle's Zeitschrift*, Band ii. Heft 3, p. 293.

Crystals in Blood. By KUNDE and FUNKE.

[These are two elaborate papers on the crystals which may be obtained from blood under the microscope by the addition of a small quantity of water, alcohol, ether, &c., to various kinds of blood, both from men and from the lower animals. We defer their analysis, as we intend to review, shortly, all the observations which have been made on crystals in organic fluids. We may notice, only, that although the inquiry is as yet merely in its infancy, it is sufficiently advanced to give us some hope that it will not be barren of results, but will eventually throw some light on the nature of the fluid of the red corpuscles. Both the authors notice the extreme difficulty of obtaining the crystals in quantity, and this is at present a great bar to a satisfactory chemical investigation.]—*Henle's Zeitschrift*, Band ii. Heft 3, pp. 271 and 288.

Crystals of Hæmatoidin in the Bloody Fluid of a Tumour. By DR. BACON.

IN the bloody fluid obtained by puncturing a large cancerous tumour with an exploring needle, the author observed cancerous elements, blood-discs, and rhombic crystals, of a fine transparent crimson and ruby-red colour. No chemical reactions were observed. In a few hours the crystals had entirely disappeared. The author considered them the hæmatoidin-crystals of Virchow.—*American Journal of Med. Science*, Oct. 1852.

[Some little confusion seems likely to arise, unless care be taken, about these crystals. The hæmatoid-crystals of Virchow are broadly distinguished by their extreme stability and their comparative indifference to reagents. The crystals described above are evidently similar to those noted by Funke in the splenic blood of the horse, and afterwards, and almost simultaneously, by Kunde, Funke, and Parkes, in human blood. These crystals are of another order, and are distinguished by their extreme destructibility.]

The Laws regulating the Bodily Temperature and the Frequency of the Pulse. By R. LICHTENFELS and R. FROHLICH.

THE authors have made a most careful series of experiments on themselves. Each experimenter is twenty-two years of age; the pulse of one of them is normally 71 per minute, that of the other 88; the normal temperature of each is 98·434. During the course of the experiments, they rose shortly before 7 A.M., took coffee between 7 and 8, had dinner at 2, and evening-coffee between 7 and 8.

1. *Daily rate of pulse, and temperature.*—The influence of the period of the

* Charakteristik der epid. Cholera. Leipzig, 1850.

day, *per se*, was very trifling, but both pulse and temperature were greatly affected by food. Before the morning-coffee the pulse was lowest; by the end of the first hour after coffee it rose, on an average of many observations, nearly 8 beats per minute; it was slightly less rapid at the end of the next hour; at the end of the third hour it was only 3·3 beats; and at the end of the fourth, 2·77 beats over the original number. The pulse did not sink to the number noted before coffee, till six hours had elapsed. The mid-day meal raised the pulse again, and this occurred apparently sooner after protein than after starchy food, but to a less extent. After the evening-coffee, the pulse, which had fallen, again rose, but to a less extent, and its declension occurred more rapidly.

The temperature of the body was affected in a similar way by food, but the augmentation occurred later than the rising of the pulse; so that the temperature was often at its maximum when the pulse had fallen considerably towards the point from which it had risen. The average amount of increase is about $\frac{1}{3}$ of Fah. The greatest average range of the thermometer in the course of the day (between 7 A.M. and 10 P.M.) was rather less than a degree of Fah.

2. *Influence of customary liquid.*—The experiments were performed in the afternoon; each lasted 100 minutes, and the greatest tranquillity of body was preserved. After *beer*, the pulse sank 6 or 7 beats in from 10 to 15 minutes; in 30 minutes, it regained its former frequency; much before this time, the subjective feelings of slight incipient intoxication were felt. In about 2 hours, the pulse was heightened nearly double as much as it had been depressed. The temperature, after the use of beer, fell about one-third of a degree of Fah. After *wine*, the pulse at first fell in the same way, and then rose greatly; the temperature fell about half a degree of Fah. The same occurred with alcohol, but afterwards the temperature rose about a quarter or half of a degree of Fah. Cold water lessened, at first, the number of the pulse, and lowered the temperature. In 15 minutes both returned to their former amount. Coffee, as already said, raised the pulse, but more in the morning than in the evening.

3. *Influence of fasting.*—Fasting for from 20 to 21 hours lowered both pulse and temperature. At the end, the pulse was from 12 to 16 per minute; the temperature as much as 1·8° Fah., under the normal. The curious observation (made also by Davy and Gierse) was noted, that at the period of customary meal-times both pulse and temperature slightly rose.

4. *Influence of muscular movements.*—Various experiments were tried with different kinds of movements. 1. A ten-pound weight was allowed to hang from the arm for five minutes, the body being tranquil; the pulse first fell in frequency, then rose; its greatest frequency was after the termination of the experiment. When the weight was on the left arm, the rise was nearly double that which occurred when it was on the right arm. 2. A weight of one pound was held out horizontally; the pulse rose and fell remarkably several times. 3. A weight of two pounds was rapidly swung round and round with one arm, while the other was placed on a table, that the pulse might be counted. This exercise produced the greatest effect on the pulse, raising it sometimes from 30 to 50 beats. 4. Long-continued moderate exercise, carried on to fatigue, raised the pulse greatly for some considerable time, but never produced the enormous rise noted in the previous kind (3) of muscular exertion.

5. *Influence of narcotic poisons.*—*Belladonna* and *atropine* at first diminished the frequency of the pulse (16 to 20 beats), but after a variable time (50 to 117 minutes), the pulse again rose (12 to 30 beats). The smaller doses produced greater primary sinking than the large, but required much longer time to do so; on the contrary, the larger doses produced much greater secondary rising; that is to say, the maximum sinking-point is inversely, and the maximum rising-point is directly, proportioned to the amount of the drug. It might be said that small doses depress, larger excite, the pulse. The temperature was diminished in all cases. *Opium*, especially in small doses, caused rising of the pulse, but afterwards there was great sinking, and the temperature diminished. The *Cannabis Indica* produced many

periods of rising and falling; the temperature rose for about four hours, and to as great a degree as ·7° or 1° Fah. *Chloroform* and *ether*, if not pushed to too deep narcotism, raised both temperature and pulse.—*Denkschrift. d. math-naturw. Klasse d. k. k. Akad d. Wiss zu Wien; und Schmidt's Jahrbuch*, 1852, Oct. No. 2.

Contractile Tissue of the Iris. By JOSEPH LISTER, Esq.

IN a fresh portion of human iris removed by operation, the author was able (in certain places) to isolate muscular fibre-cells, of which he gives representations. The length of the longest cell was 1-125th of an inch, and its breadth 1-3750th; others were shorter, but nearly as broad. He confirms Kölliker's statement of the existence of a sphincter pupillæ in the eyes of men, rabbits, guinea-pigs, and horses; and describes it as consisting, at the pupillary margin, of contractile fibre-cells without any uniting tissue. The author is inclined to doubt whether the individual cells are united end to end into fibres, as they are separated with great ease. As regards the dilating fibres, the author thinks that the fibres described by Bowman as contractile are the outer cellular coats of the vessels; but he has been able to discover in the outer part of the human iris, "long delicate fasciculi, whose faint outline, absence of fibrous character, and possession of well-marked elongated nuclei parallel to the direction of the fibre," convinced him they were muscular fibre-cells. In the horse, also, these cells can be seen, when from the anterior surface of the iris a tough membrane ("composed of short felt-like fibres gelatinized by acetic acid") is peeled off. The muscular fibre-cells of the dilator can be seen, running from the outer part of the iris, at various angles with the sphincter, and finally making a short curve, and blending with it. The fibre-cells of the dilator are held together more firmly than those of the sphincter, and therefore cannot be defined satisfactorily.—*Microscopical Journal*, No. 1, p. 8.

Contribution to the Knowledge of the Elastic Tissue. By Dr. ZOLLIKOFER.

ZOLLIKOFER studied principally the products of decomposition of this tissue. After having perfectly cleared the ligament-nuchæ of the ox of fat and intercellular tissue, he boiled it from 48 to 50 hours with diluted sulphuric-acid, saturated the clear brown solution with aqua calcis, and then boiled the whole mass, which had become white and pappy; during the process of boiling it lost its colour, and developed a smell similar to that of the flowers of berberis. After the separation by filtration of the gypsum, and the inspissation of the fluid to the consistency of syrup, besides the salts of lime, a yellowish, granular, crystalline sediment formed, which, when washed out, and repeatedly boiled with alcohol for the separation of the salts of lime and of the colouring matter, was recognised as *leucin;* it presented the following characters: crystallized out of alcohol (93°), it formed a perfectly white crystalline powder, glittering like mother-of-pearl, of a greasy touch, without smell and taste, and grating between the teeth. According to the strength and concentration of the alcohol, from which it deposited, the crystals had sometimes more the form of pillars, at other times more that of rhomboidal tables. As characteristic for the leucin obtained in this way, Zollikofer considers the formation of concentric, rosette-like groups of crystals, in which each single crystal is to be more or less distinctly recognised. By carefully heating to about 400° Fah., it may be completely sublimated, without melting, in the form of a white mist, smelling like burnt horn. It dissolves in about 27 parts of cold, or a rather smaller quantity of hot water, in 1040 parts of cold alcohol of 96°, and in 800 parts of hot alcohol of 98°; it is insoluble in ether, but most easily soluble in ammonia. The solutions of leucin do not act on vegetable colours, nor are they precipitated by any reagents; with sulphate of copper and liq. potassæ it produces a light blue solution, which undergoes hardly any change by boiling; in concentrated nitric acid it dissolves without disengaging any gas, and an acid combination of leucin and nitric acid (leucin-salpetersaure) precipitates from this solution in crystalline crusts; rhomboidal tables, of a silk-like gloss, are obtained from the solution in hydrochloric acid. The elementary analysis of leucin shows the following composition:—C 54·96, H 9·93, N 10·68, O 24·42 = $C_{12}\ H_{13}\ N_1\ O_4$.—*Ann. der Chem. und Pharm.*, vol. lxxxii. Heft 2, 1852.

On the Influence of the Sympathetic Nerve on the Animal Temperature. By Dr. T. Budge.

To the communication of Bernard's observation, that by dividing the sympathetic nerve between the first and second ganglion cervicale, the temperature of the corresponding side of the head soon rises several degrees, and remains increased for some days ('Compt. Rendus,' Mars, 1852), Dr. Budge adds the remark, that he had observed the same fact already in December, 1851. In another experiment, Dr. Budge destroyed the lumbar part of the spinal marrow, after which he observed a considerable decrease of temperature in the whole of the posterior part of the body. —*Froriep's Tagesber.*, No. 512, 1852.

Anatomy of the Male Mammary Gland. By Professor Luschka.

The *situation* of the *nipple* is by no means quite constant, although it is generally used as a fixed point in the physical examination of the chest. Among 60 cases, it was 44 times in the space between the fourth and fifth ribs, 6 times upon the fifth rib, 8 times on the fourth rib, twice between the fifth and sixth ribs. Not rarely it was different on both sides. The average distance from the margin of the sternum is about $4\frac{3}{4}$ inches, but it is sometimes half-an-inch less. Concerning the *contents* and *structure*, Luschka has frequently squeezed out from the nipple a fluid, clear like water, containing (1) molecular granules, (2) roundish globules, 0·004 to 0·006mm. in size (composed of small granules), and (3) spherical pale cells, as large as 0·012mm. with an excentrical nucleus, in form and appearance like the granular globules just mentioned. The areola and the nipple itself are endowed with *sebiparous* glands and numerous *papillæ*, as well simple as compound ones, in which the minute bloodvessels frequently form spiral slings, which might be mistaken for terminating slings of the nerve-tubes, but he could not find anything like Wagner's corpuscula tactus. As well in the nipple as in the areola Luschka met with many organic muscular fibres, such as Kölliker describes, in the female breast. The *parenchyma* of the male mammary gland is very scarce; it appears as a white fibrous mass, without showing any arrangement into lobules; it contains now and then a few small miliary vesicles, which are sometimes a little larger, and show more the form of a bag about 1·0mm. broad, and 2 to 4mm. long, ending in the nipple. By the microscope is recognised: 1. A *fibrous stroma*, formed by (*a*) *cellular tissue* as the principal component, which exhibits the different forms of development more numerously than in any other organ; (*b*) *elastic fibrillæ* in large number, without nuclei, unchanged by acetic acid: (*c*) *organic muscular fibres* in small number, with distinct nuclei. 2. The *glandular tissue* displays only few of the *glandular vesicles* which are so numerous in the female mamma; no regular acini are found, but sometimes a few of them (0·5 to 1·0mm. in size) are seen uniting by their tubular prolongation into a larger duct, which disappears in the nipple; now and then small excavations are found, seemingly formed by the combination of several vesicles. The thin lining membrane of these vesicles and excavations consists of polygonal epithelium, with a small granular nucleus; within their cavities, small, nearly round, globules (0·004 to 0·006mm.) are discovered, which appear composed of very small granules, scarcely changed by acetic acid, but easily dissolved by liquor potass, leaving only molecular granules; some of them are surrounded by a thin membrane, which seems more or less protracted at the ends, and disappears to the eye by the addition of acetic acid; Luschka considers them to be an early form of development of intercellular fibres.—*Müller's Archives*, 1852, No. 4.

On the Reproduction of Nervous Substance, and on the Structure and Functions of the Spinal Ganglia. By Dr. A. Waller.

Dr. Waller, after having made many experiments on different animals, principally warm-blooded ones, of an early age, and frogs, considers himself entitled to the conclusion: "That the old fibres of a divided nerve never gain anew their original structure and function, and that the reproduction of nervous substance does not take place merely in the cicatrix itself, but also downwards into the terminating ramifications. The *old* fibres gradually waste, and after a month or later, *new* fibres are formed, which are pale and transparent, possess no double contour, present a very unequal diameter, being on the one place very thin, on the other, varicose, like the fibres of the spinal marrow. In the peripheral part of the glosso-pharyngeal nerve of a frog, three months after the section, their size was only about one-sixth to one-third of the original fibres; they resembled, therefore, much more the ramifications of the nerve in very young frogs. In the central part of the cut nerve the fibres remain unaltered. Concentrated acetic acid dissolves the membrane of the newly-formed fibres, leaving fusiform nuclei; the membranes of the original fibres are completely dissolved, no nuclei being left. The reproduction of fibres, and the return of function, proceed in the same proportion. Of great importance are Dr. Waller's experiments for the understanding of the *structure and function of the ganglia.* While, as he has previously shown, all motor nerves, separated from their cerebro-spinal centre, become entirely changed in their microscopic appearance, the peripheral part of the sensitive spinal nerves, the root of which is cut through between the spinal cord and the spinal ganglion, remains unaltered as long as the connexion with the ganglion is maintained. Ten or twelve days after having divided one or both of the roots of the second cervical nerve, he was enabled to make the following observations: 1. That part of the sensitive nerve which is situated between the place of division and the ganglion, is disorganized in the same manner as any dissected nerve in its peripheral end. 2. Tracing the disorganized fibres into the interior of the ganglion, they are seen mixed with normal fibres; the disorganized ones appear to pass into ganglionic globules, which are likewise altered, seeming to be deprived of their contents, and to consist merely of a thin, indistinct membrane. 3. The normal fibres appear to end by very thin filaments passing into normal ganglionic globules. 4. All the fibres originating within the ganglion are in their normal state. 5. The motor fibres are completely disorganized in the whole of the peripheral part of the nerve (no motion is produced by galvanism, or any other stimulus). 6. After having divided only the posterior root, all the fibres *below*, or on *the other side*, of the ganglion were normal. 7. After having divided the nerve *below* the ganglion, or after having cut out the ganglion, all the fibres in the peripheral part were disorganized. It is evident, from this, that the spinal ganglion acts as a nervous centre for the sensitive fibres, but not for the motor ones. Dr. W. promises to give soon more detailed observations, as well on the same subject as on the function of the nervus vagus and sympatheticus.—*Müller's Archives*, 1852, No. 4, p. 392.

On the Glands of the Mucous Membrane of the Human Stomach. By Dr. A. Ecker.

From the careful examination of the stomach of several suicides, Ecker gives the following statement concerning the gastric glands. In almost the whole of the stomach are merely simple cylindrical glands $\frac{1}{2}$ to $\frac{3}{4}'''$ long and $\frac{1}{50}'''$ thick, going in a straight line through the mucous membrane, ending in a clublike swelling, very rarely exhibiting a division of the blind end. They contain round and angular cells, of a diameter of 0·017 to 0·020^{mm} with a nucleus composed of larger granules; towards the open end are seen more developed cells, towards the blind one, more nuclei and granular matter. At the *cardiac end*, besides these simple glands, other glandular follicles are situated, the blind end of which is divided and pouched; their contents are the same as just described, except that more fat-granules are seen towards the blind end. Near the *pyloric orifice* he constantly found, besides the simple, also *acinous glands*, deciding by this against Frerichs ('Wagner's Handworterbuch,' iii. 748) and Kölliker ('Mikroscop.-Anatom.' vol. ii. pp. 139 and 149), in favour of Bischoff ('Müller's Archives,' 1838, p. 515). We observe, therefore, no abrupt change in the structure of adjacent parts of the intestinal tube, but only a gradual one, single acinous glands being situated in the mucous membrane of the œsophagus, and a larger quantity of them in the duodenum.—*Henle's & Pfeufer's Zeitschrift, f. ration. Medicin.*, 1852, vol. ii. p. 243.)

Chemical Composition of the Uterine Fibres in Pregnancy. By SIEGMUND.

THE point of interest in this communication is, that kreatin was obtained from the tissue of the gravid uterus. Formic and acetic acids were also present.—*Wurzburg Gesell. Verhandl.*, Band iii. Heft 1, p. 50.

The Crystals of Carbonate of Lime in Urinary Sediments. By G. SIEGMUND.

IN the sediment often found in the urine of guinea-pigs, crystals, which chemical analysis prove to be carbonate of lime, are seen of the following forms: little rods, isolated or crossing each other at right angles, or grouped rosette-like, rods with nobby ends, and globules, two of which are often united by a rod. These last crystals are dumb-bells, and they lead the author into a discussion on the dumb-bell crystals of Golding Bird and others, from which he concludes that about dumb-bell crystals there is nothing *specific*—i. e., that it indicates no special chemical body, but may be formed by many substances. Returning to the dumb-bell and other crystals of the carbonate of lime in the urine of the guinea-pig and the horse, the author traces, with great care, all modifications to their fundamental form, the rhombohedron, and to various combinations of this. The typical form of dumb-bells is constituted by two opposed rhombohedra; the rod-shaped crystals (whose structure can be best made out when they are large) are formed by two rhombohedra joining in the direction of their main axes, and having the interspace filled up by new deposition, which goes on until the origin of the form may be undiscernible, and from the two rhombohedra a six-sided prism may arise. The mode of formation of these crystals and of the dumb-bells is, therefore, almost the same, and almost all the crystals of carbonate of lime can be thus referred to modifications of the rhomb. We must refer to this interesting paper for the full details, for the diagrams to illustrate the scheme, and for the representations of the various crystals as actually seen.—*Virchow's Archives*, Band iv. Heft 4, p. 505.

PATHOLOGY AND PRACTICE OF MEDICINE.

Contribution to the Pathological Physiology of Pneumonia.

By Dr. G. ZIMMERMANN.

ZIMMERMANN considers the most minute observation of the natural course of acute disease to be the only way of elucidating the laws of these typical processes. Although he thinks those cases most fit for the purpose in which no remedies are employed, yet he does not at all exclude those in which the treatment is active. As an instance of how to observe, he describes the course of a case of pleuro-pneumonia, treated by repeated venesection, application of blisters, and internal administration of antiphlogistic remedies, in moderate doses. From the first day of the fully established disease, till after the return of convalescence, he has noted twice daily (at 8 A.M. as the time of remission, and between 5 and 6 P.M. as the time of exacerbation) the temperature under the tongue, the frequency and other qualities of the pulse, and the number of inspirations on almost every day, the quantity of urine secreted in 24 hours, its specific gravity, and the quantity of lithic acid contained in it. Of great interest are the *critical symptoms*, remarked on the 3rd and then again on the 9th day, after which we observe a steady progress to convalescence. The first note on the *temperature* was taken four hours after an attack of general vehement rigors, which the patient himself considered as the commencement of the disease; it was then as high as 103°10, which, being 4°14, over the normal warmth of that individual (98°96), makes Zimmermann inclined to conclude, that the disease had commenced before the occurrence of the rigors. Between the evening of the 2nd and the morning of the 3rd day, a decrease from 104°0 to 99°86 was observed, coincident with a general abatement of the constitutional symptoms of pyrexia, but on the evening of the 3rd day, the temperature was again as high as 105°80 and the physical examination exhibited the signs of hepatization, which had not been present in the morning. Between the evening of the 8th and the morning of the 9th day, the warmth had decreased from 106°16 to 99°50; at the same time all the other febrile symptoms disappeared almost completely, after they had reached a very high degree on the two previous days. It is interesting to remark, that although the frequency of the pulse did not exceed the normal average after that period, yet the temperature increased again to 102°20, and remained so for more than a fortnight longer. The entrance of the change on the 9th day, does not appear to Zimmermann as merely accidental, but he looks at this day as a critical one; and also the coincidence of several alvine evacuations effected by calomel seems important to him, as he thinks that the crisis may be promoted by our remedies, if their action takes place shortly before or on the beginning of a critical day. Concerning the *urine*, the quantity secreted in twenty-four hours was between 9040 and 29,030 grains; the minimum is noted on the 5th, the maximum on the 9th day; the average quantity of 24 hours before the 10th day, was 15,215 grains, with an average specific gravity of 1·0198; between the 10th and the 40th day, 16,510 grains, with 1·0208 specific gravity. The quantitative examination for *urea* was not made before the 8th day, on which 1081 grains were contained in the urine in 24 hours; on the 9th day, only 884 grains; and a few days later, not more than 400 grains (which is about the average quantity during health—*Lehmann*, Physiol. Chem., vol. i. p. 167). The quantity of *uric acid* was almost normal during the commencement of the disease, but after the 6th day it appeared considerably increased, and reached the maximum on the 9th day, when it amounted to 3770 grains in twenty-four hours (which is at least three times more than the average during health—*Lehmann*, l. c. p. 217). Zimmermann considers this increase of the uric acid and its salts, at the period of the critical change, as important for the doctrine of *crisis*, as he had opportunity to observe the same phenomenon, not only in pneumonia, but also in typhoid fever, ague, measles, &c. From three examinations of the blood obtained during the first seven days in this case, and from his previous experience, Zimmermann considers as the most important changes in the *inflammatory blood*—that it is less coagulable than the blood in health; that the quantity of fibrine is augmented; that the coloured blood-globules are diminished in number, and possess an abnormal disposition for the formation of rolls; that the colourless globules are found in an increased proportion, and show, likewise, a tendency to join in groups.—***Prager** Viertiljahrschrift*, 1852, vol. iv. p. 97.

Decennium Pathologicum: Contributions to the Pathology of Chronic Disease. By Thomas K. Chambers, M.D.

These eight papers are the beginning of a series, designed to test, by statistics, the accuracy of our generally received opinions concerning the effects of disease. The author has, as yet, not got beyond "Tuberculosis." The groundwork is the record of about 2500 fatal cases, of all kinds, preserved in the post-mortem books at St. George's Hospital. The first step was the construction of "an index, in which each morbid appearance observed is alphabetically enumerated, with a reference to the volume and page where it is found:" on the plan, we presume, of Giambattista Morgagni's ***Index visorum in Cadaveribus***, in the Venice edition of the ***Epistolæ de causis et sedibus morborum***. Then instances of various lesions were arranged in tables, under the head of the various viscera affected or the nature of the disease, and the index and tables submitted to mutual correction. The tables are not published, but simply a numerical enumeration of various facts which appear in them, of which, of course, only an outline can be here attempted.

The two first papers are devoted to statistics of the general mortality of the hospital, as a mean of comparison with those of particular diseases, in order that facts which appear in the history of the latter may be assigned to their due causes, whether those are the peculiarity of the malady, or of the locality in which it is observed.

The results are—1st. There is an excess of about $\frac{1}{36}$ in the male deaths throughout the country.

2nd. This excess is greater in hospitals, and amounts, at St. George's, to $\frac{44}{100}$, when accidents are excluded on both sides; and to $\frac{65}{100}$, accidents included.

3rd. This real excess is augmented in the general hospital reports by an excess in surgical male patients.

4th. The causes of the real excess are in a great measure of social, and not of physiological origin.

5th. The excess of accidental deaths is on the male side at all ages, but most in the middle period of life.

6th. The excess of deaths from other causes was, up to 20, slightly on the female side, but much less so than in the ordinary population of England. After 20, it was on the side of the males, and attained its maximum at 35.

In the third paper the prevalence of tuberculosis and the influences upon it of age and sex are discussed. In no less than twenty-five per cent. of the cases examined, tubercle was found. The next important point is the great preponderance of males in those affected. "In every 100 men there were more than 27, and in every 100 women not quite 22, affected with tuberculosis." The variations of this preponderance at different ages are then shown.

The fourth paper examines the seat of tuberculosis, and then the usual position of it in the lungs, when they are diseased. The idea of its preference for one lung over the other is held to be a fallacy dependent on the usual mode of examining living patients. Pneumonia, pneumothorax, and the conversion of tubercle into chalk are then examined. In the fifth and sixth papers, the location of tubercle in other parts besides the pulmonary organs is gone into with considerable length of detail. The prevalence of tubercle in the kidneys, much greater than had been represented by previous pathologists, calls forth remarks on the connexion between those organs and the skin, and the consequent importance of guarding it from climatic influences equally with the lungs. Tuberculosis of the intestinal canal is shown to be almost entirely confined to the junior periods of life, and attention is therefore drawn to the different precautionary treatment of patients at various ages. This part of the subject is concluded by speculations on a connexion which appears, by their order in the tables, to exist between the liability of a part to tubercle and its degree of venosity; the inference being exactly the reverse of the doctrine professed by Rokitansky. In the seventh paper, the complication of tuberculosis with diseases of the heart is examined. The connexion is shown to be unusual, by a comparison of the frequency of cardiac lesions in tuberculous and non-tuberculous bodies, but still is much more common than observers of living patients only are aware of. In the post-mortem examination at St. George's, lesion of the heart occurred in seven per cent. of cases of pulmonary tubercle. In the eighth paper, the complications of non-tubercular diseases of the brain, such as softening, inflammation, effusion, &c., with pulmonary tuberculosis, are examined in the same manner as heart-disease was in the former one.

The eighth paper inquires into the connexion of tuberculosis and cerebral disease, both in respect of cases where tubercle existed in the brain and where it did not. Omitting the numbers, the conclusions of the observation of the 2161 cases examined at St. George's Hospital in ten years are—

1st. That the secondary consequences of tubercle in the brain were the same, whether the tubercle be in the substance or membranes.

2nd. That the symptoms of these secondary consequences were, in the case of inflammatory action, pretty uniform, but otherwise obscure and variable.

3rd. That independent of tubercular deposit, idiopathic inflammatory conditions of the meninges were most common in the tubercular diathesis—nay, almost peculiar to it.

4th. That the same diathesis disposed also to softening of the cerebral substance, probably of an inflammatory character; but that other diseases had nearly as great a tendency to produce softening, whether truly inflammatory or not is unknown.

5th. That serous effusion on the brain was less usual in tuberculous persons than in others.—*Medical Times and Gazette,* August, December, 1852.

The Respiration in Pressure on the Brain. By Dr. Landgraf.

Dr. Landgraf calls attention to the state of the respiration, in cases of cerebral pressure. It is frequently not stertorous and laboured, as described in books, till the agony; but it is interrupted, that is to say, after from six to twelve tranquil and easy respirations, a long pause ensues. The author details cases in proof of the existence and diagnostic value of this sign.—*Deutsche Klinik,* 1852, p. 39.

Temporary Albuminuria. By Dr. Begbie.

Dr. J. W. Begbie alludes to the phenomenon of albuminuria in the following diseases:

1. *Scarlatina Simplex.*—He confirms his former statement that about the period of desquamation, albumen can *almost* always be found; its presence is associated with renal epithelium, but not with casts of tubes.

2. *Cholera.*

3. *Erysipelas.*—Usually at resolution or during convalescence. Its presence is not constant, nor its quantity great.

The albuminuria in these three cases is called *desquamative.*

4. *Scarlatinal Dropsy.*—The albuminuria may, or may not, be temporary—blood exudation-corpuscles, and casts of tubes, accompany it.

The albuminuria in this case is termed *inflammatory.*

5. *Pneumonia,* at the period of resolution, in almost all cases.

6. *Typhus* and *Typhus Abdominalis* (typhoid).—From a consideration of the period when the albumen is observed in these last-named diseases, Dr. Begbie terms it *critical albuminuria.*—*Monthly Journal,* Oct. 1852.

On Albumen in the Urine of Various Diseases.

HELLER asserts that albumen is present in the urine in all kidney-lesions, though sometimes in small quantity, and that it exists in many other diseases, and often in greater amount.

1. *Pneumonia* and *Tuberculosis acuta.*—At the commencement of exudation, while yet the chloride in the urine is in undiminished quantity, no albumen can be found. As exudation increases, and as the chloride in the urine diminishes, a very small quantity of albumen appears, and continues for a long time. This appearance is not constant, but is very frequent. The greater the albumen, and the less the chloride in the urine, so much the worse is the prognosis.

2. *Pleurisy.*—Albumen does not appear so frequently, even when the chloride is much diminished. In the period of absorption it sometimes occurs, and is attended with carbonate and hydrothionate of ammonia.

3. *Acute Liver-Affections.*—In chronic or subacute inflammations, where the chloride of the urine is diminished, albumen appears as in pneumonia.

4. *Pericarditis* and *Endocarditis.*—In the first case albumen sometimes occurs; in the last, very seldom, even when the chloride is much diminished.

5. *Peritonitis.*—Albumen is frequently found, and continues sometimes long after the customary amount of chloride has reappeared, and morbus Brightii is then, perhaps, left.

6. *Metritis* and *Eclampsia Puerperalis.*—As in peritonitis.

7. *Cholera.*—More or less albumen.—*Archiv. für Pathol. Chem.*, Band i. Heft 8.

On Cirrhosis of the Liver. By M. MONNERET.

M. MONNERET, believing that the term cirrhosis has been applied to very different pathological conditions, endeavours in these papers to speak of it with some precision, while relating the cases of the disease which have come under his own observation. He defines cirrhosis as a chronic and apyretic affection of the liver, characterized by more or less impediments to the hepatic portal circulation, which leads to peritoneal effusion, dilatation of the collateral veins, and often to œdema of the extremities, and hæmorrhages from the various mucous surfaces.

The *anatomical changes* which almost always accompany these symptoms are induration and atrophy of the entire tissue of the gland, or of its vascular portion, with the yellow change of tissue, whether granular or not. The retraction of the hepatic substance gives rise to the diminished size of the organ, and at the same time that the portal veins become less visible, the yellow portion continues to predominate over the other, until it entirely supersedes it. The thickening of Glisson's capsule and the serous layer that lines the liver completes the anatomical character.

M. Monneret's memoir is based upon twenty-four cases, in fourteen of which autopsies were performed, this being the entire number of cases he has been able to meet with during the ten years his attention has been directed to the subject. He has compared these cases with forty others of the various lesions of the organ, as also with fifty cases of disease of the heart, in which the condition of the liver was examined.

He has taken great pains in the *measurement* of the liver, by means of plessimetry, having accurately measured in this way 100 patients. In a healthy man lying in the horizontal position, the hepatic dullness commences four centimetres (about 1½ inch) below the right nipple, and terminates at the edge of the ribs, which forms a tolerably exact natural inferior boundary. At the medium line, it is placed behind the scaphoid cartilage, passing a little towards the upper part of the epigastrium. Posteriorly and laterally it ceases at the level of the ribs. The following figures indicate the normal distances which separate the upper line of hepatic dullness from the level of the ribs. In thirty-one cases its mean height at the *median line* was 5·62 centimetres; its minimum 1·5, and its maximum 9·5. To the right *nipple* the mean was 12·64 centimetres, the minimum 7·8, and the maximum 18. In the *axillary* region, the mean was 10·57, the minimum 7·3, and the maximum 13. In the *scapular* region, the mean was 9·11, the maximum 14. In twenty-five cases the hepatic dullness commenced at four centimetres below the nipple. The *thoracic vibration*, perceptible to the hand, while the patient counts with a loud voice, extends three or four centimetres below the upper limit of hepatic dullness. In cirrhosis the normal limits of hepatic dullness have never been found exceeded. In some cases they are scarcely diminished, while in nine have they been so by more than five centimetres. The meteoric state of the intestines renders exploration sometimes difficult by pressing the liver into the thoracic cavity: but when authors speak of hepatic hypertrophy they confound other lesions with cirrhosis.

The *peritoneal effusion* is of very slow occurrence, fluctuation long continuing obscure, and the patient often not being aware of tumefaction of the abdomen. The progress of the dropsy is gradual, and there is not observed those alternations of increase and diminution seen in dropsy arising from hepatic congestion, whether connected with disease of the heart or other lesions. The fact of the dropsy becoming established before anasarca of the extremities, has been too much generalized. The integuments of the abdomen becoming infiltrated sooner than can be explained by the abdominal distension is explicable by the obstruction to the venous circulation.

In like manner the *dilatation of the veins* of the abdominal and thoracic parietes may become considerable before any notable effusion occurs. The most delicate *capillaries* undergo dilatation, so that their elegant arborescence becomes perfectly visible, and that by no means always when distension is greatest. These facts are explicable by the obstructed state of the portal circulation. In some cases, however, no such dilatation and inosculation of veins is present, ascites existing alone. In this point of view, it is interesting to remark that in five out of ten of Dr. Hillaret's cases of portal phlebitis there was no effusion. It is probable that in some of these, as well as in some of the cases of cirrhosis, the obstruction has only been partial, and hence the absence of some of the usual symptoms.

Not only, however, is the hepatic circulation thus disturbed in cirrhosis, but in all probability the *composition of the blood* has undergone change, giving rise to the *hæmorrhages* which are of such frequent occurrence. Epistaxis, slight in quantity, is the form that M. Monneret has usually met with; in some cases the stools have been tinged with blood.

We cannot abstract the details of the eleven *autopsies* M. Monneret furnishes an account of; but may advert to his summary of the most common lesions. 1. The liver is sometimes diminished by a third or one half its size. 2. Its surface presents more or less prominent lobules, separated by whitish furrows, the normal disposition of the hepatic structure being exaggerated. 3. The capsule of Glisson is thickened, whitish or opaque, more close and resisting, and intimately adherent. 4. This capsule is found in a hypertrophied state, in the interior of the parenchyma, as whitish lines, enclosing the hepatic lobules and sometimes yellow granules. 5. The change in the proportion of the two substances of the liver has long been admitted as a characteristic of cirrhosis; but while acknowledging the convenience of the expressions red or vascular, and yellow or bilious portions, and believing the affection is one which obstructs the circulation in the vena porta, Monneret doubts the correctness of these anatomical statements. M. Lereboullet believes in the conversion of the bilious into fatty cells, and Monneret has always found by the microscope that a large quantity of fat incrusted the biliary cells. He believes, however, that this fatty transformation itself is dependent upon the atrophy of some element of the parenchyma. 6. The extreme frequency of perihepatic peritonitis is of importance in the anatomical history of cirrhosis; for it may be asked whether this phlegmasia induces induration of the proper membrane of the liver, the loss of extensibility of which may be the cause of the hepatic retraction. 7. The degree of induration of the liver varies, being in some cases comparable to scirrhus, and apparently due to the predominance of the cellulo-fibrous portion. 8. There is great dryness of tissue from paucity of blood. 9. The alteration may occupy an entire lobe or even the entire organ; but it may do this in very different degrees. 10. As a negative character worthy of note, it may be mentioned that there are no lesions of the bile ducts, and the bile is apparently normal. 11. In several cases a new circulation has been found established in the fibro-cellular partitions of the lobules, a brilliant arterial network being distinctly visible. This may be regarded as a supplementary circulation of the hepatic artery, it having been observed in cases in which the vena porta was entirely or partially obstructed.

In respect to the *causes* of this affection, the habitual excessive use of alcohol is undoubtedly one; but in other cases bad and insufficient diet is alone discoverable. These circumstances explain the frequency of disease of the alimentary canal, which is observed in cirrhosis. The frequency with which inflammatory disease of the liver has preceded this condition is undoubted: but whether the thickened state of the capsule be an extension of this, or one of the lesions accompanying organic atrophy, is doubtful. Inflammation is not essential, as in certain cases it has not prevailed. The congestions of the liver which are so frequently seen in disease of the heart are not, as has been stated, first stages of this affection. Their effect is to lead to dilatation of vessels, while cirrhosis leads to their obliteration. In cirrhosis the yellow secreting tissue, formed of biliary cells, and yellow granules, is not hypertrophied, and only becomes more visible and prominent from the atrophy of the portal and vascular system. In hypertrophy the functional activity gives rise to jaundice, but not to obstruction of the circulation; while in active congestion and phlegmasia, even when slight, characteristic symptoms are present, as increase in size, tenderness, irregular fever, fibrinous blood, and icteric urine.

For the *treatment* of so fatal a disease M. Monneret has little to recommend. At least temporary benefit is sometimes derivable from alterative doses of blue pill, combined with Vichy or soda water, and alkaline or sulphureous baths. The diarrhœa and vomiting so obstinate in some of these cases are best treated by large doses of bismuth.—*Archives Générales*, tom. xxix. 385, & xxx. 56.

Ichthyosis Cornea. By H. Muller.

The author describes fully a case which, in point of severity, though not in respect of hereditariness, stands near the cases of the family Lambert. The crusts, on section, were found to be composed of a system of concentric rings, made up solely of epidermic-cells; between the rings, epidermis was irregularly arranged. The whole structure resembled Gustav. Simon's representation of a section of a wart, but the rings were not joined by the cuticle sheathing the papilla, and the masses lying between the rings by the cuticle formed by the parts between the papillæ, as in the case of warts, but each ring-system corresponded to a hair-bulb or to the duct of a sebaceous gland; spiral ducts of sebaceous glands pierced the mass. Ichthyosis, however, may be of various kinds, and especially in elephantiasis the papillæ are chiefly engaged, are long, and hardened and sheathed with abundant cuticle. The author proceeds to make some general remarks on ichthyosis and abnormal cuticular development, from which it is to be inferred that he believes ichthyosis may have, so to speak, various points of departure, and may be connected with hypertrophied papillæ, with altered hair-bulbs or sebaceous follicles, or even with degenerated sweat-glands.—*Wurzburg Gesell. Verhand.*, Band iii. Heft 1, p. 40.

Leucocythemia. By Dr. Hewson.

Charles Robinson, aged 17; never had ague, but had been in miasmatic districts; came under the care of the author; he was anemic, and had œdema of the lower extremities, and diarrhœa, and on one occasion epistaxis. The spleen, marked out by percussion, measured 8½ inches by 8 inches. The blood showed "a great redundancy of white corpuscles." He was treated with iron and quinine, to which mercury and nitre, hydrochloric acid, were added, for a short time. In five weeks he was cured: the spleen was of its normal size, and the blood was healthy. When he was seen three months afterwards, however, the colourless corpuscles were found to be too numerous, although he appeared in perfect health. The author (who is a grandson of Hewson, and who refers, with pardonable pride, to his ancestor's well known opinion on the functions of the spleen) has examined numbers of patients with splenic enlargement from intermittents, without detecting any leukæmia.—*Amer. Jour. of Med. Science*, Oct. 1852.

The Uræmic Hypothesis of Frerichs. By G. Zimmermann.

The author criticizes with great keenness the late statement of Frerichs, that the phenomena of the so-called urinary intoxication are owing to decomposed urea. The arguments against this view are chiefly drawn from an analysis of Frerichs's own observations, which are shown to be very incomplete. The following table gives the opinions of the one, and the criticisms of the other:—

Frerichs.	Zimmermann.
No bad consequences result from the injection of urea into the blood; the contrary results obtained by others, arose from the urine being unfiltered and loaded with epithelium.	The explanation is insufficient. Besides, in retention of urine, in the blood of Bright's disease, the blood is not, probably, normal; between urine injected and urine retained there may be no analogy.
Urea, at page 113, is said to be a harmless substance.	Urea, at page 50, is said to injure the cerebral organs.
The urea is decomposed by a ferment, which is generated more easily in febrile than in apyretic conditions.	The existence of the ferment is unproved. If it occurs, may not the ferment itself be the cause of the symptoms?
The urea is decomposed into carbonate of ammonia, as proved by the presence of ammonia in the breath, and by examination of the blood.	The blood of perfectly sound individuals may contain an ammoniacal salt, which passes off with the halitus sanguinis, and is easily detected by the fumes formed with hydrochloric acid. It is probable that ammonia is constantly given off through the skin (Gerlach and Schottin), lungs, and kidneys. The breath, as Marchand and Lehmann have shown, and as anybody may prove, often contains ammonia.
Carbonate of ammonia injected into the veins of dogs produces coma and convulsions.	The dogs were never killed with it. Would men be equally affected? Carbonate of ammonia given internally in large doses has not this effect.
Clinical experience confirms the hypothesis.	In Frerichs's book, blood was drawn three times in Bright's disease, but carbonate of ammonia was never sought for. The ammonia in the lung-exhalation was proved only in three cases.

Zimmermann proceeds to adduce other similar arguments, in order to show what he considers the incompleteness of the evidence brought forward in support of this ingenious hypothesis.—*Deutsche Klinik*, No. 37.

Obliteration of the Renal Veins in some Diseases of the Kidney, and especially Albuminous Nephritis. By M. Leudet.

M. Leudet, having met with two cases of albuminous nephritis, in which the renal veins were obliterated, has examined the various recorded examples of this lesion, with the view of ascertaining how far it stands in relation of cause and effect to this alteration of texture in the kidney; and whether it is also found in other affections of this organ. This latter point is answered affirmatively; for Rayer relates a case of such obstruction in hypertrophy of the organ. Rayer and Dance met examples of it in nephritis; while several cases are on record of propagation of uterine phlebitis to the renal veins. Still, it is in relation to "albuminous nephritis" that most examples have been observed. Rayer, Stokes, Peacock, and Delarnelle, have each related cases, and M. Leudet now adds two others. Yet even in this form of renal disease the lesion is rare, as, notwithstanding M. Leudet's attention has been directed to the subject for several years, these are the only cases he has met with.

Whether such lesion of the veins be cause or effect of the structural change, or a mere coincidence, is a doubtful point. Frerichs, however, regards the obstruction so produced as one of the causes of albuminous urine, and a source of a more or less rapid disorganization of the kidney. He has performed various experiments on animals, in which, by obstruction or compression of these vessels, he has been able to render urine speedily albuminous; and he refers to similar results obtained by Robinson and H. Meyer. To draw safe conclusions from these, however, the kidneys should be examined at a remote epoch; for, although ligature of the renal veins may readily give rise to albuminous urine, it is quite another question whether it will give rise to the disorganization of the kidney termed Bright's disease.—*Gazette Medicale*, 1852, No. 44, pp. 681.

SURGERY.

Statistical Account of the Ligature of the Principal Arteries. By M. Roux.

At a recent meeting of the Surgical Society, M. Roux detailed the results which have attended his numerous applications of the ligature to the large arteries. From 1808 to the present time, he has ligatured 82; of these, 33 were for true or spontaneous aneurism. These are distributed as follows: 1 of the popliteal artery for aneurism by the old mode;—46 of the femoral artery—of these, 27 have been for popliteal aneurism by the Hunterian operation, 3 for femoral aneurism, 7 for wounds and primary hæmorrhage, 7 for secondary hæmorrhage, and 2 for fungous tumours of the tibia;—20 of the brachial artery—of these, 10 were for false consecutive aneurism at the bend of the arm, 6 for arterio-venous aneurism, 1 for spontaneous ulnar aneurism, 2 for secondary hæmorrhage, and 1 for fungous tumour of the radius;—6 of the common carotid artery—1 of these was for fungous tumour of the orbit, 2 were for wounds of the face or neck, and 3 preventive ligatures in operations;—4 of the axillary artery immediately below the clavicle—1 was for true, 1 for false aneurism, and 2 for hæmorrhage after amputation at the shoulder-joint;—3 of the subclavian artery for secondary hæmorrhage;—2 of the external iliac for hæmorrhage consecutive to ligature of the femoral. Of these ligatures, 16 were applied by the old method, and 66 by the Hunterian.

The entire number of *aneurisms* so treated has been 49—viz., 33 true, 10 false, and 6 arterio-venous—of this number, all but two (which were successful) were treated by Hunter's operation. Brasdor's has never been performed by M. Roux. Of the 33 true aneurisms, 31 occurred in men and 2 in women. In 28 of the cases, the age varied from 27 to 40, and the oldest patient was aged 59.

Of the 33 *true* aneurisms, 23 were cured, and 10 were treated without success. In 2 cases superficial, and in 2 complete gangrene occurred. In 4, secondary hæmorrhage took place—viz., on the 4th, 22nd, 34th, and 50th days. The 10 cases of *false* aneurism, all arising from venesection, were all cured. Venesection also gave rise to the 6 cases of *arterio-venous* aneurism for which the brachial artery was tied, in 4 with success, while in 2 hæmorrhage and gangrene necessitated amputation.—*L'Union Médicale*, 1852, No. 124.

On Medullary Sarcoma, principally that of the Membrum Virile and its neighbourhood. By Professor WUTZER.

As the principal seat of medullary sarcoma, Wutzer considers the interstitial cellular tissue between the muscles; rarely he found it originating from the nerves. Amongst the parenchymatous intestines, it was most frequently in the testicles, where he observed the occurrence of *primary* medullary sarcoma; from these he noticed in one case its spreading to the *membrum virile* ('Deutsche Klinik,' 1841, p. 160); but he doubts, whether it ever *primarily* occurs in the latter organ, *epithelial cancer* being, according to his observation, the principal malignant affection by which it is attacked. Concerning the question of the removal of the affected organ by surgical operations, Wutzer considers the latter as prolonging life, if instituted at an early stage; as deleterious, if at a late stage, when secondary affections are present.—*Illustrirte Medic. Zeitung, Dr. G. Rübner, Muenchen*, 1852, vol. i.

The various forms of Inflammation of the Joints. By Dr. FÜHRER.

AFTER some introductory remarks, the author states that the anatomical division of the joint affections is into those which attack the synovial capsule, the other coverings of the bone, and the bone itself.

1. *Rheumatic joint inflammation.*—[By this term the author does not imply the simple joint-rheumatism, which is attended only with slight injection of the synovial membrane, increased exudation of serum, and a little velvety appearance of the cartilages.]

(*a*) Acute form.—Rapid pus-formation; plastic exudation on an intensely-injected synovial membrane; softening of the capsule, reaching even to rapid perforation, and passage of pus between the muscles and fascia; sometimes periostitis.

(*b*) Chronic form—Leads essentially to necrosis of the joint, and fistulous communications; the joint contains grayish-black, bloody pus, and the capsule is covered with exudation; the internal ligaments are destroyed; the bones, as far as they reach into the joints, are discoloured, necrosed from without, often fractured. Externally is a callous thickening of the outer capsule, and infiltration of the neighbouring parts with exudation passing into fibroid tissue.

2. *Arthritic joint-inflammation.*—It is characterized by exudation in the spongy bone substance, by rarefaction, and mollities; lastly, in the third period, by shrinking, atrophy, and disfigurement of the ends of the bones.

The chronic form is much more frequent than the acute; the spongy substance becomes infiltrated with oily marrow, and later with yellow, hardened fat. There is no effusion in the joint, and no external swelling. As the bony substance becomes absorbed, fresh layers of bone are formed around; some of the ligaments are ossified; lamellæ of bone form under the synovial membrane, and osteophytes under the periosteum. A peculiar appearance are the "corpuscula mobilia," which arise, through fibroid thickening of the synovial villi at their free ends and detachment of the same; through ossification of the same and detachment; through division of pieces of cartilage following fissures. These last are often perfectly free, or connected only by a fibrous connexion. The *malum coxæ senile* is the commencement of the process occurring in the hip. In this disease the cavities of the joint are deepened and widened by the previous mollities; the surface of the joint denuded of cartilage; the ends of the bones covered with an irregular ivory-like cortex; they are also flattened, surrounded with stalactites; the neck of the thigh bone is very much shortened and pressed in, so that the head of the femur is placed on the trochanter. The synovial membrane is dry, and causes often creaking on movement; the eburnation may occur in the depths of the bone-ends, and the abnormal ossification implicates sometimes the surrounding soft parts, and even the muscles.

3. *Podagraic Joint-inflammation.*—Both acute and chronic forms are little known. The former occurs exclusively in the small bones of the hands and feet: the ligaments and cartilages are unaltered; the faces of the joints are beset with a chalky coating; little chalk-formations are under the periosteum; and in the joint is a thick grumous fluid, which contains uric acid and urates. The neighbouring bursæ are also sometimes filled with a like fluid. The chronic form appears in anomalous gout, and, occurring in the hip-joint, is one of the forms of the *malum coxæ senile;* there is not, however, previous swelling and softening of the spongy substance of the bone, but pure "incalcination" and thickening with atrophy.

4. *Fungous Joint-inflammation*—(one form of tumor albus)—appears preferably in the knee-joint, and in the small joints of the hands and feet; is painless, insidious, and very chronic. The joint swells; the synovial membrane and the surfaces of the cartilages are thickly covered with grey-red, soft granulations. Similar fungosities grow through the chronically inflamed periosteum; the surrounding parts, fascia, muscles, &c., are partly infiltrated with exudation, and partly changed into a callous tissue, with much fat.

5. *The Tuberculous Joint-inflammation*—(one form of tumor albus).—The cartilages are penetrated with fistulæ, which penetrate into the bone substance, and communicate there with abscesses. When pus has entered the joint, the surface of the cartilages becomes softened, as if macerated; the diseased epi- and apophyses are strewed with little masses of shining exudation, or with yellow cheesy masses.

6. *The acute Empyema of Joints.*—Is an affection of the synovial membrane, passing rapidly into superficial caries. It occurs in puerperal diseases, but also often during pregnancy, after phlebitis and pyæmia, in the desquamative period of scarlet fever, in typhus, &c. The varieties are illustrated by cases.—*Virchow's Archiv.*, Band v. Heft 1.

Hernia Foraminis Ovalis. By Dr. R. Fischer.

Of an interesting paper on this subject, we can give only the following results. After describing the anatomy of the canalis obturatorius, Dr. Fischer mentions four passages, through which the hernia might occur: 1. Between the ramus horizontalis ossis pubis and the superior margin of the anterior portion of the muscul. obtur. extern., which passage is marked by the ramus abduc. posterior of the nervus obturator. 2. Between the anterior and the middle portion of the musc. obtur. extern., through the same opening with the ramus adduc. poster. of the nerv. obtur. 3. Between the membrana obtur. extern. and intern. following the decourse of the third branch of the nervus obturator. 4. Between the membran. obtur. extern. and the incisura acetabuli. Dr. Fischer himself, however, considers the occurrence of a hernia through the two last passages as scarcely possible. The *diagnosis* has been to him as difficult as to others (on account of the deep situation under a large layer of fat and the muscul. pectinæus), the principal symptoms being furnished by the percussion of that region, by the disturbance in the function of the nerv. obturat. and in the action of the bowels. Concerning the *statistics*, Dr. Fischer observes, that it is met with very rarely in children, and more frequently in women than in men—on account of the anatomical construction of the parts.—*Henle & Pfeufer's Zeitschrift für Ration. Medicin.*, 1852, Band x. p. 246.

Abscess, &c. in Bone. By HENRY LEE, Esq.

THE author details a case of abscess in the head of the tibia, cured by trephining, and one of necrosis of the femur treated in the same way. In reference to these cases and others previously detailed, it is stated that long-continued pain in bone may arise from a variety of conditions, and that the chronic irritation which precedes the deposit of new lime may depend, among other causes: 1. Upon the formation of pus within bone; 2. Upon solid deposit from mercurial or syphilitic poisoning; 3. Upon tubercular deposit; 4. Upon necrosis of the cancellated structure. In all these cases the remedy is trephining.—*London Journal of Medicine*, Oct. 1852, pp. 884.

MIDWIFERY, &c.

Extirpation of a Mesenteric Tumour, simulating Ovarian Disease. By Dr. BUCKNER.

THE case having been diagnosed as ovarian, and operation decided on, an incision nine inches long was carried from umbilicus to pubes; the tumour was then found to be not ovarian, but situated "*in the mesentery*, between the laminæ of the peritoneum, and surrounded by the small intestine." The operation was proceeded with, the tumour was dissected out, and the superior mesenteric artery, and other smaller arteries, tied. The patient recovered, and in spite of the great separation of the mesentery from the intestine, no apparent bad consequences of any kind ensued.—*American Journal of Medical Science*, Oct. 1852.

On the Effect of Prolonged Horizontal Posture in the production of the great mortality in Foundling Hospitals. By M. HERVIEUX.

M. HERVIEUX observes, that persons visiting the *crèche* of the Paris Foundling Hospital, admire the exquisite cleanliness, free ventilation, and mild temperature of that vast apartment. Still, of about 4000 infants annually admitted, about 3000, i. e. 75 per cent., die; and to explain this fearful mortality, the impoverishment of the blood of these victims of debauchery and poverty, their over-crowding, and the insufficiency of their nursing, have been referred to. All these have something to do with the result; but a chief cause of its production, hitherto overlooked, is the too prolonged maintenance of the horizontal posture. Each child is taken up, fed, and changed four times daily, and again at night, when it cries. Suppose this operation is performed six times on an average, as it only occupies about twenty minutes, the infant is lying on its back for twenty-two out of the twenty-four hours, quite unable at this age to change its position. Motion and exercise are essential to the well-being of the infant, and its proper place is its nurse's bosom, the warmth of which is imparted to it.

The children of the *crèche* die, in fact, of cold and hunger. Owing to the continuance of the horizontal posture, the temperature of the body becomes lowered, the limbs chilled, the circulation languid, and the respiration embarrassed. All the principal functions languish, the skin becomes indurated, and visceral congestions take place. Some of the children perish from sclerema, some from the so-called pnuemonias, which are only sanguineous stases, and others from various serous effusions or hæmorrhages. The definitive cause of all these disordered conditions is *cold*, not cold engendered by the diminished temperature of the surrounding medium, but cold resulting from their prolonged immovability.

We have also to inquire whether feeding infants four, six, or even eight times a day is sufficient. Books tell us that they should only be suckled at regular intervals, every three or four, or sometimes two hours; but any one practically acquainted with the rearing of young infants, must see the fallacy of this. In fact, they suck some thirty or forty times a day, absorbing, according to the calculations of Guillot and Lamperière, from three to four pints of milk. This suits them admirably, for in the first two or three years they have to acquire one-half the height and weight they will gain during the rest of their lives; and the limiting them to the periods and quantities suitable for older subjects is unphysiological and mischievous. It has been said that this so-called excess of food gives rise to the gastro-enteric affections, so frequently met with at this period of life; but, in fact, such diseases are not met with in private practice, either in the infants of the rich or of the poor, who are often so inordinately suckled, while the body of every child brought from the hospital exhibits more or less intense signs of acute or chronic gastro-enteritis. The practice of bringing up the children by hand has been assigned as a cause of the great mortality: but nothing similar to it is found among the children so brought up in the worst parts of Paris, where they, however, get abundance of milk and good nursing. At present the eighty-four infants at the *crèche* have only nine nurses and two night nurses to attend to them; while M. Hervieux considers that one woman cannot pay suitable attention to more than two infants.—*L'Union Médicale*, 1852, Nos. 139, 140.

Double Uterus. By Dr. Kelly.

An interesting and complete case of this kind is recorded by Dr. Kelly; there were two vaginæ (each had had its hymen), two uteri, Fallopian tubes, ovaries, &c. Reference is given to other cases.—*American Journal*, Oct. 1852.

Conversion of Arm-Presentation into Natural Labour. By Mr. Mayne.

In a case of arm-presentation, in which turning was impossible, Mr. Mayne pushed the hand and arm up above the pubic portion of the pelvis, and held them there for an hour and a quarter. A few minutes after withdrawal of the hand, the head was found to be descending naturally, and delivery occurred without further accident. Mr. Mayne is sure that this was a true case of arm presentation, and not merely of the descent of the arm with the head. In this case it was the left arm; the back of the child was to the abdomen of the woman, and the head rested on her right ilium.—*London Journal of Medicine*, Oct. 1852.

Case of Hermaphrodism. By Dr. Gross.

The author being called to a child, presumed to be a girl, three years old, found a small clitoris, natural nymphæ, large labia, containing each a well-formed testis, no vagina, and no penis. As sexual congress would manifestly be always impossible, the author deemed it advisable to castrate. A question then arises as to whether the operation was justifiable—the author, of course, takes the affirmative side.—*Amer. Journ. of Med. Science*, Oct. 1851.

MATERIA MEDICA AND THERAPEUTICS.

Remarks on Hæmospasia.—By Dr. T. Vogel.

Dr. Vogel strongly recommends as an important remedy the *hæmospastic apparatus* (ventouse monstre—Schroepfstiefel) invented by Dr. Junod, and improved by Dr. Ficinus, of Dresden. The intention is to expose the surface of the body, or a part of it, commonly the leg, to condensed or rarefied air. The apparatus for the lower extremity simply consists in a box of latten-plate, open on the top for the introduction of the limb; around this opening a conical piece of vulcanized india-rubber is fastened, which is on its upper end slightly narrower than the thinnest part of the thigh it is to surround. Through a small opening in the box, by means of a simple air-pump, the air is rarefied to about $\frac{1}{12}$ or $\frac{1}{8}$, or even $\frac{1}{3}$ of its original normal density, the degree of rarefaction being easily ascertained by a *manometer* screwed into another little hole in the instrument. After the limb has remained for about half an hour under the influence of the rarefied air, it appears more or less swollen and hard, generally without much pain, sometimes showing slight extravasation of blood, if the rarefaction had been produced suddenly, or carried to a considerable degree. This tumefaction disappears gradually (within 8 to 24 hours). By measuring the limb before and after the application of the apparatus, we can estimate the increase of the quantity of blood which is accumulated in it, in consequence of the exposure to the rarefied air. An increase of the volume amounting, for instance, to forty-seven cubic inches, would indicate an increase of thirty ounces of blood in the swollen leg, and a diminution of the same weight in the quantity of blood circulating in the other part of the body. The effect of *hæmospasia* must be therefore the same as that of *venesection*, if only a temporary collapsus is requested; bleeding must be preferable where an actual loss of blood appears desirable, but in that great number of cases, which show the phenomena of local congestion or inflammation, in non-plethoric, or even oligæmic individuals, the actual loss of blood must do much harm, although a temporary detraction of it to a distant organ may be beneficial; and for such cases the use of rarefied air, by a convenient apparatus, like that of Junod, can be highly recommended. The disappearance of swelling and redness in hyperhæmical and inflammatory affections of external organs, for instance of the conjunctiva palpebrarum et bulbi, or in erysipelas faciei, is often striking. The operation in pneumonia and pleuro-pneumonia appeared very valuable to Dr. Vogel; the relief of the dyspnœa, the restlessness, and pain, by every application of the apparatus, was as great as that observed after venesection; it is true that the distressing symptoms generally make again their appearance after some time, but this is not less the case after venesection, although perhaps not quite so quickly; by repeated application, however, the same beneficial effect may be obtained without the disadvantages of the real loss of blood. It will be easy after this, for every one to find the cases in which the one remedy is more suitable than the other; for internal hæmorrhage, however, and for organic diseases of the heart, it is particularly recommended by Dr. Vogel. A well-made apparatus may be had as well at Dresden as at Giessen, for 2*l*.—*Illustrirte Medic. Zeitung, Dr. Rübner, Muenchen*, 1852, No. 1.

Remarks on Paracentesis Thoracis, and Suggestion of a new Instrument for this operation. By Dr. Wintrich.

From the experience gained as well in his own practice as in that of others, Dr. Wintrich speaks highly in favour of the paracentesis thoracis, and ascribes the want of confidence placed in it by so many medical men to the imperfection of the instruments used, and to the insufficient manner in which important points connected with every case are generally taken into consideration. As indicating the operation, Dr. Wintrich considers: 1. A high degree of dyspnœa produced by sudden exudation of liquid matter. 2. Gradual accumulation of the exudation to such a degree, that suffocation is threatening, or death from pressure on vital organs. 3. Insufficiency of the other remedies to effect resorption.

Dr. Wintrich cautions against mistaking the high degree of dyspnœa arising from accessory pleuritis, flatulence, &c. (in cases of slight exudation), as caused by a considerable increase of exudation, and as indicating paracentesis. He further remarks, that the operation should never be performed after the diseased side has become perfectly immovable (by paralysis of the muscles or by rigidity from old age), as the fluid under such circumstances cannot escape without being replaced by air entering from without, the effects of which is not to be computed beforehand. The more moveable and elastic on the other side the wall of the thorax (short duration of the disease, young age), the greater is, cæteris paribus, the indication for the operation, provided we have proved (by the exploring needle) the exudation to be of a liquid nature. From the elasticity of the wall must also depend the quantity of fluid we may evacuate at once, and the space of time within which this may be done. The attributes which Dr. Wintrich considers necessary in an instrument used for this purpose, and which he finds combined in the one recommended by him, are, that the entrance of air may be prevented, that the fluid can be made to flow off in a large stream or in a thin one, or even drop by drop; that in case of obstruction of the opening (by flakes, &c.), the impediment may be easily removed without the instrument being taken out.

Dr. Wintrich's instrument consists of two flat, narrow silver tubes, with sharp brims; the narrow one ends in front by a trocar-point, behind which is a large oval hole leading into the cavity of the tube. The other tube is just so much wider as to fit exactly over the former one; it has likewise, on one side, a large opening corresponding to the one in the narrower tube. By the wider tube being moved forwards or backwards, the point of the trocar may be covered or denuded, and in the same manner, the opening leading into the narrower tube is made larger or smaller, according to the desire of the operator. For the more accurate description of the instrument, we must refer the reader to the original paper.—*Illustrirte Med. Zeitung, Dr. Rubner, Muenchen,* 1852, No. 1.

Statistics of Fractures and Dislocations treated in the Pennsylvania Hospital for Ten Years.—By Dr. Norris.

Eighty-four dislocations in 10 years; viz., 52 of the shoulder, 4 of the hip, 2 of the astragalus, 9 of the elbow, 9 of the clavicle, 2 of the radius, 1 of fingers, 3 of thumb, 1 of knee (incomplete), and 1 of semilunar cartilage. 78 were cured, 5 removed, 1 died. Of the shoulder, 39 were disclocations into the axilla, and 10 were forward, under the clavicle.

In 12 years there were 27 compound fractures of the thigh, and 139 of the leg; 50 underwent amputation, of whom 20 died, 116 were not operated upon, and 51 died; 22 of the deaths occurred within 24 hours after the accidents. Of the whole number, 53 were from railway accidents.—*American Journal of Medical Science,* Oct. 1852.

On Adulteration of Sulphate of Quinine. By Dr. Moll.

The excessive price of the true cinchona, the calisaya of Bolivia, has led to the substitution of many inferior kinds, chiefly remarkable for their containing large proportions of *quinidine.* In consequence of their lower price they have obtained admission to the quinine manufactories in large quantities, and much of the sulphate now produced is depreciated by the addition of quinidine. This substance differs from the sulphate of quinine by its greater specific gravity and less flocculent crystallization, and it is much more soluble than it in water and alcohol. The addition of both *cinchonine* and *quinidine* may be detected by means of ether; for while cinchonine is almost insoluble in this substance, quinidine is so in a far less degree than is quinine, inasmuch as sixty drops of ether and twenty of ammonia will dissolve ten grains of quinine and only one grain of quinidine. On the addition of these quantities of sulphuric ether and liq. ammonia, to ten grains of quinine, with ten drops of dilute sulphuric acid, and fifteen of water, all will remain dissolved, unless cinchonine, or more than 10 per cent of quinidine, be present, the mechanical impurities only appearing at the surface. If 10 per cent. of quinidine be present in the ethereal solution, it will soon crystallize on the surface of the ether. Traces of this substance can be yet more certainly dis covered if ether saturated with quinidine be employed, when all that exists in the suspected salt will remain insoluble. If the powder contain cinchonine, or more than 10 per cent. quinidine, it will remain undissolved at the line of demarcation of the two fluids. If it be quinidine, it is soluble in additional ether, which cinchonine is not.

To establish the purity of quinine, we must also assure ourselves of the absence of *inorganic* substances, by calcination in platina, or by a solution of the salt in alcohol. Sulphate and carbonate of lime, magnesia, &c., remain undissolved, while boracic acid, though soluble, betrays itself by its blue flame on conflagration. The absence of *organic* substances, as salicine, sugar, starch, stearic acid, is known by the colourless solution which takes place in concentrated sulphuric acid. The presence of *ammoniacal salts* is revealed by the odour which ensues on the addition of caustic alkali.—*Revue Medico-Chirurgicale,* xii. 238.

Experimental Inquiry concerning the question, whether the Purgative Action of the Neutral Salts is the effect of Endosmosis? By Dr. H. Aubert.

Aubert made his experiments with the purpose of examining the correctness of the view spread under *Liebig's* authority ('Untersuchung der Mineralquelle zu Soden und Bemerkungen ueber die Wirkung der Salze auf den Organismus.'— *Wiesbaden,* 1839), that the purgative action of the neutral salts is a merely physical process, being the consequence of exosmotical transudation from the walls of the intestinal tube, effected by the more concentrated solution of these salts within the cavity of the tube. The experiments were instituted in the following manner:—Solutions of different neutral salts were put into a cylindrical glass tube, the lower end of which was covered with a piece of membrane, from a pig's bladder; then the tube was immersed into serum of blood, and the changes going on in the solution within, and the serum without, were examined at different intervals. Another series of experiments was made by taking internally solutions of these salts. As the result of both series, Aubert draws the following inferences:

1. The purgative effect is not influenced by the degree of concentration of the solution; the number of stools produced by a certain quantity of salt will be the same, whether the salt is dissolved in six or seventy-two ounces; the water of the solution is excreted by the kidneys, the salt exercises its influence on the bowels. 2. No albumen is found in the alvine excretions, as ought to be the case, if the action of the salts was an endosmotical one. 3. The quantity of salt excreted through the urine, compared with the quantity of water contained in the discharge from the bowels, is not that which it ought to be, according to the laws of endosmosis and exosmosis (as much, at least, as they are known at present). 4. Peristaltic motion of the bowels is constantly excited by the neutral salts; the rolling and rumbling, which Aubert always observed soon after taking the salts, as well in a concentrated as when in a diluted solution, is attributed by him to the action of the salts on the nerves of the intestines, and to the reflex motion excited in consequence of this. 5. A part of the *sulphate of magnesia* appears to be decomposed within the organism, as the magnesia is excreted in a larger proportion than the sulphuric acid with the fæces; the sulphuric acid in a larger one than the magnesia with the urine. 6. The remedy produces the characteristic effect on the bowels, when a solution of it is merely infused into the veins of an animal.

It must be, however, remarked here, that *Liebig* himself, in a work of a later date ('Untersuchungen ueber einige Ursachen der Saeftebewegung im thierischen Organismus,' 1848), states, that he does not intend to explain the whole action of the neutral salts by endosmosis, but that he considers this to be one of their influencing qualities.—*Henle und Pfeufer's Zeitschr. für Ration. Medic.,* 1852, Bd. ii. p. 225.

NOTE.

Our correspondent who inquires respecting the composition of the 'Eau Pagliari,' is informed that it is the compound tincture of benzoin, which is used in its composition.

BOOKS RECEIVED FOR REVIEW.

Handbuch der Geweblebre des Menschen. Von A. Kölliker. Leipzig, 1852. 8vo, pp. 637.

A Practical Sketch of Low Inflammations. By F. S. Statham. London, 1852. 8vo, pp. 31.

A Practical Sketch of the Asiatic Cholera of 1848, in an English Village. By F. S. Statham. London, 1852. 8vo, pp. 62.

A Commentary of Medical and Moral Life; or, Mind and the Emotions considered in Relation to Health, Disease, and Religion. By William Cooke, M.D. London. 8vo, pp. 304.

A System of Practical Surgery. By William Fergusson, F.R.S. Third Edition. London. 8vo, pp. 846.

On Rheumatism, Rheumatic Gout, and Sciatica. By Henry William Fuller, M.D. Cantab. London. 8vo, pp. 403.

A Manual of Elementary Chemistry. By George Fownes, F.R.S. Fourth Edition. London. 8vo, pp. 681.

A Letter to Dr. Lyon Playfair; being a Medical Commentary on the Result of the Recent Analysis of the Buxton Tepid Water. By W. H. Robertson, M.D. London. 8vo, pp. 19.

The Nature, Symptoms, and Treatment of Consumption. By Richard Payne Cotton, M.D. London. 8vo, pp. 286.

A Treatise on Tuberculosis, the Constitutional Origin of Consumption and Scrofula. By Henry Ancell. London. 8vo, pp. 779.

A History of British Crustacea. By Thomas Bell, F.R.S. (Part 8.) Oct. 1852. 8vo.

Observations on the Treatment of Lateral Curvature of the Spine. By Edward F. Lonsdale, F.R.C.S. Second Edition. London, 1852. 8vo, pp. 126.

Demonstrations of Anatomy. By G. Viner Ellis. Third Edition. London, 1852. 8vo, pp. 822.

Report of the Proceedings of the Pathological Society of London. Sixth Session. London. 8vo, pp. 490.

A Practical Handbook of Medical Chemistry. By John E. Bowman, F.C.S. Second Edition. London. 8vo, pp. 261.

Traité de Chimie Anatomique et Physiologique Normale et Pathologique. Par Charles Robin et F. Verdeil. Tome III., with an Atlas. Paris, 1852. 8vo.

A Treatise on Corns and Bunions, and Ingrowing of the Toe-Nail, &c. By T. J. Ashton, M.R.C.S.E. London. 8vo, pp. 82.

Traité Pratique des Maladies Veneriennes. Par J. G. Maisonneuve et H. Montainer. Paris, 1853. 8vo, pp. 580.

Reports made to the Directors of the London (Watford) Spring Water Company, on the Results of Microscopical Examinations of the Organic Matter and Solid Contents of Waters supplied from the Thames and other sources. By E. Lankester, M.D., F.R.S., and Peter Redfern, M.D., F.R.C.S.L., &c. London. 8vo, pp. 52.

An Introductory Lecture, delivered on the opening of the Session of the Medical School, Charing-cross Hospital. By Edward Smith, M.D., LL.B., B.A. Lond. London. 8vo, pp. 23.

On the Action of Medicines in the System, &c., (being the Fothergillian Prize Essay for 1852). By Frederick William Headland, B.A., M.R.C.S. London. 8vo, pp. 346.

On the Contamination of Water with the Poison of Lead. By J. B. Harrison, M.R.C.S.L. London, 1852. Fcap. 8vo, pp. 196.

Pulmonary Consumption and its Treatment. By W. M. Burslem, M.D. London, 1852. 8vo, pp. 160.

Stricture of the Urethra, its Complications and Effects; with Practical Observations on its Causes, Symptoms, and Treatment. By Robert Wade, F.R.C.S. London, 1853. 8vo, pp. 360.

The Medico-Chirurgical Transactions. Volume the Thirty-fifth. London, 1852. 8vo, pp. 492.

The Fever at Boa Vista in 1845-6. By Gilbert King, M.D., R.N. London, 1852. 8vo, pp. 110.

Clinical Reports of Ovarian and Uterine Diseases. With Commentaries by Robert Lee, M.D., F.R.S. London, 1852. Fcap. 8vo, pp. 340.

A Complete Treatise on Midwifery. By Alf. A. L. M. Velpeau, M.D. Translated from the French, by C. D. Meigs, M.D. Fourth American Edition. Edited by Wm. Byrd Page, M.D. Philadelphia, 1852. 8vo, pp. 652.

A Practical Treatise on Dental Surgery. By Thos. E. Bond, A.M., M.D. Second Edition. Philadelphia, 1852. 8vo, pp. 366.

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. Fcap. 8vo.

On the Nature and Treatment of some Painful Affections of Bone. By Langston Parker. London, 1852. pp. 16.

Das Auge, Beiträge zur Anatomie, Physiologie und Pathologie dièses Organs. Von Adolph Hannover. Leipzig, 1852. 8vo, pp. 158.

The Dictionary of Domestic Medicine and Household Surgery. By Spencer Thomson, M.D. Part xii. Completion.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1853.

PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

1. *Clinical Reports of Ovarian and Uterine Diseases, with Commentaries.* By ROBERT LEE, M.D., F.R.S., Fellow of the College of Physicians, &c. &c. 1852. pp. 337.
2. *A Memoir on the Pathology and Treatment of Leucorrhœa, based upon the Microscopical Anatomy of the Os and Cervix Uteri.* By W. TYLER SMITH, M.D., Physician-Accoucheur to St. Mary's Hospital. ('Trans. of the Med. and Chirurg. Society,' vol. xxxv. p. 377.)

THE worst effect of the enthusiastic pursuit of any branch of medical or surgical science, is the tendency it has to divide the investigators into parties, by which personal interests become mixed up with the pursuit of truth. Instead of the calm weighing of evidence, the scrupulous observation of phenomena, and the deliberate judgment, we find too much stress laid upon individual interests and individual opinion, to the great detriment of science and the destruction of the tone and temper of mind befitting philosophic inquirers.

We have witnessed an unpleasant illustration of this proposition of late, since uterine diseases have occupied so prominent a position before the profession. We find one party adopting eagerly every suggestion, without much discrimination or careful estimate of its value, and carrying out their views with more zeal than discretion. Another party rejects every proposal—not so much, we fear, from any due appreciation of its demerits, as from dislike of the persons from whom it emanates. The journals of last winter bore melancholy testimony to the truth of this picture; there we read reports of the meetings of different societies, in which zeal for professional improvement was disfigured by something approaching to personal animosity, and where victory seemed to be more valued than truth. Now this is what we should earnestly wish to avoid in entering upon the consideration of uterine diseases. Discarding all personal considerations, we will accept truth from any quarter: holding this object steadily before us, we shall reject nothing that we believe to be true, merely because it is new, nor adopt any novelty, because it is new, without proof of its value. When we differ, we shall endeavour to express our opinion without asperity, and with an earnest desire to avoid giving pain to any one; when we agree, we shall as frankly express the pleasure this gives us. Above all, we shall feel it our duty to lay before the profession our reasons for approval or disapproval, with the facts upon which they are grounded, thus not only endeavouring to communicate information, but, in fact, appealing to the only tribunal capable of finally deciding upon the merits of the case.

With these views and feelings we take up Dr. Lee's book, which may be regarded, in some sort, as the manifesto of a party which has repudiated certain modern methods of more definite investigation, and the employment of certain mechanical modes of treatment.

The volume consists of five reports, with an appendix of cases to each. The *first* report contains observations on the structure, functions, and diseases of the ovaria, and the histories of 170 cases; with an analysis of 162 cases of ovariotomy, which have occurred in Great Britain. The *second* is on malformations of the uterus, diagnosis of uterine diseases, and of diseases of the Fallopian tubes; inflammation of the unimpregnated uterus, and of the follicles of the os uteri; on the use of the speculum; on the nervous structures and diseases of the uterus, and disorders of the uterine functions. The *third* is on fibrous tumours and polypi of the uterus, with the histories of fifty cases. The *fourth* is on the symptoms, morbid alterations of structure, and treatment of the cancerous diseases of the uterus, with clinical reports of 100 cases. And the *fifth* is on the pathology of the vagina, urethra, &c., with the histories of cases.

The larger portion of the observations is reprinted from Dr. Lee's articles on the subject in the 'Cyclopædia of Practical Medicine;' the cases are new and valuable: the sections on ovariotomy and on the speculum have been published already in the 'Medico-Chirurgical Transactions.'

Considering the high position and extensive practice of Dr. Robert Lee, his long experience, his careful observation, and his great intelligence, this work has upon the whole disappointed us; not merely in those points on which we are constrained to differ from the author, but on account of the omission of much information which has been of late years added to our knowledge of these diseases, and from the very small additions made by himself. We scarcely expected that descriptions written nearly twenty years ago would have been considered an adequate exposition of the present state of the science. Moreover, there is a degree of one-sidedness, a disposition to settle questions by his own sweeping condemnation, conclusions against certain instruments and operations based upon their abuse, and the entire omission of others, which we did not expect from a physician of Dr. Lee's acuteness and mental cultivation.

On the other hand, we gladly admit the excellent points of this work; although the descriptions are much too brief, they are clear, precise, and intelligible; there is neither heaviness of style nor obscurity of thought; there is even a degree of piquant humour in describing the mischievous mistakes or malpractice of others; and the cases, as we have already said, are of great value, and exhibit much care and industry.

We shall now venture upon a short notice of some portions of the reports, and some commentaries thereupon.

After a sketch of the anatomical structure of the ovaria, the author relates the cases by Mr. Pearse, Mr. Pott, Mr. Yarrell, and himself, which were the means, in later times, of calling attention to and proving the influence which the ovaries exert upon menstruation, contrary to the opinion which limited that function to the uterus; and he concludes that these facts

—"render it extremely probable that all the phenomena of menstruation depend upon or are connected with some changes in the Graafian vesicles, in consequence of which an opening is formed in their peritoneal and proper coats. Whether an entire vesicle, or only the fluid it contains, escapes through this opening at the period of menstruation, further observations may hereafter determine. There is no proof whatever that an ovum passes along the Fallopian tube into the uterus during menstruation, and it is not clearly established that this takes place even subsequent to conception." (p. 7.)

Now, if the object be merely to show the meritorious share Dr. Lee has had in calling attention to this subject twenty years ago, his omission of all notice of more recent researches is intelligible; but if his intention were to lay the whole matter before the profession for its information, why omit all account of the interesting and important observations of Knox, Girdwood, Ritchie, Renaud, Carpenter, Negrier, Pouchet, Chereau, Raciborski, Bischoff, Müller, and others, which have thrown considerable light upon the subject, and all of whom agree with the first part of Dr. Lee's opinion. They differ from the latter part, however; for nearly all believe, that on the rupture of the coats of the Graafian vesicle, an ovum does escape into the Fallopian tube, and, if not fecundated, perishes there, or in the uterus. At what exact period it escapes, whether just before, during, or immediately after menstruation, is uncertain, as well as the time and place of impregnation.

We regret much that, with his extensive opportunities, Dr. Lee has not given us some careful observations upon the condition of the Graafian vesicle after its rupture at this period, so as to throw some light upon the difficult subject of false corpora lutea, or, as they have been called, the corpora lutea of menstruation. It is quite essential, in order fully to appreciate the distinctive characters and value of the true corpora lutea. With regard to the latter, the information given to us in this volume does not extend later than Dr. Lee's own researches. After stating the opinions of De Graaf, Haller, Baer, and Montgomery, he remarks, that from his own observations we may conclude that the corpus luteum is

—"neither produced by a thickening of the inner layer of the Graafian vesicle, nor by a deposit of a new substance between its two coats; but that it is formed around the outer surface of both these coats of the Graafian vesicle, and that the stroma of the ovarium is in immediate contact with the external surface of the yellow matter." (p. 18.)

This view, however, differs from that of Montgomery, Patterson, Renaud, and others, who regard the adventitious matter as deposited between the two membranes of the Graafian vesicle; and their opinion has received strong confirmation from the recent researches of Dr. Dalton.

We quite agree in Dr. Lee's conclusion as to the real significance of the true corpus luteum, although we venture to differ from the latter part of the following passage:

"From all the observations hitherto made upon the true corpus luteum, we may conclude that it is never formed but as a consequence of impregnation. The yellow oval-shaped substances found in the ovaria of women who have never been pregnant, are produced by morbid states of the Graafian vesicles, and are essentially different in structure." (p. 20.)

That some of the yellow spots may be the result of disease is quite probable, but it is of great importance to remember that the researches of Negrier, Raciborski, Pouchet, and Dalton, have proved that many of them are the results of changes in the shell of the Graafian vesicle, after the escape of its contents at the menstrual period—menstrual corpora lutea, in fact; and that at a certain period they bear a very strong resemblance to the corpora lutea of pregnancy there can be no doubt; although we cannot agree with M. Pouchet that they are identical. The characteristic differences have been carefully investigated by Dr. Dalton, who thus enumerates the particular points in which the corpus luteum of pregnancy differs from that of menstruation: "1. It arrives more slowly at its development, and afterwards remains for a long time as a very noticeable tumour, instead of undergoing a process of rapid atrophy. 2. It retains a globular or only slightly flattened form, and gives to the touch a sense of considerable resistance and solidity. 3. Internally, it has an appearance of advanced organization, which is wanting in the corpus luteum of menstruation. 4. Its convoluted wall, particularly, attains a greater development, this portion measuring sometimes so much as three-sixteenths to one-fourth of an inch in thickness; while in the corpus luteum of menstruation it never exceeds one-eighth, and is almost always less than that. This difference in the thickness of the convoluted wall is one of the most important points of distinction. It will be much more striking when viewed relatively to the size of the central coagulum. 5. The colour is not by any means so decided a yellow, but a more dusky and indefinite hue. 6. If the period of pregnancy be at all advanced, it is not found, like the corpus luteum of menstruation, in company with unruptured vesicles in active process of development."*

We have dwelt a little upon this matter, because it involves practical points of great interest in legal medicine, and it is desirable that further investigations should be undertaken to settle the various questions connected with it.

But we must retrace our steps. After the notice of the dependence of menstruation upon certain changes in the Graafian vesicles, Dr. Lee makes a few remarks upon the rarity of inflammation of the ovaries, unless connected with parturition; and he details a case of abscess of the ovarium. He then describes, at greater length, encysted dropsy of the ovarium, previously quoting the opinions of Drs. Hodgkin and Seymour as to the nature of these formations, but differing from them as to the disease not being of a cancerous nature, nor ever degenerating into that disease.

"The injurious effects upon the system which they produce," he observes, "result entirely from the pressure and irritation which they excite in the abdominal

* On the Corpus Luteum of Menstruation and Pregnancy, by John C. Dalton, M.D., p. 73. (From the Transactions of the American Medical Association.)

and pelvic viscera, and some of the remote organs of the body. The cysts may descend between the bladder and the rectum, and becoming firmly fixed by adhesions in this situation, interrupt the evacuation of the urine and fæces. In a case which lately came under my observation in the Marylebone Infirmary, an ovarian cyst having become firmly impacted between the bladder and rectum, produced all the symptoms of stricture of the rectum. In a lady now under my care, the presence of an ovarian or uterine tumour in the pelvis, which presses upon the neck of the bladder, renders it impossible for the bladder to be emptied without the introduction of the catheter.

"When the ovarian cysts remain at the brim of the pelvis, in the progress of their enlargement they gradually produce all the usual consequences of interrupted circulation in the pelvic viscera and lower extremities. Attacks of inflammation occasionally take place in these capsules, by which they contract adhesions with the surrounding organs, and pus is poured out into their cavities. After a time, effusions of dropsical fluid take place into the peritoneal sac, and sooner or later the patient dies, exhausted from the long continued pressure and irritation of the abdominal and other viscera." (p. 11.)

As we shall see presently, the diagnosis of ovarian dropsy is not only very difficult, but of extreme importance; yet the only assistance given by Dr. Lee is the following observation:

"Encysted dropsy of the ovarium can generally be distinguished from ascites by the following symptoms:—The tumour commences on one side of the abdomen; its surface is unequal, and its fluctuation, if felt at all, is very obscure. The health at first is but little impaired, and the thirst, scanty urine, and other symptoms which characterize general dropsy, are wanting. The catamenia are usually extremely irregular, or altogether wanting. When both ovaria are diseased, Dr. Seymour states that the menses are always absent." (p. 11.)

A very good description of the contents of these cysts follows, with the *post-mortem* appearances, and a short notice of the difficulties which the tumours may occasion in parturition. The treatment, however, is rather summarily disposed of, as follows:

"Bloodletting, mercury, iodine, diuretics, emetics, long-continued friction or percussion, and a variety of other remedies, have all been employed in encysted dropsy of the ovaria, and in most cases without the slightest benefit. Though the progress of the disease cannot be arrested by these means, yet the uneasy sensations produced by it admit of considerable alleviation. Inflammation of the cyst, and irritation of the bowels from its pressure, which often arise, may both be mitigated by the occasional application of leeches to the abdomen, by fomentations, and the use of cathartics and anodynes. When the distension becomes great, recourse must be had to the trochar, and by a repetition of the operation of tapping the life of the patient may be prolonged, and considerable ease and comfort may be thus obtained under a complaint which must, sooner or later, terminate unfavourably." (p. 14.)

At first sight, such a statement would seem an ample justification for attempting a radical cure, and accordingly various plans have been adopted at different times (though Dr. Lee says nothing about them) for this purpose, by injections into the sac, removing a portion of it, making a fistulous opening, &c., but without any very encouraging success, and it remained for our own times to attempt the daring operation of removing the entire mass of disease. Nor is it in a few cases only in which this operation has been performed; 162 cases in Great Britain, a number in America, and a few on the Continent, attest, to a certain extent, the conviction of the profession, that the ordinary mode of treating these diseases is altogether inadequate. From the number of cases on record we might have hoped to arrive at some just estimate of the value of the operation; and Dr. Lee is evidently of this opinion, for he says—"On the practice of extirpating the ovarium, when diseased, it is not necessary to offer any observations, as it has been abandoned by all who have made themselves acquainted with the pathology of these organs" (p. 14); but if this be true, it was equally unnecessary for Dr. Lee to have occupied twenty pages with an abstract of cases in which it has been performed.

Now, without in the least degree coming forward as advocates of the operation, but considering that it has been, and still is, occasionally sanctioned and performed by men of ability and great knowledge, and that we read of successful as well as unsuccessful cases, we do think it would have been more to the purpose if Dr. Lee had entered fully into the question, and more effective than a sweeping expression of opinion. We will try to supply Dr. Lee's omission very briefly.

The objections to the operation adduced by Dr. Lee are—1, the great mortality, which, according to his tables, is 1 in $2\frac{18}{42}$; 2, the extreme difficulty of diagnosis, so as to be sure that the case is one which will offer no obstacles to the removal of the tumour; 3, the possibility of prolonging life considerably by other means.

To this it is answered, by the advocates of the operation:

1. Undoubtedly the mortality is very great—1 in $2\frac{18}{42}$ according to Dr. Lee, 1 in 3 according to others; but a mortality nearly, if not quite, as great, is not considered a fatal objection to other operations. If we take the major amputations of the limbs (primary and secondary) it appears that in Paris, according to Malgaigne, the mortality is upwards of 1 in 2; in Glasgow, it is 1 in $2\frac{1}{2}$; in British hospitals, it is 1 in $3\frac{1}{2}$. As to amputation of the thigh, Mr. Syme observes—"the stern evidence of hospital statistics shows, that the average frequency of death is not less than from 60 to 70 per cent." Of 987 cases collected by Mr. Phillips, 435 proved fatal, or 44 per cent. Mr. Curling states—"On referring to a table of amputations in the hospitals of London, performed from 1837 to 1843, I find 134 cases of amputation of the thigh and leg, of which 55 were fatal, giving a mortality of 41 per cent." Of 201 amputations of the thigh performed in the Parisian hospitals, and reported by Malgaigne, 126 ended fatally. In the Edinburgh hospitals, 21 died out of 43. Even if we take much larger numbers, we find the mortality very high. Mr. Inman has collected 3586 cases of "amputations generally, primary and secondary, for accident or disease, and the deaths are 1 in $3\frac{1}{10}$." In 4937, published by Mr. Fenwick, the mortality is 1 in $3\frac{1}{15}$.

The result of amputation at the hip-joint is still more unfavourable. Mr. Sands Cox has shown, that of 84 cases, 26 were successful, and 58 unsuccessful.

Again, take the operation for hernia. Sir A. Cooper records 36 deaths in 77 operations; and Dr. Inman, 260 deaths in 545 cases. Or, the ligature of large arteries, of which Mr. Phillips has collected 171 cases, of which 57 died; Dr. Inman, 199 cases, of which 66 died. Of 40 cases of ligature of the subclavian artery, 18 proved fatal. Ligature of the innominata has, we believe, been fatal in every case.

So that, taking the mortality at Dr. Lee's estimate, it is not higher than that of other operations, which are admitted to be justifiable notwithstanding.

But, although these figures show that as high a mortality occurs in other operations as in ovariotomy, we beg to remark, that the necessity for the operation is much more urgent in the former: in many cases it is the alternative of immediate death. Further, the operation of ovariotomy is of two kinds—by the long and short incision; and the advocates of the latter point to their statistics, which give a mortality of 4 in 23 cases, or nearly 1 in 6; whilst, according to Mr. Safford Lee's tables, that by the long incision is 1 in 3.

2. The errors in diagnosis have been very great, and the fair inference therefrom is, that the diagnosis is difficult and obscure. But, unless it can be proved that all improvement in this department is impossible, it is clear the argument cuts both ways. If the present deficient diagnosis entails an increased mortality, it is certain that every improvement will by so much reduce it. And we can see that it is possible that this may occur, for if all who have operated had the means of adequately ascertaining the actual presence of a tumour, of being sure that it is an ovarian, of determining the amount of adhesions, and had been sufficiently attentive to the constitution of the patient, it is clear that many of the recorded operations would never have been undertaken, and equally clear that many of the deaths would have been avoided, as a cursory glance at Dr. Lee's tables will prove. Moreover, it seems highly probable that a more accurate knowledge of the contents of these cysts may lead to important results as to the selection of the more promising cases for the operation, which may yet further diminish the mortality; and lastly, it is quite possible that some beneficial modification of the mode of operating might be adopted.

3. With regard to the prolongation of life by palliative treatment and repeated tapping, it is not easy to estimate the exact gain; it would have been a valuable argument if Dr. Lee had given us a collection of cases to show the amount of prolonged life thus obtained. If the patient be otherwise in good health, and the ovarian tumour increase very slowly, it is true that years may elapse, under careful treatment, without much distress, or any necessity for measures involving risk. In such cases, life will be best prolonged by letting the patient alone. But with those that increase rapidly, and to such an extent as to occasion inconvenience and distress, or to threaten life, something must be done to afford relief, and tapping has been the ordinary resource. We have, however, but few statistics to show the results. The only statement of the kind within our reach at this moment is one by Mr. Southam, who, in twenty cases, found that "fourteen died within nine months of the first operation, four of whom survived it only a few days. Of the remaining six, two died in eighteen months, and four lived for periods varying from four to nearly nine years. It further appears that paracentesis does not prolong life, on an average, for more than eighteen months and nineteen days, and that one in four dies from the effect of the first operation." Undoubtedly, these numbers are too small to enable us to form a correct judgment; but, so far as they go, they do not advocate very strongly the operation of tapping.

From this brief summary it appears that the admissibility of the operation will depend, not so much upon the rate of mortality hitherto, as upon future improvements in diagnosis; and when we see men of high intelligence like Drs. Simpson and Bennet, Southam, Walne, and Frederick Bird, devoting themselves to this task, we cannot doubt that a decided and practical advance will be made.

Having laid these facts before the profession, we shall leave it to decide how far Dr. Lee's condemnation is just.

We now pass on to the next report, which consists mainly of a reprint of the article, *Diseases of the Uterus*, in the 'Cyclopædia of Practical Medicine.' It possesses the merits and also the defects of the former reprint; it is a clear and condensed summary; but considering the undoubted increase of our knowledge upon this subject since it was written in 1833, the additions are far from satisfactory. Some diseases, or what have been described as such, and which we are told are very frequent, and some remedies much recommended, are altogether omitted, we presume because Dr. Lee either doubts their existence or frequency, and their utility; but still, we should have been glad to have had the expressed opinion of so able a man.

Without entering at length upon the details of diseases with which, probably, our readers may be familiar, we shall at once notice Dr. Lee's remarks upon the diagnosis of uterine disease. He remarks:

"We ascertain the presence of disease in the uterine system chiefly by the uneasy sensations of the patient, by the disordered functions of the organs, and by the changes in their situation, form, and sensibility. The mammæ, stomach, brain, and nervous system, are all sympathetically affected in many diseases of the uterus; and in most of the organic affections of the organ and its appendages there is severe burning or lancinating pain experienced in the hypogastrium, or dull, gnawing pain in the sacrum, loins, pubis, and upper part of the thighs. There are often sickness and vomiting, loaded tongue, impaired appetite, and other signs of gastric derangement. There are frequently, also, dull pain, sense of giddiness, confusion of head, and many singular nervous affections. The mammæ sometimes become enlarged and painful, as in the early months of pregnancy; the functions of the uterus, more particularly of menstruation and conception, are disturbed. There is frequently an altered secretion of the lining membrane of the uterus and vagina: and instead of the mucus which lubricates the passages, serum, pus, or blood, are poured out in greater or smaller quantities from the parts. When a female, after the middle period of life, suffers from an habitual discharge of a serous, sanguineous, or purulent nature from the vagina, with pain in the back and irritation within the pelvis, an internal examination should be made, to determine the condition of the uterus. If our object is to ascertain the condition of the lower portion of the uterus, the patient should remain in the erect position, with the back resting against the wall, while the forefinger is carried through the vagina to the os uteri. When the finger reaches the uterus, pressure should be made with the left hand over the hypogastrium, that the sensibility, form, and weight of the organ may be accurately ascertained. It is necessary for every practitioner to recollect that there is a great variety in the form of the os uteri in different women, without disease. In some individuals its length is remarkable, and in many women who have had children, in whom labour has been natural, there are irregularities, or fissures from laceration when there is no organic disease. In some women, observes M. Dugès, after repeated labours, the lips of the os uteri do not project, the orifice occupying directly the upper part of the vagina like a funnel. This I have repeatedly found to be the state of the os uteri in aged females, whether they have had children or not. Sometimes the orifice is so large as to admit the point of the finger, prolonged backwards and to the left by an oblique fissure with round edges. Not only should the smoothness, hardness, and regularity of the lips of the os uteri be examined, but the degree of dilatation of the orifice should be ascertained, if any exists, and whether it is giving passage to a tumour of any description. The size and weight of the uterus should also be determined, and whether the fluid covering the finger be mucous, serous, sanguineous, or purulent. The condition of the vagina should likewise be accurately explored, for there are few diseases of the uterus, of a cancerous or malignant nature, in which some change is not perceptible in the coats of the vagina. It is by an examination per vaginam that we become acquainted, not only with the alterations of structure in the os and cervix uteri, but with the numerous displacements to which the organ is liable." (p. 111.)

This is an excellent description of the results to be obtained by a digital examination; but there are other means generally admitted by the profession to have a certain value, and to add much to our information when judiciously employed, but which are ignored by our author. Nothing is said of examination "per rectum," which is of great importance in many cases; nor anything of the microscope, though no one can doubt its value after reading M. Lebert's admirable volume, and other recent communications on this subject. Of the uterine sound, perhaps, we could not expect Dr. Lee to say anything, though we have found it of decided use; but of the speculum he has reprinted for our use the judgment read to the Medico-Chirurgical Society, and to this we shall now direct the reader's attention, with a view of arriving at something like a correct conclusion as to the value of this instrument.

"The speculum uteri," says Dr. Lee, "has been much employed upon the Continent, in the exploration of diseases of the uterus; and in some cases of inflammation and superficial ulceration of its orifice, important information may be obtained from its use. In many more, however, and particularly in tumours and cancerous affections, I am fully persuaded that little information can be obtained from it; its introduction is painful, and when the vagina is diseased it has produced most injurious effects." (p. 113.)

In the first part of this sentence we have an admission of the utility of the speculum in certain cases, almost the only ones in which Dr. Lee thinks it useful, whilst in the conclusion we have an objection founded on an abuse. As this mode of argument seems a usual one with Dr. Lee, we shall make a few observations before entering upon the attempt to estimate the precise value of the speculum.

1. It appears, then, from cases related by Dr. Lee, that by some members of the profession the speculum is used needlessly and wantonly, and with undue frequency, to the distress and injury, physical and moral, of the patients: that diseases of the uterus have been pretended to be discovered by the use of the instrument which did not exist: that means of cure, some valuable, some severe, some objectionable, have been used for supposed diseases, which either did not exist or were not of a kind to require such remedies; and lastly, that men have traded on the speculum, by impressing the patient's mind with a belief in their peculiar knowledge of disease and possession of remedies, and have prolonged their residence in their neighbourhood for their own pecuniary profit. As regards such cases, no matter who the practitioner may be, we heartily join Dr. Lee in expressing our horror and contempt for his proceeding. Such a man can possess none of the purer, higher feelings of a gentleman and a Christian, and ought, whenever discovered, to be treated with the scorn he deserves.

2. We give up Dr. Balbirnie and all "apostles of the speculum" to Dr.

Lee's tender mercies. 3. But we must decline to argue from the abuse of an instrument against its use; we might as well object to the use of laudanum, because people have been poisoned by it; or to ergot, because it has been given to procure abortion. The speculum may have been used unnecessarily, wantonly, and mischievously, but were the examples ten times more numerous, they would really prove nothing against the proper use of the instrument. 4. We cordially agree with Dr. Lee in the following paragraph:

"An examination of the physical condition of the uterus in unmarried women, either with or without the speculum, I have always refused to make, even when requested to do so, unless pain, severe and almost constant, in the region of the uterus, existed; leucorrhœa or hæmorrhage, which did not yield to treatment, and where the symptoms did not make me strongly suspect the presence of some displacement or organic disease. In unmarried women, whatever their rank or condition in life may be, the integrity of their structure should not be destroyed with the speculum, nor their modesty wounded by an examination of any kind, without a necessity for such a proceeding being clearly shown. Even in married women who are barren, or who have had children, it is unjustifiable, on the grounds of propriety and morality, to institute an examination of any sort, unless the symptoms warrant the supposition that the uterus is displaced, or it is in a morbid condition, the nature of which cannot be determined by the symptoms alone." (p. 135.)

Or, as we have elsewhere expressed it, "to make an examination with the finger or speculum, unless it be plainly necessary, is a flagrant breach of delicacy, and in the case of young unmarried females, it is almost a crime."

Having thus cleared the ground, we may now proceed to inquire as to the true *value and use of the speculum.* We shall first give Dr. Lee's estimate, and then some comments of our own. After a short and very imperfect sketch of the employment of this instrument, with references to the works of Paulus Ægineta, Spachius, Recamier, Lisfranc, and others, Dr. Lee remarks—

"In the first great class of organic uterine diseases, which comprehends fibrous, fibro-cystic, glandular, and all other tumours which are not malignant, I have derived little or no aid from the speculum in their diagnosis or treatment. When fibrous and other tumours are found under the peritoneum, or between the muscular fibres, or under the lining membrane, and distend the cavity, their existence can only be determined by a careful examination of the hypogastrium and of the interior of the pelvis through the vagina and rectum. The uterus is usually felt large, hard, irregular, and the cervix shortened. Where these tumours have passed partially or completely through the os uteri, their size, density, the length and thickness of their roots, and the relation these bear to the os and cervix uteri, can only be determined by the touch. I have even detected a small polypus within the os uteri, or hanging through it, which I had failed to detect with the finger. In cases of this description I have, however, repeatedly employed the speculum to ascertain the colour of the polypus, and the degree of vascularity of the investing membrane, which, without ocular examination, could not have been determined. The knowledge thus acquired was of no use in the treatment." (p. 133.)

Again:

"In all the varieties of malignant disease of the uterus, scirrhus, fungoid cancer, and corroding phagedænic ulceration, the speculum has given me no assistance whatever in their diagnosis and treatment, either in the early or in the advanced stages." (p. 134.)

Again:

"In cases of obstinate leucorrhœa I have often employed the speculum in married women, after I had failed to detect the existence of disease by the ordinary mode of examination. In some of these cases there has been seen an unusual degree of redness of the os uteri, sometimes affecting the whole, and at other times limited to the inner margin, with or without swelling. The white viscid discharge has been seen issuing from the os uteri. I have never seen ulceration of the orifice of the uterus in such a case, and the condition of the interior of the cervix I have never been able to demonstrate with the bivalve or any other speculum, &c." (p. 136.)

Lastly, after a very good description of hypertrophied and granular cervix, Dr. Lee remarks, that "on its nature, diagnosis, and treatment, little or no information is derived from the use of the speculum." (p. 137.)

It appears from the foregoing statements, that the only case in which Dr. Lee has derived benefit from the employment of the speculum is in inflammation and erosion of the cervix uteri. Now, although we agree with certain portions of his statements, we cannot assent to so sweeping a condemnation; on the contrary, we have found the instrument of great practical value in certain cases, in the enumeration of which we shall endeavour rather to understate than overstate the instances.

1. With regard to fibrous tumours of the body, and upper portion of the cervix, which are beyond our reach, no one, we believe, ever supposed that the speculum is of any use; but although Dr. Lee may never have detected the small vascular polypi in the os uteri with the speculum, after having failed with the finger, others have done so, and ourselves among the number. A glance at Madame Boivin's plates, or an examination of any preparation of the kind, will convince the most sceptical that it is quite possible for one to appear within the os uteri, and yet, being above the level of the os, to be quite beyond our touch. And although it is not difficult to twist them off without the speculum, it can be done much more easily and satisfactorily with it. With polypi of a larger size, the speculum is of no use, as Dr. Lee observes, except to show the colour and the unbroken surface.

2. Of course it is easy, as Dr. Lee states, to decide by the finger alone upon ordinary cancerous disease and corroding ulcer, but we have seen a polypoid fungus growing from an apparently healthy uterus, in which the finger could decide nothing. The speculum showed the peculiar aspect of encephaloid cancer, and a microscopical examination of a small portion decided the question. When Dr. Lee quotes the "opinions of the best French writers," he must include not only MM. Tealier and Pauly, but M. Lebert and others, and then he will not find so much unanimity as to the value of the speculum in cancer of the uterus.

It is quite true that the speculum is inadmissible in ulcerated cancer at an advanced stage, because of the danger of laceration. Yet at an earlier period, after the commencement of ulceration, we have found it of great value in treatment. By local applications, through it, we have repeatedly succeeded in arresting the hæmorrhage, relieving the pain, and rendering the ulcer stationary for a considerable time; thus prolonging the patient's life, and without the least injury. It is true that this must be done before the deposition has extended beyond the uterus, and before the vagina has become contaminated; and further, that a small-sized, non-expanding speculum should be employed. Our experience on this point is confirmed by the recommendations of M. Lebert.

3. In simple leucorrhœa, uterine or vaginal, where there is no organic change, the speculum is of little or no use, and the treatment quite independent of it. But in cases of congestion, inflammation, or erosion, we not only agree with Dr. Lee, that "important information may be obtained from its use," but that a more certain and rapid cure is possible by applications made by its aid to the diseased part, than by any other means, and in this we feel sure we shall have the support of most practitioners.

4. If granular inflammation be combined with hypertrophy of one or both lips of the os uteri, of course the touch will easily recognise the latter; but if not, unless the granulations be unusually well developed, or unusually hard, we may suspect, but we cannot be sure. Some time ago, we saw a lady in the early months of pregnancy, who was suffering intensely from itching of the vulva, without any apparent cause in the part. She had been carefully examined by the finger, and no suspicion of disease of the cervix uteri was excited; but when we employed the speculum, we detected extensive granular inflammation, which was relieved in the usual way, and the itching of the vulva ceased after the first application.

And certain we are, that in this disease, as well as erosion, no treatment by injections or general remedies is equal in efficiency to the application, by means of the speculum, of nitrate of silver, nitric acid, or caustic tincture of iodine.

5. These are the principal cases in which Dr. Lee has questioned the usefulness of the instrument, and we have given the result of our own experience. Let us add a few others, in which we have found it of service. In certain cases in which it has been necessary to pass an elastic bougie through the canal of the cervix, we have found it easier to do so through the speculum. In some cases of occlusion of the os uteri and vagina, our search for the orifice at the upper end of the cul de sac has been much facilitated by the use of a small speculum. In many cases of vesico-vaginal fistula, it has enabled us to ascertain more clearly the extent of the opening, and the condition of its edges, and above all, to apply certain remedies exactly to the part without injury to the neighbouring tissues. And the same may be said of recto-vaginal fistula.

6. We have also seen, and treated by its means, some cases of simple ulceration of the cervix uteri; we do not mean erosion or abrasion, but ulceration with loss of substance, and neither scrofulous nor syphilitic. This, Dr. Lee says, he has never seen, and he has adduced a large series of post-mortem examinations in hospitals to prove its rarity or non-existence.

That the disease is not very frequent, is an assertion we are not prepared to dispute, and so far these hospital statistics may be received as a loose general expression of a fact; but to be of much weight, we should require to know the ages of the women and their habits; for if they were old, no one could expect to find this form of disease; and if they had been long in hospital, the absence of the ordinary exciting causes would prevent their acquiring the disease, and the rest and regimen of the institution would tend to its cure if present. Again: it would require very large and definite statistics of this kind to give anything like an accurate measure of frequency: for example, we find one of Dr. Lee's authorities, Mr. G. Pollock, stating, that in 600 females autopsies he found only one example of mucous polypus, and sixty of fibrous polypus. Dr. Lee will hardly contend that this is the usual proportion.

Again: such statistics can never be admitted as a counter-statement to the assertion of credible witnesses, who state that they have met with such cases. A negative argument of this kind is necessarily weak; it reminds one of the man who was accused of horsestealing, on the evidence of one who saw him take the horse, and who rebutted the charge by bringing twenty men to swear that they did not see him.

Lastly: we believe that the whole dispute has arisen from the improper application of the term ulceration by Dr. Bennet and others to the slighter forms of abrasion or erosion.

Having said thus much, we must leave it with the profession to decide whether we have not shown, that in conscientious and careful hands the speculum is an instrument of great value in certain cases, not superseding the use of digital examination, but adding to the information thence obtained; and whether Dr. Lee, in his sweeping condemnation of it, has not fallen into an error of the opposite kind from his indignation at its abuse.

The *third report,* on fibrous tumours and polypi of the uterus, is, we think, the most satisfactory chapter in the book. The following description is simple, graphic, and tolerably complete, except for the omission of the results of microscopic examination.

"The fibrous tumour, or fleshy tubercle of the uterus, as it was termed by Dr. W. Hunter, is sometimes met with not larger than a pea; in other cases it grows as large as a walnut; and occasionally is equal in size to a cricket-ball, or even the gravid uterus at the ninth month. It is generally of a globular form, or kidney-shaped; and when cut into, presents a laminated or radiated semi-cartilaginous structure, the fibres being often disposed in a concentric manner. At other times, this tumour has a granular appearance, or seems to consist of a congeries of smaller tumours, each having a thin capsule of cellular membrane. Most frequently it has a yellowish-white colour; but several specimens of the disease have been of an ash-grey colour, or approaching to a dark slate. When large, the tumour is often unequal on its surface, being lobulated, or divided by deep fissures; and arteries and veins of considerable magnitude can be traced into its substance. Cavities containing a bloody or dark-coloured gelatinous fluid are sometimes found in the central part of the tumour, probably by a process of softening which its substance undergoes. In a specimen of large fibrous tumour imbedded in the walls of the uterus, which was removed from the body of a woman who died in St. Marylebone Infirmary, there is a considerable cavity, which contains a coagulum of blood.

"In other cases, the tumour does not manifest a disposition to become softer as it enlarges; but its density gradually increases, until the whole or the greater part of the mass has become cartilaginous, or like the intervertebral substance without vessels containing red blood; or calcareous depositions are gradually formed in the substance of the tumour, until it is either partially or completely converted into a concretion, composed of carbonate and phosphate of lime." (p. 173.)

A very full account of the concretions and their analysis is given, and the author describes the variations in numbers and situation of the fibrous tumours, with the obstacles they occasionally offer to parturition.

Dr. Lee gives a very accurate account of the gradual conversion of fibrous tumours, in certain situations, into uterine polypi; and we agree with him, that

—"though the facts which have now been stated clearly demonstrate, that the greater number of uterine polypi are fibrous tumours, which have been formed under the lining membrane and stratum of muscular tissue, we are not entitled to conclude, as some have done, that these are the only tumours which make their way from the cavity of the uterus into the vagina, and which are not of a malignant nature." (p. 190.)

Dr. Lee then notes a tumour of which he has only seen two examples, and afterwards describes the cellular or vesicular polypus, and the mucous polypus.

"With respect to the treatment of the various tumours which have now been described, I have few observations to offer. Iodine, mercury, and all other remedies, have little effect either in arresting their growth or promoting their absorption. Women who have fibrous tumours formed in the walls of the uterus, should avoid mechanical pressure of the hypogastrium, violent bodily exertion, and every other cause which may excite inflammation, or a determination of blood to the organs within the pelvis. Where congestion has taken place, it should be removed by local bloodletting, mild cathartics, and anodynes. Profuse uterine hæmorrhage should be controlled by rest in the recumbent posture, cold applications to the hypogastrium, and the internal use of the acetate of lead. When any of these tumours pass through the os uteri into the vagina, they may be removed by the ligature or the knife." (p. 194.)

We should have been glad to have entered more fully into the consideration of this report, and that on Cancer, which contains many valuable observations, but our limits forbid. We have been occupied so much with the former part of the work, that we can only refer our readers to the work itself for the concluding chapters. We cannot conclude, however, without reverting to the large series of cases with which the work is illustrated. They are at once a durable monument of Dr. Lee's careful industry, and a valuable store of facts for the practitioner and professional author. A brief *résume* of each class would have made their value more apparent, and would have saved some trouble to the reader; but even without this, we feel that the profession are under great obligations to Dr. Lee for them, and we say this the more gladly, because we have been conscientiously obliged to point out deficiencies in other portions of the work, and to differ so materially from many of Dr. Lee's opinions, and much of his practice. In what we have thus stated, it has been our object to express our opinions in the least offensive manner, to avoid all harsh and unbecoming expressions, and to do justice at once to the subject and to Dr. Lee. How far we have succeeded the profession must judge; but we trust that Dr. Lee will give us credit for being actuated by a simple, earnest feeling of our duty.

The second communication at the head of this review is one of great value, not only from the original character of the information it affords, but because it is a step in the right direction, and the commencement of a series of observations which must add much to our knowledge, if pursued carefully and extensively. Microscopists are human, and make mistakes; but the microscope, being subject to physical laws, cannot err. Many more observations than have yet been made, slower and more careful induction, more cautious and modest interpretation, are necessary, before histology will deserve to rank as a science. But we may thankfully receive every contribution, and, owing to Dr. Tyler Smith's investigations, we are now in possession of the microscopical anatomy of the cervix uteri, which we have every reason to believe correct.

Our space prevents us doing more than merely exhibiting an outline of Dr. Tyler Smith's observations. He finds that the structure of the external surface of the cervix differs widely from that of the internal. The mucous surface of the former consists of epithelium, basement-membrane, fibrous tissue, vessels, and nerves, like other mucous membranes. The epithelium is tesselated, or squamous, and forms a membrane of considerable thickness. Immediately beneath, we find the basement-membrane covering numerous villi, each villus containing a looped bloodvessel, which passes to the extremity of the villus, and returns to its base, where it inosculates with the bloodvessels of the neighbouring villi. Very beautiful drawings of this peculiar disposition are given. Dr. T. Smith has not found that the external mucous surface of the cervix uteri possesses many follicles, as has been supposed.

The canal of the cervix is, of course, very different from the external surface, and Dr. Smith has given a laborious enumeration of its pillars and rugæ, the most striking peculiarity of which seems to be their almost infinite subdivision, and the enormous number of mucous follicles, amounting, Dr. Smith thinks, to somewhere about 10,000.

Villi, clothed with cylinder epithelium, are observed within the os uteri, but are less numerous than upon the outer surface. The reader will find an admirable description of the glandular structure of the upper portion of the canal of the cervix. The epithelium of this part is ciliated.

Dr. Smith agrees with Mr. Whitehead and others, that the mucus secreted by the canal of the cervix is alkaline, and that from the external surface of the cervix and vagina, acid; and upon this he has submitted some pathological considerations which are worthy of careful consideration. His researches seem to establish, upon a sound and intelligible basis, the old distinction between uterine and vaginal leucorrhœa, and we are inclined to agree with his views as to the kind of local application most suitable in both varieties. The following is Dr. Smith's description of one form of leucorrhœa:

"Leucorrhœa, in its most simple and uncomplicated form, is, then, the result of a morbid activity of the glandular portion of the cervix. A follicular organ, which should only take on an active condition at certain intervals, becomes, from a variety of causes, constantly engaged in secretion. Instead of the discharge of the plug of mucus at the catamenial period, an incessant discharge is set up. In the first instance, the leucorrhœal discharge consists of nothing more than an unusual quantity of the elements found in the healthy mucus of the cervix. Quantities of mucous corpuscles and oily particles, with particles of epithelium entangled in the viscid alkaline plasma, which gives the mucus its clearness and consistence, are found. The mucus is seen at the os uteri, extending in the form of a string through the vagina to the os externum, and also adhering to the walls of the vagina in the curdy or creamy state produced by the vaginal acid. This form of leucorrhœa is to the cervix uteri what menorrhagia is to the cavity of the fundus uteri." (p. 391.)

This is the mucous leucorrhœa of Dr. T. Smith, and the affection of the external surface of the cervix he calls the epithelial variety, which may be combined with either epithelial or villous abrasion. Of both the varieties of this complication we have interesting microscopical descriptions, which our readers will do well to study. Numerous other points of importance are elucidated in this paper, with some views perhaps not so well established; but we have no hesitation in saying, that the facts stated by Dr. Smith, if corroborated by further experience, must, to a very great extent, modify our views of the structure and diseases of the cervix uteri. We trust he will extend his researches to the cavity of the uterus, and thus have the credit of illuminating the anatomy and pathology of that organ, upon whose physiological action he has written so admirably.

We should deserve blame if we did not mention, in terms of great praise, the beautifully executed plates which accompany the paper.

Fleetwood Churchill.

Review II.

1. *The Effects of Lead upon the System:* being the subject of the Lumleian Lectures, delivered at the Royal College of Physicians for the session 1852. By James Alderson, M.D., F.R.S. ('Lancet,' vol. ii. 1852.)
2. *Some Observations on the Contamination of Water by the Poison of Lead; and its Effects on the Human Body; together with Remarks on some other modes in which Lead may be injurious in Domestic Life.* By James Bower Harrison. Fcap. 8vo, pp. 196.

Although the fathers of physic were very familiar with the acute and well-marked effects of lead on the human body, yet it is only in modern time that physicians have become acquainted with the chronic and insidious action of this metal. The former of these effects were observed in those cases where the poison had been administered in the concrete form of minium, ceruse, and litharge, but the latter have resulted from the introduction of the metal into the system in a far more subtle condition, and often in a state of casual admixture with various articles of food. This fact was first noticed about a century ago; and, although it has since been made the subject of several valuable dissertations, yet it is only just beginning to assume its proper importance.

"Colica pictonum," says Dr. Alderson, "considered as a disease depending on the absorption of lead dissolved in cider, was first brought before the medical profession in this country in the year 1767. In that year, Sir George Baker, a physician of great talent and celebrity, read an essay before the College of Physicians on the cause of the endemic colic of Devonshire. The disease had been previously noticed in the same locality by Doctors Musgrave and Huxham, but without suspicion of its real origin—nay, even Citois, from whose accurate observation of the symptoms in the wine-district of Poictou, the malady derived the name whereby it is still known, deemed it only the effect of the unripe, austere wines of the province." (p. 74.)

Ten years, however, before the date just mentioned, Tronchin published a work on the same subject, and proved that the occurrence of lead colic at Amsterdam was due to the employment of lead instead of tiles for the roofing of houses; in consequence of which the water became impregnated with the metal, and acquired poisonous properties. Important as these facts were, they nevertheless commanded but little public attention. True, they were readily admitted into our standard works on medicine, but beyond this they obtained no serious consideration. A few earnest physicians, whose names will ever be mentioned, in connexion with this subject, with gratitude and respect, Drs. Hunter, Warren, Lambe, Christison, Percival, and Burton, in this country; together with Tanquerel, Merat, Andral, Breschet, Rumbelt, and a few others on the Continent, have, at various times, endeavoured to awaken the public mind to the vast importance of the matter, but unfortunately with little success. Now, however, after the lapse of nearly one hundred years from the time of Tronchin, eighty-five from that of Sir George Baker, and fourteen from the date of Tanquerel's work, we have the whole subject re-discussed, and the facts of it presented to the profession in the form of three brief but powerful discourses, each of which has been brought forward independently of the others. These are the able review of Tanquerel in our contemporary, the 'Edinburgh Journal,' and the two productions whose titles are placed at the head of this article.

Dr. Alderson's lectures are three in number: they are devoted to a general description of the chronic effects of lead, and to an account of the best means of remedying them. It is, perhaps, too much to expect that our author should have brought forward any novelty in the matter, seeing that the labours of Tanquerel, Merat, Christison, and Burton, have been so complete, as to leave hardly anything for their successors to perform: we must therefore be contented with the fact, that he has given us an admirable summary of all that is known of the disease: in addition to which he has invested it with an interest that it did not before possess, by that display of humanity and earnestness of manner with which he presented it to his distinguished auditors.

"The subject of colic and paralysis, the result of the absorption of lead, commends itself," he says, "to the notice of this assembly, by the same peculiarities which have deterred the great body of medical practitioners from bestowing much time and attention upon it. It is, in one form at least, a disease almost exclusively confined to the labouring class, and therefore a fit subject for the benevolent consideration of those who are the true guardians of the public health, and especially of the health of that part of the public who need the protection which they have not the power of obtaining for themselves." (p. 74.)

He likewise reminded his hearers, that, "unhappily, the victims of that poisonous agency, which in days of yore was but suspected or casually remarked, are now numbered by hundreds, nay thousands, in all classes, and especially among the artisans of several trades hereafter to be particularized." (p. 75.)

Dr. Alderson believes that lead may be rendered soluble by any of the secretions of the body; and that it may therefore gain access to the system by the lungs, the alimentary canal, the vagina, the skin, and even by the conjunctiva. He discusses the various ways in which absorption from these surfaces may be effected; and he points to a fact derived from statistical observations—namely, that absorption by the lungs is more productive of deleterious consequences than any other mode of receiving the metal into the system.

He states that the disease manifests itself among all who are engaged in the manufacture or use of lead compounds; that it is not only observed among painters, lead-smelters, shot-manufacturers, sheet-lead rollers, and the workers in sugar-of-lead, white-lead, litharge, and red-lead, but it is also seen among plumbers, potters, compositors, glass-melters, sealing-wax makers, and the enamellers of German cards. Some idea may be formed of the relative frequency of the disease among these artisans, by consulting the statistics of the Parisian hospitals. We are informed by Merat, that during the years 1776 and 1811, 241 cases of lead colic were admitted into the wards of La Charité. Of these, 148 were painters, 28 plumbers, 16 potters, 15 porcelain-makers, 12 lapidaries, 9 colour-grinders, 3 glass-blowers, 2 glaziers, 2 toymen, 2 shoemakers, 1 printer, 1 lead-miner, 1 leaf-beater, and 1 shot-manufacturer. In 1841, there were 302 cases admitted into the hospitals of Paris; of these 266 were occupied in white-lead factories: and according to the report of MM. Pelouse and Rayer, who were deputed by the Academy of Sciences to inquire into the best means of ameliorating the evil consequences of such occupations, it appears that from the years 1838 to 1847 inclusive, 3142 patients labouring under lead-disease were admitted into La Charité; of which 1898 came from the white-lead manufactories in the department of La Seine alone. Their report further informs us, that during these ten years, the average number of patients had risen from 268—the number in Tanquerel's time—to 314.

"An idea prevails that the women are less frequently affected by the pernicious influence than the men. We can scarcely deny some degree of credit to a popular idea, which can be founded only on observation; but I doubt its truth to any great extent. At least, it is certain that the immunity of the females is so small in extent, that in one of the larger manufactories of this metropolis, where seventy women are employed, the average of attacks is about two in a week. The impression among the workmen is, that the fineness of the pores of the female skin in some way protects them; but I should rather attribute it to their not being employed in the more dangerous branches of labour, such as sifting, packing, &c., and also to their more cleanly habits. Their comparative temperance is also a protective, for it is found that the use of ardent spirits, or anything which tends to debilitate the system, favours an attack; and this is borne out by the French statistics, in which it is shown that a very large proportion of these patients are admitted into 'La Charité' early in the week, a time succeeding their usual days for indulgence. Stimulants are also referred to by Dr. Bright, in his Hospital Reports, as an exciting cause. The statistics of 'La Charité,' though they state that only fifty-seven women were admitted out of 1200 cases, do not assist us to form any conclusion, as they are defective in a statement of the relative numbers of men and women employed. It has been suggested by a good authority, that the immunity is due to their organization. Tanquerel, however, states, that sooner or later, in France, every individual who works in the lead-manufactories is in turn attacked by the disease. The average time of exposure previous to an attack, as given by him, ranges between three days and six years, the general average being fifty-one days; and this fact, therefore, relieves us at once from the task of descanting on the causes for the alleged immunity of the female sex." (p. 98.)

As to the question of age at which attacks are most common; this, says Dr. Alderson, is much dwelt on by the French.

"Their tables show that between 30 and 40 years of age is that between which the greater number of cases occur, the next periods being from 40 to 50, and then from 20 to 30, the extremes at either end of the scale being exempt. It is impossible to draw any results, however, from these imperfect statistics, as the duties of the workers employed, as well as the aggregate numbers, should be known, for any argumentative purpose." (p. 98.)

Of 279 cases of colica pictonum referred to by Merat, 24 were under 20; 113 were between 19 and 30; 66 between 29 and 40; 38 between 39 and 50; 28 between 49 and 60; and 10 above the age of 60.

The mortality from colica pictonum is not very great, for Dr. Christison says, that of Merat's 279 cases 15 died, or 1 in 18·6. In 1833-4-5-6, there were 1541 cases treated in the hospitals of Paris, of which 38 died, or 1 in 39·5; and in 1839-40-41, there were 761 cases, of which 31 died, or 1 in 24·5.

Among the other causes of lead-disease must be mentioned the use of food or drink contaminated with the metal. We have already alluded to this fact in our review of Dr. Normandy's work on the adulterations of food;* but we may here again briefly state, that lead-pigments, so frequently employed for colouring cayenne pepper, cheese, lozenges, snuff, &c., are very likely to produce the disease in question. The fluids which are liable to become charged with lead are, water, wine, cider, vinegar, beer, porter, milk, and, in fact, all sour and saline liquids which are stored or distributed in leaden vessels.

"It is impossible," remarks Dr. Alderson, "to say how many cases of unaccounted for disease may be referred to such remote sources; and in the obscurity which involves the first disturbances of the equilibrium of the functions of the body, it is neither trifling nor unreasonable to search out their causes in the smallest tangible shape." (p. 393.)

Mr. Harrison has discussed with great minuteness the probable modes in which water may become charged with the metal; and, as we shall see hereafter, he looks upon it as one of the commonest sources of lead-disease.

Dr. Alderson has described the symptoms in the order of their occurrence, and has directed attention to the blueness around the edges of the gums, and their liability to hæmorrhage—both of which, he thinks, are among the earliest indications of the malady; then to the waxy tint of the countenance; the emaciated, anxious look; the poorness of the blood; the quick but feeble pulse; the obstinate constipation; the attacks of twisting colic, which are relieved by gradually increased pressure; the nausea and distress from flatus; the checked secretions from the kidneys and alimentary canal; the wandering rheumatic pains; the weakness of the upper extremities, amounting at last to palsy; the amaurosis; and, finally, the fatal apoplexy. He remarks, however, that

"The effects of the imbibed poison are by no means constant: different individuals are affected in dissimilar ways. In one, colic will be the result; in another, paralysis; in a third, the effect may be merely anomalous pains in the limbs or in the body, which, without the history of the case, might seem obscure in their origin and nature; in a fourth, the morbid influence may be manifest in the form of epilepsy or convulsions, and a modification of the paralysis is sometimes found in the forms of amaurosis and deafness. These several varieties have been separated by the French into distinct forms of disease, for each of which they have elaborated a high-sounding designation, such as 'arthralgia,' 'encephalopatic,' &c.

"All these various results of the action of lead may, however, be set up in different individuals under the same identical circumstances, labouring in the same workshop, and exposed to the same deleterious influences. It is more reasonable, consequently, to regard them as a series of symptoms developed differently, accord-

* See vol. viii. p. 77.

ing to the peculiarity of individual constitution or of the especial mode of absorption of the poison. The classification will be more convenient, as well as more efficient for the purposes of treatment, if we divide the subject into the two leading forms of colic and paralysis. Colic has usually been called the acute, and paralysis the chronic form; but I have considerable doubt of the accuracy of this distinction. There are well-recorded instances of paralysis being set up after a very short period of exposure to the noxious influence, without any intervention of colic; while, on the other hand, long-continued and frequently-repeated attacks of colic occur without paralysis supervening. Colic is undoubtedly the form of most frequent occurrence, and that with which practitioners are most familiar, especially in this country. Happily, also, it is the form which is the most amenable to treatment—indeed, so manageable that it is rarely fatal." (p. 75.)

He is of opinion, that when lead gains admission into the body through the lungs, it is most likely to occasion paralysis of an acute kind, but that when it enters the system by the alimentary canal it most frequently produces colic. In explanation of the former effect, he hazards a conjecture that it may be owing to the numerous intimate communications between the nerves of respiration and those of the axillary plexus. He says, indeed, that

"Dr. Bright's observations all lead him to this origin as the point in which to look for the seat of the disease; and here it is that the axillary plexus, the phrenic, and many of the nerves of the mechanical part of respiration, have their rise. It would appear, besides, as if it were possible that the absorbent glands in the axilla might, in some degree, promote the transmission to the trunks of nerves which supply the upper extremities; and there is one remarkable fact, that all the muscles in the forearm affected by this form of paralysis are supplied by one individual nerve." (p. 392.)

These views are, he thinks, supported by the phenomena witnessed among the workers in quicksilver.

"The paralysis to which these artisans are subject differs entirely from that which affects the workers in lead. Mercurial tremblings are known to arise from the absorption into the blood of the fumes of mercury during respiration. They come on gradually, with an incapacity to direct the arms and hands; and this want of power to direct increases to shaking, and then to trembling, and in time other parts of the body participate.

"Now, we have all seen mercury given to its full constitutional effect, as evidenced by ulceration of the gums and complete salivation, and yet we have no record, that I am aware of, of mercury taken by the mouth having produced these tremblings. As far as we know at present, it would appear that it is requisite that mercury should be received into the blood by absorption through the lungs in the act of respiration, in order to occasion these tremblings." (p. 212.)

And he afterwards says:

"I do not pretend to offer this as a fully-matured explanation; it may, however, deserve so far as to draw the attention of physiologists to the subject, and to induce them to trace, through this morbid state of the nerves, some hitherto unexplained nervous connexion between the act of respiration and the upper extremities.

"One more point on which I should like to remark is, that the parts paralysed are those which are most especially called into immediate action during the processes which occasion exposure to the metallic influence, whether the paralysis consist in loss of power of motion, or of sensibility—whether in paralysis of the extremities, or in loss of sensibility in an organ of sense, as in amaurosis. Lead-pickers use their fingers and eyes; and the compositors are a stronger instance of both fingering lead and straining their sight to discriminate the small types. They are both, especially the latter, subject to paralysis and amaurosis." (p. 392.)

In those cases where colic is the result, he thinks that the specific seat of the disease is in the large intestine, the circular fibres of which have first become contracted, and then paralysed, by the local action of the lead on the tissues. Indeed, this appears to be the probable cause of the mischief in every case; for it is an ascertained fact, that the metal always exists in the tissues of the paralysed muscles; and with respect to the phenomena witnessed, Dr. Alderson remarks, that—

"The *complete* influence of lead results in the graver forms of paralysis, and the more *partial* influence results in that state of disinclination to act, or of impaired power to act, which is a consequence of the attendant pain. In the extremities there is impairment of muscular power, and there is exalted sensibility: in the bowel there is excruciating pain, with a contracted state of the circular fibre of the muscular coat; in both cases there is the same inability or disinclination to action of the muscular fibre, which is in colic the great cause of constipation. It is the astringent power of lead which in both cases prevents the muscular fibre from performing its due function; consequently colic is the result of an approach to the specific paralysis of lead, developed in the muscular coat of the intestine." (p. 167.)

As to treatment: Dr. Alderson condemns the empirical practice which is usually adopted in La Charité, and the other hospitals of Paris. He says that it consists merely in an extension of what was the practice of the ancients, as laid down by Nicander and Celsus, who prescribed vomiting and purging as the treatment for poisoning by lead, as well as by other metals, under the erroneous belief that the nature of the disease was entirely referred to spasms. From a consideration of the nature and seat of the disease, he thinks that the great desideratum is, to choose the most certain and, at the same time, least distressing purgative—the action of which is comparatively uninfluenced by combination with opium.

"Calomel alone, or in combination with the compound extract of colocynth of our Pharmacopœia, and with opium, is the aperient which may most safely be relied on in all stages—even during sickness; not, however, omitting to guard against the probability of the constitutional effect taking place, and superseding the purgative. The combination of croton oil prevents such result, by accelerating the action; and as all remedies have their specific effect upon different portions of the canal, we secure our object better by combination. Whenever the above combination has failed to succeed, we have still a resource in the use of croton oil in full dose. Its action in this disease is speedy and safe: a couple of hours scarcely elapses without satisfactory result.

"Castor-oil is a favourite remedy in self-treatment at the manufactories, and, no doubt, is of the greatest value in the earlier symptoms. The women, who are more careful of self-management then the men, have frequent recourse to it in the beginning of indisposition, and in this way perhaps obtain that protection which has been attributed to other causes." (p. 213.)

The warm bath is another remedy, of which he speaks in favourable terms; and referring to the experience of the late Dr. Pereira at the London Hospital, he says that the addition of four ounces of sulphuret of potassium to thirty gallons of water increases its efficacy. The sulphur of the alkaline salt combines with the lead which may be present on the skin, or immediately below its surface, and forms a dark discoloration. This is observed to occur even after many ablutions, and it is chiefly developed in the axilla, the abdomen, the inside of the thigh, the palms of the hands, the back, and in all those parts where an abundance of hair is met with. Dr. Alderson thinks it probable, that the discoloration is due to the lead which is mechanically entangled by the hair and sebaceous matter of those parts. Some of it, however, is perhaps dependent on the metal excreted by the skin; for in one of the cases of lead-poisoning mentioned by Dr. De Mussy, the whole of the abdomen became black after a sulphur bath, notwithstanding that the patient had never been exposed to emanations from lead, and could not, therefore, have acquired surface accumulation.

In adverting to the efficacy of these baths, Dr. Alderson has offered a suggestion which is worthy of notice.

"It appears to me that the establishment of baths &c. for the poorer class, which have been lately opened, might be especially available for the artisans engaged in any sort of lead-works, in order to obtain an habitual purification of the skin from the particles of lead which attach themselves to the surface, or exude from the pores. If the benevolent conductors of these institutions would provide the addition of even a small amount of sulphide of potassium for the use of this particular class of mechanics, they might immensely increase the benefit which they already afford." (p. 391.)

Lastly, in the treatment of lead-colic, we are told that the use of opium is indispensable as a means of relieving the distress which occurs during the night, and renders sleep otherwise unattainable.

The cure of paralysis from lead, Dr. Alderson regards as a much more difficult matter, for he says:

In this form of the disease, the change of colour, the impaired muscular power, and the wasting of the paralysed muscles, demonstrate that there is an almost broken constitution to deal with, and one which would fail to endure the trial of very active measures. In colic we find an apparent wasting of the solids, especially indicated in the shrinking of the features; but this seeming emaciation is so soon recovered from, the form so rapidly restored to its usual proportions, and the complexion to a natural hue, that it cannot but be regarded as merely illusory. In paralysis, however, there is not a seeming, but a real, change in the structure of the muscles. There is actual emaciation, and it is one of the symptoms from which, when appearing in excess, we should draw the most unfavourable conclusions as to the prospect of final recovery; for here, indeed, the altered and impoverished condition of the blood is distinctly manifest." (p. 213.)

We have not space to discuss in detail the treatment which he proposes in these cases, but we may state that he recommends immediate removal from the source of the disease, the use of mild purgatives, the employment of the sulphuret-of-potassium bath, and the application of electricity or galvanism to the paralysed limbs. He thinks that the former of these physical agents is most useful in amaurosis, and the latter in palsy. He prefers the latter, because of its low tension and high chemical power; and he has ventured to propose a means of applying it which is somewhat novel.

"Our positive metal we may consider to be the lead in the nerves and muscles and tissues, and our negative metal, plates of copper, which we must attach to the limb itself; a weak solution of acetic acid may form the oxydating and connecting link. I choose acetic acid, because the acetate of lead is soluble, and there is every ground for reasoning that the chemical and galvanic actions set up will cause the lead to be withdrawn from the muscles." (p. 214.)

We hope that this may be so, even in spite of our fears to the contrary. Dr. Alderson is strongly opposed to the practice of administering strychnia in these cases, as will be seen from the following remarks:

"It is usual to give tonics, and in a safe form there can be no doubt of their general efficacy; but of the class of alkaloids, such as strychnia, we must admit that the danger of their use hardly excuses such experimental treatment without the greatest caution. My own recollection of the reputed success of the use of strychnia many years ago in France, coupled with my knowledge, at the same time, that these reports were unsupported by real evidence, makes me very dubious of later statements on this subject." (p. 214.)

Dr. Alderson perceives the necessity for speedily removing the poisonous metal from the system, for he says, "it is obvious that the first step must be to free the nervous and muscular tissue from the offending cause;" nevertheless, he has not mentioned, or even referred to, the plan of treatment recommended by Melsens, notwithstanding that this plan offers the most hopeful means for accomplishing so important an object. We trust that Dr. Budd's translation of M. Melsens' memoir, at page 201 of the present volume, will be read with interest; and we may here, in conclusion, state, that whatever may be the result of the plan in question, when it is put to the test of actual practice, there cannot be a doubt that at first sight it presents the strongest indications of success. Analysis has proved, that in lead-disease the poison is contained in the muscular and nervous tissues of the affected parts. The metal has likewise been found in the blood, in the liver, and in the substance of the brain; from which it is highly probable, that the whole of the phenomena witnessed in these cases are due to the actual presence and chemical reactions of the metal on the tissues of the affected parts. If this be true, what more likely chance is there of restoring health, than that which offers an easy mode of eliminating the poison?

Mr. Harrison's volume is, as its title implies, almost entirely devoted to an examination of the effects produced by water contaminated with lead. It is written in a very earnest and popular style, and is evidently intended as much for general as professional readers. We should judge from the preface, and early portions of the work, that Mr. Harrison had given much attention to this highly-important subject, though it appears, that when he entered on its study he was impressed with a belief, that the mischief arising from so insignificant a source as the trifling contamination of water with lead was comparatively small.

"I supposed," he says, "that it was one of comparatively limited application. I believed that the cases in which people suffered from this cause were few and accidental, and always, in the end, sufficiently apparent to the medical attendant. I now feel convinced that the cases are very numerous, at least in my own neighbourhood, and no doubt also in others. I feel sure that they are often, for a long time, misunderstood and improperly treated, and that deaths arise not unfrequently without the true cause ever being imagined. I know that many persons leave their dwellings under wrong impressions as to the salubrity of their localities, without even conjecturing that bad water is the real source of their disorders." (p. 3.)

As might be expected, our author takes a general view of the facts which have been made out concerning the action of common water on lead. These, however, have been discussed so recently in a former number of this journal,* that we may be excused from entering on them at any length, especially as the author has not advanced any novelty for consideration. It is now, we believe, generally admitted, that rain and snow water, as

* Vol. viii. p. 88.

well as certain kinds of water which contain but a small amount of saline matter (less, for example, than a 12,000th part), as well as those which hold chlorides and nitrates in solution, cannot be stored with safety in leaden vessels. It appears, moreover, from certain cases which have been recorded by Dr. Paris, Dr. Christison, Mr. West, and Dr. De Mussy, that when water containing much saline matter is brought into contact with lead and another metal, as iron, tin, zinc, solder, &c., it is likely to become charged with the poison, in consequence of galvanic action. Acid bodies, alkaline substances, and the products of decomposing vegetable or animal matter, are also said to be a source of metallic impregnation; and finally, it may be mentioned that the so-called insoluble compounds of lead are all slightly attacked by water. Fresenius has shown that the hydrated oxide of lead is soluble in 10,000 parts of water, the sulphate in 22,816 parts, and the carbonate in 50,551. We know that the last of these results is in opposition to a statement made by Professors Graham, Miller, and Hofmann, who were appointed by the government, in the early part of last year, to report on this subject. They assert that "pure water did not dissolve a quantity of carbonate of lead greater than one-sixtieth of a grain to a gallon, or one part of lead in four millions of water; while water, on the other hand, which contained already so much as six grains of oxide of lead dissolved in it to the gallon, had the quantity of metal reduced to one-fifty-seventh of a grain, by free exposure to the atmosphere for twenty-four hours, the lead being deposited as carbonate of lead, in consequence of the absorption of carbonic-acid gas," (Report, p. 33.) But while the matter is open to dispute, we are warranted in adopting the larger amount; and if we do so, there can be no doubt that the proportions named are quite sufficient to occasion disease. In the case of the late ex-king of the French and his suite at Claremont, thirteen persons became affected with lead colic, though the water which they drank did not contain more than one part of lead in 70,000; and Mr. Herapath has reported, that he once witnessed the injurious effects of this metal on the human system, when the water made use of contained only one part of the poison in 500,000. It has, however, been stated by Dr. Smith of Aberdeen, in a paper which he read before the Chemical Society of London, in April, 1851, that though the Dee water, which is supplied to that city, dissolves one part of lead in from two to seven millions of water, yet it did not appear to cause any injurious action on the inhabitants; and he further states as his opinion, that less than the twentieth of a grain of lead in the gallon of water, or one part in 1,400,000 parts, does not produce injury to those who drink it: he believes, in fact, that the limit of danger is somewhere between the tenth and twentieth of a grain to a gallon. This view of the case is supported by another eminent physician of Aberdeen, Dr. Dyce, who says, that during a period of seventeen years he has never known an instance of illness from this cause, and he adds, that the experience of his colleagues is to the same effect. In reply to this, however, Dr. Alderson has very properly remarked, that "the testimony from Aberdeen goes no further than to say, that the effects of lead have not been noticed by the physicians there. Now it is possible that lead-disease may be classed, in reference to their perception, in the same category with many other things; but be that as it may, we are not in a position to weigh negative evidence, while we have so many fully recorded facts; and many of us have had our own personal knowledge of instances, which bring direct proof of the positive existence of the evil." (p. 417.) To this we may add, that the work of Mr. Harrison abounds with such instances; and that the report of the government chemists, though it is intended to have an opposite tendency, actually demonstrates the fact of the great danger which is likely to arise from the use of soft water. The commissioners show that such water readily attacks lead, and forms, if carbonic acid be not present, the hydrated oxide of the metal. A large portion of this compound is dissolved in the water, and it requires the access of carbonic acid to precipitate the poison, though too much carbonic acid will re-dissolve it. Suppose, however, that the quantity of carbonic acid present is just sufficient to convert the whole of the oxide into an insoluble carbonate; this gradually falls as an impalpable powder to the bottom of the cistern, whence it is very likely to be stirred up by every fresh supply of water, and thus carried forwards into the service-pipes. So that, whether dissolved as the hydrated oxide or the bicarbonate, or suspended as the insoluble carbonate, it is liable to gain admission into the human body, and thus to produce incalculable mischief.

"There is another instance," says Dr. Alderson, "in which the presence of lead, under apparently similar circumstances, may be found to vary. Water running in leaden pipes, or pumped from a leaden pump, *may* show no trace of the metal; but after a delay of twelve hours or more in the pipes, or in the body of the pump, which very often happens, the water will be found to contain it in smaller or greater degree. As an illustration of this, I quote from Dr. Clark's published evidence in the report of the General Board of Health, on the supply in Aberdeen:—'The water is brought from the iron mains in the streets to the houses by means of leaden pipes, and in general without any disadvantage, because the supply from the pipes is constant, and the use of the stop-cock very frequent in a family; but in my class-rooms and laboratory, I find that when the pipe has been out of use for a few days, the water taken from it affords a trace of lead, which disappears when the water has been allowed to run briskly from the stop-cock for a few minutes.' It is curious that Dr. Clark is an Aberdeen M.D., and is thus describing the identical water which is spoken of in the 'Report of the Chemical Commissioners' as perfectly innocuous."

Dr. Christison has shown that the same thing is true of Edinburgh water, which may be kept running over the lead with impunity, though it cannot be left standing in the metal for any length of time without becoming charged with it; and the Board of Health have with much candour acknowledged, that in their opinion "minor injuries from such partial contaminations may occur, and pass unnoticed."

Mr. Harrison, however, believes that these injuries are not of a minor character, and few who read his book will think so either: though, to speak the truth, he appears to be somewhat over-anxious in the matter; for wheresoever he goes he sees the ghastly effects of this metal, and whatsoever he examines he finds is contaminated with it. The kitchen-boiler is the plague of his life, and the very name of soft water is a terror to him. He has no faith in the prudence of servants, and he calculates that whenever the rain-cistern is nearer to the house than the well, mischief will come of it.

"So much, indeed, am I impressed with the importance of this subject, that I recommend those who are located in lodging-houses, or who are in the habit of travelling from place to place, to furnish themselves with the means of testing water, that they may make an inquiry, from time to time, into the nature of the water brought to table. This may, at any rate, be a desirable caution for those who have already suffered from lead colic, or have dyspeptic symptoms for which they cannot find relief, and are ignorant of the cause. I have chosen to speak particularly of these points, because I conceive that if there be any merit in these pages, it is chiefly in the prominence which I have given to matters apparently so trivial." (p. 23.)

Sulphuretted hydrogen is the test which he employs, and he takes care to examine the water taken from the pump the first thing in the morning, so that the water may have stood in contact with the metal as long as possible. If this precaution is not adopted, the operator may fail to detect the contamination.

But the principal of all points connected with the discovery of the disease is, according to Mr. Harrison, a proper knowledge of the symptoms. These, however, may sometimes embarrass, from the circumstance that lead is an accumulative poison; and hence the effects of it may be suddenly manifested, even after long exposure to the influence of an exciting cause. With a view of elucidating this part of the subject, our author describes the symptoms of the disease with great minuteness. He informs us that in many cases the complaint is so gradually induced that the patient becomes affected without being aware of it, or at least without having that just appreciation of the mischief which he ought to have.

"He considers himself dyspeptic, is troubled with constipation, becomes slightly emaciated, perceives his respiration to be difficult, and feels to have lost his usual elasticity of mind and of body. Pain now begins to be experienced in the epigastrium; the constipation is more troublesome; and aching sensations are felt in the limbs. The complexion gradually assumes a dirty yellow cast, and the margin of the gums has sometimes a bluish appearance, or borders the teeth in places with a deep blue line. This state may be, in part, and for a time, arrested by purgatives and medicines, but especially, and for obvious reasons, by a temporary change of place. The sufferer conceives that his residence is unhealthy, and without suspecting the true cause, blames various circumstances in his locality or dwelling. The muscles now begin to lose their accustomed strength; the patient walks badly, holds his pen with less steadiness, and vomits, apparently without adequate cause; —after a protracted period of ill-health, to which he is become familiar, and perhaps to some extent indifferent, he is suddenly seized with a griping pain in the stomach. He takes aperient medicine, but it is vomited; he repeats the dose, but without effect: the pain goes on increasing, and at length becomes intolerable and unintermitting; medicine after medicine is rejected. He tosses himself about in extreme and increasing distress; now vomiting green porraceous matter, and now vainly trying by pressure and friction to mitigate his suffering. If this state be not relieved by timely and judicious assistance, convulsions arise and insensibility comes on, and even death may follow. By this time he will be found to have acquired a dirty yellow tinge, and to have a wretched look, with some emaciation and a partially paralysed and wasted state of the muscles. He has probably walked with a shuffling gait, and felt his mind give way, and even his words falter." (p. 41.)

This, he says, is not the invariable train of symptoms, but it is one which is most notable, and, so to say, the type of the disease. Among the irregularities in the effects, he mentions rheumatism, an aching pain at the lower part of the spine, which radiates into the thighs, chronic dysentery, epilepsy, &c. He then discusses the cause, progress, and diagnostic value of each of these symptoms, and concludes in the following words:

"It may be imagined, from what I have said, that so many general symptoms are attributed to the poison of lead, that it is almost impossible to determine when the lead-affection is really present. It must be observed, then, that the more important and characteristic symptoms are in themselves sufficiently distinctive; and that the rest have only a value when found in combination with these, or when the patient is actually known to have subjected himself to the saturnine poison. Many of the symptoms are thus only corroborative of the rest, and if found isolated, would not deserve the same amount of attention." (p. 86.)

These general remarks are followed by a great number of cases, many of which have occurred in the author's own practice, and the rest are taken from the reports of Drs. Christison, Norris, De Mussy, and James Robertson. We have not space to quote from them, but we may remark, that they fully illustrate the nature of the malady, proving that it is a common affection, and that it operates slowly but surely upon large masses of the community. As an example of the insidious character of its attacks, Mr. Harrison refers to the fact, that Dr. Norris was the subject of lead-disease for several years before he was aware of it, notwithstanding that he had devoted much attention to the matter, and was actually engaged during the whole time in investigating the more acute forms of the malady in other people. He tells us, moreover, that cattle are equally liable to the disease, and that it makes horses restless, and engenders a disposition to kick while they are in the stable. He likewise refers to an interesting paper by Dr. George Wilson, in the 'Edinburgh Monthly Journal of Science' for May last, wherein it is stated, that within the short space of five months he had to make a series of analyses in connexion with the death of thirteen horses and several cows, which were poisoned by compounds of lead, transferred by the atmosphere, or by water, to the fields in which they pastured.

The treatment which Mr. Harrison recommends is of the routine kind —croton oil, combined with aloes or colocynth, to relieve the constipation, and opium to assuage the pain. He also advises the use of hot fomentations to the abdomen; and he speaks most favourably of the sulphuret-of-potassium bath. Unlike Dr. Alderson, however, he recommends the use of small doses of strychnia, or nux vomica, in combination with aloes, for the purpose of restoring tone to the paralysed muscles; and lastly, he has omitted to mention the treatment proposed by Melsens, unless, indeed, it be referred to in the following paragraph:

"Chemical remedies for the disorders produced by lead have naturally had many advocates, but it is a little singular that the advocacy has been maintained on exactly opposite views. Some have recommended agents which, they imagined, would render the lead more soluble, and thus favour its elimination from the body; others have endeavoured to form insoluble salts of lead, and thus neutralize its deleterious influence. Certain bodies, which may be supposed to operate chemically, have been found useful; but experience, rather than theory, must decide upon their merits." (p. 158.)

Both of the writers whose works we have reviewed, impress upon us the necessity for a more careful consideration of the chronic effects of lead; and if only a small part of the account given us by Mr. Harrison be based on fact—and we have reason to believe that all of it is—there can be no doubt that the time has arrived for a complete and most careful revision of the whole subject of water-supply. Whatever may be the opinions of chemists, derived from insignificant experiments made with a determinate object, in the laboratory, if the experience of the medical practitioner points to the fact that lead-disease is a common affection, and that it undoubtedly arises from the use of water contaminated with the metal, there cannot be a question as to the propriety of altogether abolishing the use of lead as a medium of general distribution. And if it should so happen that the government decide on their recently-proposed scheme for the supply of this metropolis with soft water, it must be evident, as Dr. Alderson remarks,

—"*that especial study should be directed towards devising a new mode of distribution; that the use of lead, however facile in its applications, and however sanctioned by a long line of succeeding ages, must be abandoned.* It is not for us to repose on the excuse, that a source of evil which has served for nearly a couple of thousand years, may easily be permitted to remain in the present generation. Let us rather be thankful, when a really tangible cause of the too numerous derangements of human health is brought to light; for even better than the healing art is that wise precaution which is sedulous to remove even the smallest germ of yet undeveloped disease." (p. 418.)

All the evidence which has been brought forward by the Hon. W. Napier and others of his opinion, does not go to show that lead is not attacked by soft water, but rather to prove that it is affected by hard water also; indeed, their results do not in any way detract from former ones, but they rather add to them, by proving that it is dangerous to bring any kind of water into contact with lead surfaces. Who, therefore, can doubt the propriety of at once abolishing the present mode of employing this dangerous metal? Besides which, we have a number of cheap, durable, harmless, and equally convenient materials ready at hand to take its place. In fact, iron, zinc, slate, porcelain, and gutta percha have again and again been proposed as substitutes for lead; and it is a marvel to all who are acquainted with the subject, that one or other of these materials has not long since been adopted.

Before we quit the subject, we may remind our readers, that the best mode of purifying water from such contamination, is by filtering it through sand and animal charcoal. Relying on the experiments made by Colonel Yorke, Dr. Clarke, Mr. Richard Phillips, and others, we, some years since, contrived an apparatus for filtering water, which performs its duty admirably. It is described at page 91 in vol. viii. of this journal.

In taking our leave of these authors, we cannot but hope that their labours will be appreciated; and that the members of our profession, as well as the community at large, will devote more attention to this important subject, for assuredly it deserves their most serious consideration. It is, indeed, a matter for surprise, that so manifest an evil has not long since met with a remedy; in fact, it will hardly be believed that there is at the present time almost the same apathy and general want of information on this subject, as there was in the days of Dr. Burton, who referred to it in the following language:

"From a careful perusal of the authors before named, as well as from considerable personal experience, I presume to express a strong belief, that the unobserved introduction of lead into the human body is continually taking place to a much greater extent than is usually imagined, and that it has often caused an ambiguous assemblage of morbid symptoms; for although the influence of lead on the system is readily detected when the symptoms are severe, and follow each other in expected order of succession, yet when they are mild, or do not follow each other in the regular stated order of succession, if the mind of the physician is not awake to their cause, or the cause cannot be ascertained, then the symptoms appear ambiguous, and they may be misinterpreted, without exposing the physician to the imputation of unpardonable ignorance, or of culpable oversight."*

We are sadly afraid that Dr. Burton's remarks are as applicable now as they ever were.

H. Letheby.

Review III.

1. *Handbuch der allgemeinen pathologischen Anatomie.* Von Carl Rokitansky.—*Wien*, 1846. Band iv.

Manual of General Pathological Anatomy.

2. *Lectures on Nutrition, Hypertrophy, and Atrophy.* By James Paget, Esq., F.R.S., &c. ('Medical Gazette,' 1847.)
3. *Observations on the Clinical History and Pathology of one form of Fatty Degeneration of the Heart.* By E. L. Ormerod, M.D. ('Medical Gazette,' 1849.)
4. *On Fatty Degeneration of the Heart.* By Richard Quain, M.D. ('Medico-Chirurgical Transactions,' vol. xxxiii.)
5. *On Fatty Degeneration of the Small Bloodvessels of the Brain, and its Relation to Apoplexy.* By James Paget, Esq., F.R.S. ('Medical Gazette,' 1849.)
6. *On Fatty Degeneration of the Placenta, &c.* By Robert Barnes, M.D. ('Medico-Chirurgical Transactions,' vol. xxxv.)
7. *On Granular and Fatty Degeneration of the Voluntary Muscles.* By Edward Meryon, M.D. ('Medico-Chirurgical Transactions,' vol. xxxv.)

Much has been written and said about fatty degeneration, much that is true and sound, and much that is less trustworthy. Of the importance of the change in question and of its reality no doubt can exist; but some minds who love not to be carried away by the fashion of the day, and who long for clear proofs, are rather inclined to suspect that too much is made of fatty degeneration, that it is too often regarded as the essential and primary change to the exclusion of other less manifest alterations, or that it is too confidently pronounced in some instances to exist without its being fully proved that such is the case.

The suspicion, we confess, is justified by some circumstances to which we will allude. One is, that as the visible and the manifest affect our minds always more than that which is not so clearly seen, we are apt to consider the appearance of an unusual quantity of oil in a part as of more importance than other changes which are less prominent. The existence of oil in the hepatic or renal cells is matter of sight, but the presence or absence of sugar in the former is not. Thus we may overlook the more important but less manifest change in a tissue, while our attention is fixed on some more striking phenomenon. We think that we have seen

* Medico-Chirurgical Transactions, vol. i., Second Series, p. 73.

fibrinous infiltration of a part and consecutive fatty change mistaken for primary fatty degeneration. Again, the microscopist is prone to assume that highly refracting celloid or molecular particles are always of oily nature, whereas they may be either starchy or calcareous, or perhaps colloidal, or of some other substance. We speak *experientiâ docti* of the possibility of these fallacies; we know that starch globules floating in the urine are not to be distinguished from oil without the aid of iodine; we know also that calcareous (carbonate of lime) granules may most closely resemble fat-cells in outward appearance. Dr. Jenner has recorded his discovery of this fact in a report contained in the last volume of the 'Transactions of the Pathological Society,' p. 323; regarding more particularly, however, the calcareous phosphate. We had observed it some time previously, in making examinations of the common black slug. The whole of the larger arteries in this animal are coated with a white layer, which to the eye and the microscope has all the appearance of fat. As such we regarded it and had it figured, thinking it a very proper position for fat to accumulate in, as it is wont to do along many of the vessels in higher animals. Great was our surprise when, on accidentally adding some acid to a specimen one day, the fat dissolved with copious effervescence. Unless, therefore, the reaction of ether, acid, and sometimes iodine, have been tried, we must not too confidently assume that oil-like matter is oil. A third circumstance is the following: the condition of the kidney in cats and dogs proves that a very unusual and considerable quantity of oily matter may exist in a part without there being any degeneration of it; the part is fatty, but not degenerated. It is therefore necessary to beware of confounding fatty accumulation with fatty degeneration; the one may exist without impairment of function, the other cannot.

We propose to go over all the fatty conditions of the various organs so far as they are evidently deviations from the typical state of health, to examine similar changes in morbid growths, and to compare fatty with other degenerations. We shall attempt, lastly, some inquiry into the nature of fatty degeneration, and endeavour to discriminate between the several varieties which the condition may present.

To commence with the *heart*, the subject of so much interesting examination in this respect of late. We think the essential step in the discovery of this most important local change was made by those, among whom we must mention Vicq. d'Azyr, who distinctly recognised the occurrence of fatty degeneration in the voluntary muscles. To apply this knowledge to the similar morbid condition of the heart, an organ consisting of nearly identical tissue, was an important but much easier step. It was made in the most positive and distinct manner by Corvisart and Laennec, the former, however, speaking anticipatively, and not on his own experience. The conclusions thus attained have been confirmed and elucidated, though scarcely extended by subsequent inquiries. Rokitansky's account, given in 1842, left little more to be done, save a correction which was furnished by Mr. Paget a few years later. Since then, Dr. Ormerod and Dr. Quain have published their well-known memoirs, which have tended much to diffuse an acquaintance with this important change, besides adding to our knowledge of its pathological relations. We subjoin the statements of Corvisart, Laennec, and Rokitansky. Corvisart says:

"The conversion of the muscles into a fatty substance has been pointed out by Haller, and many other writers, but no one has described it better than Vicq. d'Azyr. The subject of his case is an old man, in whom he found almost all the muscles of the leg converted into fat, so that scarcely a vestige of them remained. But (he adds) the most singular circumstance in this limb, is the *disorganization* of the muscular fibres, and their *conversion*, by insensible shades, into cellular fibres. If we examine the sartorius, from its insertion with the os ilium to that with the tibia, these changes, in their successive degrees, are observable in the most striking manner. It was not between the layers that the fatty substance was deposited, but *in the elements of the fibre itself.* The limb preserved its natural shape, size, &c. This change appears to be common to many parts which have the muscular substance for their basis. What Vicq. d'Azyr saw in the lower extremity of an old man, some modern anatomists have seen in the heart. Doubtless they will some day publish these interesting facts; for myself, I have never seen this change. . . . . It is important to observe that this morbid change does not usually deprive the muscles of all their contractile power, either because it is never quite complete, or because the muscular fibres still preserve some degree of contractility. . . . . . I doubt not that this diminution of strength is in direct proportion to the degree of change, so that the complete transformation must necessarily have brought on an entire loss of muscular power. . . . . . . I cannot say how such a transformation takes place, but will simply make some remarks suggested by this case. 1st. The fatty nature of muscles thus changed cannot be doubted, because it offers the physical characters, and some of the chemical properties, of fat. 2nd. The parts changed in the case related by Vicq. d'Azyr, and in other analogous cases, having preserved their natural shape and size, it cannot be said that the alteration arose from a quantity of fat forming *upon* those parts. 3rd. The diminution of muscular power is the necessary result of, and in direct proportion to, the degree of this morbid change. It is necessary to distinguish this morbid change from another pathological state to which it has a considerable resemblance. I mean, the state of the heart in fat people, in whom that viscus is sometimes oppressed and smothered by the immense quantity of fat with which it is enveloped on every side, particularly towards its basis. Authors are but little acquainted with the first kind of affection of the heart; but they have given numerous examples of the last."*

This passage shows that Corvisart had a clear and just conception of true fatty degeneration occurring in the voluntary muscles, and that he was prepared to apply it to the case of the heart. Laennec says:

"The fatty degeneration of the heart is the conversion of its muscular substance into a substance which presents most of the physical and chemical properties of fat; it is an alteration quite similar to the fatty degeneration which Haller and Vicq. d'Azyr have observed in muscles. The substance of the heart in the point altered is more pale than in the rest of its extent; its colour is that of dead leaves, not the natural red, and is nearly similar to that of certain softened hearts."†

He carefully distinguishes this state from that of accumulation of fatty tissue upon the heart, and between its fibres, causing atrophy and wasting (not conversion) of the muscular substance.

Rokitansky,‡ after having described fatty accumulation taking place at the base of the heart, in its furrows, and around its vessels, the accompaniment of general corpulence; and fatty transformations of the muscular substance, by the penetration of adipose tissue between its fibres coincident with their atrophy and disappearance, and not at all necessarily associated

* Hebb's translation, p. 165. † Traité de l'Auscultation, vol. ii. p. 298.
‡ Sydenham Society's translation, vol. iv. p. 206.

with obesity; proceeds to speak of a third and different form, which he depicts as follows:

"The extent, seat, and duration of the disease present numerous remarkable diversities. In some cases we observe scattered and distinct centres of inconsiderable extent, where the muscular substance is pale, flaccid, of a dirty yellow colour, and soft and friable rather than admitting of being easily torn, as is usual in relaxation. In other cases, these centres are very numerous, and are found scattered over the true substance of the heart, in the trabecular and in the papillary muscles. They are ill-defined, their margins being indistinct or obliterated. The discoloration presents a striped appearance as it follows the course of separate muscular fibres, decreasing in intensity from the centre outwards, and being finally lost in the normal colour of the substance of the heart. This anomalous condition frequently extends over the whole inner layer of the muscular substance, which, when seen through the endocardium, after it has become thin and even transparent, presents the discoloration to which we have already referred, showing, on a closer inspection, that this change of tint depends on the presence of fine yellow granules, or globules, which are deposited in great numbers, in close contact, as if strung together on strings *in* and *upon* the muscular substance, and variously entwined among the muscular fibres. The trabecular and the papillary muscles are usually diseased throughout their whole extent, as is also the muscular wall of the heart through its entire thickness, although not uniformly in all parts. This granular formation on and between the muscular fibres marks the intensity of the disease, which further corresponds with the degree of discoloration and softening of the muscular substance. A microscopic examination shows an accumulation of black and dark outlined globules, which prove to be fat, while the muscular fibres are found to have lost their striated appearance, and the fibrilli are soft, and readily break down into delicate molecules. This form of adiposity most commonly occurs in the muscular substance of the left ventricle, and, in cases of hypertrophy, also in the right ventricle. This affection is, according to our observations, the most frequent cause of the *spontaneous laceration* of the hypertrophied left ventricle."

The chief defect in this description is, that the intra-sarcolemmous site of the oil-molecules is not so distinctly noticed as it should be; the author seems, indeed, to regard the oily deposit as being at least as much external to as in the substance of the fibre. This error was completely rectified by Mr. Paget, in the excellent description given in his lectures, in which he also announced the important fact of the disappearance of the nucleus in the fibres that are undergoing degeneration.

We may enumerate the following as the chief points to be noticed in the condition of the degenerating fibre. The homogeneous sheath of the sarcolemma, in most cases, persists, and does not appear altered; sometimes, however, it is not discernible. The elongated nuclei have either disappeared, or have broken-up into a streak of oil-dots, or are in process of so disintegrating. The contractile myoline contains throughout its substance a greater or less quantity of oil, appearing either as minute opaque molecules, or actual small drops. These seem, in great measure, to occupy the place of the sarcous elements, observing either the longitudinal or transverse linear arrangement, and suggesting the idea that they result from actual transformation of these particles. The substance in which the oil-molecules lie is of a dim homogeneo-granular aspect, is rendered much more transparent by acetic acid, and does not appear materially altered from its natural state. Sometimes, however, both it and the oil-particles are tinged of a deep reddish yellow colour. The altered fibres are apt to break into very short fragments: they seem to be unusually brittle and friable; in this, the sarcolemma as well as the myoline must be concerned. It is said that a diminution of the transverse striation is a sign of commencing fatty degeneration; but we do not think this is at all a constant sign of change,—it may be very marked in simple atrophy. Dr. Ormerod remarks, that the transverse striæ of the healthy cardiac tissue are less distinctly marked than those of voluntary muscle, and that the fibres have a "singular granular appearance." In one form of degeneration, which Mr. Paget distinguishes as the "granular," the oil-molecules are so minute and so uniform, that they convey the idea of a granular structure; this may not be quite clearly distinguishable from the granular appearance mentioned by Dr. Ormerod, unless acetic acid be added, which renders the natural tissue transparent, leaving the oil-molecules, if they be present, unaltered. The peculiar importance of this form of fatty degeneration is, that it generally affects the whole heart; whereas that which is described by Rokitansky may be confined to a few spots, and leave much sound tissue to carry on the work of the organ.

The columnæ papillares may undergo fatty degeneration, in common with other parts of the heart; but we are inclined to think that they may undergo a secondary fatty change, in consequence of their being invaded by fibrous tissue. In one instance, we found the apicial part of a column, when divided through its centre, to be converted, for some distance, into an opaque, whitish, fibroid substance, continuous with the chordæ tendineæ which were attached to the apex. The microscope showed that this whitish material consisted, in great part, of fibroid tissue, formed in great excess, in which there lay numerous muscular fibres in various stages of degeneration, many being converted into mere streaks of oil-drops. The fatty change in the muscle appeared to be secondary to the fibrous formation, which, starting from the chordæ tendineæ at their point of attachment, invaded the muscle, and caused atrophy of its fibres as it extended into it. There may coexist with this state, more or less, fatty change of the substance of the heart generally.

The localization of the morbid change to particular spots is a point on which we wish to remark. Dr. Quain says,—"In making a section it is sometimes possible to obtain a small diseased point, occupying two or three fibres, surrounded by tolerably healthy texture." This, certainly, proves that a very important element in the degenerative process must be a local decay of the vitality of the fibre. The granular degeneration affecting the whole heart might proceed from more general causes, as some condition of the nervous system or of the blood; but a local degeneration, confined to a few small spots of an organ, must acknowledge a cause equally limited.

It is of much importance to remember, that a true degeneration of the fibre not unfrequently occurs without any corpulent tendency, or accumulation of fat on the surface of the organ. In many cases, where sudden death has occurred, fatty degeneration of the heart has been found, and has been the only circumstance that could at all account for the fatal event. It is, therefore, most just to regard it as a condition which may produce such an issue. We think it, however, well to state, that it is not the only cause of sudden death proceeding from the heart, as the following cases show:

E. H., æt. 27, had been subject to palpitation and dyspnœa since an attack of rheumatic fever; dropsy had lately appeared; the urine was albuminous. Pulse feeble and irregular; cardiac dulness increased; regurgitant mitral murmur. The heart's action became more tumultuous; pulse scarcely perceptible; and she died suddenly. The heart was very greatly enlarged, and all its cavities distended with dark loose coagula; the walls of the left ventricle were slightly thickened, and its cavity much dilated, as well as those of the other chambers, to some extent. The left auriculo-ventricular orifice was enlarged, the pelvis healthy; but there was a good deal of thickening in the free edge of the mitral curtain. The kidneys appeared to be in an early stage of Bright's disease. Microscopic examination showed that the transverse striæ of the muscular fibres of the heart were not well marked; the structure was sometimes indistinct; there was, however, no trace of fatty degeneration, and the nuclei of the fibres were occasionally visible. Debility of the heart, not depending on fatty change or atrophy, was doubtless the cause of death.

In another case, death occurred suddenly while the person was riding; there was no account of his having had any recent illness. The heart was much enlarged; the cavities were dilated and their walls thickened, especially those of the left ventricle; the apex, however, of this, was remarkably thinned. The mitral valve was healthy, the aortic reduced to two flaps; the aortic coats were affected by fibrinous deposit. The transverse striæ were absent in many of the muscular fibres; they appeared as confused granular bands. Acetic acid brought nuclei into view, but no great amount of oil. The fatty change in this case, although it had clearly commenced, does not seem at all sufficient to account for the fatal event.

McE., æt. 55, had remained several weeks in St. Mary's Hospital, complaining chiefly of obscure abdominal pain. He had enlarged liver; there was a regurgitant aortic murmur, and a doubtful mitral. He expired suddenly one night while getting up to go to the night-chair, having been more cheerful and talkative that evening than he had been at any time before. The heart was much hypertrophied and dilated; it weighed 19 oz. The aortic valves were thickened and stiffened; the mitral was healthy. The left ventricle was more enlarged than the right; the tissue of both was of good colour, not flabby, and showed no more trace of fatty degeneration than the conversion of many of the nuclei into streaks of oil-molecules. In the right ventricular wall, however, there was some intrusion of adipose tissue among the fibres, but not to a great extent. The aorta was extensively affected by fibrinous deposit; the liver and the kidneys were the seat of degenerative change. The brain was healthy, except a cyst in the choroid plexus, and some fatty deposit in the walls of the small vessels. Certainly, in this instance, there was nothing whatever discoverable in the heart that could account for the sudden death.

On the other hand, we have seen cases which corroborate the statement of Dr. Walshe, that "fatty disorganization of the heart is by no means necessarily fatal. I have known," he says, "extensive destruction of the kind exist, where death had occurred from unconnected chronic disease of other organs." The existence of fatty degeneration must impair the contractile energy of the heart in proportion to its extent and degree, but probably not more than simple atrophy of the fibre; and in no case does it sufficiently account for a *suddenly* fatal event, when there have been no preceding signs of failing power in the central organ of the circulation. It is difficult to conceive how a condition, which must have existed some length of time, should produce no effects until the last moment. Are we not, in ascribing the fatal catastrophe solely to the fatty change, in danger of over-estimating the seen to the neglect of the unseen?

The following case shows that several signs indicative of fatty degeneration of the heart may be present, and yet dependent on other causes:

E. G., æt. 52, mother of two children, had suffered from palpitation six years; anasarca and ascites had appeared lately; pulse 134, small and weak; arcus senilis present; urine not albuminous, specific gravity 1·014; cardiac dulness increased; a systolic murmur below heart, over sixth and seventh costal cartilages and ribs. Pulse continued to be extremely weak and quick, sometimes irregular; the lungs became congested, and effusion took place in the pleuræ; she suffered much from dyspnœa, but died suddenly from apoplectic effusion into the brain. The heart was found large; the left ventricle hypertrophied; the valves all effective; the muscular tissue appeared healthy to the eye, and was found, on microscopic examination, very tolerably so, though there was more oil present than in the perfectly healthy state. The kidneys were in an advanced state of granular degeneration, and contained numerous cysts. The exceedingly feeble action of the heart, its enlargement, the existence of an arcus senilis, and the apparent absence of serious renal affection, seemed decidedly to point to the conclusion that the muscular structure of the heart was primarily and chiefly affected; and yet it clearly was not so;—the blood, rendered unhealthy by the renal disease, was an insufficient stimulant to the action of the heart, and the probably diseased vessels of the brain gave way under the venous congestion to which they were subjected.

In perusing the three cases of "granular degeneration" of the heart recorded by Mr. Paget, the question occurs to the mind, whether it be possible to consider the fatty change as one of recent and rapid occurrence, in fact, as an acute disease. The second case is the one which most forcibly suggests this idea. It is altogether impossible to understand how, with a degenerated heart, a man could pursue, "as amusement," what other men call "feats of strength and endurance." Dr. Ormerod, from whom we quote these expressions, solves the difficulty by supposing the animal spirits in this individual to be of an extraordinary vigour, by means of which some few fibres remaining healthy, were sufficiently toned to carry on the circulation effectually. This explanation we cannot accept, when we remember how dependent the emotional, as well as the intellectual, centres are on a duly sustained circulation for the exercise of their functions, and how much the love of active exertion and a cheerful buoyancy of spirit are impaired by the strange undefined consciousness of something amiss about the heart. As we are obliged to remain in the regions of hypothesis, we certainly think it more probable that the fatty change, in this second case at least, was of comparatively recent occurrence, than that it should have existed through years of vigour and activity. We would suggest, that in a case of this kind it would be worth while to attempt some examination of the ganglia and nerves connected with the heart, though even if these were found healthy, it would not disprove the possibility of death having occurred from some sudden suspension or alteration of nervous influence operating in the way of shock.

Another form of fatty degeneration of the heart, though less properly so termed, is that which consists in the growth of adipose tissue upon the surface and between the fibres. Rokitansky calls this "fatty metamorphosis of the muscular substance;" Dr. Quain distinguishes it more accurately as "fatty growth on the heart." The former says—

"The fat surrounding the heart penetrates inwards, and by gradually insinuating itself between the muscular fibres, tends in this way to displace the muscular substance. The apex of the heart and the right ventricle are especially subject to this form of degeneration, which, according to Laennec's observations, originates at the first of these points. When the left ventricle is implicated, the disease is usually limited to the apex, from whence it advances towards the right ventricle. It is only in its more intense stages that it affects the main part of the left ventricle. The muscular substance at the apex of each side of the heart, and consequently in the right ventricle, is frequently observed to be reduced to a layer which, from its extreme thinness, scarcely admits of being measured, and appears like a mere muscular investment covering the fat. In cases of intense degeneration, the muscular wall of the left ventricle has even been found only from 2-1½ lines in thickness, or the columnæ carneæ may even appear to arise from a mass of fat. The muscular substance is flabby and much relaxed, of a faded colour, capable of being easily torn, and infiltrated with free fat. The valves of the heart are, at the same time, thin and transparent, while the papillary tendons are softened."

This state does not occur only together with corpulency and accumulations of fat in other parts, but also with general emaciation, the result in some cases of tuberculous disease, and also "under circumstances that have not yet been explained." Dr. Quain, speaking of the condition of the muscular fibres, says, "that even though they are overwhelmed with fat, they may still retain their organization, though their course and direction is more or less modified and distorted." In the case illustrative of this form of fatty change of the heart, given by Dr. Ormerod, it is mentioned that the organ, which was "rather larger than natural, was entirely overlaid with a layer of dirty yellowish fat, separated by a distinct boundary from the muscular substance," which was "pale, like dead leaves, contrasting forcibly with the bright red colour of the abdominal muscles," and passed rapidly into decomposition, while other muscles had undergone no change. From the combined testimony of these observers, it seems warrantable to conclude, (1) that in some cases, as the fat encroaches upon the wall of the heart, advancing inwards from beneath the pericardial layer, the muscular tissue simply wastes, and is replaced by fat, the fibres, even when invaded by the fat intruding among them, remaining tolerably healthy; (2) that in others, under the same circumstances, they do not merely atrophy and disappear, but degenerate fattily in their own texture. We are much inclined to think, that between the condition of fatty growth upon the heart, with wasting of its tissue, and that of degeneration of its fibre, no broad essential difference exists. In both cases, we believe that the original evil lies in a lowering of the vitality of the muscular tissue, which, in the one case, simply wastes, in the other, degenerates. Both changes are atrophic, and may be shown, we think, to resemble each other considerably by the following consideration. It is not at all to be supposed that the adipose tissue forming upon the surface of the heart, and penetrating between its fibres in any way of mechanical pressure, causes starving and wasting of the tissue. The fatty growth does not bring about the atrophy, but is itself a consequence of it; the fat collects around the wasting heart, just as it does about a paralysed muscle. Now, as we have on another occasion described,* the first step in the formation of fat consists, not in the development of granular or transparent nucleated cells, which afterwards become filled with oil, as the common account has it, but simply in the separation of oil-drops from the plasma, which coalesce together, and after a time become surrounded by a film of proteine matter, constituting an homogeneous, membranous envelop. In some cases, owing to unknown causes, the separation of oil from the effused plasma takes place within the fibre itself, and thus is produced fatty degeneration. In other cases, the oil separates from the plasma in the interstices of the fibres, and while these waste, undergoes the lowest grade of organization, that of becoming fat-cells.† As Rokitansky and Ormerod testify, the two processes may go on together. Between these two varieties of degeneration and the mere addition of a quantity of fat to a *healthy* organ, the interval is very great indeed. As Mr. Paget says, "the muscular fibres of the heart, or of the voluntary muscles, may be imbedded in adipose tissue, and yet may be themselves free from the least degeneration."

The fatty changes of *voluntary muscles* are precisely the same as those of the muscular tissue of the heart; the fibres may either simply waste and be replaced by fat, or they may undergo true fatty degeneration, while more or less of interstitial adipose tissue simultaneously springs up. As the microscopic appearances are identical with those we have detailed, we shall not mention them, but only quote Rokitansky's description of the changes perceived by the naked eye.

"The earliest change is one of colour. The muscle has a pale reddish appearance; and is found, on close examination, to be not uniformly discoloured, but stained irregularly of a yellowish or fawn colour, as well as marked with longitudinal pale reddish streaks, which follow the course of the fibres. As the stains of fat increase in size, and coalesce, the muscle acquires an almost uniform fawn colour; but its fibrous arrangement still remains distinct. With the advance of the disease, it becomes altogether of the colour of fat; sometimes being yellow, sometimes remarkably white, and resembling, accordingly, either tallow or spermaceti. No trace of its fibrous structure remains, except some of its tendons or the cellular sheaths of its fasciculi. Up to this stage of the disease, the outline of the muscle has been preserved; but in the last stage, the mass of fat into which the muscle is changed mixes with the adipose structures around it. . . . . We may, then, find in a limb nothing of its muscles but remnants of tendons, and aponeuroses with their prolongations inwards."

It is very instructive to inquire into the causes of this degeneration of voluntary muscles. We enumerate them after Rokitansky, as "advanced age, a sedentary, luxurious kind of life, the misuse of alcohol, and complete inaction of the muscles," the result of paralysis, of anchylosis of a joint, &c. The one thing which is manifestly common to these several conditions is a lowering of the vital power, either of the muscles themselves, in con-

* Medical Gazette, May, 1850, p. 933.

† It deserves to be borne constantly in mind, that "fat is no part of an animal," as Hunter has it. It does not rank with other tissues; it does not appear to be a necessary part of the plan of the organism.

sequence of disease, or of the whole system generally. Luxury and spirit-drinking promote the formation of fat, no doubt, by increasing the quantity of oily matter in the blood, as well as by lowering the vital energies. But besides these causes, whose operation we can in some measure understand, there is another, which is quite unknown to us, and which produces the apparently spontaneous degeneration of muscles. Rokitansky seems to have observed this change in the muscles of the calf of the leg, in which it gave rise to considerable pain. Dr. Meryon has recently recorded* some very interesting cases, in which the muscles throughout the body gradually lost their contractile power, so that the patients at length could neither walk nor stand. What is the more remarkable, is, that in all the eight cases, the disease occurred at an early period of life; the four first, brothers of the same family, were affected almost from the birth, but rapid deterioration seems to have commenced about the age of ten or eleven, and one died, apparently in consequence of the disease, at the age of sixteen. In the last two cases, the muscular power of the legs began to fail when the patients were about twelve years old, and when they were twenty-five, both legs and arms were quite powerless. It is also very remarkable, that in all the three families to which the patients belonged, the females were entirely exempt from any trace of such affection. This peculiar limitation is most marked in the first instance, in which all the six girls appear to have been entirely free, while the remaining four boys were more or less affected. A thoroughly satisfactory examination in one of the fatal cases, shows that the sole lesion (at least, the only one detectible) was fatty degeneration of the fibres of all the voluntary muscles. "Throughout the entire body they were atrophied, soft, and almost bloodless," and destitute of their natural deep-red colour. The change was not a loading of the fibres with enveloping adipose tissue, as might indeed have been supposed, from the circumstance of the patients being apparently well-grown and fleshy, but an actual fatty degeneration of the fibres. It is curious that the heart's tissue was not at all affected, the transverse striæ of the fibres "were as discernible as in ordinary healthy hearts." Dr. Meryon argues against the possibility of the change having depended upon a "defalcation of nervous energy," from the circumstance that the fibres were actually degenerated, and not only wasted and overgrown with fat. We are quite disposed to agree with him in believing the essential cause of the degeneration to lie in the muscular tissue itself, but not for the reason he assigns, which we believe to involve an erroneous statement. There is no doubt that in many cases of paralysis the muscular fibre truly degenerates, and does not only waste. Professor Simon states this very clearly, at page 90 of his Lectures on Pathology, and Mr. Paget speaks to the same effect. The same authority mentions, that *one* of the muscles of an extremity, or of the back, is sometimes found thoroughly atrophied, while the others are healthy; and that no account can be given of its failure. This fact puts in a still more striking light than even the above-cited cases, the supreme importance of the vital power of a muscle, as, indeed, of any part, in preserving and maintaining its healthy organization. Compared to many other organs, the muscles certainly enjoy a considerable immunity from disease, and the cause of this, in part, may be the continual exercise to which they are sub-

* Medico-Chirurgical Transactions, vol. xxxv, p. 73.

ject, which, according to the general law, maintains their natural energy and assimilative power. The nerves distributed to many other organs convey influences only from ganglionic centres, which are endowed, as it were, with only a material life, itself liable to torpor and decay; but those supplying the voluntary muscles are the channels of impulses emanating from that mysterious power, which, itself indestructible, is but rarely inactive, and can sometimes impart an almost superhuman energy to limbs that are cast in no Herculean mould. One more fact of interest, and also illustrative of the same point, we quote from Dr. Meryon's paper. He states, on Mr. Quekett's authority, that the ostriches confined in the Zoological Gardens are subject to fatty degeneration of the voluntary muscles, which so weakens the power of those of the lower extremities, that the birds drop down never to rise again. It is clear that the confinement, as well as the change of climate, must operate prejudicially on the nutritive processes in these animals, so as to predispose their tissues to degenerative disease; but the deprivation of the natural exercise of the muscles concerned in running must, somewhat in the same way as in a case of paralysis, specially incline these organs to fatty change. Mr. Hallett, in his interesting paper on adipification of muscle,* states, that "the circumference or surfaces of a muscle undergoing fatty transformation are always first converted into fat; in other words, that the adipose matters are first deposited in the outermost fibres, and subsequently in those towards the centre." Why this should be, no reason can be given, but the same appears to be the case in some measure in the heart, as the spots of degeneration are, for the most part, situate at first beneath the endocardium or pericardium. The same author also remarks, and Dr. Meryon's observations appear to be confirmative, that no diminution of bulk may occur in muscles that are completely degenerated; nor does the form appear to be changed. This is an excellent characteristic of a true degeneration, and forms the distinction between it and simple atrophy. In the latter, the replacement of substance during the process of molecular change is imperfect, more is carried away than is laid down. In the former, there is no want of equilibrium between subtraction and addition in regard of quantity, but much in respect of quality: the quaternary proteine compound is replaced by the ternary oily. We think this fact respecting the voluntary muscles has also its parallel in the case of the heart, both as regards the individual fibres and the whole organ; it seems to be of some importance, and we may refer to it again. Of course, atrophy may and does occur at *later* periods in degenerated muscles, and that to an extreme degree.

The proportionate quantity of oily matter contained in degenerated muscles is very considerable. Mr. Hallett states that a portion of the pectoralis major, weighing 1oz., gave only 2 scruples of substance not fatty, and but 46 grains of residue were obtained from a like amount of the rectus femoris, the rest being fat.

Mr Paget notices particularly the great diversity which exists in the condition of the tissue intervening between the fibres in fatty degeneration of voluntary muscle. In some instances the quantity of fibrous tissue is very great, with no unusual amount of fat; in others, the fat is accumulated in great quantity. He is much inclined to believe that the

* Edinburgh Medical and Surgical Journal, 1849, p. 267.

increased and toughened state of the areolar tissue only takes place in muscles which, being atrophied, have had to resist stretching after the manner of ligaments, and that the increase of fat is found only when a muscle has been very long atrophied and remained completely at rest, and the fibres themselves, after degenerating, are being removed and replaced by a formation of common adipose tissue. Mr. Adams has recorded* a series of observations upon the condition of the extensor-reflexor muscles of the foot in cases of talipes. As far as any conclusion can be drawn from these observations, which were made by Mr. Quekett, it seems that in talipes of paralytic origin adjacent muscles of both kinds may be very diffently affected, one being tolerably healthy while the other is completely degenerated. In talipes of spasmodic origin the large muscles of the calf were much degenerated, while the extensors were healthy, and only in one (tibialis anticus) was there excess of fibrous tissue. In talipes without paralysis or spasmodic affection the extensor muscles were tolerably healthy, the large muscles of the calf more or less degenerated, and having their fibres imbedded in a large quantity of fibrous and adipose tissue.

We proceed to fatty degeneration of the *coats of arteries*, the common atheromatous condition. On the inner surface of the aorta, which is for the most part earliest and most extensively affected, we find in the commencement of the process numerous whitish yellow patches, spots, or streaks of small size, which are sometimes scattered irregularly, but, as Hasse remarks, in youngish individuals are arranged in two interrupted lines, near the origin of the intercostal arteries along both sides. They are at first, he proceeds, perfectly flat, but after a while slightly raised. This point we have particularly noticed as of some importance, and we quite agree with Hasse's statement. If a thin layer of the internal coat be raised up with the forceps and peeled off the surface, carrying with it a small commencing patch, the character and situation of the deposit can be very well seen. Such a layer comprises little more than the longitudinal fibrous coat, the fenestrated membrane according to our observations seldom existing in the aorta. It is quite translucent, and exhibits a rather indistinct fibrous structure, which is not much altered by acetic acid. At the margin of the atheromatous patch, or for some distance around it, minute oil-drops are scattered throughout the tissue, which, on advancing towards the centre, are seen grouped together in small irregularly-shaped masses, or streaks. Occasionally the groups resemble very closely the glomeruli or granule-cells, but we have never seen any appearance of their being formed by the filling of a pre-existing cell with oil-drops, as the latter commonly are. The deposit is not confined to the superficial layers, but exists frequently among the deeper. It has appeared to us in all cases of pure atheroma (by far the most common) to be primary, that is to say, the oily matter is deposited at once as such, and does not result from the breaking up of any previous exudation. Instances are not uncommon in which more or less of atheromatous change is combined with that which results in the white cartilaginiform patch, but we are convinced, from careful examination, that the two processes are quite distinct, in which we are supported by Hasse's authority. The atheromatous patch, as said, consists of oily matter from the first; the cartilaginiform, when pure, presents only a

* Report of the Pathological Society for 1851–52, p. 472.

translucent homogeneous substance, which seems to have been infiltrated while fluid among the fibres of the tissue. The unaided eye distinguishes a small point of atheroma amid the cartilaginiform (fibrinous) stratum by its yellowish colour and dead opacity; the microscope detects the faintest traces of such commingling by the oil-drops, which at one or more spots interfere with the translucency of the infiltrated homogeneous matter. The pathological relations of the two deposits we believe to be different; atheroma belongs to the class of true fatty degenerations, the fibrous deposit to that which includes cirrhosis of the liver, granular kidney, contraction of the cardiac orifices, and stiffening of its valves. Hasse states, and we coincide with him, that the fibrous patches never ossify, as the atheromatous commonly do. There is another alteration of the inner arterial coat, which we believe is not generally known, and which it seems desirable to notice, as Rokitansky appears to confound it in some degree with the atheromatous process. He describes it as "consisting in an excessive formation and deposition on the lining membrane of the artery derived from the mass of the blood, and at the same time constituting hypertrophy of this membrane." He says "it is the most frequent form of disease affecting the arteries, and is on that account of the greatest importance." The deposit "may vary in thickness from a quarter of a line to two lines and upwards, and extends in extreme cases over the whole trunk and main branches of the aorta, implicating the entire arterial system." We have as yet only found this condition in the external iliac arteries, but have scarcely searched for it elsewhere. When the vessel is laid open, the surface appears of a greyish colour, and is curiously thrown into transverse wrinkles, which sometimes become very prominent, and enclose between adjacent ones saccular depressions. There is no staining of the surface, nor does the elasticity of the vessel in the longitudinal direction seem much impaired. A vertical section parallel to the long axis of the vessel shows internally the wavy whitish lines of the hypertrophied internal membrane, next a thickish reddish grey stratum, and externally an equally thick more opaque whitish one. A thin section of this kind in acetic acid, under the microscope, exhibits externally a coat composed of yellow and white fibre; in the middle, the contractile fibrous coat; and internally, that of the fenestrated membrane, which forms several layers, one over the other. It is this coat which by its hypertrophy gives rise to the wrinkled condition of the internal surface. Rokitansky seems to us to imply that the atheromatous deposit takes place only in this hypertrophied internal coat; he says, still speaking of the laminar formation, "The metamorphoses through which the above-described deposit passes after it has become completely opaque, are the so-called atheromatous process and ossification of the arteries." The atheromatous process consists in the metamorphosis (disintegration) of the deposit into a pulpy mass, compared by the French to a *purée* of peas, consisting of a large number of crystals of cholesterine, fatty globules, and of molecules exhibiting various degrees of consistence, from coarseness to extreme fineness, and consisting of albumen and calcareous salts." We can scarcely believe that Rokitansky intended to convey the idea that atheroma has its seat commonly in an hypertrophied fenestrated membrane, at least so far as the aorta is concerned, for daily observation shows the contrary. Calcareous matter is probably not uncommonly deposited in the superposed fenestrated layers, as we have seen it, and oily matter *may* no doubt also be deposited there, but we cannot think that this change any more than the cartilaginoid has any essential relation with atheroma.

As the oily deposit in a patch of atheroma becomes still more heaped up and condensed, the tissue in which it lies degenerates and perishes, from its nutrition being interfered with, or primarily decaying. In this way the thin covering layer which separates the oily mass from the current of the blood is broken down and removed, and the oily matter itself being evacuated from its nidus, a kind of ulcer is left. There is no trace of inflammation around softening and disintegrating atheromatous patches; but if they advance at all deeply and tend to penetrate externally, as they have been known to do, the adventitious coat usually becomes thickened and its vessels injected. The matter which is expressed from a softened atheromatous patch consists almost entirely of variously sized oil-drops, quite fluid, and capable of coalescing, and of cholesterine tablets. The calcareous change is quite as common in atheromatous matter as that of softening. The two often proceed simultaneously; one part indeed of a patch may be seen calcifying, while another is breaking down by softening. Hasse thus describes these formations:

"Ossifications of this kind mostly assume the form of very brittle layers of a pale yellowish colour. The internal membrane passes over them unchanged; sometimes, however, it disappears, so that the stream of blood comes into immediate contact with the bony plate. It enlarges at the expense of the middle membrane. . . . . Although the bony sides adhere pretty firmly to the arterial membranes at their margins, yet they sometimes crack and tear at the centre, thus favouring the ulcerous disposition of the parts, and projecting in their partially loosened state into the cavity of the vessel."

When this is the case they are very apt to become the seat of fibrinous coagula, which, especially in small vessels, may obstruct more or less completely the current of blood. "In some instances," as Rokitansky and Hasse testify, atheromatous matter may be taken up again into the circulation by absorption. "The internal surface of the artery then exhibits blackish pits, with scar-like puckering and attenuation of all the membranes." Rokitansky states "that the seed of arterial ossification is in the lining membrane of the vessel, which is itself produced in anomalous excess." This certainly does not hold good of the common calcific change in atheroma; the phosphate and carbonate of lime settle down in the degenerate and feebly vitalized organic matter as they do in tubercle when it ceases to soften, in fibrous tumours, and other morbid products. No doubt the hypertrophied fenestrated membrane often does calcify, and this is perhaps especially the case in old persons when the arteries ossify (as it is said) extensively, without having been atheromatous. Rokitansky states further, that "in the more highly developed stages of this deposit (that of fenestrated layers), and when the atheromatous and ossifying processes have become fully established, and even made considerable progress, the circular fibres each presents a dirty brown, gold-like colour, and is soft, lacerable, and cleft. . . . This disease of the circular fibrous coat," he adds, "depends on fatty degeneration, by which, analogously with the process observed in the so-called fatty metamorphosis of the muscular tissue, its peculiar ramifying fibres and its elasticity are destroyed." We have not observed this fatty change of the contractile coat; we have seen it altered in instances of hypertrophy of the fenestrated membrane, showing its fibres less distinctly than natural, but it was not the seat of oily deposit. There is nothing peculiar in the atheromatous process when it affects the *endocardium;* the deposit of oil we have satisfied ourselves is primary, the drops are seen distinctly at the margin of a patch dispersed amid the fibres which are quite unaltered; but in the centre the oily seems to be mixed with a certain quantity of soft granulous substance. The fibrinous deposit which stiffens and thickens the valves has the same relation to atheroma here as its analogue in the arteries. It need scarcely be remarked, that in the case both of the arteries and the endocardium the oily matter is a deposit from the blood which traverses the vasa vasorum, and not from that which is contained in the heart or the artery.

The important pathological condition which Mr. Paget has recently noticed of the small *bloodvessels of the brain*, deserves especial notice. He finds in the least degree of the disease minute, shining, black-edged particles, like molecules of oil, thinly and irregularly scattered beneath the outer surface of the smaller vessels. This state is observed in capillaries, arteries, and veins, even of portions of brain which appear quite healthy. "As the disease makes progress, the oil-particles may increase in number till the whole extent of the affected vessels is thickly set with them, and the natural structures, even if not quite wasted, can hardly be discerned." The oil-molecules increase to the size of distinct drops, or aggregate together in groups resembling granule-cells or masses of irregular form though similar aspect. The coats of the vessels become atrophied, the external layers being most affected, so that vessels even of $\frac{1}{150}$th of an inch in diameter come to resemble tubes of mere homogeneous membrane, thickset with fatty particles. Sometimes the outer layer of the wall is lifted up by clusters of oil-particles, and sometimes there are aneurismal dilatations or pouches of the smaller branches. The capillaries are less affected, speaking generally, than the arteries or veins. The principal seat of the deposit is, in arteries, the transversely fibrous coat; in veins, it is the corresponding layer; in vessels which have only a nucleated homogeneous tissue, the substance of this membrane is the first seat of the deposit. In the first and second cases selected by Mr. Paget, there was some atheromatous change in the bloodvessels at the base of the brain; in the third there was none, in the second and third there was coincident atheroma of the aorta. In all three cases the part which had been the seat of hæmorrhage was much softened, and in the second and third there were granule-cells in the broken-up tissue. Dr. Quain has recorded an instance of fatty degeneration of the small vessels of the corpus striatum, in a case of hemiplegia, with subsequent epileptic seizures; the part was much softened and disorganized. Dr. Sibson has recorded in the same volume a case of "red and white softening" of parts of the right hemisphere of the brain, in which the vessels are spoken of as being in a state of fatty degeneration. From our own examination of this specimen we were rather inclined to consider that the change was of an inflammatory kind, producing exuda-

* See Report of the Pathological Society for 1851–52, p. 246.

tion-granules on the surface of the vessels, in the manner described by Dr. Bennett. The history of a year's headache, chiefly affecting the right side, and increased in severity during the last four months of life, also supports this view. We have several times been able to corroborate the statements of Dr. Bennett, respecting the coating of the vessels with patches of oily-looking granules, and the existence of similar granules in the surrounding cerebral substance, both free and in the form of granule-cells. We have seen this in cases of hydrocephalus in children; in scrofulous inflammation of the ventricles in the adult, and in a case of acute meningitis. On the other hand, we found very recently in the brain of a man dying suddenly from heart-disease, the small vessels of the hemispheres exactly in the state described by Mr. Paget. Had we not known that the brain was quite healthy in appearance, and that no cerebral symptoms had existed, we should have been very much inclined to suppose that this coating was the result of inflammatory action. In this case, however, there were no oily-looking granules or glomeruli in the cerebral substance. We have examined also a portion of one hemisphere of perfectly healthy appearance, from the brain of a man who died with hæmorrhage into the pons Varolii, and a cyst in one hemisphere. All the minute vessels just pre-capillary were coated over with a deposit of orange-yellow refracting corpuscles. These were very similar to those commonly met with in the spleen, sometimes occurring singly, but for the most part in groups. They were little affected by liquor potassæ or acetic acid, and were mingled with a few colourless oily granules. They were situated chiefly in the areolar sheath of the vessels, and did not encroach upon the inner coats; the circular fibrous tissue in particular was unaltered. The smallest vessels were much less affected, but they and the capillaries sometimes appeared less purely homogeneous than is natural, as if dotted over with granules. There was slight atheromatous deposit in the basilar artery. A large branch of the anterior cerebral and its offsets were quite free from the yellow matter. From the statements of others, and our own observation, we think it must be concluded that there are two conditions of the small cerebral vessels which resemble each other very much, the wall being in both covered over with a coating of oily-looking granules. In the one, the change is the result of an inflammatory exudative process; in the other, it is simply a degeneration of the nature of atheroma. The first is related to delirium, pain in the head, and erethism of the nervous centres; the second, to hæmorrhagic extravasation. In the former there is almost always simultaneous exudation of similar granules in the adjacent cerebral substance, with softening, and often vascular injection; granule-cells also are frequently present. In the latter there are no such changes in the surrounding brain or its *dêbris*, unless inflammation have supervened in consequence of irritation. Cases, however, we can well conceive, may occur, in which it would be very difficult to decide to which of the two classes a given instance should be referred. We think Mr. Paget's discovery one of the most important contributions to pathology that has been made for some time. Rokitansky and others have noticed the calcified condition of the small cerebral arteries, "the brain appearing as if filled with stiff wires;" but no one before him, that we know of, had demonstrated the frequency of fatty change in this structure and its important bearing upon the phenomena of disease. That many of the vessels throughout a brain, which appears healthy to the naked eye in almost every respect, should be so seriously weakened and disorganized, is a most weighty fact for the physician to consider. How important a service would statistical research render in showing at what age and under what pathological conditions such a change occurs most frequently.

A fatty change in the *brain and nerves*, as such, is not known to us; but we may remark, that in a state of white softening there is always much *débris* of the white nervous matter strewed about, which has very much the aspect of oily material, and is, from chemical analysis of the brain, shown to be so. Here, then, in the disintegration of a tissue, we have a good instance of the appearance of oil in quantity, which is not a new deposit in the part, nor the result of a change in albuminous matter, but only the original constituent liberated, in some measure, from the state of organic combination in which it normally exists. This, of course, is not fatty degeneration, but is worth a notice, as it bears a considerable resemblance to that state.

The *choroid plexus* in a middle-aged female, who died in a state of toxæmia from granular disease of the kidney, with a fatty liver, presented the following condition of its covering epithelium. The cells contained each a distinct drop of reddish, yellow, oily-looking matter, of larger size in some than in others. Many such drops were seen floating free, as well as some colourless ones. There were numerous whitish concretions of oval shape, situated beneath the epithelium, which appeared as vesicles with a distinct envelop enclosing oily contents. One such concretion, of the size of a small seed, consisted of perfectly normal fat-cells. The fatty condition, in this case, did not amount to actual degeneration; but it seems not improbable that it must have interfered, in some measure, with the function of the organ.

The milky-white colour of soft cataract suggests the presence of fatty matter in the *altered lens;* and this is confirmed by the microscope detecting, as Mr. Dalrymple states, many oil-globules, and sometimes plates of cholesterine among the *débris*. We found no trace of fatty degeneration in a hard cataract which we had the opportunity of examining, with our friend Mr. White Cooper, to whom we are indebted for the following account from the pen of M. Lebert:

> "In hard cataracts there is seen an opaque, granulous matter, interposed between the laminæ of the crystalline, which is not susceptible of becoming absorbed; and the laminæ themselves are, as it were, rendered horny and atrophied. In soft cataracts there is found an effusion of a lactescent liquid, and in this liquid, crystals of cholesterine; the lamellæ are softened and hypertrophied."

Though the exact nature of the change in *mollities ossium* is not yet known, we have sufficient evidence of the great increase of oil in the tissue to justify us in ranking it among instances of fatty change. Enlargement of the Haversian canals and of the lacunæ is observed, with diminution of the size of the bone, and absorption of its earthy constituent. At the same time, some important change occurs in the animal basis of the bone; it no longer yields healthy gelatine, and is saturated with oil. The quantity of fatty matter, in a specimen analyzed by Dr. Garrod, amounted to 20·35 parts per cent., the normal amount being generally about 2 or 3. Lehmann has found 29 to 34 per cent. of fat in bones thus affected; and

Von Bibra, 8 to 13 per cent. It is worth noticing, that the oil, in some at least of these cases, is not contained in cells, but lies as a free fluid in the cancelli and medullary canal, and readily drains out when the bone is placed in an inclined position. Its colour is also peculiar, presenting bright yellow, pink, and deep crimson hues. Rokitansky states that fatty degeneration of the muscles is often present when mollities ossium exist; but it is not certain whether the change in the muscles is dependent only on inaction, or whether it is the result of the same cause that produces the disease of the bone. Were it not for reasons to be mentioned, the conclusion would seem very probable, that mollities ossium, such as it is described by Mr. Paget in his lectures on Nutrition, is a true fatty degeneration, homologous to the like change in muscular fibre; while some kinds of osteoporosis, in which the texture of the bone is expanded and atrophied, while its enlarged cavities are filled with fat, are more analogous to wasting of muscles with fatty growth upon their surface. But the evidence afforded by Mr. Solly's and Mr. Dalrymple's observations, of the existence of a process of active change, shown by the striking afflux of blood to the parts affected, and the abundant cell-growth in their interstices, compel us to acknowledge that the two metamorphic processes, though both resulting in atrophy, are not quite identical.

It has not been customary to speak of fatty degeneration of *cartilage*, but Mr. Adams has lately directed especial attention to a change of this kind, in a communication to the Pathological Society.* He describes a first stage, in which the solid contents of the cartilage-cells have become converted into very minute spherules of oil, by which the cells are more or less completely filled.

In a more advanced stage, the spherules of oil before noticed, by their coalescence, have formed globules of various sizes, by which many of the cells appear distended and enlarged, and no trace of cell-membrane being distinguishable, the cells present the appearance of irregularly-shaped cavities excavated in the intercellular matrix, and filled with particles of oil. The intercellular matrix next undergoes the same morbid change as the contents of the cells and their cell-walls; this is indicated by the appearance of minute globules of oil, arranged either in irregular lines, and thus channelling the cartilage, or of globules congregated together in large cavities of various forms. When the disease is thus far advanced, and nearly all the cartilage is converted into oil, then those globules which were formed at the expense of the matrix cannot be distinguished from those produced by the degeneration of the cartilage-cells and their contents. When this condition of the articular cartilages is accompanied with disease of the synovial membrane, the part of the cartilage nearest to the membrane is much more affected than that next to the bone; so that a vertical section of cartilage affected with this disease exhibits the fatty degeneration in all its stages.

Rokitansky, describing the degeneration of cartilage which takes place when the tissue becomes ulcerated, or otherwise atrophied, speaks of the inter-cell substance changing into a kind of filamentous tissue, while the cells appear round, as if distended, and project from the fibred blastema; in many of them the nucleus is indistinct, and breaks into small rounded points, or degenerates into a fat globule; in the latter case there is a good

* See Report for 1848–49, p. 116.

deal of free fat in the blastema, and the cells are filled with small shining molecules of fat. When in this state, just as in atrophy, the cartilage is gradually worn down and lost. Rokitansky seems to consider the change in the cartilage as secondary to diseased states of the bone which it encrusts, or of the synovial membrane of the joint to which it belongs. Dr. Redfern, speaking of the removal of articular cartilage by atrophy or ulceration, says the essential parts of the process are the softening of the inter-cell substance, and the release of the cells, which, at the same time, are often disintegrated. He notices the presence of oil-globules as an occasional circumstance in the contents of the enlarged cells, but evidently does not regard the process as one of fatty degeneration. With this, our own observations on ulceration and wasting of cartilage quite coincide. We have noticed* the great enlargement of the cells, their filling with an endogenous growth of young cells, and the softening and transformation of the inter-cell substance into fibroid tissue; we have observed oil in small quantity among the contents of such cells, as also indeed in quite healthy ones, but we have never seen it in such quantity as to indicate that the degeneration of the tissue was a fatty transformation.† We do not in the least wish to question the accuracy of Mr. Adams's observations, but we are inclined to read the facts he has recorded rather as instances of the accidental presence of oil in unusual quantity in the degenerating tissue, than as examples of actual fatty degeneration. We feel sure that a "conversion of cartilage into oil" is not the ordinary way in which it is disintegrated; and we do not think, therefore, that a true fatty degeneration occurs in cartilage homologous to that taking place in muscle or bone.

The opaque arc of the *cornea*, which usually marks the advance of old age, has lately been discovered to be another instance of fatty change. Mr. Canton shows, that oil-drops and molecules are deposited in the corneal tissue, which becomes somewhat swollen and elevated above the level of the unaffected part. It is not unfrequently coincident with fatty degeneration of the heart, but is not any absolute indication of this condition being present. Mr. Canton, however, speaks confidently of the corneal degeneration, when well marked, being always accompanied by a similar change of the heart. The elevation of the tissue in the seat of the change makes it probable that there is rather a simple deposition of oil in the part, than a transformation or destruction of tissue. The anterior and posterior elastic laminæ do not become affected.

The atrophy of the *pulmonary tissue*, which takes place in emphysema, has been described by Mr. Rainey as depending upon fatty degeneration of the membranous wall of the air-cells; but the presence of oil in the wasting tissue is certainly not constant, and there is no sufficient reason for supposing that the atrophy depends upon such fatty transformation. Oily accumulation, to some extent, occurs very commonly in the apparently healthy lungs of dogs and cats; the molecules collect in the epithelial particles, which they convert into bodies indistinguishable from granule-cells. These, after a time, break up into shapeless patches of opaque oily matter. We have once seen a large group of true fat-cells either in or

* See Report of the Pathological Society for 1848–49, p. 104.

† Recent observation has shown us, that in the chronic disintegration termed "usune" of the cartilages, there is no fatty degeneration; the cells increase by multiplication, but contain little or no oil; the matrix breaks up into fibres.

just beneath the thickened and inflamed pleura of a man who died with diseased kidneys. The hint that we seem to get from the history of pathological conditions of the lungs is, that homogeneous membranes and fibres have little tendency to degenerate fattily, while the albuminous corpuscles within them readily do.

Glandular organs are among those most liable to fatty change. The *liver* has long been known as especially prone to this transformation. Cases are by no means infrequent in man, in which it is converted into a huge mass of oily matter, and such is actually its normal condition in many fishes. An excess of oil may be present in the liver, either as a simple *addition* to the normal structure, in which case "oily accumulation" only may be said to have occurred; or it may take the place of the natural granulous contents of the cells, which are almost entirely broken down and destroyed. In the one case, the cells remain perfect and undiminished in number, though they may contain more oil than usual, and a great part of this substance is free and diffused between them. In the other case, the individuality of the cells is lost; they are fused together, and their *débris* appear as mere fibro-granular detritus entangling huge oil-drops. In the liver of the cod there is absolutely no trace whatever of cells, except some groups of oil-drops aggregated together in a celloid shape. There are but few nuclei to be seen, and these look pale, and as if imperfectly formed. A very small quantity of fibroid tissue, with the vessels and ducts, is all that exists besides the enormous quantity of free oily matter and albuminous plasma. In fatty livers from the human subject, we think the free nuclei which are to produce new cells are also less perfectly formed than in the healthy state, and new cells do not seem to be produced, or but scantily. We believe it is a general fact that the natural secretion of the hepatic parenchyma is absent in thoroughly fatty livers, whether this condition be natural or morbid. We have failed to detect sugar in the fatty livers of fish, or in those of the human subject, and our friend, Mr. L. Blyth, has arrived at the same result. The oily matter shows a remarkable tendency in the liver to be deposited chiefly in the marginal cells of the lobules, which it often converts into a dull, whitish zone, forming the periphery of each lobule, and contrasting strongly with the central congested part. The deposit, however, may be central, occupying about one-third or one-half of the lobule around the intra-lobular vein. This, however, is the common seat of yellow pigment, which collects here in extraordinary quantities in cases of obstructive heart-disease occasioning great engorgement of the venous system. It is very remarkable how exactly co-terminous the sanguine congestion and the yellow-pigment deposit are, so that one can scarcely avoid the conclusion that the pigment results directly from alteration of the hæmatine, just as it does when blood is extravasated in other parts. Oily accumulation is chiefly observed in cases where much of this substance has been taken in the food, as in sheep fed on oil-cake: we have produced it ourselves, by feeding a kitten, for several days, on milk and butter, with a little bread, to prevent the bad effects of an uniform diet. The fatty condition of the liver in fish can scarcely, we think, be regarded as other than one of degeneration, for the tissue is remarkably altered from what analogy indicates to be its natural state, and which we actually find, in some instances, belonging to the same class. One principal cause of the liver being thus converted into oil must be the low type of the respiratory process, in consequence of which the blood cannot be so thoroughly decarbonized as in the air-breathing vertebrata. The very different condition of the cetacean's liver, in which the cells are quite distinct, and contain scarcely any oil, tends to support this view. At the same time this cannot be the whole truth, for there is oil enough in the blubber of a cetacean to make his liver fatty to the last degree, were it so disposed. We believe we must fall back on that real but incomprehensible influence of the organizing, assimilating power of tissues, which, being comparatively low in the fish's liver, allows it to degenerate into a mass of oil; but being more vigorous in the air-breather preserves the normal nutrition of the liver, and organizes the oil into a subcutaneous stratum of fat-cells. Our speculation on this subject suggests the question to which we may, on some future occasion, try to return an answer, how far the presence of an unusual quantity of oily matter in the blood tends, *per se*, to induce a true degeneration of any of the tissues or organs. The fatty condition is also developed, as Rokitansky states, and others have observed, in consequence of a luxurious and indolent mode of life, and especially as a result of dram-drinking. In such cases we are inclined to think that the oil is chiefly within the cells, but at the same time these are not so much broken-up as they are in states of more decided degeneration. We should consider that the liver of drunkards became loaded with oil, not solely from ingestion of much hydro-carbonaceous matter, but in great measure also from defective energy of the general assimilative and formative powers. Lastly, we have cases where, in consequence of emaciating and exhausting disease (not of phthisis alone), the liver is found in the extremest degree of fatty degeneration, the cells so broken-up that few retain anything of a distinct and separate form. Here, besides the presence of an excess of oil in the blood, there is beyond doubt a lowered state of the vital assimilative power of the liver; the nutrition of the organ fails in a greater degree than that of other parts. This opinion, grounded on the altered condition of the tissue, is confirmed by the absence of the glucose.

We have recently, through the kindness of Mr. Pollock, had the opportunity of examining an interesting specimen of local fatty change in the liver. The organ generally appeared healthy; its cells contained very little oil. At one part of its surface there was a raised nodular mass, of a dead whity colour, which was tolerably well defined from the surrounding healthy structure. It consisted of a dense fibrous structure, in which was imbedded a very large quantity of oily matter. The fibrous stratum was rendered translucent by acetic acid, and shown to be loaded with nuclei. The oily matter appeared in the form of streaks, patches, and celloid particles, and seemed pretty certainly to have resulted from the disintegration of some albuminous structure or substance imprisoned within the fibroid stratum. The condition might have resulted from a spontaneous cure of cancer by fibrous degeneration, or from a local cirrhotic process. In the one case, the cancer-cells; in the other, the hepatic, would have furnished the oily matter.

The hepatic ducts are not often, indeed scarcely ever that we have seen in man, seriously affected by fatty change. Sometimes there is a small quantity of oily molecular deposit between the nuclei of which their parietes chiefly consist, but not enough to entitle one to say that they were in a state of degeneration. Even this small amount of alteration is by no means associated with a fatty state of the parenchyma; the ultimate ducts of livers in the extremest degree of fatty degeneration commonly appear quite or nearly natural. In most, if not in all London dogs, however, the epithelium of the whole efferent apparatus of the liver is remarkably affected by a fatty change, which, however, does not appear to be at all degenerative, or to impair the exercise of its function. If thin sections of these livers be viewed under a moderate power, the ultimate and the larger ducts are seen very readily, as cylindrical, opaque tracts, running in the canals and fissures, skirting, for the most part, the margins of the lobules, and but seldom entering their substance, and only to a little depth. The interior of the gall-bladder and of the larger trunks of the ducts are of an opaque white. This opacity depends on the presence of a multitude of oily molecules or drops in the prismatic epithelial particles, replacing their usually granular contents. Similar molecules are deposited between the nuclei of the parietes of the ultimate ducts, and often seem to be especially clustered round them. The parenchyma of these livers is not in a fatty state, nor does the bile seem at all unhealthy, nor is there any indication of a scrofulous or other general morbid diathesis. This fatty state of the epithelium, especially of parts which have not, we believe, a merely mechanical office, but are actually concerned, in some considerable degree, in the formation of the bile, appears to us of considerable interest; it raises the question, how far the place of albuminous may be effectively supplied by oily matter.

A fatty state of the *kidney* is found in two conditions of the organ, either when it is diseased and degenerating, or when it is in a state of apparent health and certain efficiency. In all London cats,—and the same seems to be the case with German ones, according to Frerich's experience,—the epithelium of most of the cortical tubes is loaded in the most remarkable manner with oil. On making a section of these kidneys, the opaque dead-whity aspect of the cortical structure is very striking, and contrasts strongly with that of the tubular. The tubuli are easily seen to be quite sound, their basement-membrane is perfect, their Malpighian tufts are unobscured, and no other alteration can be detected, except that a prodigious quantity of oil has been mingled with their epithelium. This oil is, for the most part, in a free state, not contained in the interior of cell-particles; in this respect, however, it does not differ from the albuminous granular matter, which always forms, together with free nuclei, a considerable part of healthy renal epithelium. The above-described condition of kidney has no relation to a fat or lean condition of the body, nor is it at all connected with a fatty state of the liver; it presents itself simply as a constant occurrence in individuals who have been very differently circumstanced as regards food, &c. It is an instance of fatty accumulation, without any structural degeneration, similar to that just noticed in the hepatic ducts of dogs; and it seems, like that, to be of much interest, as showing that the function of a gland may continue unimpaired, although the granular matter which constitutes the chief bulk of the epithelium is, to a great extent, replaced by oil.

The fatty kidney of disease is, at least in the great majority of cases, only a variety of the degenerative process, which ends in the condition of wasting and granulation. The organ is large, of a whitish colour throughout, or presents opaque whity-yellow spots here and there. In the former state the oil is more generally diffused through the epithelium, in the latter it is more especially deposited in certain parts. The epithelium is variously altered, but always in such a way as marks its degenerating condition. It may be either bulky and coarse, or very much wasted. The tubes often contain fibrinous casts, or the remains of hæmorrhagic extravasations; their basement-membrane is often less distinct than natural, and they show a marked tendency to break down into shapeless detritus. The Malpighian tufts are many of them obscured or shrunk. In these, which are the essential circumstances, the fatty kidney does not differ from a non-fatty one, there are the most various grades of fattiness, so that it appears to us that the presence of oil to any amount in the degenerating tissue is a thing of no great moment, and certainly that Bright's disease is not fatty degeneration of the kidney. The two last instances of fatty kidney which we have seen, were associated with fatty change of the liver; but the two degenerations are not by any means constantly associated together. Whether there be a different kind of fatty degeneration of the kidney quite homologous to that of the liver we stand in some doubt.

Of the *testis*, Dr. Walshe testifies that it is liable to fatty destruction, the fat accumulating in the oil-globule from without and within the tubules. This we have never seen, nor is it mentioned by Rokitansky. Mr. Curling, however, mentions an instance in which one testicle, which was wasted to one-fifth its natural size, contained both adipose tissue beneath the serous investment, and a quantity of free oil-globules between the wasted tubuli. What was the cause of the atrophy does not appear. The epithelium of apparently healthy testicles contains a good deal of oily matter, which often accumulates in the cells to such an extent that they resemble, very closely, the glomeruli of inflammation.

The *gastric tubuli* which, in the greater part of their extent, have all the characters of truly glandular structure, are not unfrequently found remarkably opaque in their deeper third or half. This depends on the presence of multitudes of minute oily molecules in their granulous epithelium; it scarcely appears to be a morbid condition, as it may be seen in the stomachs both of men and animals, which have been clearly quite healthy and efficient. A condition, however, is sometimes met with, which, though apparently of the same kind, is evidently the result of disease. In the stomach of a female dying with pneumonia and an hydatid in the right lung, with kidneys in an early stage of degeneration, the mucous membrane was throughout of a reddish-brown colour, but did not appear to the eye otherwise unhealthy. On a vertical section being examined under the microscope, the tubes were seen quite opaque, filled in every part with an oily-looking mass, which consisted of solid fatty particles and granular matter. These solid fatty particles were much about the size of the normal nuclei, and were probably the same in a degenerate and altered state. In one part, where there were some traces of cells or nuclei, both had lost their natural aspect, and seemed more like masses of oily matter.

In a female, æt. 37, dying extremely fat with bronchitis and tubercular cavities in the lungs, the mucous membrane of the stomach was remarkably injected, in little patches of ramiform aspect. In some parts the tubes were opaque from the presence of oily contents, especially at their lower parts; in others they had almost disappeared, and there was only a mass of nuclear particles with scanty granulous matter forming an uniform layer in the fibrous tissue. In one part, which was apparently softened, and which was of a more uniform and darker reddish tint, the tubuli had perished very completely, their remains appeared as streaks of granulous matter containing oil-drops. We hope to prosecute the subject of degeneration of the gastric glandular structure by further research, but it appears to us at present that there may exist a fatty condition of the epithelium, analogous to that of the cat's kidney, which is consonant with health, as well as a degenerated fatty state, which, in its more advanced stages, issues in breaking up and atrophy of the mucous tissue.

The *pancreas* is known to undergo fatty degeneration, but this seems to be rather of the nature of fatty growth than a primary alteration of the glandular tissue. It is thus described by Rokitansky:

"The pancreas is liable to excessive accumulation of fat which may terminate in the conversion of the entire organ into one mass of fat. This affection rarely occurs without a coincident accumulation of fat in the abdomen. The disease proceeds from without inwards, and in very obese persons a direct communication may be traced between the surrounding fat and the pancreas; the cellular tissue gradually absorbing the lax greasy fat, the acini, which are of a dirty yellow colour, being reduced, and gradually disappearing. When the disease has attained its extreme limits, a mere pultaceous strip of fat, retaining the general outlines of the gland, is found in its place; only scattered remains of the acini are discoverable, and in the delicate and thinned duct there is a whey-like fatty fluid. The disease occurs frequently in drunkards, associated with fatty liver and the formation of biliary calculi."

The *supra-renal capsules*, very frequently in man, and less constantly in animals, present a highly fatty condition of their cortical substance. In the sheep this consists of well-formed granulous, celloid particles, arranged in rows, which contain no great amount of oily matter; but in several other of our common animals, and in our own species, these particles are so gorged with oily matter, that they consist of scarcely anything else, and one can scarcely regard them otherwise than as in a state of fatty degeneration. The medullary portion consists of well-shaped nuclei, lying in a granulous blastema; and the commencement of the cortical substance where cells begin to exist is also pretty free from the overwhelming fatty deposit. Our entire ignorance of the function and use of the gland prevents our forming any idea as to the cause and import of the fatty state described. It is comparable to the exceedingly common marginal fatty condition of the hepatic lobules, and like it affects the region where the blood passes first into the capillaries.

The *thymus gland* affords an instance of an organ which normally, at a certain period, loses its natural structure and degenerates into fat. This it does in most animals and in man once, at the same time progressively wasting and disappearing, but in some hybernators it seems to persist in the form of a mass of fat, which becomes greatly enlarged at the approach of the winter sleep. According to our observations the ordinary atrophy of the thymus takes place with accumulation and penetration of its interspaces by adipose tissue; it affords an excellent example of fatty encroachment upon a diminishing organ. In the thymus of a calf the whole surface was marked all over with interlacing white streaks, consisting of fat-cells, and enclosing the real thymic tissue. The ultimate cavities of the gland were perfectly defined by the rows and groups of fat-cells, and were filled with the usual nuclei, none of which were found developing fat-cells. In some specimens, however, there is a deposit of oily molecules and drops within the glandular cavities, and Mr. Simon has seen perfect fat-cells in great numbers in this situation. Atrophy, therefore, seems to take place in the thymus, both in the way of fatty growth and degeneration.

The *thyroid gland* rarely undergoes any change that can be termed fatty degeneration, but we have examined one specimen which presented many of the characters of this process. The gland was enlarged considerably, of a reddish, glossy aspect, and destitute of the natural acinous structure. The vesicles were for the most part destroyed, some traces only of their epithelium remaining, and some large cells or globules, composed of aggregated oil-molecules, being discernible in some few still existing cavities. The bloodvessels, which were prodigiously enlarged and irregularly dilated and varicose, were so encrusted with oily matter that they appeared, by direct light, as whitish cylinders. Some parts of the gland presented to the naked eye a whiter aspect than others, and in them it was evident that the deposit of oily matter along the vessels and the destruction of the glandular tissue had proceeded to the greatest extent.

The *uterus* has lately been described* to undergo fatty degeneration as a normal occurrence, after fulfilling its parturient function. The fibres of one, three weeks after the birth of a child, were so shrunken as to be only one-fourth the breadth of those of the fully developed organ; they were at the same time so soft that they broke up readily under dissection, and could not be isolated in their whole length. In most of the fibres there were minute molecules of oil, which in many formed a series all along the middle. The uterus to which the above description refers was in a state of inflammation, and we are much inclined to attribute the appearance of fatty degeneration of its texture to the deranged state of its nutrition, and not to any normally occurring process of inversion. In this view we are confirmed by having recently examined the uterus of a female who died with granular kidney some time after childbirth; the organ appeared quite in a healthy state, though larger than in the unimpregnated condition; its fibres were inextricably interwoven together, but neither in water nor in acetic acid did they present the least trace of oil. We think, however, that it is very possible that the muscular tissue of the uterus may undergo true fatty degeneration; and we quote the following description from Rokitansky, which, though it refers only to a state of diminished consistency without particularizing the cause, seems very much to indicate the occurrence of a change of this kind.

"Diminished consistency is not only consequent upon the exhaustion induced by parturition, or arising from paralysis of the uterine fibre in puerperal disease, but it also occurs in a distinct form, as pulpiness, slight friability, or fragility. It very frequently affects the decrepit uterus, involves chiefly the fundus, and appears

* See Report of the Pathological Society for 1851–52, p. 395.

generally to result from exhausting uterine discharges. The tissue of the affected uterus is of a pale or yellowish red, or sometimes ashy colour; it is torn by the slightest effort; its vessels are thickened, rigid, and sometimes ossified."

Perhaps fatty degeneration of the fibres may be the cause of some of those spontaneous ruptures which have occurred.

Of fatty degeneration of other involuntary muscles we have very little account. Rokitansky only says, speaking of the true degeneration of the heart's fibre, "We have occasionally observed this form in hypertrophied muscular membranes when the paralytic habitus is established, as, for instance, in the hypertrophied muscular coats of the intestine and the bladder." Mr. Hancock, in his excellent Lettsomian lectures, after remarking that he found the columns of the dilated and hypertrophied bladder to consist not of muscular but chiefly of elastic cellular tissue, observes, that there is only a certain degree of hypertrophy of muscular fibre consistent with the persistence of that fibre as muscular tissue, that beyond that point it appears to lose its original character, and to pass into a state of fatty degeneration. We have ourselves found the muscular coat of an old diseased bladder, long affected with paralysis, in a state of complete conversion into adipose tissue. In another, also the seat of chronic inflammation, there was no such change.

Previous to the researches of Professor Kilian and Dr. R. Barnes, pathologists were unacquainted with fatty degeneration of the *placenta*. Though the true nature and history of the changes observed have not yet been fully ascertained, it is very clear that an important step has been taken, and that further research in this direction will both contribute materially to the elucidation of fatty changes in general, and to our knowledge of the diseases of an organ with which we are too little acquainted. We subjoin an abstract of the accounts given by the above authors. Dr. Barnes mentions the uterine surface of the placenta to have been studded with fatty *masses*, of size varying, in the two cases, from that of a pea to that of a pigeon's egg. They were of dense structure and firm consistence. These masses extended inwards into the substance of the placenta, and the largest occupied the whole thickness, and was visible on the fœtal surface." (First case.) In the second case, it is said, "adipose matter appeared to be aggregated into firm, defined masses, isolated from the surrounding placental tissue." Again, speaking of masses on the fœtal surface: "the masses were of solid consistence, and appeared as *hard* bodies imbedded in the softer, spongy structure of the healthy placenta." We have lately, through the kindness of Mr. Druitt, had an opportunity of examining portions of placenta thus affected. We observed the same comparatively hard, whitish masses, well defined from the surrounding soft, spongy tissue, though continuous with it, and strongly suggesting the idea of there being some local deposit of firm consistence. Microscopic examination of these portions convinced us that the solid masses were produced by the effusion of a fibrous matter, which had coagulated, partly in the substance of the villi, partly around them, matting them together with other structures, and obliterating the bloodvessels. Numerous patches and streaks of oily molecules were seen, sometimes much resembling granule-cells. In some parts, the oily matter preponderated, infiltrating the villi throughout. It appeared to us, however, that it was not the primary deposit and the essential cause of the disease, but the result of a secondary change, set up in the disintegrating fibrinous effusion, or in the tissues which it had spoiled. The obliteration of the bloodvessels renders it still more probable that it was not, at least for the most part, a primary deposit. The resemblance between these masses and blocks of fibrine in the spleen appeared to us very close, both in their general aspect and structure, and their changes. We must not, however, any longer delay to quote the interesting and careful microscopy of Dr. Barnes' specimen, executed by Dr. Hassall. He says—

"The maternal structures presented to a great extent their normal characters; the walls of the vessels were nucleated, and the cells of the decidua were of the usual size. The important difference, however, was noticed, that both vessels and cells were studded over with numerous minute spherules, some of which appeared to be upon the surface, but others evidently were contained within the coats of the vessels, and in the cavities of the cells. The maternal portion of the placenta was therefore not free from disease. The fœtal portion of the placenta, as already noticed, consists of the umbilical vessels and chorion. On placing a small portion of one of the diseased lobes in water, the first thing which strikes the observer is, *that the tufts of villi do not expand or float out in the same way as in the healthy placenta;* and on endeavouring to separate the fragments into its component villi with needles, the extreme brittleness of the whole structure becomes apparent. Examined with the half-inch object-glass, the villi are observed to be much broken-up, and darker than usual, especially near their terminations, which reflect a yellowish colour. Viewed with a glass of 420 diam. lin., a variety of structural changes are detected. 1st. We observe that the villi are thickly studded with innumerable minute spherules of oil. 2nd. *The chorion is much altered; it is thickened, and destitute of nuclei.* 3rd. The walls of the vessels no longer contain nuclei; these having, in all probability, become degenerated into spherules of oil. 4th. The spherules of oil are contained, some in the chorion, some in the walls of the bloodvessels, and many in the intervals or spaces between these. 5th. The cavities of the vessels are almost invariably free from fatty deposition. 6th. The vessels are destitute of blood. Such was the usual condition of the several component structures; sometimes, however, in places, the disease appeared to have progressed still further, and to have produced almost complete disorganization and disintegration of tissue. Turning our attention to those lobes of the placenta, which to the eye present a normal appearance, we detect in them manifest evidences of the same destructive changes in progress; considerable fatty deposition is visible, and the nuclear structure of the parietes of the bloodvessels and of the chorion is to some extent implicated. In these portions, the distribution of the oil-molecules coincides exactly with the course of the blood in the vessels. This observation shows that the condition of the blood itself is intimately connected with the origin of the deposit."

We have italicised two passages in the above description, which seem to us to indicate the existence of another deposit beside that of oil. The strongest fact in favour of the oily deposit being primary, is the presence of similar changes in the adjacent apparently healthy portions of the placenta. According to Professor Kilian's description, it would appear that the oil-globules in his case were *inside* the vessels, filling their canals up at the terminal loops, and excluding the blood-corpuscles, while, in the earlier part of their course, the oil-globules were progressively replaced by blood-globules; the walls of the vessels also, which at their terminations had been loaded with fat, resuming their healthy appearance. We cannot help doubting the presence of oil *within* the looping placental capillaries; we rather suspect Professor Kilian's instrument did not define accurately enough to distinguish between vessels coated with oil and those filled by it. Some time ago we communicated to the Pathological Society the detail of an examination of a placenta, which, though it was not diseased in the same way as those now described, may help to throw some light upon the nature of the changes we are now considering. The fœtal surface presented several masses, which consisted of ramifying villous structure, loaded more or less with oily molecules; between these masses the placenta was much thinner, and the fœtal villi were found in this part either infiltrated with oily matter, or with mingled oily and calcareous, or simply pale and atrophied. No bloodvessels were visible in them; their surface was seen in some parts still covered by the remnants of the cell-growth of the decidua. The whole of the maternal portion was converted into a dense leathery or vellum-like layer, consisting of fibro-homogeneous substance, containing some elongated nuclei. Our interpretation of the appearances was, that fibrinous effusion or deposit had taken place in the maternal portion, completely obliterating its bloodvessels, and therewith abolishing the function of the organ; that, in consequence, atrophy had set-in in the fœtal portion which in some villi had produced simple wasting, without any oily deposit; in others, deposition of oil and calcareous matter. We regarded it as an instance, which showed that from a single cause the simple and fatty forms of atrophy might result. Now there appears to us some reason to think that the condition which was general in our case, was local in the cases of Dr. Barnes; that a quantity of fibrine having been effused or deposited in the placental tissue, produced the hard, circumscribed masses. The circulation in those parts of course was stopped, or at least materially impeded, and, in consequence, fatty degeneration commenced just as it would in a mass of fibrine in the spleen. Much of the oily matter, no doubt, proceeded from the disintegrating fibrine; but we think it probable that this in the substance of the villi may have been gradually deposited there by the failing blood-current. The case related in the note appended to Dr. Barnes's paper is evidently an instance of this kind, and is recognised as such by him.

We proceed to review some instances of fatty transformation presented by morbid products. Extravasated *blood* which is undergoing absorption, is described by Rokitansky as exhibiting, besides varying quantities of yellowish, red, or brown pigment, free fatty matters of different kind, in the form of small, dark, outlined, separate, or coalescing molecules, of great clear drops, of cholesterine crystals, together with the finest dust-like substance of albuminous, oily, or earthy nature. The chief part of the oily matter seen in the disintegrating clot must, doubtless, be derived from the dissolution of the combinations in which it naturally existed, from its 'freiwerden,' as the German well expresses it. A block of extravasated *fibrine* in the spleen, when undergoing absorption, consists of an amorphogranulous matter, imbedding oil-drops, large and small, with some slight traces of fibre, and in many cases, glomeruli of various size. Exudations of croupous fibrine (that less healthy kind which Mr. Paget names corpuscular, from its tendency to form pus-like corpuscles) are said by Rokitansky to leave behind, as they become absorbed, a residuum abounding in free oily and calcareous matter. Mr. Paget describes this change in fibrine at some length, as follows:

"Fibrine is subject to a degeneration which we may compare with fatty degeneration. In the coagula or solid parts of effusions that are found in the lower forms of inflammation, or in very unhealthy persons, the fibrine is usually not clear and uniform and filamentous, but rather opaque or turbid, nebulous or dotted, presenting just such an appearance as marks the earliest stages of fatty degeneration in the muscular fibrils. In such coagula as these, also, one sees not unfrequently minute, shining, black-edged particles, which we may know to be drops of oil, while some general alteration in the composition of the fibrine is shown by its not being made transparent with acetic acid. In all such cases as these, the fibrine is also very soft, and easily broken."

Our own observations have quite convinced us of the accuracy of the above descriptions. Two of them we subjoin here, as illustrating this fatty change in fibrine, although they refer to intra-vascular coagula; for we have the authority of both Rokitansky and Paget, that there is no essential difference between the fibrine of the blood and that of exudations; and, besides, it is of special interest to note the occurrence of similar changes in different situations. The coagulum in the heart of a man dead with toxæmic symptoms was almost black and soft, consisting of a delicate, scanty, fibrinous mass, entangling normal red globules. The fibrinous matter showed scarcely any trace of fibre, was homogeneous and punctiform, and contained oil-drops and vesicles much resembling fat-cells, as well as several spherical bodies much like glomeruli. The fatty condition in this specimen was very marked; it must have occurred, one would suppose, during the closing hours of life.

A youth died of empyema and pulmonary tubercle in the Brompton Hospital, where we witnessed the inspection, by the kindness of Dr. H. Roe and Dr. Cursham. For some time before his death both lower limbs had been very œdematous, one having been first affected, and afterwards the other. The veins in both, as well as the iliac and the vena cava inferior, for some distance from its commencement, were filled and plugged up by coagula of blood, which had evidently existed for some time. The coagula in different parts were more or less decolourized, and more or less adherent to the parietes; in the smaller veins they were quite solid, in the larger they presented various degrees of central softening and excavation, so that they formed sometimes a thick mould of decolourized fibrine on the walls of the vessels. The ragged, irregular, internal surface of this layer exhibited, under the microscope, an extraordinary number of corpuscles of different size and aspect, very feebly formed, and rather indistinct. Very many of the nature and appearance of glomeruli were mingled with them, and the whole were imbedded in a great abundance of punctiform and oily material. Some of the glomeruli were seen to be forming by the congregating of oil-drops upon a clear vesicle. A colourless coagulum in the right ventricle presented the characters of healthy fibrine—viz., an homogeneous basis substance, interwoven with a network of minute fibrils, and containing little punctiform matter, and few corpuscles. There can be no doubt that the softening and breaking-down of the coagula in the veins was a process of conversion of firm filamentous fibrine, like that in the heart, into a mass of granulous and oily matter, by the agency of the corpuscle-formation taking place in it. One of the products of this change was a large quantity of oil; but some doubt must exist whether it was produced merely by "freiwerden," or by an actual transformation of proteine substance.

In other inflammatory exudations besides the fibrinous, oily matter is often present in considerable quantity; we have seen it ourselves in portions of hepatized lung in the state of large free drops, as well as in the more common form of granule and molecule, either imbedded in greyish amorphous matter, or within cells developing into glomeruli. M. Nat. Guillot's observations are highly illustrative of this point. He has shown that while healthy lungs contain no more than 10 per cent. of fatty matter, hepatized or tuberculized may yield 50. In the brain, as we have seen, oily matter is found in the seat of inflammatory softening, both coating the vessels, and dispersed through the tissue in the form either of separate granules, or of glomerular aggregations. Inflamed and ulcerated mucous membranes show more or less of oily matter in the exudation which has infiltrated their texture. It is most probable that part of the oil in these cases simply separates from the albumino-fibrinous fluid, as it assumes a solid state, and does not result from a transformation of proteine matter. From the unusually large proportion of oil in many cerebral exudations, it would seem probable that the chemical constitution of the tissue has some influence on that of the exudation.

We may here particularly allude to the formation of the *glomerulus*, or *granule-cell*, as a very well-marked instance of fatty change. Reinhardt has shown, and we have repeatedly observed the same thing, that the presence of these particles in a tissue is no certain indication of its having been the seat of inflammation. They may be produced by an alteration in any cell having albuminous contents; thus, they are seen in non-inflamed lungs, in the tubuli seminiferi of the testis, in granular kidneys, and in ovarian corpora lutea. They may be formed, according to Reinhardt, from the epithelial particles of the pleura and peritoneum, the ciliated ones of the bronchi, and the columnar of the intestinal surface in slight diarrhœa. The corpuscles of the colostrum afford an instance of similar transformation in the epithelium of the mammary gland. Of course, where no cell-structure exists, as in the medullary substance of the brain, the presence of glomeruli would very strongly indicate the prior occurrence of some inflammatory process, which had caused the outpouring of some blastematous matter, in which they became developed. We have observed glomeruli in supplementary menstrual fluid from the breast. From repeated observations we feel convinced that the formation of the glomerulus does not take place according to one uniform plan. The oily molecules which characterize them may collect *around* a nucleus in a little amorphous matter; they may appear *in* a half-formed cell, or in a perfectly formed one, or collect *upon* either the incomplete or perfect one; lastly, they seem occasionally to aggregate together, as if by mutual attraction, and form a mass by themselves. In most instances, the enlargement of the cells, and their impletion with oily molecules, seem to proceed together, so that in the smaller cells there is usually a greater proportion of albuminous, in the larger of oily contents. This, as well as the circumstance figured by Vogel, of the oily granules collecting on the surface of the changing cell, seem to show that the transformation does not only affect the cell contents, but the exterior blastema also.

A case which we observed recently shows that formation of fatty granules in the interior of exudation-cells, may take place at an early period of the inflammatory process. A man died on the third day of an attack of peritonitis; there was some injection of the membrane, and much lymphy exudation. A thin section of the inflamed membrane showed a vast number of small granule-cells, each containing several oily molecules floating in a clear fluid, lodged in the interstices of the fibres. The separation of the oily matter from the albuminous must, in such cases, take place very quickly; and as the cells, beyond doubt, are actively growing at the same time, we can scarcely speak of their condition as one of fatty degeneration. The low type of the inflammation, and the hurried formation of the cell-growth, are probably causes of the early presence of oil within them, which would thus appear as an indication of imperfect organizing action. We do not think the formation of granule-cells belongs at all solely to retrograding exudations and decaying cell-growths; we are convinced that cells which are actively growing and enlarging, may fill themselves with fatty granules, and pass into the condition of glomeruli. This, doubtless, indicates "a degraded and bad" nutrition, but still it is a nutrition and not a decay. The white corpuscles of the blood occasionally present a number of opaque dots in their substance, which are probably of fatty nature; but we have never seen such an accumulation of them as to make the cell at all similar to a glomerulus.

Pus is subject to a kind of fatty degeneration; its corpuscles, according to Rokitansky and Paget, may change into granule-cells, and its serum also may give origin to much oily matter. In chronic abscesses there are found "mixtures of pus-cells, granule-cells, and molecular matter diffused in more or less liquid; or all the cells may be broken up, and their *dêbris* may be found mingled with minute oily particles, which appear, in all such cases, to be always increasing; or with these there may be abundant crystals of cholesterine, or such crystals may predominate over all other solid contents." In pus formed under an unhealthy condition of system, such as that in many cases of pyæmia, the fluid at the *outset* appears degenerate, and abundance of oily matter is seen, both diffused free in the fluid and in the substance of the corpuscles.

The cellular contents of *cysts* are liable to degenerate with a coincident development of oily matter. In a cystic sarcoma of the testis, which we recently examined, the contents of some of the cysts were firm, closely apposed layers of epithelial scales, with but little oily matter; others, which had softened down in the centre to a pulpy mass, consisted of a mixture of swollen epithelial scales, and multitudes of small cells containing minute oil-particles, and embedded in an abundance of granular and oily matters; in one, where the contents were very soft, the oil-drops in many cells were large and conspicuous.

Tubercle, even in the crude state, contains much fatty matter, and it is affirmed by several observers, that during softening this constituent increases, but we have been unable to find any evidence to show clearly that such has been the case. We may mention, by way of *cautela*, that in a specimen of soft tubercle becoming obsolete, and contained in a pseudo-cyst, we found the corpuscles and the granular stratum presenting very much the same peculiar opacity which oily matter does; and we were much inclined to think that a considerable quantity of this substance was actually present, especially as the mass did not feel gritty to the touch.

On adding some muriatic acid, however, the opacity quickly disappeared, with copious effervescence, and no oily matter was anywhere perceptible. This shows that not only calcareous granules, but calcareous amorphous matter, may simulate the aspect of oil.

Cancerous growths unquestionably undergo fatty transformation— saponification, as it has been termed by Rokitansky. In this instance the degenerative change is actually a beneficial one, at least for the parent system, nullifying the destructive vitality of the morbid organism. Rokitansky speaks of it as a process which commences in a solid inflammatory product, remaining in a crude state analogous to yellow tubercle, and causes its disintegration with a simultaneous conversion of its proteine elements into oil, afterwards extending to the cancerous cells themselves, changing them into glomeruli by filling them with fatty granules. He allows that this change may also occur spontaneously in the cancerous blastema; that is, we suppose, independently of inflammation. It takes place chiefly in the encephaloid variety, so rich in oil and albumen, a circumstance which indicates strongly the probability of the oily matter being merely set free by decomposition of its organic combination with albumen, and not produced by a metamorphosis of the latter. As, however, it also occurs in scirrhus, constituting the not uncommon c. reticulare, and in colloid, giving rise to the c. areolare pultaceum of Cruveilhier, it is probable that an actual conversion of other substance into oily matter occurs, and this would be especially true in the case of colloid.

Handfield Jones.

(*To be concluded in the next Number.*)

Review IV.

Traité de Chimie Anatomique et Physiologique, Normale et Pathologique, ou des principes immédiats normaux et morbides qui constituent le corps de l'Homme et des Mammifères. Par Charles Robin et F. Verdeil. Tomes III. Accompagné d'un Atlas.—*Paris,* 1853.

Treatise on Anatomical and Physiological Chemistry, or on the immediate normal and abnormal principles which compose the body of Man and of the Mammiferæ, &c.

This treatise, both from the numerous novel facts which it contains, and from the peculiar and original manner in which these facts are arranged and are brought to bear on each other, must be considered as one of the most remarkable works which have lately issued from the press. It is, perhaps, destined to exert an immense effect on medical science, and it must ever be looked upon as a most valuable repertorium of facts.

The opinions of MM. Robin and Verdeil are in many respects so different from those commonly entertained, that they demand a separate and complete exposition. This we trust soon to lay before the readers of this journal. At the present time, our purpose is to make use of some of the facts recorded in this great work, and to offer a succinct account of the constituent principles of healthy urine. We shall endeavour, in accordance with the title of this review, to trace the history of these principles from their entrance into or their formation in the system to their exit from it. As far as our limits will permit, we shall describe their physical and chemical characters, as well as the physiological functions they perform while still constituents of the animal economy. In this way we shall be able to point out the latest arrangements which chemists have made of the constituents of normal urine, and in a future number we shall follow up this sketch by an account of the pathological conditions of that fluid.*

Before enumerating the various constituents of healthy urine, we may remark, that the study of the chemical substances which constitute the organized system must be considered as a department of *anatomy*. The chemical constituents of the frame exist in as definite *compound* forms as the histological elements. If we wish to display the structure of a muscle, we separate its fibres, vessels, or nerves with the scalpel and forceps. When we investigate the composition of an animal fluid, we use, in the place of the scalpel and forceps, water, alcohol, and ether. The agents are different, but the principle of the method is the same. The substances obtained from the animal fluid by the water, alcohol, and ether, are as essential to its composition as the fibres, vessels, and nerves exposed by the scalpel are essential to the composition of a muscle.

According to Robin and Verdeil, the organic immediate principles are the final products, solid, fluid, or gaseous, resulting from an accurate anatomical analysis, without decomposition of the substances examined. The anatomical analyses of an animal fluid require three conditions:

1. They must be easily performed.
2. The principles must be obtained in the same state as that which they possessed when constituting component elements of the animal fluid under examination.
3. These elements must be extracted in a perfectly pure condition.

1. The manipulations necessary for the analysis of an animal fluid are most simple, and may be performed very easily at any time, the implements required being test-paper, a small water-bath, capsules, funnels, and filters; with water, alcohol, and ether. With the test-paper the primary reaction of the liquid is ascertained; the water-bath is required for the evaporation of the fluid which by such means will deposit its immediate principles at different stages of concentration, to be examined afterward under the microscope. With the alcohol and ether these principles are separated and obtained in their pure state.

2. It is of the greatest moment that the substances extracted should be obtained in the very state they had assumed as immediate principles of the fluid examined. The importance of this condition has unfortunately been hitherto overlooked; thus we are not yet satisfactorily acquainted with the various forms urea can assume in urine. This objection again holds good in the case of creatinine, which is obtained from concentrated urine by the addition of chloride of zinc. Reagents, however, are frequently very

* The writer takes this opportunity of expressing his thanks to MM. Robin and Verdeil for the assistance and valuable advice he received from them, while he was engaged in physiological and chemical pursuits in their laboratory.

useful to precipitate certain substances detected by the microscope as constituting an element of an immediate principle. For instance, the microscope will show the presence of urate of soda or ammonia in urine, from which the uric acid may subsequently be obtained by the addition of another acid. The immediate principle in this case is urate of ammonia, and not uric acid.

3. It is much to be regretted that volumes of elaborate researches have been written upon the constitution of animal fluids whose value is annulled in consequence of the organic constituents obtained from them having been described as alcoholic or aqueous extracts, which are nothing but a mixture of immediate principles. In regard to the tests for ascertaining whether an immediate principle is quite pure, the crystalline form which the greatest number of them assume, and the mere inspection of its crystals by the naked eye or with the microscope, will be a sufficient indication. The uncrystallizable principles, such as the albuminous compounds, may be purified by repeated washing with water, alcohol, and ether. Finally, chemistry proves very useful as giving methods of analysis to be employed in testing the purity of the immediate principles.

These few preliminary observations will show the importance of studying not only the crystalline forms of the animal immediate principles which are so accurately illustrated in Robin and Verdeil's Atlas, but also the methods used for their extraction, and for ascertaining their chemical properties. Should this study be neglected, the investigator will meet at every step with such difficulties as will prove irremovable obstacles to the prosecution of his researches.

The immediate principles constituting healthy human urine may be considered under two heads:

I. The Inorganic Principles.
II. The Organic Principles.

I. *Of the Inorganic Immediate Principles of Healthy Urine.*

The following are these principles and the order by which they will be described:

1. Water.
2. Chloride of sodium.
3. Chloride of potassium. (?)
4. Chloride of ammonium.
5. Carbonate of lime.
6. Sulphate of lime.
7. Sulphate of potash.
8. Basic phosphate of lime.
9. Acid phosphate of lime.
10. Ammoniaco-magnesian phosphate.
11. Neutral phosphate of soda.
12. Acid phosphate of soda.
13. Phosphate of potash.

1. *Water.*—Water is taken into the body with the food and through the cutaneous system. The amount of this fluid in the economy is equal

to a little more than two-thirds of its own weight. Urine, saliva, perspiration, tears, and pulmonary vapours, contain the greatest proportion of this principle. Its quantity in the urine appears to vary according to a number of circumstances, as the nature of the ingesta, the temperament, the climate and season, the exercise, &c., and ranges from 930 parts per 1000 upwards.

The use of water in the animal frame is evidently that of dissolving a certain amount of the crystallizable and uncrystallizable principles, so that they may be brought, by imbibition or endosmose, into close contact with the different solid elements of the various tissues. By this process a reaction is induced of the fluid on the solid, and *vice versa*, upon which depend the phenomena of nutrition. Water has also the property of moistening tissues for the penetration of gases, of giving elasticity to certain semi-fluid parts, flexibility to cartilage, and tenacity to bone.

The urinary secretion eliminates this principle from the system, where it serves for the removal of a number of other elements which have become useless or injurious to the economy. It appears, however, singular, that certain principles when obtained from urine in the pure state become insoluble in water. A similar phenomenon can be also observed in the blood, which has been shown by Lecanu to contain in solution free fatty acids. These results we have lately confirmed in Robin and Verdeil's laboratory, by following a new method of investigation, and one less liable to error.*

In order to obtain the amount of water contained in urine or any animal fluid, a known quantity of it is evaporated to dryness on the water-bath at a low temperature, and the residue dried over sulphuric acid under the air-pump until its weight no longer varies. It sometimes requires many days to ascertain with great accuracy the weight of the water contained in an animal solution. By taking the specific gravity of the liquid an approximate notion of its proportion of solid constituents may be arrived at; but this method should not be employed when accurate results are required.

The solid inorganic principles of urine are all crystallizable from their solution in water. When heated upon a platina spatula no charring occurs. They do not in general combine with an organic principle; except, however, in the case of urea, which probably unites with chloride of sodium.

2. *Chloride of Sodium.*—Chloride of sodium, or common salt, forms a large proportion of the solid constituents of human urine. It is found also in blood, chyle, lymph, milk, saliva, gastric juice, bile, perspiration, and muscular tissue. After its absorption from the stomach by the veins, it at once plays an important part in the functions of the blood. As a constituent of muscle, it exists but in very small proportion; and from Braconnot's observations, it appears that none can be detected in oxen's heart. The amount of chloride of sodium found in the blood is not liable to variation, but the urine contains more or less of it, according to the nature of the ingesta, its average proportion being 4·45 per 1000. (Berzelius.)

This immediate principle is indispensable to the phenomena of assimilation or formation of organized substance. It acts in the blood as one of

* Comptes Rendus et Mém. de la Soc. de Biologie. Paris, 1851.

the dissolving agents of albumen, and perhaps of the fatty elements. Its principal function appears to be that of promoting the phenomena of endosmose and exosmose of the aqueous solutions, which find their way from the intestines into the blood. Salt is a particularly important ingredient of the food of cattle. According to Boussingault's experiments, it does not increase the amount of the muscular tissue or of the milk, but it seems to act favourably on the quality and general appearance of the animals, so that when fed with salt they will fetch a higher price in the market. Salt, therefore, does not produce flesh, but neutralizes the conditions which are unfavourable for its development, and resulting from external physical causes. The addition of salt to food excites the secretion of saliva, and facilitates digestion; vegetables containing a comparatively small amount of chloride of sodium are for that very reason more difficult to digest; and herbivorous animals are naturally induced to lick the saltpetre from walls, and to eat a much larger quantity of salt than carnivorous. Finally, chloride of sodium dissolves nitrogenized substances out of the body; and when common salt is added in excess to blood, it diminishes considerably the rate at which the fibrine coagulates. It appears also indispensable for the preservation of the physical character of the blood-globules.

Chloride of sodium is eliminated from the body by the urine when it exists in the free state, and probably in combination with urea.

Extraction.—If urine be evaporated at a very gentle heat on the water-bath, or in a hot stove, the chloride of sodium will crystallize. It is easily recognised under the microscope by the cubic form of its crystals, which resemble those of creatine; they are, however, readily distinguished by the polariscope. Common salt does not transmit polarized light, whilst creatine polarizes readily. When mixed with other animal principles, common salt becomes soluble in alcohol, and may be obtained in a crystallized state from the evaporation of its alcoholic solution. In this case, the crystals generally assume the form of octohedra with well-defined angles. If ether be added to the alcoholic solution, the crystals will assume other forms, still deriving from the octohedron. They are often found grouped in various ways, and intermingled with long prisms, giving rise to a peculiar arborescent structure. Other circumstances besides the nature of the extract modify the crystals of common salt obtained from urine, and especially the temperature at which the concentration of the solution has taken place. The extracts containing chloride of sodium are very deliquescent, and the crystals of common salt rapidly disappear.

The amount of chloride of sodium contained in the various animal fluids may be ascertained by the addition of nitrate of silver, which precipitates the insoluble chloride of silver. This method, however, is not satisfactory, for by its means we can only ascertain the total amount of chlorides present in the fluid examined. Lecanu has observed that the urine of a child nine years old voided in twenty-four hours, contained from 15 to 30 grains of common salt; the urine of an old man, from 20 to 60 grains; and the urine of a woman, from 30 to 60 grains.

3. *Chloride of Potassium.* (?)—This salt has not yet been detected in human urine, where, however, it probably exists. Margraff found it to be a constituent of cow's urine, and Vauquelin, of the urine of the horse,

rabbit, and guinea-pig. Chloride of potassium has been extracted from milk, muscular tissue, liver, cephalo-rachidian fluid, blood, nasal mucus, saliva, bile, and gastric juice.

When examined under the microscope, this substance is very similar to chloride of sodium, but the precipitate occurring upon the addition of chloride of platinum, will suffice to show the presence of the potassium salt.

3. *Chloride of Ammonium.*—This salt is found as one of the immediate principles of the gastric juice of ruminating animals, of saliva, tears, and urine. According to Simon, urine contains 0·41 parts in 1000, and to Berzelius, 1·50. Chloride of ammonium exists, as a constituent of the economy, in the dissolved state.

It has not been ascertained whether this salt is introduced into the animal system with the food, or whether it is formed in the body. The urine eliminates chloride of ammonium from the organism.

Extraction.—The amount of muriate of ammonia contained in an animal fluid, is ascertained by treating that salt with soda or baryta. The ammonia which is evolved is collected in hydrochloric acid, and this chloride of ammonium is precipitated by chloride of platinum. The double chloride of ammonium and platinum thus obtained is weighed. This method, however, cannot be applied to human urine, which contains ammoniaco-magnesian phosphates and urate of ammonia.

The presence of chloride of ammonium can be detected under the microscope, by the appearance of its crystals, which have a peculiar arborescent structure. It sometimes happens that chloride of sodium assumes a crystalline form not very unlike that of chloride of ammonium; but the common salt crystals are always much better defined than those of the former, and can be easily distinguished by a little practice.

4. *Carbonate of Lime.*—This immediate principle, though detected but occasionally, and in small quantity, in human urine, when it is alkaline, is important, from its being a constituent of bone.

Carbonate of lime was detected by Cl. Bernard in that portion of the saliva secreted by the parotid gland. This distinguished physiologist, having obtained, separately, the saliva secreted by the parotid, the submaxillary, and the sublingual glands of a dog, observed, in the first, a short time after its extraction, a crystalline deposit, which was found to be carbonate of lime. Those crystals are regular rhombohedra, slightly flattened and grouped in masses radiating round a central point. Otolithes are mainly composed of crystallized carbonate of lime.

The carbonate of lime contained in the animal system is derived from two sources. A small quantity is taken in with the fluid ingesta, as a slight excess of carbonic acid will effect its solution in water. Another portion is derived from the organic salts of lime, as the tartrates and malates taken in as food, and eliminated through the agency of the urine, in the form of carbonate of lime, after having lost their hydrogen and a part of their carbon.

5. *Sulphate of Soda.*—Urine contains a comparatively large proportion of sulphate of soda. It is also found in the ashes of blood, where its presence, however, might result from an oxidation of the sulphur yielded by the blood's albumen. Nearly every part of the animal system contains sulphate of soda; but it is completely absent in milk, bile, and gastric juice.

This salt is constantly in the dissolved state as an animal immediate principle. In the blood, its function is probably that of dissolving the fibrine, and of preserving the elasticity of the globules.

Extraction.—The sulphate of soda has never been extracted directly by crystallization from animal tissues or fluids. It is obtained by calcination, baryta being added to the dissolved ashes, the precipitate washed with hydrochloric acid, dried and weighed. The microscope has not yet detected its presence as an immediate principle, but it is often seen when sulphate of soda is formed artificially, if sulphuric acid be used for the extraction of any other principle.

6. *Sulphate of Potash.*—Prout, and afterwards Thénard, showed the presence of sulphate of potash in urine, which, according to Berzelius, contains 3·71 per 1000, and to Simon, 3, or 3·500 per 1000 of that salt. It is also a principle of the blood, in which A. Marsh found its amount to be 9·35 parts per 1000. Its organic properties and method of extraction are the same as in the case of sulphate of soda.

7. *Subphosphate of Lime, or Basic Phosphate of Lime.*—Phosphate of lime has been shown to exist in urine by Gmelin, Fourcroy, and John. Fleitman has found in the ashes of urine 25·7 per 1000 of this salt. It assumes the solid state in bones, teeth, nails, and hairs. Though insoluble in water, it is found dissolved in the blood by the presence of the carbonic acid and bicarbonates; in urine, by the phosphate of soda. The fæcal matters appear to contain a large amount of phosphate of lime, but it probably results, in this case, from that part of the ingesta which has not been absorbed. This salt also occurs in milk, saliva, and in the albumen of the serum of oxen's blood. It is an important constituent of bone, where it is united with a peculiar organic substance, called by Robin and Verdeil, *ossein.* The albumen of blood is found to contain 1·80 per 1000 of that salt, and the fibrine of venous blood 0·69 per 1000. Casein, when obtained coagulated, cannot be separated from a small quantity of earthy salts.

Extraction.—To extract phosphate of lime from bones, they are treated with weak hydrochloric acid, and the solution being neutralized by ammonia, the phosphate will precipitate. It is obtained from residues after the evaporation of animal fluids, or from tissues by calcination. The ashes are treated with water, and the insoluble part is dissolved in hydrochloric acid; upon the addition of ammonia to this solution a precipitate containing phosphate of lime will occur. This precipitate is redissolved in an acid, and the solution mixed with ammonia and a salt of magnesia; this will yield the ammoniaco-magnesian phosphate, which is easily detected by its solubility in acetic acid and its crystalline form.

8. *Biphosphate of Lime, or Acid Phosphate of Lime.*—This principle has been found by Scheele in human urine, and in dog's urine by Robin and Verdeil. It probably results from the action of uric acid, or of any other free acid in the urine, upon the neutral phosphate. Fourcroy and Vauquelin have shown long ago the conversion of the neutral phosphate of lime of bones into acid phosphates, when acted upon by weak acids.

Extraction.—When urine is evaporated, an amorphous acid phosphate of lime may be observed floating on the liquid, mixed with small masses of urate of soda. A second crystallization will show these crystals to be elongated hemi-octahedra. When extracted from dog's urine, this salt sometimes assumes the form of isolated plates.

9. *Phosphate of Magnesia.*—Fourcroy and Vauquelin, and after them Wollaston, showed that urine contained phosphate of magnesia. This salt also exists as a constituent of muscular tissue, where, according to Liebig, it is more abundant than phosphate of lime. It is an element of blood, bone, milk, and nail. Berzelius found excrements to contain 12·9 per 1000 of phosphate of magnesia, after a hard diet; and Fleitman, 10·67 per 1000, after a mixed diet.

This principle generally occurs in the dissolved state, but it assumes occasionally the crystalline form, especially in pathological products. Phosphate of magnesia enters the system, probably, with the vegetable food, for it seldom occurs in the mineral kingdom. When in excess in the body it is expelled by the urine. We can infer, from the large proportion of this salt extracted from excrements, that the system derives but a small amount of it from the ingesta; but the excrements also eliminate from the body a considerable proportion of phosphate of magnesia, derived from the pancreatic juice, and from the other intestinal secretions.

Extraction.—This immediate principle is to be extracted from the ashes of a calcined tissue, or fluid residue. They are dissolved in weak hydrochloric acid, and then treated with ammonia and chloride of ammonium, when the ammoniaco-magnesian phosphate will precipitate. Phosphate of magnesia may be obtained crystallized from the urine of certain herbivorous animals, merely by evaporation. The crystals are oblique prisms, with rhombohedral bases, or large plates occasionally aggregating so as to form a stellate mass. Robin and Verdeil have observed similar crystals in human urine. They are insoluble in cold water, but easily dissolved by weak acids.

10. *Ammoniaco-Magnesian Phosphate* (*triple Phosphate*), *or Phosphate of Ammonia and Magnesia.*—Berzelius detected this immediate principle as a constituent of urine. Donné showed its presence in healthy urine after a vegetable diet, or after the taking of alkaline carbonates. This salt is generally found in fluid undergoing decomposition, when the ammonia evolved has combined with phosphate of magnesia. It sometimes occurs floating upon urine as large brilliant crystals, twenty-four hours after micturition. The deposits observed in alkaline urine often consist of ammoniaco-magnesian phosphates.

This principle does not exist in the mineral kingdom; its presence in the body must therefore depend upon the chemical phenomena going on in the circulation, and probably results from a combination of ammonia with phosphate of magnesia.

Extraction.—The addition of ammonia to a fluid containing this salt will cause it to precipitate. The precipitate, when viewed under the microscope, may be seen to consist of confused stellate groups, with dentate margins, or of small, shining, needle-like prisms; both varieties developing beautiful colours when examined with polarized light.

11. *Neutral Phosphate of Soda.*—Neutral phosphate of soda can be detected in every solid and fluid constituent of the animal system. Robin and Verdeil found human urine to contain both neutral and acid phosphate of soda. Its amount in the urine of herbivorous animals is much less con-

siderable than in that of man, or of carnivorous animals. The former, on the other hand, contains a larger proportion of carbonate of lime. This phenomenon can be easily explained, if it be considered that the organic acids, as tartaric, malic, or oxalic acid, are very liable to yield carbonates; whilst flesh, which contains a great deal of phosphate of soda, will increase the amount of the phosphates of the urine. Its presence in urine may also result from a double decomposition occurring between the chloride of sodium and phosphate of potash in the circulation. According to Berzelius, urine contains 2·94 per 1000 of this salt.

Phosphate of soda is always found dissolved in the economy; it probably acts as a dissolving agent for the insoluble phosphates, and the other sparingly soluble immediate principles. From its possessing the same alkaline reaction as carbonate of soda, and the same property of fixing carbonic acid, it may be considered as acting the same part in the blood as the carbonates, so that, according to circumstances, one may be substituted for the other. This fact enables Liebig to explain why the use of a vegetable or an animal diet has no ultimate influence on the animal functions. Verdeil has analyzed the ashes of oxen's blood, and those of the blood of a dog fed with meat. The former contained a very small amount of phosphoric acid, combined with soda or potash (3 per 1000); compared to that, he detected in the latter 12 per 1000. The blood of the same dog, after a potato diet, yielded no more than 9 per 1000 of phosphoric acid.

Extraction.—No satisfactory process has yet been discovered for extracting phosphate of soda from the organism, as by incineration this salt may be modified. Moreover by burning albuminous substances, some inorganic products are obtained, which those substances cannot be made to yield even to protracted washing: the ashes of albuminous substances will therefore always be found to contain phosphates.

There is also another obstacle to the study of the alkaline phosphates, resulting from the different degrees of saturation phosphoric acid is liable to assume, according to circumstances. To examine the alkaline and neutral phosphates contained in a fluid we must, therefore, obtain this salt in the crystalline form; and for that purpose, it is necessary to remove first the albumen and fatty matters, which generally interfere with crystallizations; after this operation, the phosphates may be separated and made to crystallize. If alcohol be added to concentrated urine decanted from the first deposit of crystals, phosphate of soda will gradually crystallize on the sides of the vessel. These crystals assume the form of tables derived from the rectangular, or right rhombohedral prism; they are sometimes irregular and striated, and transmit polarized light with the occurrence of beautiful colours. The crystals of neutral phosphate of soda occasionally take the shape of an octahedron.

12. *Acid Phosphate of Soda.*—This immediate principle has only been detected in urine, and is probably the cause of its acid reaction. The variety of reactions urine is capable of assuming is quite consistent with the presence of unstable phosphates in that secretion. The importance of the instability of this principle in urine will be at once understood, if it be considered that blood must always possess the same alkaline reaction; so that if, during digestion, acids are obtained from this fluid, a propor-

tional amount of alkali is also to be excreted, and that alkali the phosphate will yield to urine. This, moreover, agrees perfectly with Dr. Bence Jones's interesting and important researches on the various reactions urine is susceptible of assuming at different periods during the day. He found it to be most alkaline during digestion, when the blood yielded the greatest amount of acid secretions, and most acid just before meals, when an excess of acid existing in the blood had to be given off.

Extraction.—Acid phosphate of soda can be obtained crystallized in urine, by the same method as that recommended for the extraction of the neutral phosphate. It crystallizes three or four days after the latter, and is more soluble in water. The crystals derive from the rectangular or right rhombohedral prism. From their being very transparent, they are better observed under the microscope when made to rotate in a small current of water.

13. *Phosphate of Potash* (?)—Though phosphate of potash has not yet been detected in urine, it probably exists there in small quantities. This salt may be considered as one of the most important principles of muscles, its amount in that tissue greatly exceeding that contained in the blood. From this circumstance, Liebig considers phosphate of potash as being necessary to the plastic conditions of the blood.

II. *Of the Organic Immediate Principles which constitute Human Urine.*

The following are the organic immediate principles which can be obtained from human urine:

1. Oxalate of lime.
2. Uric acid. (?)
3. Neutral urate of soda.
4. Acid urate of soda.
5. Urate of ammonia.
6. $\left\{\begin{array}{l}\text{Urate of lime.}\\ \text{Urate of magnesia.}\end{array}\right\}$ (?)
7. Hippuric acid.
8. Hippurate of soda. (?)
9. Two new acids.
10. Creatine.
11. Creatinine.
12. Urea.
13. Double chloride of sodium and urea.
14. Colouring matter.

The organic constituents of urine are all crystallizable, with the exception, however, of the colouring matter, and may be readily extracted from animal fluids, and detected with the microscope, to be finally tested by chemical analysis and reagents. They probably result, partly from a peculiar metamorphosis, whether it be oxidation or splitting, which tissues undergo previous to their elimination from the system; and partly from an excess of food which is not assimilated, acting as an indispensable agent for the functions of secretion and respiration.

1. *Oxalate of Lime.*—This principle was first detected in the body as a

constituent of renal calculi, by Bergman, who merely mentions the existence of oxalic acid, omitting the base. Some time afterwards, Brugnatelli discovered the presence of oxalate of lime in urinary deposits. This salt is very frequently found in the urine of herbivorous animals, and often occurs in healthy human urine, especially if the person has partaken of sparkling wines, and beer containing a great deal of carbonic acid. The amount of oxalate of lime in urine is also increased, after the use of alkaline salts or alkaline bicarbonates.

This principle is occasionally found crystallized in certain tissues. Its crystals are octahedra derived from the cube. They do not transmit polarized light, and might therefore be taken for common salt, were it not for their complete insolubility in water. When viewed under the microscope, they assume a peculiar appearance, from their superior and inferior angles being seen at the same time, in consequence of the great transparency of the crystal.

Oxalate of lime is not a constant constituent of the economy. Donné observed, that a few spoonfuls of oxalis acetosella sufficed to cause a deposit of oxalates in the urine in less than two or three hours. They disappear shortly afterwards. Other substances taken into the stomach may generate oxalate of lime. Wöhler and Frerichs have detected large quantities of this salt in the urine of a dog, after injecting into a vein essence of bitter almonds, free from hydrocyanic acid. The urine in this case was found to contain, instead of urates, crystals of oxalate of lime, with a trace of urate of ammonia. It has been detected by Garrod in the blood.

It is very probable that oxalate of lime is introduced into the system with vegetable food, but it can also be generated in the body; so that we have no precise knowledge as to its real source.

Extraction.—If urine be suspected to contain oxalate of lime, the specimen examined is to be left undisturbed in a glass for some hours, the deposit is then subjected to the microscope; if no apparent precipitate has occurred, a drop of the liquid which occupied the bottom of the glass is examined with the microscope. The crystals are sometimes very small; it will be advisable in that case to use a magnifying power of about 500 diameters. According to G. Bird, oxalate of lime sometimes assumes the shape of dumb-bell crystals; this opinion, however, he has modified of late, and he now considers those crystals as oxalurate of lime.

2. *Uric Acid.*—It is still doubtful whether uric acid in the pure state has been detected in healthy urine, or in any other part of the economy. It occurs combined with soda and ammonia as important constituents of urine.

3. *Urate of Soda.*—This salt is found in the urine of man and of herbivorous animals when starving; in the latter it disappears as soon as this secretion becomes alkaline. M. Regnault showed its existence in the liquor amnii; it has also been detected in blood, but only as one of its pathological constituents.

When in the pure state urate of soda crystallizes in tuberculated clusters. Its amount in human urine has never been ascertained. The presence of uric acid in urine is closely connected with the ingesta, for Dr. Bence Jones found its amount in that secretion greatly increased during the period of digestion. Uric acid, however, is generally considered as resulting from the combustion of nitrogenized substances, the degree of oxidation being inferior to that which causes the formation of urea. In corroboration of this theory it is alleged that reptiles which produce a less degree of animal heat in consequence of deficient oxidation, emit uric acid instead of urea. This hypothesis cannot be maintained, if it be considered that the urine of herbivora does not contain any urate or uric acid except when the animals have been deprived of food for a considerable length of time; the amount of food they take, however, contains about the same proportion of nitrogenized substances as that of carnivora. It must now, therefore, be admitted, with Messrs. Robin and Verdeil, that the oxidation theory is not altogether without objections. The quantity of urate of soda contained in urine can only be ascertained by examining separately, with the usual chemical means, the uric acid and the soda.

4. *Acid Urate of Soda.*—This salt is constantly present in human urine. Landern has also found it in perspiration. Acid urate of soda is one of the first crystalline deposits which occur in concentrated urine; it is then mixed with phosphate of lime and the urates of soda, lime, and ammonia. This principle assumes the shape of spherical masses resulting from the aggregation of fine needles; they assume an opaque dark colour, slightly yellow. The needles are not always distinct; they appear to shoot from the centre to the periphery. When treated with acetic acid the groups of aggregate crystals first dissolve and then gradually assume the acicular appearance of uric acid.

5. *Urate of Ammonia.*—Urate of ammonia is one of the constituents of normal urine. Its amount in that secretion has never been ascertained.

No method has been suggested to separate this salt from the other substances occurring in the deposit from concentrated urine. When obtained from calculi dissolved in hot water, this substance crystallizes on cooling, and assumes the form of groups composed of very long and fine needles. These small masses are colourless, but become opaque when they acquire a certain degree of thickness; some groups have a fan-like structure, the needles radiating from a centre, others are fasciculated. Finally, spherical groups of these crystals are sometimes observed to have a dark centre, with a great number of extremely fine needles at the periphery.

6. *Urate of Lime and Urate of Magnesia.* (?)—These two salts have been found as pathological constituents of urine. They probably exist also as normal principles.

7. *Hippuric Acid.*—Robin and Verdeil observed a deposit of crystals of hippuric acid in the urine of a healthy man aged 30, who, being in business, was prevented from taking exercise. When he drank stimulants, as wine, coffee, or spirits, the urine passed had a strong acid reaction, and deposited crystals of hippuric acid. Humfeld detected the presence of this principle, or perhaps, of hippurates in the urine of a child nine months old. It has also been detected in many other cases.

Extraction.—Whenever hippuric acid exists in the free state as a constituent of an animal fluid, it is to be dissolved by ether; the solution evaporated in a low temperature will yield that substance in two crystallized forms. Under the microscope the crystals appear as small prismatic needles, sometimes grouped in spherical masses, occasionally superposed, and often isolated; they polarize readily. When obtained in sufficient quantity, this substance may be recognised at once by its chemical properties, and especially by the peculiar aromatic smell it evolves when burnt on a platina foil. It is very sparingly soluble in cold water, but dissolves easily in boiling water, alcohol, and ether.

8. *Hippurate of Soda.*—This salt has not been clearly demonstrated to exist in human urine, but it most likely occurs as one of its constituents, for hippuric acid is never found in the free state in that secretion, and Verdeil and Dollfus have extracted hippuric acid from the blood where it is combined with an alkali.

Extraction.—Hippurates have not yet been separated in the crystalline form; but the following is the method given by Verdeil and Dollfus to extract hippuric acid from the blood.

After the removal of its fibrine, the blood is mixed with an equal volume of water, and the albumen coagulated by heat. The fluid strained through calico is concentrated to a syrupy consistence, and then mixed with alcohol; an abundant precipitate then occurs, and the filtered liquid is distilled until the whole of the alcohol has been removed. To this fluid residue a little sulphuric acid is added, which precipitates the fatty matters, they are likewise separated by filtration. The filtrate is then neutralized with carbonate of lime, evaporated to dryness, and the residue treated with boiling alcohol; the hippurate of lime is dissolved and yielded by the alcoholic solution on cooling or by evaporation. A single emission of blood from the patient's arm will suffice to show the presence of hippuric acid under the microscope.

Hippuric acid may be obtained in large quantity from horse's urine when it exists as hippurate of lime. For that purpose it is merely necessary to decompose the concentrated secretion by hydrochloric acid. In any fluid where its amount is but small, it will be advisable to treat it first with lime, and then add hydrochloric acid to the concentrated fluid, allowed previously to cool; this mixture is shaken in a flask with ether and the supernatant ethereal solution decanted. This solution is mixed with a little lime and distilled, when, after due concentration, the hippurate of lime will crystallize.

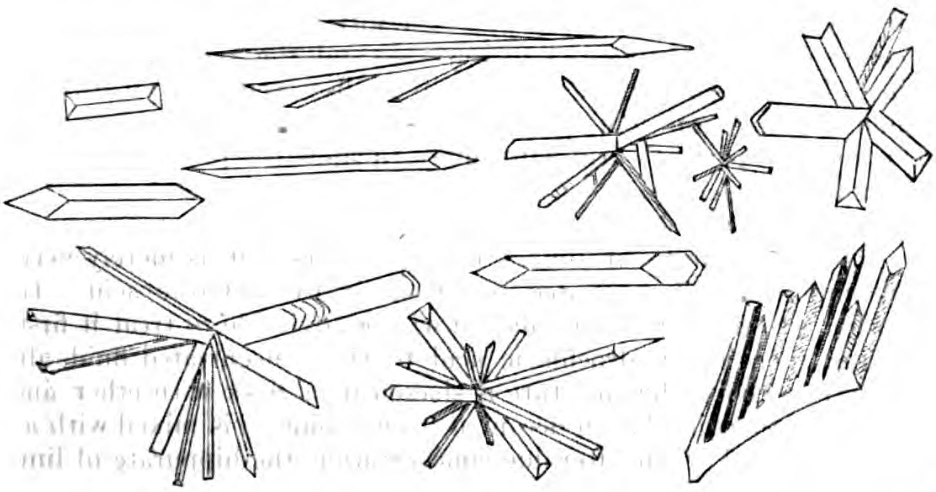
9. *Two new Acids obtained from Human Urine.*—Whilst engaged with some researches, in regard to the immediate principles of healthy urine, in Messrs. Verdeil and Robin's laboratory at Paris, we obtained from that secretion two acids which had not been previously described. The following is the method to be employed for their extraction:

After the extraction of the urea by the addition of ether to the alcoholic solution of dried urine,* the strongly acid mother liquor is evaporated at a low temperature, baryta water being previously added, to avoid the decomposition of the organic substance contained in the solution. The acids are then set free by the addition of sulphuric acid in excess to the remaining aqueous solution, which is shaken in a flask with alcohol and ether, and allowed to stand; after a few minutes the ether rises to the surface possessing an acid reaction, though not from the presence of the sulphuric acid, as it

* See Urea, p. 373.

remains, with the small quantity of urea left behind by the first operation, dissolved in an inferior layer of water. The supernatant ethereal solution is decanted and repeatedly washed with water and alcohol, ether being occasionally added to supply the deficiency caused by evaporation during the above-mentioned process. An acid solution in ether is finally obtained quite free from urea and sulphuric acid, and remarkable for its beautiful pink colour when observed under transmitted light. This solution is decanted for the last time through a filtering funnel, the inferior layer of water being allowed to run out; and the acid ether is then submitted to a slow evaporation at the temperature of the atmosphere. Twelve hours afterwards the remaining liquid is found covered with prismatic colourless needles, which can be collected and dried upon filtering paper; the sides of the vessel are occupied by the deposit of an amorphous pink substance.

First Acid.—The crystalline substance, figured in the adjoining woodcut, when examined under the microscope, appears to assume the form of oblique rhombohedra or of prisms derived from that type, aggregating occasionally in stellate groups, but generally branching off from a main crystal or long prism. They transmit readily polarized light. This substance is soluble in ether, alcohol, and boiling water; it imparts to its solution an acid reaction, and can be again obtained crystallized by concentration.



When heated upon a platina spatula the crystals fuse, emitting a peculiar smell different from that of hippuric acid, and after charring without taking fire, finally disappear, leaving no residue. This substance, therefore, is not *hippuric acid,* to which, however, in many respects, it bears great analogy.

Second Acid.—The pink deposit which occurs on the sides of the vessel during the evaporation of the mother liquor, has a very strong acid reaction; after standing for a week, it is found to contain colourless concentric groups of radiating crystals; but the very small quantity obtained did not admit of their minute examination. Robin considers the pink amorphous sediment as *urrosacine,* or the colouring principles of urine obtained by Dr. Harley.* This pink substance is soluble in ether and alcohol, but not in

* See Urrosacine, p. 376.

water. Its smell is peculiarly aromatic, increasing when exposed to heat on a platina spatula; it chars, emitting an odour of burnt oil, and leaves no appreciable fixed residue. We have not observed whether iron is present.

10. *Creatine.*—This principle of urine, recently discovered by Liebig, who ascertained its presence in that secretion, is one of the most important constituents of flesh. It exists also in blood, where Verdeil and the reviewer have detected its presence.* Creatine may therefore be considered as a peculiar form assumed by muscular tissue, to aid its elimination from the system. It is formed in the muscles, taken up by the blood, and excreted with the urine.

Extraction.—To obtain creatine from flesh, that of an ox, a horse, or a fowl, may be conveniently employed. The finely minced flesh is digested for twenty-four hours in lukewarm water strained through calico, and the filtrate heated to the boiling point for the extraction of its albumen, which will then coagulate, taking up the whole of the colouring matter. The fluid is again filtered through calico, and the coagulum pressed to avoid losing any of the extract. The filtrate neutralized with baryta water, which is to be added as long as it produces a white precipitate, is evaporated at a temperature of about 80° cent. It is advisable to use a moderate heat, especially when the concentration is coming to an end, lest the creatine should undergo decomposition. When about one-half of the liquid extract has been evaporated, it is filtered, and the clear filtrate again concentrated; as soon as it is reduced to about one-twentieth of its original volume, and has acquired a thickish consistence, it should be placed under a moderately warm temperature, and left to evaporate slowly. Very soon small, distinct, short, colourless needles appear on the surface, which increase on standing and on cooling, until the sides of the vessel become covered with them.

For the extraction of creatine from fresh urine, this fluid is concentrated on the water-bath, having been previously treated with lime water and chloride of calcium to precipitate the phosphates; the liquor is filtered, and the filtrate evaporated down will deposit the crystalline inorganic salts. The decanted liquor is to be treated with chloride of zinc, and allowed to stand for a few days. A mass of crystals will thus be obtained, consisting of creatine and of the compound of creatinine with chloride of zinc. These crystals are washed, and boiled with hydrate oxide of lead suspended in water; by which process the chloride of zinc and creatinine salt will be converted into an insoluble basic chloride of lead, oxide of zinc, and creatinine. The filtrate from this precipitate containing therefore nothing but creatine and creatinine, is evaporated to dryness, and treated with boiling alcohol, which takes up the creatinine, leaving the creatine.

To obtain creatine from the blood, this fluid mixed with an equal volume of water is heated to the boiling point, and then strained through calico in order to separate the albumen. The filtrate neutralized with milk of lime is heated, passed through a filter, and concentrated under a low temperature; after a few days this fluid will deposit crystals of creatine.

The crystals of creatine are colourless, perfectly transparent, and of a pearl lustre; they belong to the klinorhombic system. When a small

* Verdeil and Marcet: Recherches sur les principes immédiats qui composent le sang de l'homme et des principaux mammifères. Journ. de Chimie et de Pharmacie, 1851, t. xx.

amount of extract is subjected to examination, they not unusually assume the form of the right rectangular prism. When viewed under the microscope, small square and rhomboidal tables, with prisms and needles, are seen, readily transmitting polarized light. If a solution of creatine be evaporated rapidly, the crystals will coalesce in small groups; otherwise the plates are generally isolated. A few crystals of creatine will suffice to show, undoubtedly, its presence; for that purpose, they are treated with a drop of hydrochloric acid, and the mixture is evaporated to dryness in a water-glass, until the whole of the acid has been removed. The solid residue, dissolved in a few drops of water, will yield, by spontaneous evaporation, broad transparent scales, of an acid reaction, consisting of chloride of creatinine.

Creatine has no action upon test-paper. It is soluble in water, but alcohol and ether fail to dissolve it. When burnt on platina foil, it evolves a characteristic smell of burnt meat, which is often useful to test its presence.

11. *Creatinine.*—Creatinine is a constituent of muscle; we have detected its presence in the blood (Verdeil and W. Marcet). It can be extracted from urine, and Scherer has traced its existence in the liquor amnii.

Its functions in the animal system are probably similar to those of creatine and of urea; it is formed in the muscle, taken up by the blood, and finally eliminated in the urine.

Extraction.—Creatinine can be separated from its concentrated solution in any animal fluid (which has been previously treated with lime and chloride of lime, to precipitate the phosphates and sulphates) by means of chloride of zinc, when the compound of chloride of zinc and creatinine will subside, sometimes immediately, but often only after the lapse of some days. This compound assumes a crystalline form, occurring as small short needles, occasionally isolated, but generally grouped in round masses, radiating from a dark black centre; the extremities of the needles are seen projecting at the circumference. From this compound, creatinine can be easily obtained, if it be boiled for some hours with hydrated oxide of lead suspended in water, which causes the oxide of zinc, with the chloride of lead, to precipitate.

Creatinine, crystallizing from its solution in water, assumes the shape of fusiform plates, thickened near the centre, and, from this circumstance, appearing bi-convex; they are grouped in a variety of ways, and polarize under the microscope. When obtained from an alcoholic solution, the groups of creatinine crystals assume an arborescent structure. This substance is soluble in water and in hot alcohol, but insoluble in ether. Its reaction is decidedly alkaline, and its taste slightly caustic.

12. *Urea.*—Urea is one of the most important immediate principles of urine, where it is known to exist in the free state, and, probably, also combined with chloride of sodium.

Urea was discovered, in 1771, by Rouelle, junior, who observed its presence in urine. Cruikshanks, in 1798, obtained it in the crystallized form. Fourcroy and Vauquelin gave it its present name. Its existence in the aqueous and vitreous humour of the eye was detected by Millon; and Wöhler found it in the liquor amnii. Pettenkoffer traced it in the saliva; and, according to Stass, urea may be extracted from the blood of the placenta. M. Paul Hervier has obtained urea from human blood, when operating only upon 250 or 500 grammes of it. The blood he used had been taken from patients suffering under rheumatism, pneumonia, or erysipelas; he concludes, however, that urea is normally present in that fluid. According to Prout and Dumas, the blood contains an excess of this substance after the removal of the kidneys. At the approach of death, when the intestinal secretions diminish, Bernard and Barreswill have also observed a decided increase of the urea existing in the blood.

The amount of the urea of urine has been found to vary according to a number of circumstances; but, unfortunately, the methods for the extraction of this principle have led to no satisfactory result. We are much indebted to Liebig, who has very recently pointed out a new process for ascertaining the amount of urea existing in the urine, which cannot but lead to very accurate results. The urine of young children hardly contains a trace of urea. According to Lehmann, normal urine will yield from 31·45 to 32·90 per 1000 of this substance. The amount of this principle secreted in the twenty-four hours is modified by a variety of circumstances, including especially the nature and quantity of the solid and fluid ingesta.

Urea is always found dissolved in the urine, from its being exceedingly soluble in water. Several theories have been given as to the source of urea in the system; it is known, however, not to be formed by the kidneys, for the excision of those organs causes an increase of urea in the blood. According to chemists, this substance is the result of an oxidation of tissues. Their carbon and hydrogen are converted into carbonic acid and water, to be eliminated through the lungs, whilst the nitrogen, combining with hydrogen, will assume the form of ammonia. This gas, in the presence of carbonic acid, is supposed to lose one equivalent of water, and the final combination will be urea. Other theorists suppose that cyanate of ammonia is formed in the blood (from the oxidation of its nitrogenized elements), which is subsequently converted into its isomeric compound, urea.

Urea is secreted from the blood by the kidneys, and thus expelled from the body. An excess of that principle in the blood is not necessarily followed by an increased amount of it in the urine; and if its proportion in the urine be diminished, as happens in some diseases, we are not certain of finding its quantity in the blood larger than usual: for M. Cl. Bernard has observed, that urea is often secreted by the stomach and intestines, where it loses its nature, and assumes the form of ammoniacal compounds. This vicarious secretion comes into action whenever the kidneys are insufficient to remove all, or the greater part, of the urea contained in the blood.

Extraction.—To extract urea from the urine, that fluid is first evaporated to almost one-tenth of its bulk, and then mixed with a small quantity of nitric acid free from nitrous acid. There occurs instantly, in a concentrated solution, or after a few minutes if it be diluted, a crystalline precipitate of nitrate of urea, which can be easily purified by repeated crystallizations. This compound will yield pure urea if it be treated with carbonate of lime, or carbonate of baryta, to separate the nitric acid as nitrate of lime or nitrate of baryta. This fluid, evaporated to dryness, is treated with alcohol, when the urea will crystallize by concentration. We have observed that the presence of sulphuric acid does not prevent the formation of nitrate of urea; on the contrary, it appears to hasten the combination, which assumes, in this case, a beautiful pearl lustre, much more remarkable than when nitric acid is made to react upon urea in pure water. It is sometimes advisable to use oxalic instead of nitric acid; the operations required in both cases are exactly the same. We have employed another method for separating urea from urine, which has enabled us to obtain this substance directly, without having recourse to a series of combinations and decompositions. The urine is first evaporated on the water-bath, and the solid residue thoroughly dried over sulphuric acid, under the air-pump. The brittle mass is then to be treated with boiling absolute alcohol, until it yields no more colouring matter to that fluid. It is advisable, in order to obtain this extract, to employ repeated small quantities of alcohol, the solution being decanted after each operation. By this process the whole of the free urea contained in the urine examined may be extracted, together with a small quantity of common salt: test-paper will show this alcoholic solution to have a strong acid reaction. Sulphuric ether is then added to the acid alcoholic extract, care being taken to pour it gently down the sides of the glass, to prevent the fluids from mixing. At first, a cloudy precipitate will appear, at the line of contact of the two liquids, gradually extending below and above. Five or six hours afterwards the precipitate disappears, when the sides of the beaker will be found covered with beautiful, needle-like crystals of urea, sprouting from the sides and bottom of the glass to the centre of the fluid. Ether is now added until the precipitate ceases to appear, and another crop of crystals is obtained. We have frequently observed the crystals of urea, at the end of the operation, to sprout from one side of the beaker to the other, being interlaced in a variety of ways. Large leaf-like masses of crystals are often seen adhering to the others, and resting upon their thickest extremity, or that connected with the glass. From their being mixed with a little common salt, the crystals of urea deliquesce very readily as soon as the mother liquor is decanted; but another crystallization in water will produce them in the pure state. The acid liquor may now be used for the extraction of the peculiar acid, already described.* From the above method for the extraction of urea, we may safely conclude that by far the largest proportion of this substance exists in the free state, as one of the immediate principles of human urine.

If an alcoholic extract of urine be concentrated, needle-like crystals of urea are deposited, mixed with the inorganic salts also dissolved by the alcohol; these crystals will occasionally lie parallel to each other, but generally coalesce into stellate groups adhering to the bottom of the capsule. We have frequently observed this crystallization to occur in the concentrated alcoholic extract of dog's urine, which contains a large proportion of urea. If a drop of a solution of urea be evaporated on a glass plate, the crystals will assume a peculiar arborescent structure, transmitting, very readily, polarized light. It also often happens in this case that a number of parallel crystals of urea will be connected to each other transversely by shorter crystals. If urine be evaporated to the consistence of a syrup, and left undisturbed for a day or two, a crystallization of urea will appear, assuming the form of long prisms or large striated needles, with a granular surface.

* See p. 370.

To extract urea from blood, the fibrine has first to be removed, and then the fluid, mixed with an equal bulk of water, is heated to the boiling point and strained through calico. The serum free from fibrin, from albumen, and from colouring matter, is evaporated on the water-bath to a thickish consistence. The addition of alcohol to the residue will cause an abundant precipitate to appear, and the filtered alcoholic solution is treated with oxalic acid, to separate the fatty matters which have now become insoluble. The fluid is again filtered and rinsed with ether; the latter floating on the water will contain all the hippuric acid of the blood; it must be decanted; the aqueous solution neutralized with carbonate of lime is finally evaporated to dryness under the air-pump. If this dry residue be treated with alcohol, it will yield an alcoholic solution of urea, from which the latter can be easily obtained in the crystallized state or combined with nitric or oxalic acid.

Urea is very soluble in water and in alcohol, but dissolves sparingly in ether. When burnt upon a platina spatula it emits a peculiarly nauseous smell, which is often sufficient to show its presence. It combines with nitric and oxalic acids, but not with sulphuric acid; we have observed, however, that the presence of the latter appears to hasten the formation of nitrate of urea in dilute solutions. The combination of nitric acid with urea when viewed under the microscope is seen to consist of superposed crystallized plates, or rectangular and rhombohedral prisms. If nitric acid be added to urine concentrated in a watch-glass, thick lozenge-shaped crystals often occur, two of them aggregating in the shape of a cross, or several joining to produce an arborescent structure. Nitrate of urea is sparingly soluble in cold water; it dissolves in hot alcohol, but not in rectified ether. When oxalic ocid is added to concentrated urine, tabular groups of crystals similar to the former are obtained, occasionally assuming the form of very flat prisms. When the crystallization has been rapid, we may detect groups of elongated crystals resulting from small imbricated prisms. Along with these, several other crystalline forms occur, as spherical masses of laminated crystals. The crystals of oxalate of urea have a peculiar yellow colour; they transmit polarized light.

Liebig has discovered lately a very accurate method for ascertaining the amount of urea contained in fluids. For this purpose he prepares a normal solution of acid nitrate of mercury in water, and determines, by pouring it from a graduated tube, how much of this solution will precipitate a given quantity of urea. By adding this fluid to an unknown solution of urea until the whole of it has precipitated, the quantity of the urea precipitated and therefore contained in the solution will be at once detected. The amount of urea obtained from urine by this method is slightly larger than that yielded by the other processes.

13. *Chloro-sodate of Urea, or Compound of Urea with Chloride of Sodium.*—If urine be saturated with chloride of sodium or muriate of ammonia there is obtained a combination of urea with the two other salts; the double salts thus obtained crystallize in octahedra and not in ashes like common salt or chloride of ammonium. According to Liebig, the urea existing in the blood in the vitreous humour of the eye, and a certain amount of the urea contained in urine, occurs in combination with chloride of sodium. It is very probable that this compound, if it really exists in

the body, possesses some important organic properties that we are not aware of. Its occurrence in urine, however, might be the result of the artificial combination of chloride of sodium with urea during evaporation; we hope that further researches will resolve this interesting question.

14. *Urrosacine, or Colouring Principle of Urine.*—The colouring matter of urine has been extracted and examined by Cruikshanks, Prout, Vauquelin, and lately by Vogel, who considers it as an acid closely connected with uric acid. Prout described this substance as purpurate of ammonia; G. Bird calls it purpurine; and Lehmann, simply urine-pigment. Dr. Harley has just published * an account of his researches on the colouring principle of urine: he was enabled by a new method of investigation to obtain this substance in a perfectly pure state, not only from the human urine, but also from that of the ox, horse, pig, &c. In every case he found iron existing as a normal constituent of the colouring matter. The following method was employed for the extraction of this substance:—A large quantity of urine was evaporated almost to dryness on the water-bath, the chloride of sodium and other inorganic salts being removed by crystallization. From this residue the colouring matter was extracted by means of boiling alcohol. He then boiled the dark-red alcoholic extracts with hydrated lime, added in small quantities at a time, till the liquid became colourless; the vessel being well shaken after each addition. The whole was thrown on a filter, and the compound of lime and the colouring matter thoroughly dried, and then washed with boiling ether, to ensure the complete removal of any fatty substance which might be mixed with it. The lime compound was again dried, acted upon by hydrochloric acid, to set free the colouring matter, and treated with alcohol. This alcoholic solution mixed with an equal bulk of ether was allowed to stand for several days, and on the addition of water the ethereal solution of colouring matter formed a distinct upper layer, which was removed without difficulty. This solution had a rich port-wine colour, it still contained a trace of hydrochloric acid, which was removed by distilled water added to the coloured ether in a filtering-funnel stopped by the finger, so that the water lurking at the bottom of the former was easily withdrawn by partially removing the finger. This and the after distillation left a pigment which, when dried, appeared as a brownish-black glistening mass.

As to the properties of this substance, Dr. Harley states that it is insoluble in water, but soluble in alcohol and ether; the presence of fat or oil adds greatly to its solubility. On being burnt, it evolves an odour similar to burning horn, and leaves a slight residue soluble in hydrochloric acid, which is a salt of iron, as was satisfactorily shown by its being tested with ferrocyanide of potassium: the sulphocyanide of potassium, when added, produced a fine red colour. Besides iron, Dr. Harley found this substance to contain carbon, hydrogen, nitrogen, and oxygen; from this circumstance, he considers the colouring matter of urine as a modified form of hæmatin, or colouring principle of the blood; the only difference appearing to be in the amount of iron. He observes, moreover, that the colouring principle of the bile, and also melanin, bear the same close resemblance to hæmatin as urine-pigment, the amount of iron in the bile-

* Pharmaceutical Journal for Nov. 1852.

pigment appearing to be less, while in melanin it would be greater. He found as much as one-and-a-half per cent. of iron in melanin prepared from a melanotic tumour taken from a horse. We have not ascertained whether iron existed as a constituent of the pink colouring matter obtained from urine by our process, but it is very likely identical with the substance prepared by Dr. Harley, mixed with a peculiar acid, which crystallizes after a lapse of several days. The substance Vogel called *Rosacic acid*, was undoubtedly a mixture of the colouring matter with this crystallizable acid. As to the source from which the colouring principle of urine is derived, nothing precise is known; it probably results from a modification of the blood's colouring principle.

The animal colouring matters have been lately the subject of most elaborate researches made by M. Verdeil, who, among the interesting results he obtained, traced a great analogy between the colouring principle of blood and the green substance of plants: his experiments cannot fail to throw a new light upon the formation of colouring matters in the body.

In the table of the organic immediate principles which constitute human urine, we have purposely omitted to mention *fatty matters*, which however are very likely to be present in normal urine; but little or nothing at present is known on that subject.

In a future review, we shall discuss the pathological conditions of the urinary constituents.

William Marcet.

Review V.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XXXV.—*London,* 1852. pp. 492.

Many papers of great merit are contained in this volume of the 'Transactions.' We shall reserve some of them for separate consideration, and shall give an abstract of the remainder.

I. *Statistical Report upon cases of Disease of the Heart, occurring in St. George's Hospital, especially in relation to Rheumatism and Albuminuria.* By Dr. Barclay, the Medical Registrar.

Ninety-two records of post-mortem examinations are arranged in a tabular form, under the following heads: sex; age; lesions of pericardium; alterations in muscular structure and cavities; condition of aortic and mitral valves; history of acute rheumatism; state of kidneys; other important lesions; general observations. From this table deductions are then made.

Unfortunately, the total number of patients admitted during the time that these deaths occurred, and the total number of rheumatic cases, are not given. Some interesting information is thus lost. For example, we are told simply that eight cases of acute rheumatism proved fatal during the rheumatic attack; but by the above omission we are left uninformed what the per-centage of the mortality may be. With this exception, the paper seems to us a worthy successor to the able memoirs recorded by the author in previous volumes of the 'Transactions.' It is rather adapted for reference than for analysis, and we shall therefore merely select some of the most interesting points for comment.

Of the eight fatal cases of so-called rheumatism, two were instances of suppuration in and round joints, and were, probably, not really cases of rheumatism. In five other cases, the immediate causes of death seem to have been pericarditis. In the last case, the patient died from an obscure cause, delirious and comatose, with a basic, endocardial, inorganic, systolic murmur. It appears, then, from the experience of St. George's Hospital, that when rheumatism is fatal, it is so either by a non-rheumatic complication or from pericarditis. Endocarditis is less immediately dangerous. There were, among the ninety-two cases, sixteen (or rather, fourteen, as two seem to be doubtful) of recent non-rheumatic pericarditis. The analysis of these cases corroborates the rule laid down by the late Dr. John Taylor, that (excluding the specific fevers) non-rheumatic pericarditis is, in the great majority of cases, connected either with kidney-disease, or with pleurisy.

There were four cases of recent *non-rheumatic endocarditis;* and in three of the four, there was kidney-disease. *Old pericarditis* was present in a great number of cases, in thirteen of which there was no evidence of prior acute rheumatism. Admitting that acute rheumatism was really absent in these cases, Dr. Barclay points out the curious fact, that the *majority of the cases of adherent pericardium were non-rheumatic;* and that while all the cases of rheumatic pericarditis appear to have been severe, *only one terminated in complete adhesion.*

Valvular lesion, old and recent, was found in 62 cases. Of these the disease was decidedly of rheumatic origin in 20, while in 26, there had been no rheumatism. From these cases the following table is made up:

	Rheumatic.	Non-rheumatic.
Mitral and aortic valves affected	12	10
Mitral valve alone	7	11
Aortic valve alone	1	5

The tendency of rheumatic endocarditis to affect both mitral and aortic valves is clearly brought out; the proportion is, perhaps, rather greater in fatal cases than it appears to be from an examination of living subjects, but the general rule in both cases seems to be the same.

With respect to albuminuria and valvular lesion, it appears that among 61 cases, in 22, or in 36 per cent., there were diseased kidneys and albuminuria; and in 13 other cases, there was more or less certain evidence of kidney-disease.

The tricuspid valves were affected ten times among the 92 patients. In three, this was associated with rheumatism; in two, with Bright's disease.

With respect to *hypertrophy* of the cardiac walls, and to *dilatation* of the cavities, Dr. Barclay calls attention to the important fact, often discussed, but never fully explained, of the very frequent association of some kidney-disease, and alteration in the walls and cavities of the heart. In 141 post-mortem records, in which the kidneys were noted as evidently diseased, there was hypertrophy in 55, and dilatation in 36 cases.

The association of *rheumatism* and cardiac lesions is discussed with considerable care. Dividing the rheumatic cases into acute, subacute, and chronic, it appears that cardiac lesions occurred in 44 per cent. of the first, in 11·2 per cent. of the second, and in 3·9 per cent. of the third class.

And as in some of the cases of sub-acute rheumatism there had been a previous acute attack, the immense preponderance of cardiac disease in the acute as distinguished from the less acute attacks, is undoubted.

The tables show, also, that the tendency to cardiac complication, highest both in acute and sub-acute cases between ten and fifteen years, subsequently declines, and in the sub-acute cases especially so after the twenty-fifth year.

The influence of sex appears to be somewhat marked, and females are slightly more liable to cardiac complication than males, although, taking all causes, males are more subject to heart-disease than females.

In estimating the influences of age and sex on all cases of confirmed heart-disease, of which notes have been kept in St. George's Hospital (82 fatal cases, 124 not fatal = 206), the rules already known receive confirmation. Thus in males, confirmed heart-disease is more common than in females; in both sexes rheumatism is its grand cause at an early period of life, but after middle age is superseded as a cause by other conditions, chiefly kidney-diseases. Thus, "up to the age of 20 almost the whole, and even as far as 30 considerably more than half, of the cases are ascribed to a rheumatic origin; between 20 and 30 the non-rheumatic cases first appear; in the two succeeding periods they nearly double the rheumatic, and after 50 there are scarcely any rheumatic cases at all." (p. 23.)

II. *Case in which a communication appears to have existed for several months, between the Stomach and transverse Colon of a Child five years old.* By Robert Jones, Esq.

An interesting case, reported rather briefly, and leaving us very much in the dark, both as to its cause and symptoms. The stomach and colon were adherent, and the opening between the two was large enough to admit the finger. So little is said of either stomach or colon, that we are unable to discover whether the primary disease was ventricular or colonic. The symptoms were, emaciation, repeated attacks of violent abdominal pain, and vomiting (at first of purulent and afterwards of stercoraceous matter), alternating with constipation and diarrhœa.

III. *On Alkalescence of the Urine from fixed Alkali in some cases of disease of the Stomach.* By H. Bence Jones, M.D., F.R.S.

Two cases of dyspepsia with vomiting of an acid fluid, containing sarcinæ ventriculi, and one case in which the acid fluid was free from sarcinæ, are narrated to show that the reaction of the urine was influenced by the quantity of acid ejected from the stomach. When this was large, the urine became alkaline from fixed alkali; when it was small, the urine was acid. This is corroborative of the inference contained in the author's paper on the influence of digestion (i.e., of acid in the stomach) on the reaction of the urine. This is a point of great importance, to which we shall return on another occasion.

IV. *A case of Constipation from Scirrhous Rectum.* By John Adams, Surgeon to the London Hospital.

We reserve this and other similar cases reported in the volume for separate consideration.

V. *Remarkable case of Foreign Bodies in the Stomach and Duodenum.* By John Marshall, Esq.

A mass weighing nine ounces, formed of aggregated pins, was found after death in the stomach, and a similar but heavier mass in the duodenum, of a woman who had been addicted to swallowing pins. The mass was plainly felt at least five years before death; at that time there was flatulence, distressing vomiting (occasionally of blood), pain, and emaciation. These symptoms subsided, and for five years the patient remained free from serious affection, but finally died with return of incessant vomiting, and spasmodic abdominal pain. The most interesting point about the case was the complete impaction of the mass in the duodenum, which appears to have completely obstructed the intestine, and in the opinion of Dr. Cowan, given in a letter to the narrator, could have allowed no solids and very little fluid to pass. Below the obstruction, the intestines, especially the large, were shrunken. The functions of digestion, absorption, nutrition, and excretion, appear nevertheless to have been tolerably well performed. Unfortunately, nothing is said about the urine, and we are therefore uncertain whether this case supports or not the practical rule laid down by Dr. Barlow, that the amount of fluid excreted by the kidneys can be used as a diagnostic indication of the seat of the obstruction.

VI. *A successful case of Parturition in a patient who had previously undergone Ovariotomy by a "large incision."* By John Crouch, M.R.C.S.

In August, 1849, Mr. Crouch extracted, by an abdominal incision nine inches long, an ovarian cyst, weighing 14lbs. The catamenia returned a few months subsequently; the patient married in April, 1850, and was at the full time delivered safely. During pregnancy the cicatrix increased in length three inches, and in breadth one sixth of an inch, but after delivery returned to its previous size.

VII. *On Granular and Fatty Degeneration of the Voluntary Muscles.* By Edward Meryon, M.D., L.R.C.P.

This paper is referred to in the article on Fatty Degeneration.

VIII. *Case of Stricture of the Colon successfully treated by operation, with an Analysis of forty-four cases of Artificial Anus.* By Cæsar H. Hawkins, Surgeon to St. George's Hospital.

IX. *Case of Inversion of the Uterus after Parturition, proving fatal in eighteen months; with a tabular statement of the results of cases treated by operation.* By John Gregory Forbes, F.R.C.S.E.

After a tedious labour, terminated with the forceps, the patient continued to go on favourably for three days, when sudden and irreducible inversion of the uterus took place. The patient died in eighteen months from the effects of repeated hæmorrhage, which produced complete anæmia and its consequences. The author annexes references to thirty-four similar cases.

X. *Remarks on the Surgical operations usually adopted for Retention of Urine.* By Edward Cock, Surgeon to Guy's Hospital.

The surgical operations referred to are, 1, forcible entry by false passage into the bladder; 2, opening of the urethra behind the stricture; 3, puncture of the bladder through the rectum or above the pubes.

The first method is condemned, as "neither scientific in its conception, surgical in its performance, nor anatomical in its details." The second method is said to be difficult in execution and unsatisfactory in results. Of the two operations comprehended under the third heading, Mr. Cock prefers puncture through the rectum, and the object of the paper is to prove the superiority of this mode of proceeding. Forty cases are related, in thirty-eight of which the operation accomplished its purpose.

XI. *On the Deposition of Fibrin on the Lining Membrane of Veins.* By Henry Lee, F.R.C.S., Surgeon to the Lock Hospital.

The deposits on the lining membrane of veins, in the so-called phlebitis, are usually spoken of as exudations from the surface of the membrane itself, in the same way as the lymph in the pleura or pericardium is considered, and no doubt, in this case, correctly, as the product of these structures. Mr. Lee, however, having experimentally determined that mechanical irritation of the lining membrane of veins produces no exudation on its surface, is of opinion that deposits of lymph in the interior of veins are in reality derived, not from the lining membrane, but from the contained blood. The experiment in question was performed on a donkey, whose left jugular vein was laid bare, secured between two ligatures, and filled with cotton wadding, to prevent regurgitation of blood when the ligature nearest the heart was removed. In forty-four hours the animal was killed; no exudation was found on the smooth and polished lining membrane of the vein.

This experiment is corroborative of those already made and recorded by Meinel.* This observer, in fourteen experiments on animals, succeeded in no single instance in causing alteration of the lining membrane of veins by mechanical and chemical irritation, although hyperæmia of, and exudation in, the cellular and middle coats were produced. The deposits, therefore, in the interior of the vein, were considered by Meinel, as by Mr. Lee, to be derived from the blood, and to be attributable to particular conditions of this fluid.

The various experiments performed by Virchow,† prove that the lining membrane of arteries is as little influenced by mechanical and chemical irritation as that of veins, and that in this case, also, the deposits upon the lining membrane are derived at once from the contained blood.

* Physico-pathologische Untersuchungen über Phlebitis. Wunderlich's Archiv., für Phys. Heilk., 1848. We are not acquainted with the original, but quote from Loebel's Abstract, in Canstatt's Jahresbericht for 1848.

† Die akute Entzundung der Arterien, von R. Virchow. Archiv. für pathol. Anatomie, &c., vol. i. p. 272.

The evidence on this point is indeed so strong, as to warrant the assertion, that the prevalent doctrines of phlebitis, which have originated chiefly with French writers, are little else but a tissue of errors. Virchow* has pointed out, with great clearness, the exact point at which even so acute an observer as Cruveilhier stumbled, in his investigation of this subject, and interpolated, almost unawares, an untenable hypothesis, in the sequence of facts.

If the coagulation of the blood, and the deposits upon the lining membranes of veins and arteries, are not attributable to inflammation of this structure, to what are they owing? Mr. Lee accounts for them by throwing the coats of the vein in the back-ground altogether. The coagulation appears, in his interpretation, to be always primary; the changes in the external and middle coats of the vein, secondary. A vitiated blood can separate from itself a "fibro-albuminous element," and this "lines the cavities of inflamed and obstructed veins;" or rather, lining the veins produces inflammation of the outer and middle coats, "for the purpose of getting rid of a foreign substance lodging in the vein."

We question whether this view is not too great a reaction against the doctrine of local inflammation of the lining membrane of veins, and we believe that the truth lies, as usual, in the middle. We do not doubt that frequently coagulation does occur, from some cause, spontaneously in the blood of a vein, and that secondary inflammation of the vein follows upon this. But neither the experiments of Meinel, Virchow, nor Lee, appear to us to destroy the opinion, that inflammation can occur primarily in the outer and middle coats of veins and arteries, that liquid exudation may then permeate through the extra-vascular lining membrane, and may either partly solidify on its surface, or, as in most cases, may be carried away by the moving blood, or may cause its coagulation. In the endocardium these changes are more easily followed, and we perfectly agree with Luschka in his enumeration of them as they occur in endocarditis; in which case, also, as in veins and arteries, the other and separate phenomenon—viz., the direct deposition from the blood, may be combined with the exudation.

We are inclined, then, to believe, that "inflamed and obstructed veins" may have arrived at this state by two different roads, which at a certain point unite. The spontaneously coagulating blood may originate inflammation of the vascular coats of the vein; the spontaneously inflamed vein may, by the exudation which penetrates its extra-vascular lining membrane, originate the coagulation of the blood. Mr. Lee's researches have already thrown great light on this subject, but they do not exhaust it.

We must not, however, at present, enter into the difficult subject of phlebitis and contaminated blood, but must content ourselves with hoping that Mr. Lee will continue his observations, which are as important as any which have been lately made in pathology.

XII. *On the occasional Organic Union of Contiguous Teeth.* By S. I. A. Salter, M.B.

Two instances of organic union of teeth are added to others previously recorded, and the mode of union—viz., by calcification on closely adjacent

* Archiv. für pathol. Anat., vol. i, p. 13.

pulps in the same sac, is explained. The union is not formed by bone, but by dentine, which is continuous from one to the other tooth.

XIII. *Two Cases of Intestinal Obstruction.* By William James Clement, F.R.C.S.S.

XIV. *A Case of Intestinal Obstruction from Disease of the Rectum.* By Alfred Baker, Surgeon to the General Hospital, Birmingham.

XV. *A Case of Intestinal Obstruction.* By J. Luke, Senior Surgeon to the London Hospital.

XVI. *Some Observations on the Effects of Cholagogue Medicines, and some Remarks on Morbid Changes in the Liver.* By C. Handfield Jones, M.B. Cantab., F.R.S.

The structure of the liver, and various of its pathological conditions, have been so well investigated by Dr. Handfield Jones, and by other observers, that the time appears to have arrived when a general summary of our knowledge of the structure, functions, and diseases of this important organ becomes expedient. We shall therefore reserve the pathological facts recorded in this paper, and notice only the experiments bearing on the action of cholagogue medicines.

Mercurial preparations, given in various doses to cats and dogs, were found in most, though not in all, cases (exceptions being referred to in the appendix), to cause an evident increase in the quantity of yellow pigment contained in the hepatic cells. Considerable congestion was also produced. The effect on the bile-acids was not determined. *Colchicum* did not appear to produce much increase of yellow pigment in the cells, but acted apparently as an emulgent on the ducts, and emptied the liver. *Tartar emetic, aloes, oil of turpentine, rhubarb,* and the *nitro-muriatic acid bath*, appeared to have the same effect, of causing bile to be poured out, but not to be formed in increased quantity. *Taraxacum* appeared to have little effect. *Muriate of manganese* increased the quantity of pigment. The following quotation sums up the general results:

"Mercury, muriate of manganese, and colchicum, are the only substances which seem to increase the production of yellow matter in the cells of the liver. I have placed them in the order of their potency. That they also increase the production of glycocholate and taurocholate of soda, I think very probable; but, unless we were assured that the quantity of these principles is always proportionate to that of the yellow pigment, which does not seem to be the case, the above experiments say nothing as to this point. It is clear that the cholagogue action of a medicine, its emulging effect on the ducts, is distinct from that which it exerts in the production of biliary pigment." (p. 258.)

XVII. *On some of the Principal Effects resulting from the Detachment of Fibrinous Deposits from the Interior of the Heart, and their Mixture with the Circulating Blood.* By W. Senhouse Kirkes, M.D., Demonstrator of Morbid Anatomy at St. Bartholomew's Hospital.

In this paper, Dr. Kirkes advances and sustains, by several very able arguments and observations, the statement, that the fibrine deposited upon the valves of the heart may become washed off by the current of blood, and that masses of it may be carried for some considerable distance, until they become arrested in vessels too narrow to allow of their transit. The idea here submitted differs from that of Rokitansky and others, who have spoken of such detachment of fibrine, but who have supposed that contamination of blood merely results therefrom, and not any such blocking-up of vessels as Dr. Kirkes believes occasionally to occur. It is, however, nearly the same idea as that advanced by Virchow,* in his paper on Acute Arteritis, already referred to. In this paper, Virchow speaks of blocking-up of vessels, by coagula forming round substances brought from distant parts, and narrates various cases of this kind. In two of these cases, in the arteries of the extremities, coagula were found, containing in their interior, chalky, fatty masses; while on the valves of the heart were similar adherent masses, and spots from which other masses had evidently been detached. Virchow has developed, with great clearness and precision, the various consequences of such coagulation and obstruction, but Dr. Kirkes has still more completely elucidated this important subject. The observations of Virchow are, however, so confirmatory of Dr. Kirkes' views, as to render it unnecessary for us to enter into a critical examination of the evidence brought forward in their support, and we shall therefore proceed to give merely a brief statement of the chief deductions which Dr. Kirkes has felt himself justified in making. These are—that vegetations on the valves of the heart, and especially the " globular excrescences" of Laennec, are liable to detachment in masses of size sufficient to obstruct an artery of some calibre. If detached in smaller masses, these fibrinous particles may obstruct by leading to coagulation of the blood. In either case effects ensue according to the part obstructed. Dr. Kirkes relates some most interesting cases, in which, in the course of heart-disease, masses were detached from the valves of the left side of the heart, and blocked-up some of the cerebral arteries; then ensued rapid cerebral softening, and hemiplegia. In other cases, the masses detached from the tricuspid or the pulmonary valve, passing into the pulmonary arteries, appeared to be arrested in the minuter divisions in the lungs themselves; but on account of the rarity of disease of these valves, Dr. Kirkes has not traced out the facts with the same precision. If the masses from the left side pass into other parts of the body, coagula may form in any artery, even the largest; hence there may ensue sudden arrest of blood, coldness of an entire limb, even gangrene. Sometimes these masses, smaller in size, are carried completely into the texture of an organ, and are there arrested, and give rise to many of those deposits often referred to " capillary phlebitis." We have had no hesitation in stating, dogmatically, these facts, as the independent observations of Virchow and Kirkes have convinced us fully of their correctness.

Arteries may not only be blocked-up by detached masses and coagula, but, as Dr. Kirkes has pointed out, smaller particles of fibrine detached from the valves of the heart may contaminate the blood, and give rise to symptoms indicating poisoning of this fluid. It is well known that obscure and dangerous symptoms of delirium, and then torpor and sinking, come on occasionally in the course of rheumatism, and it is not improbable that they arise in the way now pointed out.

A very interesting subject touched on by Dr. Kirkes, is the rapid cere-

* Op. cit. vol. i. p. 272.

bral softening which follows the obstruction of the middle cerebral artery. The mass detached from the heart, and passing up the carotid, tends to find its way more easily into this artery than into the anterior cerebral, the other branch of the carotid, because it is not only larger, but maintains more nearly the original direction of the vessel. If the middle cerebral artery is once blocked-up, all the parts to which it runs become deprived of blood, as its branches receive no anastomosing branch from any of the other divisions of the circle of Willis. Hence on this almost complete deprivation of blood, Dr. Kirkes believes that rapid softening occurs. Many arguments might be brought forward in support of this view, and we believe that not only can softening occur, when there is this sudden obstruction in an artery, but also in cases in which coagulation occurs from other causes, in the cerebral arteries.

We look forward to important results in pathology, from the facts now made known by Dr. Kirkes, and we shall watch with interest for his future communications on this subject.

XVIII. *On the Diminution of the Chlorides in the Urine, or their absence from that fluid in cases of Pneumonia, and on the Chemical Composition of the Sputa in that disease.* By Lionel S. Beale, M.B. Londin.

That there is a remarkable diminution, and even complete absence, of chlorine in the urine of pneumonic patients, was first noticed, we believe, by Heller. Subsequently as stated by Dr. Beale, Redtenbacher published a similar statement, and Dr. Beale has now, by an elaborate series of observations, added to our knowledge on a very interesting subject. Redtenbacher affirmed, from an examination of the urine in eighty cases of pneumonia, that at the period of hepatization, the chlorine was invariably absent, but returned during resolution. Dr. Beale, from analyses of the urine in eight cases, confirms this statement, and also adds, that the absence of the chlorine indicates a corresponding deficiency in the blood. During the course of pneumonia, the changes in the urine appear to be, from Dr. Beale's observations, as follows:—At first the urine contains a large quantity of organic matters, and a small quantity of fixed saline matters; after hepatization has set-in, the saline matters, and especially the chlorides, are at their minimum. As resolution advances, the organic matters diminish in amount, and the soluble salts augment; but of these the chlorides are the last to return. During the period of absorption, the quantity of chloride in the urine becomes often very considerable.

While these statements are, we believe, quite correct, as far as they go, we cannot help wishing that Dr. Beale had pushed his observations still further. We believe that while in pneumonia, as in all febrile affections, the proportion of soluble salts in the urine is greatly reduced, this is owing especially to the diminution in the chlorides and phosphates. The sulphates are, on the contrary, increased. This was noticed in pneumonic urine by Heller; and though in two cases of pneumonia, and in one of pleurisy, Lehmann states that he did not observe the same fact, it is doubtful whether his evidence is to be accepted, as he did not sufficiently regard the influence of diet on the sulphates when the patient had become convalescent. Allowance being made for this, it is probable that, even in Lehmann's cases, the quantity of sulphate eliminated was greater than that passed by these individuals in the state of health.

To return, however, to the chlorides, Dr. Beale very justly observes, that however remarkable may be their disappearance from the urine in pneumonia, it is not a peculiarity of this disease, but occurs in other cases. Heller and Tomowitz have noticed it in typhus; Heller in various other diseases. We have observed it in cases of rheumatism, although it does not appear to be usual.

At the time that the chlorides are absent from the urine in pneumonia, they are present in great quantity in the sputa. Dr. Beale's analysis of the pneumonic sputa is full of interest. In one case in which three analyses were made, the sputa were neutral on the fifth day, slightly acid on the seventh, and very acid on the eighth. Dr. Beale suggests that this may be owing to an excess of the pneumic acid of Verdeil, but there is at present no proof of this, and, in point of fact, there is no certainty that there may not have been a formation of acid after the expectoration had been discharged. On the fifth day, the sputa contained a large proportion of organic matter, insoluble in boiling water (albumen, &c.), and a smaller, but still a large proportion, of organic matter soluble in water (extractives). On the 8th day, the proportions were reversed; hence Dr. Beale surmises, that in the progress of disease the albuminous exudation may change in chemical character, and pass into the soluble non-coagulable substances classed together under the vague but convenient term of extractives. On the seventh day, the sputa contained sugar. The soluble salts, and especially the chloride of sodium, were in large quantity on the fifth day, but diminished greatly on the seventh and eighth.

During resolution, the chloride thrown out with the albuminous exudation is reabsorbed, and passes off by the urine. It is not impossible that this may become useful as a clinical guide in the treatment of pneumonia.

Dr. Beale examined, also, congested and hepatized lungs as compared with healthy lungs. We trust he will investigate, as soon as opportunity occurs, the statement of Verdeil, that the proportion of pneumic acid is as great again in hepatized as in non-hepatized lung.

The cause of the disappearance of the chlorides from the urine and their accumulation in the lung cannot, we think, be at present explained. To say that the chlorides are "attracted to the inflamed lung," is a mere translation of the statement of the simple fact. That the process is not necessarily allied to exudation into the lung is evident; whether it is constantly allied to exudation somewhere or not, time will show. Certainly the cases in which it has been chiefly observed—viz., pneumonia, pleurisy, pericarditis, and rheumatism—seem to indicate this.

We know, however, too little of these processes to venture on any close shifting of facts. Lehmann, in his instructive chapters on transudation and exudation,* has brought together a number of striking facts, which we trust soon to consider more at length. We will only remark now, that his observations seem to show that the phosphates and the salts of potash (the salts, viz., of the blood-particles) are more concerned in the formation of plastic exudations than the chlorides or the salts of the serum; and the pneumonic sputa have been too little investigated in respect of these salts

* Physiologischen Chimie, vol. ii. p. 300, and vol. iii. p. 125.

to permit us to discuss their genesis with safety. Dr. Beale suggests that a large proportion of chloride of sodium is present whenever cell-development is proceeding rapidly, but he properly refrains from pursuing the subject, as it would demand much wider treatment than the subject of his paper justifies.

In concluding this short abstract of a very important paper, we cannot help expressing our belief, that such investigations must add important facts to medical science, and that Dr. Beale bids fair to be a most successful labourer in this direction.

XIX. *On the Pathology and Treatment of Leucorrhœa, based upon the Microscopical Anatomy of the Os and Cervix Uteri.* By W. Tyler Smith, M.D., Physician, Accoucheur to St. Mary's Hospital.

This paper has been elsewhere reviewed.

XX. *On the Protection against Small-pox afforded by Vaccination, illustrated by the returns of the Army, Navy, and Royal Military Asylum.* By J. Graham Balfour, M.D., Surgeon to the Royal Military Asylum, Chelsea.

This is a short but very able statistical paper, compiled from the sources given above, and satisfactorily proving the immense protective influence of vaccination. All soldiers and sailors are instantly vaccinated on entrance into the service, if they have not been so previously. They are, of course, distributed over various countries, and are sometimes in places where small-pox is raging, yet it appears that in eight years, from 1844 to 1851, out of a total number of 1,125,854 soldiers, only 745 cases of small-pox occurred, or 66 to every 100,000 men; while among 363,370 sailors, there were 417 cases of small-pox, or 115 in every 100,000 men. The deaths from small-pox have been only 130 in nearly 1,500,000 men. Among the boys in the Military Asylum, who are all vaccinated, or have had small-pox, there have been only 39 cases and 4 deaths among 31,705 boys. It appears, also, that almost as many of these cases occurred in boys who had had small-pox, as among those who had been simply vaccinated. All the four deaths were in boys who had had small-pox. This last result is extremely important, and, as the author remarks, bears strongly against the advice lately tendered, that inoculation should be legalized.

XXI. *A case of Hæmorrhage from Inversion of the Uterus, in which the operation of Transfusion was successfully performed; with remarks on the employment of Transfusion generally.* By John Soden, F.R.C.S.

An apparently desperate case of sinking from profuse hæmorrhage after delivery was treated by transfusion; although apparently not more than an ounce made its way into the circulation, the patient was immediately revived and ultimately recovered. In connexion with this case, Mr. Soden gives a table of thirty-five cases collected from various sources, from which it appears that there were only seven fatal cases among thirty-six.

Mr. Soden used a simple apparatus, and did not defibrinate the blood.

The operation is certainly to be recommended in some cases, and Mr. Soden throws out an incidental suggestion of considerable value, that salines might be advantageously mixed with the blood. A little carbonate of soda would certainly hinder the coagulation, and might, perhaps, have a stimulating effect on the heart.

XXII. *An Analysis of one hundred cases of Cancerous Disease of the Uterus.* By Robert Lee, M.D., F.R.S.

This paper is reviewed in another part of the journal.

XXIII. *A case of Pulsating Tumour, in which the Urine contained Cancer-Cells.* By Charles H. Moore, Esq., Surgeon to the Middlesex Hospital.

A large tumour, lying apparently over the external iliac artery, and stretching on either side of that vessel, presented a marked pulsation of such a character as to lead to the probable diagnosis of aneurism. Although doubts were felt on the point, it was decided to tie the common iliac artery. After death, however, the pulsating tumour was found to be formed by a mass of enlarged and encephaloid glands—cancerous tumours were scattered about in other parts, and one of these joined on to and merged into the tissue of the prostate. In the urine after death " cancer-cells" were found.

Mr. Moore, in the course of some interesting remarks on this case, seems to think that marked pulsation in an encephaloid tumour is unusual. We are disposed to think it not uncommon. A short time since we had under our care a pulsating tumour of this kind on the dorsum of the ilium, which was diagnosed as either aneurism of the gluteal artery, pulsating tumour of bone, or encephaloid tumour. After death it was discovered to be the latter. In this case encephaloid disease of the kidney was correctly diagnosed before death; and in the urine, cells very much resembling cancer-cells were frequently seen. They did not, however, afford so much aid to the diagnosis as Mr. Moore supposes the cells seen in his case after death might have done, had they been detected during life, as it was found impossible to affirm that the cells seen by us were not epithelium from the pelvis. Virchow has also noticed the extreme occasional resemblance of pelvic epithelium to cancer-cells.

XXIV. *An account of a case of Biliary Fistula.* By G. Robinson, M.D.

A woman, æt. sixty-four, in a state of intense jaundice, presented a tumour as large as the fist in the epigastric region two or three inches above the umbilicus, which finally pointed, was opened, and discharged two quarts of mingled bile and pus. A fistulous opening remained, through which bile constantly passed, and the quantity of this was reckoned at eight ounces in the twenty-four hours. It appeared altogether to pass out by this channel, as the fæces continued always white. The patient died emaciated about five months afterwards; on examination, the ductus communis choledochus was found completely impacted by a calculus composed of inspissated bile; the liver adhered to the abdominal walls, and the fistulous opening passed into a dilated biliary duct. The biliary ducts and ductus hepaticus were dilated, but the state of the gall-bladder and of the liver-substance is not mentioned.

Review VI.

1. *Impfversuche mit dem Blute und den Ausleerungen Cholerakranker.* Von Dr. Joseph Meyer.—('Archiv.,' von Virchow und Reinhardt.) Band iv. 1852. pp. 29—54.

Inoculation-experiments with the Blood and Dejections of Cholera-patients. By Dr. J. Meyer.

2. *Charakteristik der Epidemischen Cholera, "Die Contagiositätslehre vom Standpunkte der Experimentalkritik."* pp. 77. Von Carl Schmidt.—*Leipsic*, 1850.

Characteristics of Epidemic Cholera. By C. Schmidt.

3. *Ueber den Durchfall der Kinder.* Von Dr. C. F. Eichstedt.—*Greifswald*, 1852. pp. 120.

The Diarrhœa of Children. By Dr. Eichstedt.

4. *Manuscript Notes of Experiments on the Communicability of Asiatic Cholera to Domesticated Animals.* By J. Marshall, F.R.C.S., &c.

By aid of the above-mentioned treatises, and of other materials now before us, we are enabled to take a rapid survey of the principal facts hitherto observed concerning the *natural influence* of cholera upon brute animals, and to give a sketch of the results of a large number of *direct experiments* on the possibility of communicating that disease from the human subject to those creatures.*

Many desultory attempts at inoculation were made in Russia, Gallicia, and at Warsaw, in the epidemic of 1831. A few experiments were performed by Magendie, at Paris, in 1833. Similar inquiries were instituted by Namias, at Venice, in 1836, and soon after by other Italian physicians—viz., Novati, Borsani, Freschi, Calderini, and Semmola. Lastly, the investigations of Schmidt, Eichstedt, and Meyer, and those undertaken by ourselves, were almost simultaneously pursued during the epidemic of 1848-9, in Dorpat, Greifswald, Berlin, and London.

Various animals, such as the dog, cat, rabbit, guinea-pig, goat, and common fowl, have been employed in these experiments, but the dog more frequently than any other. The method of inoculation has also varied. In some, the blood of cholera-patients, drawn during life, or the blood taken after death, has been introduced, with or without delay, into wounds in the cellular tissue beneath the skin, or directly into the veins of the animal operated upon; in others, the vomited or dejected matters have been introduced into the subcutaneous tissue, into the veins, or into the alimentary canal; lastly, animals have been made to breathe an atmosphere charged with the exhalations from cholera-blood and cholera-dejections.

It will be found, as we proceed, that the earlier experiments of the Russian, Polish, French, and Italian physicians were objectionable or fruitless; but, at least, we feel entitled to promise our readers, that those more recently performed have yielded more striking results, and possess certain claims to serious consideration.

* The term "Cholera," in this paper, unless expressly qualified, implies the Asiatic disease. The word "Contagion," is used in its generic sense, to include all possible modes of communication of that disease.

To some, indeed, it may probably appear that such an inquiry must be wholly unprofitable to medical science. The pure contagionist, for example, might object, on the one hand, that the failure to extend the cholera by direct inoculation from man to animals would avail nothing against his theory of contagion, since cholera may be essentially a human pestilence; and, on the other hand, that even the production of a like train of symptoms in animals, would not significantly add to the reliable evidence of contagion already derivable from observations on the onslaught of the disease upon communities of men. To the pure non-contagionist, also, this experimental inquiry may seem, *à priori*, to be utterly futile; for if genuine cholera did occur in the creatures submitted to experiment, might it not possibly be referrible to the epidemic influence prevailing at the time; whilst a negative result would but imperfectly support, by a questionable analogy, the views already deduced, in reference to man, as to the non-communicability of the disease from one individual to another? Lastly, by those who are induced to conceive that cholera, though essentially epidemic, may be contingently contagious, or essentially contagious while only contingently epidemic, each of the foregoing objections might in turn be urged against either positive or negative results.

But the subject cannot be so easily dismissed. All the points in dispute concerning the phenomena and laws of that wonderful disease—or, as its so fatal tendencies would almost justify us in calling it, that mysterious mode of death, which has been named Asiatic Cholera—are surrounded by so much obscurity, that little apology perhaps is due, even from those who seek by experiments the most eccentric, and by methods apparently the least promising, to discover some encouraging ray to guide us to their ultimate elucidation. The question of its contagion, for example, still rends the medical world unequally in twain; and if such multiplied observations as have now been made on the invasion and dissemination of the disease, in human families and communities, have resulted only in this division of opinion,—if the profoundest as well as the most superficial etiologists, after fluctuating in their belief, are still at variance with each other, repose in a state of compromise, or are wholly unsettled in their views,—any attempt to bring to bear upon the inquiry the powerful instrument of actual experiment may be welcomed, if for nothing more, at least for its practical as well as its scientific aim.

It is impossible to rid oneself of the conviction, that cholera has some definite agent, self-itinerant or conveyed, as its constant effective cause. Suppose this agent to be material, whether inorganic or organic, produced in one human body, and thence capable of reacting upon others, and of reproduction in them: or, suppose it to be material, inorganic or organic, and originating external to man; call it atmospheric, paludal, or terrestrial,—a miasm, an effluvium, or an epidemic germ; conceive it to be chemical, vegetable, or animal, or trace it to catalytic or zymotic forces; follow it from filth-spot to filth-spot; imagine it to be borne through the air, to float in the waters, to creep along the land, or to be transported by men; no matter whether, when once introduced into the body, it be capable of multiplication therein, and hence of subsequent propagation; or whether, incapable of such reproduction, its residual quantity, with exhausted but not extinguished power, be simply transmissible from the first affected to another person;—then on any one of these different hypotheses, experiments on the communicability or transmission of the disease to animals might lead to more accurate knowledge as to the mode and media of propagation of this unknown morbid agent, if not to the ultimate detection of its nature and its source. Suppose, however, that the cholera agent, though material, be non-reproductive and incapable even of transmission in any way, but that it is arrested, exhausted, modified, or destroyed, in its attack on the individual first affected by it; or, lastly, conceive it to be a dynamical agent or influence, either physical or vital, and of course incapable of self-multiplication or of reflected action,—then, the negative results of our experiments on animals would still have some scientific interest, and perhaps be suggestive of important subordinate considerations.

To the very obvious objection, that animals, even the nearest allied to man, differ so from him that no safe analogy can be drawn between them in regard to morbid phenomena, it may be responded, that the structure and economy of the higher animals render them liable to diseases, simple, contagious, or epidemic, more or less like those of the human species; that similarity of diet and community of abode especially approximate some of the domesticated animals to man; and that certain specific diseases, the catalogue of which future inquiries may extend, are really, though with difficulty, capable of transmission from one to the other, though they may undergo modification in their transference.

Freely admitting, then, the cogency of objections which we ourselves would be the first to urge, we nevertheless venture to maintain the importance of all such comparative pathological experiments, in which morbid agents of so great subtlety as the causes of specific disease, are studied, not only by the aid of microscopic and chemical analysis, which they appear to elude, but by their effects on an organized test, a vital reagent—viz., the *living animal body.*

I. *Natural Effects of Cholera on Animals.*

In briefly reviewing our accumulated knowledge of the reputed natural effects of cholera on the higher animals, we would, in the first place, dismiss, with the most passing notice, the numerous conjectures which are to be found in professional as well as non-professional records, as to the relation between the cholera and recent great and widely-spread epizootics, which have occurred, in almost every country, either before, concurrently with, or after the invasions of the human pestilence. The widest and best known of these have been either influenza-like, catarrhal, dysenteric, or pustular in their character, and are identical with visitations previously well understood. The pleuro-pneumonia which has raged on the continent, and, since 1838, in this country, as so fatal an epizootic, and the typhoid disease of modern date, cannot by any but the most rude analogy be associated with human cholera.

In the next place, we would advert, at somewhat greater length, but with equal reserve, to certain more partial or distinctly localized epizootics, which have been noted in districts or towns of India and Europe, contemporaneously with the human pestilence. Thus, in the Marquis of Hastings' army, in 1818, on the banks of the Sutlege, great numbers of oxen are said to have died, during the prevalence of cholera, suddenly, and from unknown causes. In the hilly districts, according to Chalmers, cholera being then very fatal in the towns, cattle died even in a greater ratio than the people. In Rajpootana, according to Ranken, camels, and especially goats, died of diarrhœas and other ailments; and, on the authority of Mr. Searle, the same thing occurred in the Madura and Coimbatore districts. The disease among the poultry, in Mr. Searle's compound, may also be noticed here: and we are informed, by an intelligent officer in the Indian army, that in 1832, at Rajahmundry, on the Godavery, all his ducks and geese died, with symptoms, which he and his native servants supposed to be truly choleraic. In Calcutta, in 1827, numbers of dogs died in the streets with choleraic symptoms; at Charcolly, also, fifteen-sixteenths of the dogs perished in the same way, during a second visitation of the disease; and at a later period, half the dogs in Madras died, with vehement vomiting, and purging. At Macassar, in Amboyna, about the year 1818, monkeys, dogs, and oxen perished under similar circumstances; whilst, quite recently (1849), horses died, as it was said, of cholera, at Penang, in great numbers. Merely glancing at the piscine mortality during the cholera period at Marienberg, which seems in one case to have been connected with the opening of a sewer into a pond; and alluding transiently also to the deaths of fishes in the rivers of Germany, and in the carp-ponds of the departments of the Seine and Oise in France, we find that at Astrachan, Moscow, and Warsaw, if not at Tagaurog, in 1831, many examples occurred of poultry and other animals dying with symptoms resembling cholera. So, too, during the epidemic in Bohemia, according to Nekola, dogs died in a few days, with loss of appetite and activity, diarrhœa, and convulsions. In many parts of Austria the same circumstance was noted; and in Dantzic, the cholera was ushered in by a canine epizootic of a choleraic form, so that dogs were said to be the first to indicate the approach of the disease. The Austrian reports particularly mention poultry, dogs, and cats; and the French report by the Royal Academy of Medicine, on the Russian and Polish epidemics, refers especially to the mortality amongst dogs. In 1832, cholera then raging in Paris, a sudden and singular mortality occurred amongst the poultry of the village of Choisi-le-Roi, situated on the banks of the Seine, about five miles *lower down* than Paris, though no cholera prevailed in the village at the time: the combs of the fowls attacked were cold and livid; a thready mucus appeared in the mouth and gullet; whitish liquid dejections were passed; the intestines were found red in patches; and the blood was thick and tarry. A similar epizootic was observed in two or three other villages in the department of the Seine, and also on the Rhone. Since that period, this disease of poultry has from time to time reappeared, but more particularly again in 1849-50, both in France and near Utrecht in Holland, during the last cholera epidemic. Investigations and discussions as to its nature have been held by MM. Renault and Delafond Alfort, and the former concludes, that the disease resembles cholera more than it does any other epidemic. It appears to be communicable by inoculation, and to affect rabbits also. In the winter and spring of 1831-2, a remarkable cholera-like epizootic, recorded by Professor Dick, occurred amongst cattle and horses, in the neighbourhood of Edinburgh, during the prevalence of cholera there, although it had somewhat preceded that disease in its earliest appearance. It is worthy of note, that the same epizootic, having apparently ceased during the cessation of cholera, reappeared near Leith at the return of cholera in that place. Swine and dogs did not seem to be affected. The reputed horse-cholera, at Denny, in Scotland, in 1832, may also be alluded to in this place. In 1832-3, cholera then prevailing, a porcine dysenteric epizootic is also on record in Ireland. In this category we would, lastly, include the unusual mortality, with more or less striking choleraic symptoms, amongst cats, goats, and dogs, mentioned in the journals of the time as having been noted in Malaga, in 1834, and also in Algiers, Tunis, Cairo, and Constantinople, during the last epidemic visitation of the disease.*

Now, in reference to the majority of the examples which we have thus collected together of *local* epizootic diseases, more or less coincident with and resembling cholera, it unfortunately happens that neither the symptoms nor the post-mortem appearances have been made the subject of accurate medical investigation. Hence we at once reject them as positive evidences of the effects of a cholera agent upon the animals concerned. And in regard even to those instances in which we have had the advantage of more precise medical observation, both before and after death,—as in the case of the poultry by Mr. Searle, and MM. Carrère, Renault, and Delafond; and in that of the epizootic described by Professor Dick,—instances which have been by some regarded as examples of cholera in animals,—we more than hesitate, on a careful consideration of the symptoms and morbid anatomy, to admit their sufficient analogy with the human disease. The weight of both veterinarian and medical authority inclines to this opinion, which is, moreover, strengthened, when we remember the liability of animals, and of the herbivora in particular, to colics, diarrhœas, and dysenteries, independently of the presence of cholera amongst men,—the tendency of the human mind to magnify the importance of merely coincident phenomena,—and lastly, the certainty, that if cholera had really reigned epizootically, as it has done epidemically, such powers over the animal kingdom would long ere this have been universally acknowledged.

In conclusion, however, whilst we maintain that the occurrence of epizootic cholera has not yet been satisfactorily demonstrated, we would draw attention to the circumstance, that as we narrow our field of observation—for example, from the animals of a continent or a country, to the cattle of a camp, the poultry of a village, or a compound, or the dogs of a populous city—we meet with a greater positiveness in the description, and a greater tendency on the part of those who observe and record, as well as of those who subsequently review the facts, to regard them as being more or less dependent on the same cause as that which produces the contemporaneous pestilence amongst men.

Let us now examine a few of the reputed cases of cholera more particularly described in animals. No instances, as far as we know, have been met with either in cattle, sheep, or pigs. From November to March, 1831-32, as recorded by Mr. Dick,† seven horses died, one after another, at long intervals, in from three to five hours, having had violent watery purging, followed by coldness of the mouth, lips, tongue, ears, and legs. The blood became thick and *treacly;* the intestines, after death, contained

* See Indian, French, Russian and Austrian reports: also M. Tardieu; and the Medical journals.

† Veterinarian, 1833, p. 144.

a whitish slime. In August, 1833, a zebra, in the Zoological Gardens, Regent's Park,* died in five hours, after copious whey-like purging of a yellowish tinge, great prostration, and coldness of mouth, muzzle, and limbs. On venesection, the blood proved thick and treacly. Intestines contained a similar whey-like fluid; blood black; urinary bladder not mentioned. In September, 1834, near Huntingdon,† a mare died after copious watery purging, much collapse, coldness, oppression of breathing, and suppression of urine. Two hours after the attack began, three quarts of thick blood were drawn. Sixteen hours after, the blood was too thick to run; the pulse quite imperceptible, the tongue and lips blue, and cramps came on. Post-mortem examination very imperfectly made. The editor of the 'Veterinarian' ventures to name the disease cholera, on the ground of its identity in many of the symptoms with the Asiatic form of that malady. In the harbour of Oporto, it is reported by Mr. Lardner, M.R.C.S.,‡ that a racoon, removed perfectly well from a healthy ship to another alongside, in which cholera was raging, and of which the hold was very foul, was seized, after a few hours, with vomiting and cramps, and died. In October, 1847, a horse is said to have died in twenty-four hours, of excessive watery purging; it had coldness and spasms, and the blood was tar-like.§ Two other similar cases, though not fatal, are recorded by Mr. Cherry.‖ It is stated by Dr. Schmidt, that he never knew, or heard from others, of the dogs or cats of families sick with cholera, being affected with the symptoms of that disease; and we believe that common experience in this country would coincide with that statement. But a very interesting account is given by Mr. Bevan, M.R.C.S., of St. Ives, Cornwall,¶ of a dog belonging to a poor woman, which, on the 14th September, 1849, cholera then prevailing, was

—"seized with sudden vomiting and purging. In a few hours the alvine evacuations as well as the fluid ejected from the stomach assumed the *rice-water* character of cholera discharges; all the visible mucous surfaces assumed quite a leaden hue; the dog died the next day, violently cramped, after twenty-four hours' illness. The woman wrapped the dog in flannel and kept him on her lap, and also put him in a bath. On the 16th, at 3 A.M., the woman herself was taken ill; by 7 A.M. was in a state of collapse; and at 4 A.M. of the 18th she died, having [as the writer states, we must remark] contracted the disease from her canine companion."

In reference to cats, we find it stated, that in the wards of M. Chomel, in the Hôtel Dieu, a cat, belonging to one of the *rêligieuses*, who at the time was daily witness of the cholera, died with symptoms of that disease; an opinion assented to by the medical men.** Dr. Sylvain de Barbe, of Ozouer-le-Voulgis (Seine-et-Marne), also relates, that all the cats at one farm, and about a dozen others at different houses, died after two or three days' purging and vomiting, during the prevalence of cholera in that place.††

Again, many examples of both dogs and cats dying rapidly with choleraic symptoms, after having partaken, of their own accord, of the oral or alvine evacuations of their sick masters, are recorded as having been noted in Gallicia.‡‡ Lieutenant K.'s dog, in Poland,‖‖ the case of a

* Youatt: Veterinarian, 1834, p. 427.
† Mr. Ball: op. cit., 1834, p. 548.
‡ Lancet, 1833, vol. ii. p. 301.
§ Lancet, 1849, vol. i. p. 267.
‖ Veterinarian, 1849, p. 533.
¶ Op. cit., 1849, p. 661.
** Gaz. des Hôpitaux, 1849, p. 231.
†† Idem, p. 297.
‡‡ Œster.: Med. Jahrbuch, Band xviii. p. 240.
‖‖ Ibid.

dog which fell under the notice of Otto, in the hospital at Breslau,* another case in a dog, most graphically detailed by Dr. Meyer,† and a fourth case, related by Dr. Sylvain de Barbe, are individual examples of the same kind, in which, as will be hereafter described more fully, the symptoms and post-mortem appearances were highly characteristic of true cholera, to whatever cause we may attribute them.

Although related by competent observers as very remarkable cases, approximating very nearly indeed in character to the Asiatic disease in man, and proving the liability of these animals to cholera of some kind or other, the instances above quoted in the zebra and the horse cannot be regarded as perfect examples of it. The scanty narration of facts, the omission of important information, the want of microscopic and chemical observations on the blood and evacuated fluids, and the occurrence of some of the cases during the absence of epidemic cholera, are sufficient to create doubts; whilst the similarity of all the attacks to one another, the liability of horses to colic, and the fact that even in man English cholera may assume most closely the features of the Asiatic disease, and yet not be due to the same causes, suggest the probability that these cases were produced by local circumstances, and not by the same widely extended agent as the human epidemic.

The symptoms and post-mortem phenomena, described as having been manifested by cats, and especially by individual dogs, so unequivocally and so intimately brought into relation with the human sick, wear a much more imposing aspect. Either these must have been cases of common diarrhœa, or of a simple reactionary diarrhœa and vomiting, produced by the swallowed cholera-vomits or discharges, or else of a *specific cholera*, which was transmitted indirectly by those fluids, or propagated by exhalations from the sick, or engendered in the animals directly by the prevailing epidemic cause. Finally, the case at St. Ives, apparently spontaneous in its origin, marked by the rice-water evacuations, and followed by cholera in its owner's house and person, is the most striking instance of all. Still, we have even here to lament the want of minute pathological research, and to remember the chances of a deceptive coincidence. Whatever be the explanation of these cases, we cannot fail to observe, that, just as in contemplating the phenomena of general epizootic diarrhœas, we found the limited pestilence of the camp, the compound, and the town (especially that amongst the dogs of Indian and European cities), arresting the attention of pathologists more than the wide-spread mortality of a continent, so here, in turning from the inhabitants of the pasture, the stable, or the stye, to the dog,—which shares in our food, participates in our habits, is domiciled in our houses, and will follow us to our hospitals,—is our companion in health, and our adherent in sickness,—we find much more frequent examples of and a much closer approximation to the symptoms and post-mortem appearances of cholera as it is recognised in ourselves.

It must not be forgotten, however, that in comparison with the millions of human beings who have been destroyed by this pestilence, few—how very few!—of even the most domesticated animals, have died under circumstances suggestive of an actual identity of cause. But are we right in demanding so strict a correspondence in the effects of any morbid agent upon animals and man? Is not a real influence on the former compatible

* Hering: Patholog. für Thierarzte, 1849, p. 369. † Op. sup. cit.

with many differences and peculiarities in symptoms and effects? Is no allowance to be made for generic or specific distinctions in the blood, the glandular apparatus, the general organization or constitutional idiosyncrasies of the animals in question? Have the manifestly imperfect observations hitherto made, in the urgency of more absorbing duties during so serious an epidemic, been so entirely negative as to justify a denial of any influence of the cholera agent upon animals at all; or is it possible that, with all their defects, they point to a future of clearer results? Can this agent, so powerful against man, be wholly inoperative upon brutes? If epidemic, would it not in some way occasionally assail them; if contagious, would it not sometimes overleap the difficulties of transference, overcome resisting susceptibilities, and operate on those creatures which are most in proximity to man?

Construing the literal facts, the scientific pathologist cannot positively assert that animals, even dogs, have yet been *proved* to be naturally subject to Asiatic cholera; but, influenced by general considerations (which can never be altogether lost sight of), conjointly with our hitherto imperfect data, we believe that it still remains an open question, pending the advent of a final solution, whether they are not, in proportion to their intimacy with man, occasionally subjected to the morbid influence of the special agent of that disease.

II. *On the Communicability of Cholera to Animals.*

In this division of our subject, we shall notice the accidental and intentional inoculations of the human subject with the cholera-fluids.

1. *Experiments with Cholera-Blood.*—Accidental punctures received during the post-mortem examination of cholera patients, which must have occurred to numbers of medical men, as well as in the experience of Stilon, M. Le Gallois, Moreau de Jonnes, Pirogoff, Schmidt, and ourselves, are not, so far as we can ascertain, followed by any peculiar mischievous results. Exceptional cases, such as that of Mr. Penman, of Sunderland, in 1832,* the only one we can find, may be due to general epidemic influence, or, if to contagion, to simple inhalation of the poison; so that, unless very numerous, and constant in their result, such cases are unimportant.

The inoculation experiments made on animals with cholera-blood taken from the *dead* body, are very numerous; and since undue reliance has been placed upon their negative results, we think it right to mention them here, although they are open to serious objection. Thus, Namias† took clots of blood from the heart, and inserted them beneath the skin of rabbits, closing the wound by suture. In many cases the animals died in from two to eight days, but not with symptoms of cholera. A clot from the blood of a rabbit already so destroyed, being in the same way introduced beneath the skin of another, also produced fatal results without cholera. If blood-clots from persons not dead of cholera were employed, the animals survived. In a subsequent series of experiments, it was thought by Namias that real cholera symptoms ensued; but this is more than doubtful, and, after still further trial, he himself hesitated to pronounce on the cause of death. The similar experiments of Novati, at first negative, afterwards gave results of a more positive kind. Those of Borsani, Freschi, and especially the numerous trials of Semmola, were also followed by negative results.

* Haslewood and Mordey, pp. 133–5.

† Omodei: Annali, Nos. 77–85.

Meyer did not employ cholera-blood from the dead, but he made some counter-experiments, by the same method as Namias had used, with blood-clots from the heart of a phthisical body, closing the wound in the rabbit's skin by suture. Of two animals, one died in thirty-six hours, the other in three days. He also put healthy rabbit's blood, twelve hours after it was drawn, under the skin of two rabbits; and blood from a pneumonia patient, sixteen hours after being drawn, in another. The wounds in the skin were closed with collodion as well as by suture, and the animals continued well. Hence, Meyer concludes correctly, that the fatal results of Namias' experiments were probably due to the state of the blood employed, to the amount of injury inflicted on the animals, or to the access of air to the wounds.

In August and September, 1849, we performed seven experiments with blood taken from the bodies of persons who had died in the collapse stage of cholera. The blood, diluted with about equal parts of distilled water, so as to permit the fibrin to be removed, was injected into the external jugular vein of the animals employed, from which a small quantity of blood was always first allowed to flow. The time after death at which the blood was taken, is stated in hours. The injection was always made within half an hour afterwards.

Experiments 1 and 2.—Blood $15\frac{1}{2}$ hours: 2 drachms into a kitten; $1\frac{1}{2}$ drachm into a rabbit. Both animals were seized with exhaustion and prostration, which lasted six hours. They refused food for thirty-six hours, and both were slightly purged on the next day. On the third morning they appeared well.

Exp. 3 and 4.—Blood $12\frac{1}{2}$ hours: 2 drachms into a large rabbit; 4 drachms into a dog. The rabbit became depressed, and took no food until the third day, when it seemed as usual; no purging was detected. The dog was languid for a few hours, was purged of a yellowish slime, but ate a little meat in the evening, and was well the next day.

Exp. 5.—Blood 5 hours: 10 drachms to a dog, which, as in the last experiment, became languid, and refused to eat until the following day, when he began to feed, but looked ill. Fæces as firm as usual.—N.B. A kitten on which 6 drachms of the same blood was employed, died instantly, from over-distension of the heart, as no air was found in the veins.

Exp. 6.—Blood $\frac{3}{4}$ hour: 6 drachms into a dog. This animal suffered likewise from depression, laid himself down, was purged during the first night, of black and white fæces covered with yellowish froth, and gradually recovered the following day.

Exp. 7.—Blood 20 minutes: $4\frac{1}{2}$ drachms into a dog. Symptoms similar, not so severe. Recovery on following day.

To these observations we attach only a small significance; for one could hardly expect less from the introduction of a like amount of dead blood, charged with the products of its own decomposition within and without the body from which it had been taken. Magendie,* Gaspart, Trousseau, Leuret, and others,† had already shown, that even ten or twelve drops of putrid animal matter, injected into a dog's veins, would cause prostration, excitement of the pulse, hard respiration, and a black or bilious vomiting.

Nor can we trace any choleraic symptoms to the accidental or intentional inoculations of the human being, with *fresh* cholera-blood drawn from

* Leçons sur le Cholera, 1832, p. 138, *et seq.* † Gaz. Méd. de Paris, 1849, p. 73.

living patients. Thus, a complete immunity from mischief was experienced after the contact of recent cholera-blood with wounds of the fingers, by Dr. Sokolov, of Orenburg, often whilst bleeding his patients; by Schmidt, who during an experiment had the blood in contact with a fresh wound for ten minutes; and by Dr. Molison, at Newcastle, who pricked himself with a lancet just used in a very decided case of cholera. The negative results of the self-inoculations of Dr. Foy, at Warsaw, of Dr. Jannichen of Dresden, and of MM. Veyrat and Pinel, are also well known.

Fruitless inoculations with fresh cholera-blood were made on animals at Warsaw.* Namias inserted perfectly fresh cholera-blood under the skin of two rabbits, in the way already mentioned, but without any evil result. Calderini inoculated a dog and two hens with cholera-blood yet warm, from a patient in the algide stage of the disease, also without any bad consequences. Eichstedt obtained no effects from the administration of some fresh cholera-blood to a rabbit by the mouth. Dr. Schmidt injected 13 grammes ($3\frac{1}{3}$ drachms) of fresh defibrinated blood of a cholera patient, (who had had diarrhœa twenty-four hours,) into the external jugular of a cat, which had already been confined in a box charged with the vapours of cholera blood and dejections. In two hours the animal ate, played about, and continued well until the fourth day, when it was let loose. No vomiting occurred; fæces natural. Meyer injected into the external jugular of a large dog, about two drachms of the fluid part of one ounce of cholera-blood, taken from a patient who had been seized early the same morning, and died twenty-four hours after. No alteration in the fæces, nor any other symptoms, followed during the next twenty-one days. Magendie injected various quantities of cholera-blood into dogs without specific effects; but he relates† that his prosector, M. Loir, having removed eight ounces of blood from a living dog, replaced it by the same quantity of human cholera-blood defibrinated. The dog died in eight hours, with symptoms resembling cholera; i.e., both vomiting and purging, but of what nature is not described. After death, the veins contained very black blood, and the intestines reminded him, in appearance, of those of a cholera body.

Our own experiments with fresh cholera-blood were six in number. The blood was, in all cases, conveyed in stoppered bottles, slightly diluted with distilled water, defibrinated, and injected, within twenty to thirty minutes of its abstraction from the body, into the jugular vein of the animal employed.

Experiments 1 and 2.—Blood from a patient, late in the algide stage; died soon afterwards. Nearly 3 drachms into a large rabbit: animal drooped for a few hours only; then recovered itself. 6 drachms into a small dog: prostration for rest of day; bowels slightly relaxed; fæces opaque white, covered with greenish yellow mucus; no appetite until next morning, when dog was hungry. Diarrhœa through the day, yellowish. On second day, fæces quite natural.

Exp. 3 and 4.—Blood from a patient only 6 hours after seizure; collapse slight; death 23 hours after. 3 drachms into a cat: animal became quiet and suspicious; fæces at first hard, afterwards relaxed, blackish, and mixed with mucus. Next day, well. 4 drachms into a large dog: no symptoms but those of temporary weakness and refusal to eat.

Exp. 5 and 6.—Blood from patient 10 hours after attack, 15 before

* Rapport de l'Acad. Paris, 1832. † Leçons, &c., pp. 126, 138, 158.

death. 3 drachms into a kitten six months old: only symptom, loss of appetite and prostration for some hours. 4 drachms into a dog: the same debility, indifference to food, and suspicious aspect, continuing for the rest of the day; fæces twice passed; very soft in the first night, the second more so than the first; recovery complete on second day.

Two counter-experiments may be added to these, in which 4 and 6 drachms of fresh human defibrinated blood, from a case of injury to the chest, and from one of pleurisy, were injected into the veins in two dogs, without other effects than those due to the operation: the animals ate food and appeared comfortable in half-an-hour; fæces quite natural.

Now, it is important to note, that in those experiments where considerable quantities of *fresh* cholera-blood have been used by injection into the veins, care has always been taken to remove the fibrine from it. In this way, the injury known to result from the introduction of pure human blood into animals of a different organization was avoided; for, as Bischoff has shown, this mischief is almost wholly due to the fibrine in the blood. In M. Loir's experiments only, if blood had not previously been removed from the animal, was the quantity large enough of itself to cause injury in the way pointed out by Dr. Blundell and others. Nevertheless, freed from these objections, as well as from those which belong to experiments made with blood taken after death, the results of inoculation with fresh cholera-blood are not very decided. We can scarcely feel surprised that in the accidental inoculation experiments on the human person mentioned above, and in the self-inoculations of M. Foy and his coadjutors, no result followed, and we confess that the importance attached to such negative results by Schmidt, seems to us much exaggerated; for a quantity of poison may exist in the entire blood, capable of killing, and then of transmitting a disease, and yet be inoperative in such small proportion as would be contained in a few drops of that fluid. The negative results of Eichstedt, Calderini, and Namias, may be due to similar defects as to quantity. Even to larger doses Schmidt denies any influence; but what are $3\frac{1}{2}$ drachms (the quantity he employed), or even 10 drachms, out of a total quantity of several pounds of blood. In M. Loir's experiment only was the quantity employed (8 ounces) large enough to avoid this source of fallacy; and in it, as well as in some of our own observations, cholera-like symptoms were produced by the injection of fresh cholera-blood. But even in these they are not sufficiently characteristic to prove the positive communication of the disease, for the symptoms may be referrible to physical or chemical alterations in living cholera-blood, and not to the presence of a contagious zymotic substance, or true cholera agent. At the same time, we are disposed to agree with Dr. Meyer, that it by no means follows, that a specific agent may not at some period or other, and especially at the beginning of the disease, exist in the blood. Hence, as indeed is also suggested by Dr. Meyer (who, we may remark, appears to be a decided contagionist), further inquiries are undoubtedly needed, on a greater variety and number of animals, to avoid the accidents of idiosyncratic or generic insusceptibility, and with larger quantities of blood, taken from a variety of patients, and especially at the very onset of the disease, during the preliminary stage of nervous depression, even anterior to the premonitory diarrhœa, to get rid of the chances of an occasional absence or dilution of the poison. The contagious substance, he argues, may exist in too minute a quantity to operate, in certain patients and at certain periods; as is illustrated by the observation of Ricord, that syphilis cannot be communicated by the specific pus, if that fluid be too much diluted with the urine; and also by the experiments of Hertwig on Rabies, and more especially by those of Viborg on Glanders—a disease which certainly requires (according to his investigations) a very large quantity of the morbid blood to propagate it.

2. *Experiments with Cholera Ejections and Dejections.*—There seems no evidence to show that the mere contact of the vomited and dejected matters with the skin, is productive of evil results to man. In the course of their ordinary duties, and in post-mortem examinations, the gastric and intestinal fluids must incessantly act on the skin of the hand, as of nurses, or even enter wounds and punctures in the fingers of physicians and surgeons; but no sufficient proofs of resulting evil have been recorded. Dr. Jenisch, a Russian physician, rubbed his upper and lower limbs with the vomited fluid, put on and wore for eight days the shirt of a Cossack just dead of the disease, and bedaubed his face with the cold and clammy sweat of a dying patient. He had already had cholera, and experienced no harm from his experiment. Dr. Sokolov, in Orenburg, saw frequently the vomited matters spirted into the face of mothers or nurses of the sick, without any consequences ensuing. By M. Foy, at Warsaw, the vomited matters were *tasted* with impunity, but not *swallowed* (as many have supposed, and repeated).* Drs. Jannichen, Veyrat, and Pinel, also confined themselves, and with safety, to tasting the gastric discharges; and, so far as we can discover, no experimenters have even tasted, much less swallowed, the alvine evacuations. Schmidt states, on his own knowledge, that a drunken man swallowed half a beer-glass of the vomited fluids, continued intoxicated, and was quite well afterwards.

Turning our attention from man to animals, we find several instances of inoculation experiments with the cholera dejections recorded by Namias, who introduced the fluid, by means of needles, under the skin of rabbits, but without effect. In Warsaw, also, the dejections had been introduced into the cellular tissue of animals with no results. We have ourselves injected the pure rice-water fluid (from the bowels, not from the stomach) into the jugular veins of dogs and cats. In all cases the fluid used was freed from the flocculi and other finer particles by repeated filtration under cover; it was generally free from feculent odour, and invariably had an alkaline reaction.

Experiment 1.—Fluid colourless, having a faint smell, passed 13 hours after seizure. 5 drachms injected, one hour after its passage, into a dog: lassitude, slight purging, for two days; fæces white, black, and greenish yellow, with slime of a leaden hue; appetite nearly as usual; recovery.

Exp. 2.—3 drachms of same fluid, half-an-hour later, into a cat: animal purged also; refused food until next day, when it seemed well.

Exp. 3.—6 drachms of fluid, colourless, smelling faintly, passed 10 hours after first attack, and injected into a dog three-quarters of an hour after it was passed: symptoms same as in experiment 1; but the dog was larger.

Exp. 4.—On a rabbit: fatal from accidental introduction of air: animal distressed exceedingly, and dead in 4 minutes.

Exp. 5.—Fluid colourless, but having a slight feculent odour when warm,

* Du Choléra-Morbus de Pologne. Paris, 1832, p. 3, *et seq.*

passed 5 hours after seizure. 5 drachms injected 40 minutes after into a dog: animal languid, distressed, and moaning, for many hours; fæces relaxed for two days; recovery on the third day.

After voluntarily eating the fluid dejections of cholera-patients, both dogs and cats were observed, in several parts of Gallicia, during the first epidemic, to die with choleraic symptoms.* In some cases, however, no harm resulted therefrom; and Schmidt also observes, that it is common to find dogs eating with impunity the vomits of their sick masters.† Four cases, to which we have already had to refer, may here be more particularly related. Lieut. K.'s dog, after eating a large quantity of the cholera-discharges evacuated by his master, died with symptoms and post-mortem appearances quite resembling those of cholera in man. The case recorded by Otto of Breslau, is one in which a dog is said to have followed his master into the hospital, ate of the vomit, taken cholera, and died. The facts of Dr. Meyer's case are these: about four o'clock in the morning, the master discharged, per anum, a fluid resembling chamomile tea, of which the dog, previously quite well, soon after partook twice; the dog then lay under the bed until two o'clock P.M., when his master died. Hereupon the dog smelt his master all over, and was afterwards—i. e., about ten hours after swallowing the dejections—seized with a vomiting of a whitish fluid, and purging of a highly-offensive thin mass. He moaned feebly, lay with his paws out stiff, and died at nine at night. At the post-mortem next day, a perfectly *rice-water fluid* escaped in quantity from his mouth, and also from the stomach, when it was opened. There was injection, both venous and arterial, of the peritoneal coat of the intestines; flocculent whitish masses covered the mucous surface, which exhibited a fine redness throughout from stomach to rectum. Peyer's patches were injected; many of the gland-capsules turgid and white. In the cæcum and rectum, the fæces were greyish-green; mucous coat also injected. Heart and veins contained blackish blood, entirely like that of human cholera-blood. State of the bladder not mentioned. In the fourth case, related by Dr. Sylvain de Barbe, a dog entered the house of a cholera patient, and licked up the vomited matters which were near the bed. Two days after, it became depressed after vomiting and purging, uttered cries, became stiff and cold, and died in forty-eight hours. Its young mistress, seventeen years of age, who had given it drink, had embraced it, and was licked on the face (perhaps on the mouth) by the animal, had cholera two days after, and died in eight hours.

Lastly, we find experiments, in which the vomited or dejected matters have been intentionally introduced into the alimentary canal of animals. In Warsaw and Gallicia, this was done in the case of dogs, cats, rabbits, and fowls, but with contradictory results.‡ By Eichstedt, leaves frequently moistened with the cholera dejections were given to two rabbits, which ate them greedily. The one, a strong animal, was taken with purging at night, had convulsions, and died next day. The stomach was full of food; the small intestines empty and contracted; the cæcum full of semifluid fæces; colon and rectum empty. Mucous coat nowhere particularly injected; blood not treacly; brain, cord, and membranes injected. The other rabbit

* Sup. cit.

† We do not remember to have heard of dogs eating the evacuations in this country. May we assume this to be owing to the better-fed condition of the dogs, and to the different habits of the people, as compared with those observed on the continent, and in India?

‡ Arch. Gén. de Méd., t. xxviii., Rapports; also Œster.: Med. Jahrbuch.

recovered after some days, but was evidently indisposed, and ate nothing. A tablespoonful of the watery dejections was given to a third rabbit without any effect. A common fowl ate greedily of some cholera evacuations; in two hours, it lay as if stupified, was roused only when touched, but could not walk for twenty-four hours: in a few days it recovered. Schmidt gave to a fasting cat, 30 grammes (about one ounce) of cholera dejections: it suffered no detriment, took its food as usual five hours after, and was well next day. Meyer made in all seven experiments of this kind. The dogs, whose evacuations had been watched several days previously, were kept fasting ten or twelve hours before the experiments.

Experiment 1.—Dejections passed at eight in the evening; patient died at night. 4 ounces of rice-water fluid, having a slight feculent odour, given to a dog, by mouth and anus. During the night, animal took no food; its bed was moistened either by urine or stools; next day he ate briskly, but died the following morning at six, without any action of bowels. On lifting him up, a *whitish fluid* gushed out of mouth; peritoneal coat of intestines pale, soft, and slightly injected; contents of stomach and small intestine a greyish-white mucus with epithelium; slight injection of mucous coat of duodenum; Peyer's patches especially marked, tumid, and many gland-capsules burst; injection and sugillation of large intestine, which contained pretty firm fæces. Blood brownish and clotted (not treacly, apparently). Kidneys congested; bladder not described.

Exp. 2.—Dejections of a patient in blue stage; had taken no remedies; died sixteen hours after. 1 ounce, almost like water, free from smell, given to a large dog by mouth. Animal ate and seemed well all day; fæces softer than before; in afternoon passed a thin, blackish fluid; at eleven in the evening was lying weak and moaning; died in the night, leaving no further signs of vomiting or purging.—*Post-mortem:* Muscles livid, containing black blood; peritoneal coat of intestines injected; in stomach a dirty-grey mucus; in small and part of large intestine, black fluid, consisting mostly of altered blood-corpuscles and epithelium; gastric mucous membrane red; punctiform redness of duodenum; Peyer's patches turgid; glands surrounded by red villi; mesenteric veins and right side of heart contained black, greasy blood; kidneys congested; in the bladder, a small quantity of urine, which coagulated with heat.

Exp. 3 and 4.—Dejections from a case slightly collapsed; offensive; patient died. 2 drachms to a dog one year old, and 1 ounce to another of same age. In the first, no effect produced. The other ate well, but for five days had repeated purgings of a thin, sweetish-smelling, black substance, like that in experiment 2.

Exp. 5.—Dejections passed twelve hours after attack; patient blue, died. 3 ounces, almost odourless, and filtered to remove flocculi, were given to the dog last mentioned, twenty days after the former experiment, which had caused him a five-days' diarrhœa. Next day, animal ate and was lively; at noon, he had a loose, yellowish evacuation, and vomited. In afternoon, a *whitish* mucous fluid was ejected from the mouth; soon after, a more watery, yellowish motion. No cramps, coldness, or pulselessness noted; observations were interrupted; dog died in the night.—*Post-mortem: Whitish* mucous fluid in stomach and intestines; spread also on the colon; loose, frothy, yellowish matter in the rectum only; appearances of mucous coat as in experiment 1; bladder contracted.

Exp. 6.—Ejections of a pulseless girl. 2 drachms, clear, watery, with a few black flocculi, given to a dog. No effect.

Exp. 7.—Dejections, fifteen hours after attack, rather collapsed. 7 ounces, of a dirty white colour, and a strong smell, given six hours after its evacuation, and when the flocculi had subsided, to a dog, partly by mouth, partly by anus. At six the next evening, animal had passed soft, yellow fæces, and vomited a whitish fluid mass, containing its food. Up to eight o'clock he had eaten nothing; copious, watery, yellowish stools passed; but these gradually assumed their ordinary character.

The results of our own experiments, three with the ejections, and six with the dejections, given as soon as possible after they were passed, were very like those of Dr. Meyer.

Experiment 1. Ejections twelve hours after attack; saline treatment: no calomel; death. 1 ounce, white, slightly acid, to a small dog at noon. Animal ate as usual; at six P.M. seemed languid; in the night passed urine and firmish fæces. Next day, ate very little food; had two loose, yellowish stools; passed some urine; next night and day motions firmer.

Exps. 2 and 3.—Ejections four hours after a severe seizure: no treatment up to that period; fatal. 10 drachms, slightly acid, to a cat. Animal as usual for twenty-four hours; then purged several times, until the third day. 5 drachms of same fluid to a guinea-pig. Animal continued well until next day, when it passed hard and loose fæces mixed, and appeared unnaturally quiet; afterwards recovered.

Exp. 4.—Dejections of same patient as in experiments 2 and 3, passed at same time. 3 ounces, pale-yellow, watery, smelling slightly, alkaline, containing a few flocculi, given to a white terrier at eleven o'clock A.M. Animal remained well all day, and ate bread and meat. In night, passed an ordinary, whitish, firm motion; then had loose fæces, black, yellow and greyish slimy substance mixed, four times up to the evening of the second day; ate food, but had become very thin. On the third day, at ten A.M., 3 ounces more of dejections, colourless, flocculent, slightly nauseous, from another patient, six hours after seizure, were given to the dog; he ate afterwards; in the night he passed pale-yellowish, frothy stools, and had vomited his food, and a whitish slimy mucus. During this day he ate nothing, was very thirsty, continued to be purged, at last of a *quite cream-like* substance; walked unsteadily, crept into a corner, and whined. No cramps, blueness, or coldness observed. In the night he died.—*Post-mortem:* Stomach contained 2 ounces of clear whitish mucus. Intestines contracted; containing the *same creamy substance* as that which had been passed; no coloured fæces. Stomach not very red; whole intestinal tract reddish; Peyer's patches very distinct. Blood dark and clotted; not distinctly tarry. Liver dark; gall-bladder with bile; kidneys dark; bladder contained 2 drachms of muddy urine. The creamy substance passed and found in intestines was neutral or slightly alkaline; diluted with water, made a *smooth* emulsion, like rice-water motions, but *without the flocculi;* contained epithelium, granular cells (mucous corpuscles?), amorphous matter, and fatty particles, mixed with traces of semi-digested starch-cells and muscular fibre.

Exp. 5.—Dejections, six, eight, and eleven hours after seizure of patients; rice-water. $3\frac{1}{2}$ ounces in all, given in three doses, at intervals of two days. Purging at first green, then whitish and frothy, then *creamy;* vomiting on fourth day. Death on fifth.—*Post-mortem appearances:* Stomach and intestines contracted; contents cream-like, free from bile; bladder empty; blood thick. Creamy substance like that in dog (experiment 4), but contained ova of an entozoon in moderate numbers; but of course no fragments of animal food.

Exp. 6.—Dejections, thirteen hours after seizure: saline treatment; calomel. 10 drachms to a rabbit. Purging for two days; recovery.

Exp. 7.—6 drachms of same to a guinea-pig, with the same temporary results.

Exp. 8.—Dejections, six hours and fourteen hours after seizure: saline treatment, and calomel. 4 ounces, clear, and allowed to settle, to a cat, in two doses, on two successive mornings. Vomiting, purging, and death. Fæces loose, blackish, then yellow, then *creamy or milky.* Microscopic characters as before; ova of entozoon numerous.

Exp. 9.—Dejections, all passed in blue stage. 12 ounces in all, having been allowed to settle, given to a goat, at three times. The only effect produced was softening of the motions; the animal ate well.

The subsidence of the epidemic put a stop to further experiments.

From this long list of facts what conclusions can we draw as to the existence of a specific contagious substance in the cholera evacuations? In the first place, whenever only small quantities of the vomited or dejected matters can have come into operation, as in the contact of these with the skin, or with wounds in the skin, or in the inoculation experiments of Namias with needles, or in the mere tasting of the vomited fluids by M. Foy and his coadjutors, entirely negative results have followed. As to the last-named experiments of M. Foy and others, they never can be of any value, for their consequences cannot be distinguished from the effects of epidemic influences, and they never can be sufficiently numerous to overcome objections on the ground of want of susceptibility. The non-occurrence of symptoms in the drunkard is not a fact of much importance; and as to our own experiments on the effects of the filtered dejections introduced into the veins, though they are interesting as showing what may happen in such cases, it must be remembered that the phenomena produced might be entirely dependent on simple and not specific poisoning. Lastly, we have to compare the cases in which the cholera evacuations were administered by the mouth or anus, by Eichstedt, Schmidt, Meyer, and ourselves, with each other, and with the accidental cases of Lieut. K.'s dog, and the dogs observed by Otto, Meyer, and Dr. Sylvain de Barbe. From these it certainly appears, that although when small quantities of the dejections were employed little or no effect was produced, serious consequences generally followed the administration of larger doses. Eichstedt refers the results in his experiments to a poison acting on the nervous system. Schmidt, who wrote, however, without a knowledge of Meyer's experiments, denies the influence of the evacuations altogether. Meyer, with whom our own observations would lead us so far to coincide, concludes that the cholera-stools are capable of producing vomiting of a whitish mucus, and purging of blackish or yellowish fæces; and that these symptoms are sometimes followed by death with asphyxia, and with post-mortem appearances, very much like those observed in the cold stage of cholera among men. In some fatal cases, we would add, the evacuations of the animals experimented on have a creamy or milky character. It will further be seen, that although the phenomena produced are generally proportionate to the quantity of evacuations administered, such is not always the case; a discrepancy which Meyer suggests may depend on idiosyncrasy of the animal, or more likely *on the dilution* or absence of the deleterious agent. The presence of flocculi in the evacuations employed does not seem necessary to the production of the morbid phenomena, for the most remarkable of these were produced by specimens containing none or very few. The results cannot be due to the inorganic constituents of the dejections, for these are innocuous, and Meyer prepared a solution corresponding with Wittstock's analysis, and gave it to an animal, without any bad results. Neither can much be attributed to the admixture with the evacuations of remedies given to the patients, for quite as positive effects ensued where no remedies had been employed. Finally, any supposed influence of coincident causes is rendered improbable by the number and agreement of the whole series of experiments. That some deleterious agent exists in the cholera evacuations is thus, we think, abundantly proved; we think, also, that the quantity of this agent may vary in different evacuations. But is it *peculiar?* is it *specific?* Let us here direct attention to a counter experiment of Meyer's and three others made by ourselves. Meyer introduced into the stomach of a dog an ounce of a highly-coloured bilious motion, passed from a patient who had had diarrhœa for three days, and who died three weeks after with *medullary disease* of the stomach and mesenteric glands. The animal next day had bilious vomiting and purging, and died in seventeen hours.—*Post-mortem:* Redness of the gastric and intestinal mucous membranes, especially of the villi; Peyer's patches evident; contents of intestines, yellow frothy mucus; liver dark; bladder contracted; blood dark. We ourselves gave a dog 6 drachms of a yellow, offensive fluid evacuation, from a patient suffering under cancer of the mamma. In about a quarter of an hour the dog vomited, drank freely of water, and was soon well. Ten drachms of a lemon-coloured, frothy, sour motion from a phthisical patient were given to another dog; efforts at vomiting began within five minutes, and were soon effectual; he shrank away, ate some meat, and was no further distressed. About 4 drachms of a yellow, frothy, sulphuret-smelling evacuation from common diarrhœa were given, diluted with equal parts of water, to the same dog, about one week afterwards, but he was speedily sick, and appeared to be then uninjured. In none of these cases were the fæces of the animal changed. These experiments, however, suggest, that the effects of the administration of cholera evacuations may be due, not to any *peculiar* poison, but to the mere introduction of some deleterious substance common to any evacuation, into the upper part of the alimentary canal. But there are several considerations to be remembered here—viz., the great difference between the disagreeable, feculent, and almost fetid dejections employed in the above-mentioned counter-experiments, and the rice-water discharges of cholera; the fact that the *vomited* cholera fluid, which would not, from its nature, be so repugnant to the stomach of animals, and which has been eaten by them frequently and voluntarily, has produced as positive results as those caused by the dejected matters; the more marked occurrence of an interval before the commencement of the symptoms produced by the use of the cholera fluids; and, lastly, the support given to inferences drawn from the experimental cases in which the cholera evacuations were employed, by the phenomena of the accidental cases recorded by Otto, Meyer, and Sylvain de Barbe, and by the case at St. Ives, Cornwall; unless, indeed, we regard all these last as examples of non-specific coincident diarrhœas. It is evident, however, that further and comparative observations are needed before we can positively affirm that the deleterious substance in the cholera evacuations is *peculiar* to them; although Meyer, indeed, perhaps too sanguinely, maintains that from the entire evidence this is probably the case. Until, however, this peculiarity be proved to exist, we are scarcely entitled to advance to the further question—Is this deleterious agent a specific poison capable of reproducing Asiatic cholera? in other words, Is it the cholera agent sought? Certainly, all the symptoms of cholera have not been produced by it; unequivocal rice-water discharges, blueness, coldness, cramps, tarry blood, and non-secretion of urine, are not yet in the catalogue of its effects; and so long as we know so little, as a ground of comparison, of the pathology of *natural* cholera in animals, we cannot draw safe conclusions from the phenomena produced by the administration of the cholera evacuations.

3. *Exhalations from Cholera Patients, their Blood and their Evacuations.*—It is scarcely necessary to repeat, in regard to cholera symptoms following ordinary exposure to exhalations from living or dead bodies of cholera patients, that unless they occurred very frequently, which is not the fact, they could not clearly be distinguished from those attributable to epidemic influences. Dr. Jannichen, M. Foy, and others, however, purposely inhaled the breath of cholera patients without harm. Drs. Deynert and Mavroyein did the same at Moscow, where also an attendant is said to have ridden for hours together in a close carriage with sick people, and yet escaped all evil consequences. (Zoubkov's Report.) Persons have slept in the cholera wards as well as upon the cholera beds (Searle and others), even with a pillow near them, still moistened with the vomited matters (Zoubkov), and have worn the linen of the dead (Jenisch and others), all with perfect impunity.

It is remarked by Schmidt, that he worked for hours in a warm atmosphere charged with the vapours from cholera-blood and discharges, whilst engaged in analysing these fluids, and yet without any inconvenience. Other experimenters must have had the same experience. Schmidt placed a cat in a ventilated box, having a false perforated bottom, under which he put 1 litre ($1\frac{3}{4}$ pint) of cholera dejections, and 30 grammes (about 1 ounce) of cholera-blood from a patient yet living, 30 hours after his seizure. No bad symptoms arose in the cat, during a 48 hours' confinement. Two subsequent trials with another cat, one of four days', the other of three days' duration, gave similar negative results. Just before the disappearance of the epidemic from London, we ourselves confined two rabbits in close hutches, with pans containing cholera dejections. The animals suffered in no apparent way.

The absence of effect in all the preceding cases, both in men and animals, suffices to show, that exhalations in small quantities from the breath, the skin, the blood, or the evacuations, are not deleterious; and that in larger quantities, injurious effects, if possible, may be counteracted by non-susceptibility. So far as animals are concerned, further experiments are necessary.

There is little doubt, that *if ever*, under circumstances of great concentration or otherwise, cholera be extended by contagion, it is probably sometimes communicated by emanations of some kind or other passing through the air, and acting on the gastro-pulmonary mucous membrane, reputed examples of which being very abundant, need not be quoted here. One set of facts should not, however, be altogether neglected—viz., those relating to the supposed infectious character of the clothes of cholera patients, probably soiled with the evacuations and vomits, charged with exhalations from the skin and lungs, and left to dry or to be packed-up without having been washed. We will not do more than allude to the many instances in which, during the progress of the disease across the waters of the Black Sea, the Baltic, the German Ocean, the Channel, the Mediterranean, or the Atlantic, cholera has been supposed to have been conveyed by means of the dried, packed-up, unwashed bedding or clothes of those who had died on board ship of the disease. These general statements, though confessedly resting, for the most part, on unscientific testimony, and generally explicable by reference to a prevailing epidemic influence, are sufficiently numerous to be at least impressive.* Of particular instances of a striking kind, we shall mention two. Fourteen days after the death of a person from cholera, at Leeds, a box, containing this person's clothes, was opened by a man, one evening, at Monkton, twenty miles off. The next day the man was seized with cholera, of which he died on the fourth day. A husband, ten months after his wife's death from cholera, at York, opens a drawer for the first time, which contained her trinkets and clothes. He wept over the cap in which she died; took cholera that evening, and died next day.†

To test this most important point we had projected some experiments on animals with linen supposed to be infected, or purposely charged with cholera fluids. We had also intended to try the condensed perspiration and breath; but whilst gradually advancing in our inquiry, the epidemic of 1849 happily ceased in London.

Having expressed our opinion freely on each set of facts, as they have now been brought before the reader, and having stated enough, we hope, to vindicate the importance of the inquiry, we refrain from the recapitulation of formal conclusions. We desire only to reiterate our belief, that, though short of actual proof, the evidence, both general and particular, accidental and experimental, is not unfavourable to the notion of a certain susceptibility of the dog, and perhaps, too, of the cat, to the influence of the cholera-agent, whatever that may be. In future experiments, therefore, the former animal should probably be preferred. Sufficient trials, we think, have been made of small quantities of cholera-fluids, and also of *dead* cholera-blood; but large quantities of the blood, and of the various secretions and excretions, taken at very early periods of the disease, have yet to be fairly tried.‡ So also should linen, saturated with the discharges and exhalations,

* Consult Van Swieten, Cullen, Meade, Pringle, and Lind. † Simpson on Cholera, p. 137–9.

‡ Parturient women, suffering from decided cholera, either fatal or not, have given birth to infants, which have been seized with that disease, *immediately* or after a *few hours*, and have rapidly died, (Lancet, 1834-5, vol. ii.; 1849, ii., p. 131 and p. 240.) As infants rarely take cholera, it would

and shut up so as to be allowed to dry spontaneously. Careful observations, moreover, should be made on the diseases of animals during the cholera visitations; and the whole inquiry must also be repeated by careful collateral investigations in ague, yellow fever, plague, typhus, marsh, and other fevers, and the exanthemata.* May we hope, when this is accomplished, patiently, honestly, and skilfully, the microscope and the test-tube assisting, that the mystery which now veils the causes of cholera and other cognate pestilences, may, to a certain extent, be cleared away? The presence of some material and communicable poison, as the agent of this disease, may or may not be thus established. If it be, it will no longer be impossible, as now, to discuss with advantage its nature, its properties, its seat, and its mode of extension. Particularly, we might learn whether it acts on the organism without entering the blood,—or passes into that fluid and is there destroyed,—or passes through it into one or more of the secretions, and in this way is simply transmissible,—or whether it multiplies or reproduces itself, on the surfaces, in the blood, or in the secretions eliminated from that fluid, and is thus a truly contagious poison.

In conclusion, we especially tender our thanks to Dr. Meyer for his vigorous and interesting essay, without the appearance of which our own imperfect and interrupted experiments, unless somewhat extended hereafter, would probably never have been published.

John Marshall.

REVIEW VII.

Du Prognostic et du Traitement Curatif de l'Epilepsie. Par TH. HERPIN, Docteur en Médecin des Facultés de Paris et de Geneve. (Ouvrage couronne par l'Institut de France, en 1850.)—*Paris*, 1852. pp. 622.

The Prognosis and Treatment of Epilepsy. By TH. HERPIN, &c.

THIS treatise gained, in 1850, a reward of 1500 francs from the Institute of France. When the author received back his treatise, he added to it such new matter as he had in the interim been able to collect, and published it. The great length of the work arises from the fact, that every case of epilepsy is detailed at length, and that several of these cases carry the history of the patients over many years. Altogether, sixty-eight cases of epilepsy are recorded, and on these the author bases his conclusions. A critical examination of the greater part of what has been written on epilepsy is added, and the opinions of others are compared with those independently formed by the author.

The nature of the work will be best understood by a statement of the manner in which the materials for it have been collected. From the year 1823 to 1837, during the first fifteen years of the writer's medical life, he had treated a number of epileptics in various ways, and with various success. Having

seem as if the disease had here passed through the blood of the mother; but we have excluded such cases from the text, since inhalation after birth, or epidemic influence, *may* have originated the malady.

* The inoculation experiments by Dedier and others in plague, by Guyon, Chervin, &c., in yellow fever, by Ceeley in small-pox, by Hertwig in rabies, by Viborg in glanders, by Auzias in syphilis, by Vicq d'Azyr, Renault, Delafond, and others, in various epizootics, are highly interesting; but the whole subject of the transference of disease from man to animals, from animals to man, and from one species to another, especially by means of large quantities of the suspected fluids, demands elaborate investigation.

observed that one special medicine, the oxide of zinc, appeared in many cases to be eminently successful, he determined to test fully the value of this remedy by employing it in a series of cases. During the succeeding ten years, from 1837 to 1847, he employed it in every case of epilepsy, and recorded with the greatest exactitude all the particulars. In this period he collected thirty-eight cases, and at the end of the ten years he analyzed them. He then selected a number of cases which had been treated prior to 1837, and having analyzed them also, he found that the conclusions drawn from the two series corroborated each other. He then presented the work to the Institute, and received a prize. During the time that the work was under consideration, he collected a third series of cases, and the results deducible from these were found to accord with those already made. These two last series are added together, and form a group of thirty cases.

The thirty-eight cases collected from 1837 to 1847 and which included all that were treated by M. Herpin in this period, are divided into four classes:

1. Three cases, which were lost to view soon after the commencement of treatment.
2. Cases which were cured, either spontaneously or by treatment.
3. Cases which were bettered.
4. Cases which were intractable.

The cures and ameliorated cases were, in many instances, kept sight of for years, so that the permanency of the improvement was decided.

The thirty other cases occurring before 1837 and after 1847 are subsequently detailed. As far as we know, so many valuable records of epileptic cases have never been brought together, and certainly so large a number have never illustrated the effect of a single remedy.

Although especially devoted to this therapeutical end, the work is nevertheless one of much larger scope, and the symptomatology and ætiology of epilepsy receive a full discussion. We shall abstract the most important parts of the chapters on these heads.

M. Herpin commences his work with a very interesting chapter on the method to be employed in estimating the value of a remedy. This we shall at present pass over.

After the narration of the cases, which occupies 287 pages, M. Herpin proceeds to the "estimation of the facts," under the following heads:—Causes; Date of the disease; Progress, and Nature of access of the attacks.

A. Causes of Epilepsy.

1. *Hereditary Causes.*

After stating all the facts which he has been able to learn respecting the health of the grandparents, the parents, and the other immediate relations of the patients, he shows that the difficulty of ascertaining precisely even these comparatively simple facts is very great. However, he considers that he has obtained, with sufficient accuracy, information of the health of 380 relatives of the 68 epileptic patients. The following table gives the number of attacks of cerebral disease among the relations:

	First Series. 38 cases. Condition of health determined in 243 relations.		Second Series. 30 cases. Condition of health determined in 137 relations.
Epilepsy	in 7 cases	....	in 3 cases
Insanity	18 "	....	6 "
Suicide	1 "	....	—
Melancholia	2 "	....	—
Hypochondriasis	1 "	....	2 "
Hysteria	1 "	....	1 "
Chorea	1 "	....	1 "
Sleepwalking	2 "	....	—
Nervous excitability	2 "	....	1 "
Apoplexy (and hemiplegia)	11 "	....	(?)
Cerebral softening	1 "	....	—
General paralysis	... "	....	2 "
Meningitis & chronic hydrocephalus	7 "	....	6 "
Mortal convulsions	1 "	....	—
Tetanus	... "	....	1 "

Such are the facts; but it is not very easy to know exactly what they are worth. Among the 243 relations of 38 epileptics, there were 7 epileptics,—but was this anything more than hazard and coincidence? Was the disease in these 7 cases instrumental in any way in producing the same disease in any of the 38 patients who were allied to them by blood, or did external circumstances produce the disease in ancestors and descendants, without aid from any bodily organization proper to each? To answer this question, it is necessary to know the proportion of epileptics in the population generally. M. Herpin decides, from some statements furnished by Rayer, that among the French population generally, the number of epileptics is as 6 in 1000 persons. If anything, this is over the mark; but this is an error on the right side. Now, among the 243 relations of the first series of 38 cases, there were seven epileptic persons, or at the rate of 29 per 1000, nearly five times as many as among the French population generally. Among the 137 relations of the second series of 30 cases, there were 3 epileptics, or at the rate of 22 per 1000, or nearly four times as many as among the population.

It appears, then, that among the relations of epileptics there is an undue proportion of epilepsy, which is equivalent to saying that the disease is connected with relationship of blood, or, in other words, is hereditary.

In the same way it can be proved, that insanity is also an hereditary cause of epilepsy. Among the population generally, the proportion of madmen is as 1 to 1000. M. Herpin, for particular reasons, elevates this figure in Geneva (where his observations were made) to 3 in 1000. Among the relatives of the 38 epileptics, the proportion was no less than 74 per 1000, or twenty-four times as many as among the population generally. Among the 137 relatives of the other 30 epileptics, the proportion of insane persons was 44 per 1000. It is, then, certain, that the families which are peculiarly liable to epilepsy are liable also to insanity; or, in other words, insane persons transmitted epilepsy to their descendants.

With regard to the other cerebral diseases, M. Herpin thinks that his figures are too few to allow him to draw any conclusions, except in the cases of apoplexy and tuberculous meningitis. The laborious observations of M. Marc d'Espine, to which M. Herpin has had access, prove that among 1000 persons dying in Geneva, 50 die from apoplexy (cerebral hæmorrhage?) Now among the families of the first series, only 45 persons per 1000 died from apoplexy; so that this malady would appear to have no influence on epilepsy. Among 1000 persons at Geneva, 38, according to Marc d'Espine, die from tuberculous meningitis. Among the 380 relatives of the 68 cases of epilepsy, 13 or 36 per 1000 died from this disease. So that tuberculous meningitis and epilepsy are not connected.

2. *Anatomical Causes.*

(*a.*) *Sex.*—Thirty-one cases were male—thirty-seven female. Most writers have stated that females are more subject to epilepsy than males. Some few have cited figures. J. Frank gives an account of 40 females and 35 males. Esquirol, 389 women, 162 men; Georget, 342 females and 160 males. But as the observations of Esquirol and Georget were made at the Salpêtrière and the Bicêtre, and as the whole number of patients is not given, there is some uncertainty about their statements. To estimate exactly this point, the number of males and females in the population of the district from which the diseased persons came, should be known.

(*b.*) *Stature.*—From a series of measurements of all the cases, it appears that individuals of small, are more disposed to epilepsy than those of large stature. This appears to be explained by the next observation.

(*c.*) *General conformation.*—Among the epileptics there was a great excess, as compared to the population generally, of rickety and feeble persons, and a deficiency of strong and well-built persons, so that M. Herpin concludes, that a marked deficiency of general development is an accompaniment of, or gives a predisposition to, epilepsy.

(*d.*) *Incomplete conformation of the Skull.*—In 3 cases of 68, the skull was compressed laterally, so as to form a ridge along the vertex. In the other cases, the skull appeared of good conformation.

3. *Physiological Causes.*

(*a.*) *Age.*—From a lengthened analysis of the influence of age, it is concluded that epilepsy is rarely congenital; commences occasionally during the first six months, and seldom during the second six months. The number of first attacks are about equal (the influence of population being equalized) in the periods from birth to 5 years, from 10 to 15 years, and from 15 to 20 years. In the period from 5 to 10, there is a singular diminution in the number of cases commencing at this time. After 20 years, the number of first attacks gradually declines; and in the period from 30 to 70 years, very few cases of epilepsy commence. After 70 years, there is an increase. Herpin's cases were all taken from among the middle and upper classes; but he proves, by reference to 100 cases collected by Leuret at the Bicêtre, that the same rules hold good among the poor and indigent.

(*b.*) *Temperament.*—Temperaments are so vaguely defined, and the comparative prevalence of each, in a given population, has been so little studied, that it is difficult exactly to estimate their influence. Nevertheless,

M. Herpin has not wished to neglect altogether this important point, and arrives at the conclusion (with Tissot, Esquirol, and Foville), that the nervous and lymphatic temperaments, separate or combined, are more frequently met with than others among epileptics.

(*c.*) *Dentition.*—Among 68 cases, only two commenced between six months and two years of age: this fact alone would prove that the influence of dentition has been overrated. Neither of the two cases showed any indications of anything wrong with the teeth.

(*d.*) *Puberty and Menstruation.*—Before puberty, the two sexes appear equally disposed to epilepsy, or, if anything, males are most predisposed. After fourteen years, however, there is a sudden increase in the relative number of females attacked, and this increase continues beyond the period of puberty, and lasts, indeed, throughout life. Dividing life into two periods, before and after fourteen years, the invasions in males commenced in 17 before fourteen years of age, and in 14 after it; in the females, the invasions began in 13 before fourteen years of age, and in 24 afterwards. Menstruation *per se* did not appear to have a marked influence; but this point will be again alluded to.

(*e.*) *Social condition.*—As far as can be made out from the small numbers available, celibacy in females is a predisposing cause; this does not appear to be the case in men. In one sex celibacy usually implies continence; in the other not.

4. *Hygienic Causes.*

By adopting the rule of M. Villermé, that in the same place the amount of fortune is in the same ratio with favourable hygienic conditions, M. Herpin finds that all classes seem equally predisposed—the rich do not escape more than the poor. In reference to the observation of Copland and Foville, that epilepsy is more common in the lower orders, he believes that this opinion has arisen simply from the fact, that among these classes the disease is more often avowed.

With regard to the effect of *onanism,* to which some writers have attributed a great influence, satisfactory investigations were made into 27 cases (in 11 boys and 16 girls), commencing between ten and twenty years. In only one case was the existence of the habit proved.

Drunkenness has been considered an incontestable cause of epilepsy. Leuret has even attributed as many as 20 cases out of 100 to this cause. In 35 persons, aged from 15 to 78 years, there were two cases of drunkenness; and in two others, the use of white wine, not to great excess, appeared to exert an influence on the invasion of the disease. These numbers are, then, so far, opposed to the notion that drunkenness is a very important cause, although it may have some effect.

Forced intellectual effort has been assigned as a cause. In one case, perhaps in two, this may have had some influence, but this is not certain. In four cases, moral depression and chagrin appeared to have some effect.

The influence of hygienic causes appears, then, to have been slight in these 68 cases.

Anterior diseases.—After an analysis of all the anterior diseases, it is decided that the convulsive affections of infancy and scrofula are the only affections which appear to have had an influence on the production of the subsequent epilepsy.

5. *Accidental and Determining Causes.*

In 22 of the 68 cases, the relatives attributed the first invasion of the epilepsy to special causes—viz., to fright in 10 cases; to profound chagrin in 6; to sudden emotion in 1; to abuse of white wine, insolation, and extraordinary fatigue, in 1. Only in one case was menstruation referred to as the determining cause.

With regard to the influence of menstruation on the attacks, it was found, by a very careful comparison of the attacks and the menstrual periods in five cases, that the catamenial periods were always marked by a double frequency of attacks, or of giddiness. This increase was most marked at the end, or after the completion, of menstruation.

B. Course of the Disease.

(*a.*) *Frequency of Access of the Fits.*—In one case there were 100 fits in twenty-four hours; in another, an interval of $4\frac{1}{2}$ years between two fits. These were the extreme cases. The frequency of the fits in all the cases may be stated in the following formula:—Taking only M. Herpin's 68 cases, it may be said, that of 100 cases of epilepsy, 22 will have one or several fits daily; 36 will have from one to six fits per week; 16 will have one to four fits per month; and 26 will have one to eleven fits per year.

(*b.*) *Succession of Fits.*—Two different modes of accession can be observed; the fits are isolated, or are grouped in paroxysms. 46 cases belonged to the former class; 15 to the latter; the 5 remaining cases of the 68 were single attacks. Occasionally these different modes of attacks were combined in the same patient.

Perfect periodicity was seldom observed. In half the cases, the intervals varied from 25 to 45 days. In five cases, the interval occupied a month; two of these cases were in men, one in a young girl, in whom the fit occurred half-way between the menstrual period; in two, in women at the catamenial periods.

(*c.*) *Duration of Fits.*—The most usual period was a single day, and in more than half the cases the attack did not last three days. In one case it lasted twelve days.

(*d.*) *Number of Fits composing a Paroxysm.*—The average number was 5; the greatest, 36.

(*e.*) *Period of Attack.*—The number of attacks during the day was twice as many as during the night.*

(*f.*) *Nature of Access.*—We shall not follow M. Herpin closely through this chapter, but select such points as appear to be original.

Premonitory symptoms were present only in 9 of 35 patients—i.e., scarcely in one-fourth. These symptoms were chiefly irritability, turbulence, sadness, depression, agitation, vertigo, headache, somnolence, general fatigue, dark areolæ round eyes, nervous tremors, hot skin, shiverings, nausea, and diarrhœa.

One of the most curious parts of the work is an examination into the so-called *aura epileptica*.

* On this point some doubt has been thrown by the observations of Everts and Van Leeuven; for an abstract of whose paper on Epilepsy we refer to the 'Chronicle.'

A certain number of cases, in which favourable opportunities occurred of studying the commencement of the attack, presented, in one or other part of the body, a kind of convulsive or tetanic contraction of muscles, which immediately ushered in the fit. In one case there was cramp of the fingers of the left hand, followed by closure of the hand and flexion of the arm; two or three cries were uttered, and consciousness was lost, just as the patient fancied that the same sensation of contraction was spreading from the affected arm to the whole body. In another case there was in the same way contraction of the toes, which was so painful as to produce cries; the contraction rapidly invaded the leg, the thigh, the abdomen, and the chest; the fingers of the hand on the same side were then attacked with cramp and painful sensation, which spread rapidly up the arm to the shoulder, the side of the neck, and the face, to the vertex; when it reached this point, consciousness disappeared. From these, and other cases equally carefully recorded, Herpin draws the conclusion, that "when the symptoms of the attack are not simultaneously produced, a partial tonic convulsion precedes the cry, the loss of consciousness, and the fall."

This convulsion, rapidly passing from one muscle to another, was the only phenomenon which Herpin could discover which seemed to approach to the *aura epileptica* described by authors. He therefore entered on a critical examination of authorities, for the purpose of knowing how the term *aura epileptica* originated, and what were the phenomena it was intended to denote. The result of the examination was not a little curious. We give here simply M. Herpin's statements, as we have not been able to undertake the laborious task of examining into their accuracy.

No mention of the aura is found in Hippocrates. Aretæus describes accurately the partial convulsions which often precede an attack of epilepsy, in a passage which Herpin states has never been properly adverted to by writers on epilepsy. The first mention of the aura is in Galen,* who, in mentioning two cases in which the attack began by some kind of sensation in the extremities, which spread upwards, says that one of the patients described it as a sort "of cold vapour;" whereupon Pelops, his master, surmised that it was the propagation by continuity of some unknown alteration, or the transmission of some vapour or poison. M. Herpin shows, by a critical examination of this celebrated passage, that the original expression, *aura epileptica*, was a comparison, and that the remark of Pelops was an hypothesis. He shows also, that while Ætius merely copies Galen, Alexander of Trallus accepts the "cold vapour" as a reality, and Actuarius joins to it the hypothesis of Pelops, and speaks decidedly of a kind of epilepsy in which "a cold and corrupted vapour" passes up towards the head. It is, then, evident that the comparison of a sensation to a cold vapour became converted, in the hands of the successors of Galen, into an actual and potential aura. The Latin writers contain nothing on the point. Passing to the writers of the 16th century, Herpin points out that the commentators on Galen, and those who copied from them, handed down this phrase, until the existence of an aura seemed to be accepted as an ascertained fact. We shall not follow our author through his long analysis of the writers of this and of the succeeding centuries, down to the present time, but shall merely say, that if he has cited all the passages referring to

* De Locis Affectis, lib. iii. cap. 7.

the *aura epileptica*, he has made out his case—viz., that a term used on one occasion to describe a sensation has been converted into the expression of a supposed frequent phenomenon. He also shows, clearly enough, that when the aura is described from personal observation, some kind of cramp or convulsion is narrated. During the present century the term *aura* has been less frequently used, and its place has been taken by more exact descriptions of tremors, painful impressions, spasms, cramps, and convulsions. The closest observations have been carried on at the Bicêtre, Salpêtrière, and at Charleston, but any sensation corresponding to the "aura frigida" of Galen has been vainly sought for. M. Herpin finally decides that the aura is merely the first convulsive manifestation of the attack, commencing in some point distant from the brain.

The question thus raised is certainly extremely curious, and we have no doubt that the description of aura given by certain modern writers is merely an imaginary translation of Galen's term. Yet we think that the point is worthy of further investigation, especially as to the nature of the sensations felt just before the attack by epileptics, and the connexion of these with muscular contractions.

The cry is the second phenomenon of the attack. Herpin believes it to be an expression, partly of pain arising from the convulsion, and also in part, as surmised by Beau, of surprise. The cry occurred sometimes in more than half the patients, but was constant in only one-third. The fall is the third symptom of the invasion, and its suddenness appears to be generally in the ratio of the violence of the fit.

The author then describes very carefully the phenomena of the fit, and the nature of the access, but it is unnecessary to follow him through these chapters.

After the description of the fits, our author enters on the important question of

C. The Prognosis of Epilepsy.

In order to estimate fairly the value of treatment in any disease, it is necessary to know what is the necessary and natural course of the disease. In order to say that a treatment is useful, it must first be affirmed that the course of the disease, left to itself, would be towards other and less favourable terminations. It is, however, hardly possible to obtain this information, and therefore the efficacy of medicines must be inferred from a comparison of various modes of treatment, and from a record of such cases as have been left altogether untreated.

The prognosis of a disease left to itself cannot, then, be absolutely fixed; still some data can in most cases be collected.

The prognosis is general or special; that is, we judge of the chances of a given disease having such and such a termination because it is the character of such disease to follow this course, or we judge of its tendency in a particular patient whose condition of body may influence the usual course of the malady.

The *general prognosis* of epilepsy, in cases under treatment, has not been fixed with accuracy. That it is an extremely obstinate disease, *opprobrium medicorum*, is a matter of notoriety. But when we come to a numerical statement of incurable and curable cases, we find the materials very scanty.

In considering this point, Herpin proposes some questions which he seeks to solve by a critical examination of many of the ancient writers, and of the great majority of French modern authors; he scarcely refers to English or German physicians. These questions are—

1. Can spontaneous cure of epilepsy occur; and if so, in what proportion?

Few data are extant for answering this question. All authors agree that cases occasionally recover without treatment; and, chiefly from the observations of Maisonneuve, the number of spontaneous cures is fixed by Herpin at about 4 per cent. His own numbers give 3·64 per cent.

2. Has art any influence over this disease; and if so, what are its limits?

On this point, the opinions of authors are widely discrepant; some practitioners of good repute have asserted that half their cases have been cured; others, of equal authority, have denied altogether the beneficial influence of treatment. Herpin, unable to find any stable ground for argument on the recorded experience of others, enters on an analysis of his own cases, and draws the following results. Excluding all cases about which any doubt exists, and those which were lost to view too soon, there remain fifty cases. These give the following proportion:

Cured by medicine	54 per cent.
Ameliorated by medicine	21 „
Incurable	25 „

Now, since the number of spontaneous cures is supposed to be only 4 in 100, it follows, that the effect of medicine in epilepsy is really very considerable. In fact, Herpin's average of cures is equal to that of the most favourable statement yet made.

How is it, however, that these results should be so different from those announced by many excellent modern observers? Some reasons for the discrepancy will be found in a consideration of the

Special Prognosis.

The general prognosis is determined chiefly by the numerical method; the special demands other means of investigation. "In the one case, we count; in the other, we consider." The general prognosis is liable to all the fallacies which necessarily attend a method which forms general rules on mean quantities, and which arbitrarily presumes all the units to be equal.

The special prognosis is established by a consideration of the following particular circumstances, which may be supposed to affect it:

1. The hereditary nature of the disease.
2. The sex.
3. The stature.
4. The conformation.
5. The age at the commencement of the disease.
6. The temperament.
7. Menstruation and pregnancy.
8. Marriage.

9. The mental state.
10. The degree of worldly ease and comfort.
11. The age of the disease.
12. The frequency of the fits.
13. Their return by day or night.
14. The nature and number of the fits.

Each of these circumstances is examined thoroughly by the aid of the sixty-eight cases, and the opinions of writers are also alluded to and discussed at the same time.

We are, of course, able to give only the broad conclusions; but these are most interesting.

Contrary to general belief, hereditary *is more easily cured* than acquired epilepsy. In forming, then, the special prognosis, the hereditary nature of the disease in the patient under treatment should be no element in the calculation, or should be a favourable one.

Females are more easily cured than males.

Stature has little effect.

Conformation has a great effect. All the patients who were finely formed (*belle conformation*) were cured or relieved; those of good (*bonne*) conformation, were also more frequently cured than those who were badly formed. Vicious formation of the head is very unfavourable.

Age has a great effect. As stated by Posidonius, the prognosis of epilepsy is worse when it commences in youth than in old age. The prognosis is most favourable when the disease commences after fifty years; then from ten to twenty, then before ten; and it is most unfavourable when the disease commences from twenty to thirty years. This point is so important that we annex the table:

Age at commencement.

	Cured.	Relieved.	Incurable.	Total.
From 0 to 10 years	5	6	4	15
„ 10 „ 20 „	13	3	4	20
„ 20 „ 50 „	5	1	3	9
„ 50 „ 80 „	5	0	1	6

With regard to temperament, the cases are too few for any decision.

Contrary to the general opinion, Herpin's cases have not presented a single case benefited in any way by the appearance of the menses. Herein he agrees with Tissot, but differs from numerous other writers.

Mental activity appears to have some influence. Epileptics of feeble intelligence are seldom cured; those of great mental power are decidedly more amenable to treatment.

The effect of worldly ease and comfort is not marked.

The duration of the disease has greater influence. The chances of success are very great when treatment is commenced within three weeks; they diminish afterwards,—yet cases are cured at all periods.

The frequency of the attack has some influence on the prognosis; infrequency of fits is, to a certain extent, a favourable sign.

The return of the fits by day or by night appears to have no influence. The nature of the fits furnishes, also, no general rule for the prognosis.

It follows, then, that, so far, the prognosis of epilepsy in any special case can be scarcely determined. Some conditions are more favourable than others, but none give us a safe estimate of the chances of cure.

There is, however, one other possible element—viz., the number of the fits. When a patient comes under treatment, is it of any importance to the prognosis to know how many fits he has had? M. Herpin's cases prove it to be of the most signal importance: the following table will show this at a glance. In fifty-eight cases, the number of attacks were known. Of these the results were as follow:

	Cured.	Relieved.	Incurable.	Total.
Fewer than 100 partial or complete attacks	33	6	6	45
Between 100 to 500	5	0	3	8
Beyond 500 attacks	0	0	5	5

That is to say, among those patients who had not had 100 fits, M. Herpin cured no less than 73·33 per cent.; in those who had had more than 100 attacks, he cured only 62·5 per cent.; and in those who had had 500 attacks he cured none. This appears to be a most remarkable and unexpected result, and one which must be tested by future observation. Herpin sought to control it by the experience of writers, but he found that, in spite of the number of published cases of epilepsy, they had not been so carefully recorded as to permit him to compare them with his own cases. Yet his own observations are sufficiently numerous to lead to the belief that the rule here pointed out is really a very valuable one, and that, regard being had, of course, to exceptional cases, a favourable prognosis may be given when the fits have been few, and an unfavourable one must be given when they have been numerous. The various other circumstances previously discussed may also be taken into account in each case, and may be used to modify or strengthen the inference drawn from the one grand criterion now indicated.

This tremendous disease, this opprobrium of medicine, of whose treatment so many able physicians have written despondingly, appears, then, in the hands of our author, to be an affection infinitely less incurable than many others, with the results of whose treatment medical men are better satisfied. Nor, on looking carefully through the evidence, do we think it possible that M. Herpin has deceived himself. His observations have been made with the greatest accuracy, and extend over a period of many years. They include all the cases presented to him in the course of a long and busy life. In a word, the facts brought forward are more numerous, and apparently more accurate, than those in any other book on epilepsy that has ever been published. The final conclusion is, that of all cases of epilepsy, half are cured, and that among patients who have not suffered 100 fits, no less that seven-eighths are cured. If this be correct, it is an incontestable testimony to the power of the medical art, since of cases left to themselves, not more than four in 100 recover spontaneously.

We come now to the last and most important chapter in the book—viz., the details of

D. The Treatment.

In attempting the cure of epilepsy, the greatest perseverance and patience are necessary. Both patient and physician must prepare themselves for a long and tedious course of treatment, and no apparent failures must tempt them to turn their faces away from the path they have entered. The remedies which were employed by M. Herpin, in fifty-five cases which underwent full and prolonged treatment, were as follows: the oxide of zinc; the ammoniacal sulphate of copper; valerian, and the valerianate of zinc; an umbelliferous plant, the Selinum palustre; the Artemisia vulgaris; the powder of the mole-cricket;* ammoniacum, and the horse-radish. Of these remedies, the salts of zinc, the ammoniacal sulphate of copper, and the Selinum palustre and valerian were found to have the best effects. The following is the summary of the results of each remedy:

The oxide of zinc was employed in forty-four cases, but two of these have to be eliminated, as other remedies were used. Of the remaining forty-two there were

		Cured.	Incurable.
Cases which had had under 100 fits	31 ...	26 ...	5
" 500 fits	5 ...	2 ...	3
" more than 500 fits	6 ...	0 ...	6

The oxide of zinc was commenced in quantities of from six to eight grains daily, given in divided doses, one hour after each meal. It was augmented every week by two grains daily, until a dose of forty-five grains was reached. It was then administered in the same dose for three months longer. It was given, of course, in proportionate doses (one grain per diem), to children under one year, and was gradually proportionably increased. At first it sometimes produces nausea and malaise, but these symptoms soon pass off. With regard to the time when the remedy may be supposed to have had a fair trial, the following rules are laid down :—In children under one year, a total quantity of seventy-five grains should be administered before the remedy is given up. In cases over two years, and in which the prognosis is only moderately favourable (cases of more than 100 attacks), as much as four ounces must have been given during the whole period of treatment, before another remedy is had recourse to.

After the cessation of the fits, the remedy must not be discontinued, as a relapse may occur. As much zinc should be given *after* the cure as has sufficed to make the cure, in order to guard against a return.

The ammoniacal sulphate of copper has been less used. Twelve cases were treated with it.

		Cured.	Incurable.
Cases in which the prognosis was favourable (under 100 fits)	6 ...	4 ...	2
Cases in which the prognosis was less favourable .	1 ...	0 ...	1
Cases in which the prognosis was unfavourable .	5 ...	0 ...	5

The dose of the copper is, to children of five years of age, one-tenth of a grain daily, in divided doses, augmented gradually to one-quarter of a grain daily. In older persons, proportionably large doses must be given. The physiological effects of the copper are, gastralgia, anorexia, nausea, borborygmi, and diarrhœa. It must be continued for some time; and for

* The mole-cricket was introduced into medicine about 100 years ago, when there was a kind of rage for using animal medicines. It has been occasionally employed since, and has sometimes been found of use. Herpin's cases, however, do not at all prove the existence of any curative power. In his hands it appeared inert.

an adult at least seventy-five grains should be altogether taken before it is given up.

We need not occupy space with a detail of the result of the valerian and the Selinum palustre; both were successful in some cases, and, considering the small number of cases in which the latter was employed, the proportion of cures was larger even than those following the use of zinc.

Besides these remedies, Herpin alludes, in an appendix, to the employment of nitrate of silver, which some of his colleagues have found very useful, but which he has not tried; partly because he wished to try fully the effect of the oxide of zinc, and partly because he feared the effect on the skin. He also alludes to the occasional remarkable effects of belladonna; and a recent case is referred to in a note, in which a girl who had had more than 1200 fits, was completely cured by the union of zinc and belladonna.

Since these observations were published, we have not had time to test thoroughly the statements relative to the oxide of zinc, except in one case; in which, although age, conformation, and the number of previous fits, led us to a favourable prognosis, the oxide of zinc, given rigorously in the method of Herpin, produced no effect. We observe that Marshall Hall, our great authority in epilepsy, speaks unfavourably of this medicine. We commit, however, M. Herpin's facts to the consideration of our readers, with the assurance that the amount of utility of this vaunted remedy will soon be satisfactorily determined.

E. A. Parkes.

REVIEW VIII.

On the Pathology and Treatment of Hysteria. By ROBERT BRUDENELL CARTER, M.R.C.S.—*London,* 1853. 12mo, pp. 161.

IT is refreshing, in the midst of the crowd of treatises upon female complaints of various kinds, that tends to heap the reviewer's table with a pile of books, in which the old and vapid generalities, expressive rather of our ignorance than of our knowledge, are served up with a seasoning of crude novelties adapted to the prevailing taste for positive facts and local treatment,—it is refreshing, we say, to meet with a book whose author has obviously thought out its subject for himself, and is actuated by the simple desire to place before his professional brethren the deductions of sound reasoning, based upon the results of protracted and careful observation. Although Mr. Carter cannot himself lay claim, we believe, to any length of experience, he presents himself as the expositor of the doctrines of the late Mr. Mackenzie, of Leytonstone, who was extensively known for his successful treatment of a certain class of the most inveterate hysterical disorders. It had long, he informs us, been the intention of Mr. Mackenzie to publish his own results; but this intention was frustrated by his sudden and untimely death; and Mr. Carter, who was well acquainted with his opinions, who had continually witnessed and assisted in his practice, and who has adopted both the former and the latter, —has determined to put these doctrines forth in his own mode, in order that Mr. Mackenzie's results might not be lost to medical science (he having left no papers behind him); and in so doing has exercised, we think, a wise discretion.

We are not about to enter upon a discussion of the whole subject of Hysteria, for this our limits entirely forbid; nor (for the same reason) shall we even notice the history of opinion as to the nature of the disorder, to any greater extent than will be absolutely requisite for the comparison of Mr. Carter's doctrines with those previously entertained. The spirit in which he enters upon the discussion is a most laudable one, and cannot be too strongly enforced on medical inquirers generally, although it is peculiarly needed by those who devote their special attention to the Neurological department of pathology. "Ignorance," he remarks, "is often veiled under a disguise which imitates, in some degree, the appearance of knowledge; and a phrase representing only the algebraic x is considered explanatory of the phenomena ranged under it. . . . . The inquirer has no safety except in the framing of exact conceptions, or in the distinct avowal of ignorance on those subjects which have not yet been thoroughly explained."

Although every medical practitioner has a notion, more or less defined to himself, of what constitutes Hysteria, yet it is extremely difficult to frame a good definition of the disorder; and feeling this difficulty very strongly, Mr. Carter has fallen back on its first manifestation in the ordinary hysteric fit, as affording the best diagnostic characters. In this we think him wise; for although in practice this test may not be found universally applicable, yet we believe that in those perplexing cases in which some one of the secondary manifestations of this proteiform disorder simulates a more serious disease, the evidence afforded by antecedent paroxysms of primary or ordinary hysteria is of essential value, where it can be obtained. Mr. Carter regards this primary convulsive affection as "the essential characteristic of the disease," and looks at all other phenomena manifested during its course as "non-essential and secondary." And we believe this to be true of hysterical disorders in their pure or special form. But every medical practitioner must have met with cases, in which the symptoms of other diseases present themselves under a kind of exaggeration which may be appropriately termed hysterical, as arising out of the same mental and bodily condition as that which constitutes ordinary hysteria, although not existing in a degree sufficient to produce its independent development. In this class of cases (to which we have not found any distinct allusion in Mr. Carter's treatise) we are by no means satisfied that a primary hysterical paroxysm must be always looked for, any more than a regular attack of gout must be regarded as the test of the gouty diathesis. There is no difficulty, however, in bringing these also under the general statement which Mr. Carter regards as expressive of all forms of true or ordinary hysteria—namely, that it is essentially a manifestation of *emotional excitement*, not *depending upon* any local affection, although liable to be *modified in its symptoms* by the condition of particular organs. To this fundamental proposition we are much disposed to assent; and believe that, in thus fixing attention upon the *mental* condition of the patient, as of the first importance in the treatment, Mr. Carter expresses the conclusion at which most intelligent practitioners have arrived, who have made themselves fully acquainted with the disease in its ordinary and extraordinary forms. In a case that was under our immediate observation for several years,—in the course of which almost every kind of cerebral and spinal disease was simulated by turns, life being several times endangered by the fixation of the respiratory muscles in the pseudo-epileptic paroxysm, and the symptoms being always aggravated at the catamenial periods,—we distinctly traced the origin of the disorder to disappointed affection; and its favourable termination was as distinctly traceable to the formation of a new and reciprocated attachment. The dysmenorrhœa remained behind, and continued to be a source of great suffering, even after marriage, until relieved by a cautious dilatation of the os uteri, which was soon succeeded by pregnancy. Now, in this case, the intellectual powers were highly developed, and the will was almost masculine in its vigour; but the emotional part of the character was also developed with such peculiar intensity, as to exert a most undesirable supremacy, until it found free vent in the natural channel afforded by married life. It is worthy of remark, that this individual had exerted her very strong will to the utmost, to repress all manifestation of her mental disquietude, and had succeeded in doing so for a considerable period, during which her position demanded an extraordinary self-control; until, at last, when circumstances occasioned or permitted a relaxation in this temporary strain, the intensity of the previous struggle manifested itself suddenly in the most violent paroxysms of hysteric tetanus, on the abatement of which the extensors of the legs were found to be paralysed,—a condition that endured through all the changes of convulsive disorder that subsequently manifested themselves. This very striking fact will be found to be in full accordance with Mr. Carter's theory of the disease.

Our author's first chapter is devoted to an exposition of "The Effects of Emotion," which are fully and very philosophically discussed. The Emotional state, whatever may be its special direction, is one of *activity*, and must have some kind of vent. All metaphysicians recognise this in the name which they give to the various passions, feelings, sentiments, &c., which come under this general category—namely, the *active principles* of the mind. And several among them seem to have perceived, that most of the emotions may be considered as compounded of the simple feelings of pain or pleasure with other states more purely intellectual—a doctrine which has been definitely expressed by the writer of this article in the following terms: "The *ideas* which are excited by sensations, if associated with *feelings* of pleasure or pain, constitute those emotions." This definition would seem not to have been understood by Mr. Carter, who quotes the writer as describing emotions as "sensations associated with an idea of pleasure or pain;" a statement which, although using the same terms as the preceding, is very far from being identical with it. Thus, Hope, according to the first definition, is the pleasurable idea of the future; whilst Fear is the painful idea of something to come; both these anticipatory ideas having their origin in past sensations. So Benevolence is the pleasurable idea of the happiness of others; whilst Malevolence is the painful idea of their happiness, or the pleasurable contemplation of their unhappiness. To call any of these states of consciousness *sensations*, is a complete perversion of the ordinary use of terms, which has much surprised us in Mr. Carter's case, his clearness and accuracy being elsewhere so conspicuous; and the matter is rendered worse by his giving this definition, in the form of a quotation, as being Dr. Carpenter's, differing from it as it does in a transposition of the words in which it is couched, whereby its meaning is entirely changed. The importance of keeping to the original formula, moreover, becomes apparent in the latter part of the treatise; for here Mr. Carter recognises and adopts Dr. Carpenter's doctrine of *ideo-motor* action, as explaining many of the phenomena of hysteria, without seeing its very close connexion with *emotional* action. In each case, as we think it would be easy to show, the mind is under the domination of an *idea* or *set of ideas;* and the ideational state expresses itself *emotionally*, when it is associated with a vivid feeling of pain or pleasure, whilst it expresses itself *rationally*, when the mind is possessed by an idea without any such accompanying excitement.—Mr. Carter's general view of the modes in which the emotions operate upon the bodily frame, is so good, that we have scarcely any critical remark to make upon it. We must, however, express our surprise that he should ignore the influence of the depressing emotions in producing muscular *relaxation* (p. 15): the phenomena of syncope induced by violent mental *shock*, being so perfectly identical with those which are consequent upon organic lesions; and similar phenomena being often witnessed in less intensity, when the shock has been only sufficient to weaken the heart's action (without loss of consciousness), and to relax the sphincters. We would suggest, too, that the influence of the emotions upon the *quality* of the secretions is more probably to be accounted for by attributing it to an alteration in the condition of the *blood*, of which the altered secretion is only the exponent, than by the supposition that the emotional state acts upon the secreting organs themselves. Thus the sudden disengagement of a large quantity of gases in the intestinal canal, or the exhalation of ammoniacal vapours from the skin, which constantly occur in some individuals under almost any kind of emotional excitement, seem to us far more easily accounted for by changes in the blood, than by local alterations in the secreting surfaces. And if there be any truth in the idea that habitual emotional states on the part of the mother influence the development of the *fœtus in utero*,—an idea that appears to us to be sanctioned by a body of evidence which can scarcely be disregarded,—the blood is the only channel through which that influence can be propagated. We lay the more stress on this point, because Mr. Carter, in a later part of his book, sets up his emotional theory of hysteria as opposed to the toxic hypothesis which has been put forth by Dr. Laycock and some other writers. To us it appears that the two are perfectly reconcilable;—the habitual emotional state having the power to determine perversions of the blood, whereby various changes in the nutrition and vital actions of the solid parts may be brought about. And there appears to us to be much evidence in favour of the idea, that certain diatheses, especially the gouty (as urged by Dr. Laycock) are much more favourable to the development of this morbid condition of the blood, than a state of perfect health would be. In the very severe case to which we just now referred, there was a decidedly gouty predisposition, derived from the father, whom the patient much resembled, both in physical and mental character; and this manifested itself in the course of her long illness, in a peculiar arthritic inflammation of the wrist-joint, as well as (we suspect) in chronic inflammatory action in the fibrous tissue of the uterus, referable to the same type. Although our space is so limited, we must find room for the following quotation, which contains the most remarkable and authentic example of which we have ever heard, of a local disorder of nutrition occasioned by a powerful emotional idea.

"A lady, who was watching her little child at play, saw a heavy window-sash fall upon its hand, cutting off three of the fingers; and she was so much overcome by fright and distress, as to be unable to render it any assistance. A surgeon was speedily obtained, who, having dressed the wounds, turned himself to the mother, whom he found seated, moaning, and complaining of pain in her hand. On examination, three fingers, corresponding to those injured in the child, were discovered to be swollen and inflamed, although they had ailed nothing prior to the accident. In four-and-twenty hours, incisions were made into them, and pus was evacuated; sloughs were afterwards discharged, and the wounds ultimately healed." (p. 24.)

Now, with the fullest confidence in Mr. Carter's veracity, we must own that we should like to see this remarkable case authenticated by the testimony of the "medical friend under whose observation it fell, and whose exactitude and veracity may be implicitly relied on." We see no reason to discredit the statement; since the fact, if truly narrated, is only to be considered as an unusually striking exemplification of the principle which recent physiological and pathological inquiry in this direction has brought into clear view,—namely, that the strong direction of the attention to any part of the body, especially if accompanied with emotional excitement, is capable of altering its organic action in a manner which the will cannot effect; a principle on which, as Dr. Holland has well pointed out, the phenomena of hypochondriasis are to be explained, and which will be found no less applicable to the diversified forms of hysterical disorder. But as we know by experience, that even when passing through the most truthful and cautious narrators, remarkable circumstances are modified by slight unintentional alterations or omissions, and as we have constantly to make this kind of reservation of belief in the case of the marvels of mesmerism, homœopathy, &c., which are continually being urged upon us by intelligent and honest-minded friends, we feel that we have the same right to ask for a confirmation of the statements in question, by the gentleman in whose practice it occurred.

We quite agree with Mr. Carter in his view of the importance of giving vent to the emotions, in some way or other, when they are once strongly excited; and of directing their manifestation into some channel that shall "work them off" with the least injury to the individual or to others. And we fully accord in the remark, that "when any portion of the organism is the subject of continued and close attention, it is not only very prone to gradual changes of nutrition, but is also rendered liable to be the outlet for any strong emotion which is experienced at the same time." But we scarcely think that he has dwelt enough, either here, or in the latter part of his treatise, on the importance of *preventing the development* of the emotional state, and of the power of the will to effect this. The prevention of the development of emotional excitement, and the repression of its manifestations when it *has* taken possession of the mind, are two entirely different objects; for the former is to be aimed at by that habitual cultivation of the voluntary control over the current of thought, which shall enable the individual to draw off the mind from the contemplation of objects which cannot be dwelt on without excitement; whilst the latter can only be accomplished by the imposition of a forced restraint, which only determines into some other, and it may be, less desirable channel, a force which *must* find vent in some mode or other. That woman's psychical nature differs from man's, in the predominance of the emotional over the volitional and intellectual, is universally admitted by all who have studied her character; and we agree with Mr. Carter in the belief, that derangements of her system which spring out of this peculiarity, are more common and severe in females than in males, not merely on account of the greater relative strength of their emotions, but also on account of their being more frequently, or even constantly, under the necessity of concealing them. Hence it is that we are strongly impressed with the importance of so disciplining the will, in female as well as male childhood and youth, that the individual may gain the power of directing her own current of thought, and may be exposed as little as possible to those struggles which arise out of violently contending feelings, and which tend to produce those forms both of bodily and mental disorder to which she is peculiarly prone. We quite accord with Mr. Carter in the statement (which has been controverted by those writers who insist on the uterine origin of Hysteria), that males occasionally, though rarely, exhibit a true hysteric paroxysm; as also, that such paroxysms most frequently occur in men who have been accustomed to repress their sexual tendencies.

We have thus had occasion to notice, in criticising our author's introductory chapter, so many of our points of agreement with him, as well as of those (of far less moment) in which we think him in error, that a very brief account of the remaining contents of his volume will now suffice. In discussing the Hysteric Paroxysm, in the second chapter, he employs the term *primary* to express those forms of the attack which depend upon an emotion directly excited by objects perceived, remembered, or imagined. By *secondary* hysteria, he distinguishes those paroxysms which follow the suggested or spontaneous remembrance of the emotions to which the primary fit was due. And he calls those *tertiary*, which are designedly excited by the patient herself, through the instrumentality of voluntary recollection, and with perfect knowledge of her power to produce them. These distinctions (which are partly due to Mr. Mackenzie) we look upon as valuable, both theoretically and practically; for it is only by a careful analysis of the patient's condition, that we can attain a true appreciation of her real state; and it is only under the guidance of such an appreciation, that any rational plan of treatment can be conducted with a probability of successful results. Paroxysms of the primary and secondary kinds may often be induced, as Mr. Carter justly remarks, by the operation of an exciting cause so powerful, that (when once the emotional state has attained a certain intensity) no effort of the will would be sufficient to prevent their taking place, even if it succeeded in delaying them for a time; but such cases (Mr. C. continues) are rare, when compared with those in which the occurrence of the fit, although not volitional, is yet a matter of surrender, and might be prevented under the pressure of an adequate motive. Such motives, however, seem to us to have a much more cogent operation in preventing the development of the emotional state, than they have in repressing the paroxysm when the excitement has already risen to the point adequate for its production; and the case cited by Mr. Carter (pp. 47, 48) affords a strong confirmation of our view.

In the Third Chapter, Mr. Carter discusses the "Moral State and Motives" of hysterical subjects, with a keenness of observation and analytical power which has enabled him to give, within a very small compass, all the most important points by which this class of subjects is characterized in the several phases of the malady; and all who have had opportunities of witnessing the more obstinate forms of hysterical disorder, and especially the strange moral perversion which is frequently displayed, will attest the truthfulness of the portrait. We suspect, indeed, that Mr. Carter's experience has chiefly lain among subjects, in whom the mental perversion has been a more prominent feature than the disorders of the physical frame; and he has thus been led to set down as "Complications of Hysteria" (discussed in the Fourth chapter) affections, of which many are to be regarded, in our estimation, as manifestations of that abnormal state of the blood which we are disposed to look upon as the essence of the disease. Without going the length of some authors, who regard such affections as essential to the disease, we yet must regard them as a part of it when they do present themselves; and we altogether dissent from Mr. Carter's statement, that they do not occur in the primary and secondary forms of hysteria; since we are satisfied that we have often witnessed them where the tertiary state had never supervened. Looking at many of these local complications as manifestations (like the local congestions of chlorosis, which is frequently associated with the hysteric disorder, although essentially independent of it) of a particular condition of the blood, induced by an habitually disordered emotional state, we yet fully recognise, with Mr. Carter, their liability to aggravation from the special direction of the patient's thoughts to them; and we are quite in accordance with him as to the line of treatment to be pursued, which is completely antagonistic to the notions of the speculum doctors, who would make every functional disorder of the uterus into a case for local treatment. Every discriminating observer must have come to the conclusion, that the present fashionable methods of practice tend to produce, in no small proportion of cases, the very class of maladies which they are intended to cure; by attracting the minds of the patients *to* an organ over whose states emotion has a peculiar influence, and *from* which it is most important to their welfare that their thoughts should be directed. We commend Mr. Carter's remarks upon the whole of this subject to the special attention of our readers; and this the more earnestly, since we have reason to know that they will be fully concurred in by some of the most eminent authorities in the special department of practice to which they relate.

We must express our surprise at the almost total omission of the several forms of convulsive disorder; such as tetanus, trismus, epilepsy, and chorea, from the category of "complications," or (as we should say) of manifestations of the hysterical state; and this circumstance confirms our surmise that Mr. Carter's experience has not lain among such cases to any great extent. We would add somnambulism, double consciousness, and delirium, as forms of cerebral disorders which have come under our own observation, under circumstances which precluded the doubt of their essentially hysterical character.

In the Fifth Chapter are discussed the "Hypotheses of Previous Writers," and especially the toxic hypothesis, which Mr. Carter considers to be essentially antagonistic to his own. We have already pointed out a way, however, in which the two may be fairly, and, as we believe, truly reconciled.

The Sixth Chapter, on "Treatment," and the Seventh, "On Hysteria among the Poor," are eminently characterized by the best features of Mr. Carter's mind. And of the whole of this little book we can candidly say, that we do not think that any member of our profession who is capable of appreciating it, can read it without profit.

W. B. Carpenter.

Review IX.

1. *Report on Bethlehem Hospital, by the Commissioners in Lunacy, &c.* (Ordered by the House of Commons to be printed.)—*London*, 1853. 8vo, pp. 451.
2. *An Account of Bethlehem Hospital, abridged from the Report of the late Charity Commissioners.* Second Edition.—*London*, 1853. pp. 47.

Not half a century ago, the treatment of lunacy was conducted on principles alike revolting to the common sense of men, and to the common feelings of humanity. Men attempted to control insanity by severity, and to cure it by terror. The acts of cruelty which took place within the walls of those prisons, which it was mockery to call asylums, were not surpassed by the barbarous scenes which have given so sombre a fame to the Bastile or to the Tower. Among the most civilized nations of the earth, whose boast it was to exhibit the refinement, without the harshness, of an earlier age, a lunatic was caged like a wild beast, as if suffering and misfortune had not rather drawn closer than destroyed his relationship to a race, subject at every point to imperfection and decay.

Of late years, these things have become matters of tradition. Advancing civilization has brought a gentler spirit; and it is not too much to hope that a merciful religion has received a better application. Men whose names will live in the annals of philanthropy, have applied to the treatment of insanity the humane instincts of the age. The iron bar and the strait-waistcoat have disappeared; the narrow crib and the dark cell have widened into pleasant halls; the hours which would have been passed in apathy or delirium, are now solaced by interesting occupations; moral influences are brought to bear continually, though imperceptibly, on the perturbed soul; and on every side a wise control and careful guidance lead it once more towards the paths of health.

This great reform has been the work of a few men, who have found, from the intelligent part of the public, sympathy and support. It has not been accomplished without opposition and misgiving, but it has stood the test of time, and has won the suffrages of all. So thoroughly has it been accepted, that the people of this country would as soon think of again pirating slaves from the plains of Africa, as of permitting the lunatic once more to be scourged into quiet, or starved into obedience.

To guard against cruelty, however, it is necessary to do more than pro-claim that cruelty is useless and hurtful. If all men who have charge of lunatics were educated and conscientious, nothing more would be needful. But the keepers and nurses of asylums are necessarily drawn from the lower classes of the community; and, rough by nature, and coarse by nurture, they are often ignorant and careless, and sometimes brutal. They have a continual tendency to relapse into old habits, and to do by violence what must be done, with greater trouble, by kindness.

In order to prevent, as much as possible, the carelessness of superiors, and the harshness of subordinates, the English government has established a system of inspection and control, which is imperfect, only because it is limited. Under the 8 & 9 Vict., six Commissioners in Lunacy were appointed, whose duty it is to visit the various lunatic asylums, to observe the system, examine the inmates, suggest improvements, and reform abuses. So indispensable is some such system of *surveillance*, that no reasonable man can doubt, that without it, the teaching of science, and the opinions of the public, would alike have been inoperative. The government at any rate thought so, when it gave the Commissioners wide powers, to visit and license, to call and examine, to confine and set free.

With strange inconsistency, the government empowered the Commissioners to inspect all the asylums in England, except two or three, where, as it happened, inspection was most needed. By a happy adaptation of the sliding scale, the Commissioners could visit and suggest at York, could visit but not suggest at St. Luke's, and could do neither the one nor the other at Bethlehem, which needed both.

On what grounds is it conceivable that the government consented to this exclusion? Is the asylum in Yorkshire or Suffolk visited because it was built yesterday, and is Bethlehem excluded because it was founded in the dark ages? Do the magistrates of Yorkshire require such close control, while the magnates of the city of London may be safely trusted to their own ways? Is the medical superintendence in the county asylums so defective, that six gentlemen are to be continually on the watch, to set it right; and is Bethlehem so transcendently perfect, that such *surveillance* would be a waste of time? On the contrary, the county asylums are more commodiously built than Bethlehem; the county magistrates are fully as intelligent as city aldermen; and county superintendents are not less skilled, and are much more careful, than Bethlehem doctors. Is the cause of the exclusion from Bethlehem to be found in the fact, that the city of London demanded it, and the government bowed before the menace of opposition from that powerful and unscrupulous corporation? Or did Bethlehem possess some powerful friend, who, deceived by the representations of interested men, obtained for it, in an unfortunate moment, an exemption that was alike erroneous in principle and hurtful in execution? It is needless now to inquire into this point, although there be no doubt that an incalculable amount of misery has resulted from the docility with which the government allowed the affairs of Bethlehem to be withdrawn from their cognizance and control.

Although the Commissioners in Lunacy were excluded from Bethlehem, the government inserted a proviso in the Act, that the Secretary of State should possess power to order a visit if he deemed it necessary. This power was exercised last year, when certain statements were made to the

Commissioners, and communicated, after inquiry, to the Secretary of State. The result of this visit is contained in the work before us, in which, besides the very able report of the commissioners, the evidence on which it was founded, the rejoinder of the governors, and the statements of the medical and other officers, are found.

The facts discovered by the Commissioners are precisely of the kind which might have been predicted. The organization of the hospital was found to be utterly unsound. Lax superintendence, a divided authority, and intestine jealousies among the superior officers, had produced, among the inferior, carelessness and neglect. The governors, composed chiefly of members of the corporation of London, delegated their authority to a managing-committee, which divided itself into various sections, and among these was a sub-committee of seven governors, who were supposed to attend weekly at the hospital. A President, a Treasurer (living in the hospital), two visiting Physicians, one consulting Surgeon, a resident Apothecary, and a Matron, constituted the working staff. Into this vast hospital, besides incurable persons and criminal lunatics, there are admitted yearly 315 patients, who come in expressly to be cured. None of these has been ill longer than twelve months, and after twelve months' residence in the hospital they are discharged, if incurable. Bethlehem is not, then, a mere asylum for the safe keeping of lunatics, it is an hospital where all the modern means of cure ought to be in full and perfect operation. The construction of the building is, however, unsuitable for the purpose; the physicians, wedded to old systems, have required pressure from without, before they would introduce measures elsewhere in universal operation, and therefore Bethlehem has never enjoyed a very high reputation among the asylums of this country.

The physicians, Dr. Monro and Sir Alexander Morison, were ostensibly responsible for the treatment of the patients, both as respects medicines and moral treatment. They fulfilled their duties very laxly; the visits were hurried; the cases were not investigated; many patients were not seen; the general details of occupation, which form so important a part in the treatment of insanity, were carried on in a system of routine, were not looked into by the physicians, but were administered on the male side by the apothecary, and on the female by the matron.

The duties of the resident-apothecary, Dr. Wood, were, to make up the prescriptions of the physicians, to keep the case-books and records, and to attend to the treatment of the patients in the absence of his superiors. But, either from zeal or inclination, this officer stepped out of his course to fulfil the general medical duties. He appears to have been almost entirely occupied with the investigation of cases, and neglected a very important part of his own duty, the keeping of the books. Evidently, too, he assumed more authority than was attached to his office; his power over the attendants was very limited; and the management as to occupation of the female patients was improperly taken from him by a verbal order from the treasurer, and was given to the matron. Probably from the position in which he was placed in respect of the matron, whose authority was co-equal with his own, or even in some respects superior, Dr. Wood appears to have been imperfectly informed of what was going on, on the female side, where, it is remarkable, all the cases of cruelty and neglect occurred. He was ignorant that dirty female patients were laid naked on stone floors and were mopped with cold water, and although he knew that there was a custom of placing female patients naked, on loose straw covered with a blanket, he did not remonstrate against this custom with sufficient energy. Still, whatever good was done in the hospital was apparently owing to this gentleman. He claims to have introduced improvements into the treatment, especially as regards the discontinuance of methods of restraint; and we are quite ready to admit the force of his plea, that his duties were excessive, and his authority very little. We only blame him for placing himself in a false position; he should have seen that to attempt duties without authority was suicidal. His wisest course would have been to have retired from so anomalous a position; he did not do so, and he has paid the penalty for becoming a party to faulty arrangements and official indolence.

The matron appears in the report as a rival authority with the apothecary. She evidently greatly neglected her duty; her health was bad, her visits to the wards not sufficiently frequent; her superintendence of the attendants lax, and her opposition to Dr. Wood strenuous. She was perfectly ignorant of the objectionable practices daily resorted to by the nurses, such as mopping dirty patients, and forcibly feeding those who refused to eat.

With such undeniable remissness on the part of all the officers, no system could have worked well; but the pretensions of Dr. Wood and the matron were quite irreconcilable, and the hospital was divided into hostile camps. There was, however, one supreme authority, the treasurer; but this gentleman was not in daily and hourly communication with the attendants, and could not watch the constant working of this intricate machine with the same care as the autocrat of an asylum ought to do. He appears, too, to have been a bad manager and an unsuccessful arbiter, and to have on many occasions adopted courses which could only have the effect of lessening still more Dr. Wood's authority with the attendants, and of rendering that gentleman's position still more onerous.

As to the sub-committee, their visits were nearly formal, and though they do not much appear on the scene, when they do so, their entrance does them no credit.

After this sketch of the organization of the place, the conclusions of the Commissioners will excite no surprise. They found that five patients, whose cases were looked into, had been treated "harshly and improperly" by the attendants, and had been "neglected" by the medical officers; that the sub-committee were inefficient; the treasurer injudicious; and the visiting-physicians inattentive; that the apothecary's duties were not properly performed, and those of the matron neglected; that the rules and orders of the hospital were not understood, and the case-books and other records very imperfectly kept. They recommended a thorough revision of the whole system, and this recommendation has been partly complied with.

It is not our intention to go into the details of the individual cases. We have satisfied ourselves of the correctness of the conclusions drawn by the Commissioners, and if the evidence had been insufficient, Dr. Wood's letter to the Secretary of State, written in justification of his conduct, would have been enough to show the utter unsoundness of the whole system pursued at Bethlehem.

Finding how completely he has been sacrificed by the exigencies of his position, Dr. Wood has evidently determined not to allow his cause to be damaged by further reserve. He details without scruple the petty annoyances to which he was subjected, the opposition he encountered, and the enmity he provoked. He commences his letter with a statement, that the form of government at Bethlehem "affords a good illustration of the evils arising from imperfect organization, and from ill-defined and irresponsible power," and ends it with the declaration, that "imperfect organization, divided authority, little co-operation, and the demon of discord," have neutralized the good that might have been expected from an hospital boasting Royal patronage, and enjoying princely revenues.

This inquiry into the condition of Bethlehem has already borne its fruits. The organization of the hospital has been changed; and a resident physician, with plenary power, has been appointed. Further alterations appear in the distance. It is, we believe, the intention of government to repeal the clause which exempted Bethlehem from visitation, and to bring its inmates under the cognizance of the Commissioners.

So far is good; but why should the government pause at this point, and go no farther? Bethlehem must not be merely patched up and mended; it must be thoroughly re-organized. We are told by the man who has the best right to know the truth and force of his expressions, that an inefficient system has greatly marred the intentions of that benevolence which has endowed Bethlehem with almost princely wealth. More than 20,000*l.* a year flows into its coffers, and the greater part of this large sum was intended, not to found merely an asylum where lunatics might be kept, but an hospital where they might be cured.

Can this noble income be properly applied, even with the present improved machinery? Are no more than 300 patients to be benefited yearly by this wealthy hospital? Can the intricate treatment even of this number be efficiently directed by a single man? Above all, can the superintendence of so vast an establishment be safely entrusted to a sub-committee of city governors?

If Bethlehem is to be a model asylum, the reformation must be thorough. The very building is incommodious and ill-adapted; the site is most objectionable. The inmates have scarcely breathing room in their narrow exercise grounds, and the various out-door occupations and recreations which the lunatics in a county asylum enjoy, are denied to the inmates of Bethlehem. This must all be remedied before the great metropolitan asylum can take an equal rank with similar continental and provincial institutions.

The government has now an opportunity of retrieving past neglect, and of ensuring future usefulness. By every consideration which can affect its members as ministers and as men, they are bound to look into this matter thoroughly, and to act in it boldly. We demand nothing in detail; but we claim the right to be assured that this inquiry, so important to the happiness of many Englishmen, shall not be slurred over by incompetence, or be perverted by private influence.

REVIEW X.

A Treatise on Operative Ophthalmic Surgery. By H. HAYNES WALTON.—*London*, 1853. 8vo, pp. 628.

To those read in works on eye-diseases, the title of the present may suggest that of Jüngken, Die Lehre von den Augenoperationen, published at Berlin in 1829, in which the laborious professor takes up the surgical operations which are performed on the organ of vision, each in its order; gives a definition of it, a history of it, explains its object, its indications and contra-indications, its prognosis, and its value; expounds the preparations necessary for it, and the instruments it requires; first describes it generally, and then details its various steps; states the dressing necessary after its performance, enumerates the bad consequences it may give rise to, and directs the after-treatment. But the work before us is somewhat different; it pretends not to the minuteness nor to the logical precision of Jüngken; and instead of being confined to operative ophthalmic surgery, it embraces a large portion of the pathology of the eye. This is not to be quarrelled with; only the name ought to have been, a 'Treatise on the Surgical Diseases of the Eye,' which would have conveyed the true bearing of the work.

The following are the subjects of the twenty-two chapters, into which the work is divided—viz., 1. History; 2. Chloroform; 3. Instruments; 4. Injuries; 5. Foreign bodies; 6. Affections of eyelids; 7. Affections of lacrymal passage; 8. Caries of orbit; 9. Nævus; 10. Scarification; 11. Squint; 12. Tumours; 13. Protrusion of eyeball; 14. Staphyloma; 15. Conical cornea; 16. Opacities; 17. Cataract; 18. Entozoa; 19. Artificial eyes; 20. Malignant affections; 21. Artificial pupil; 22. Extirpation.

A considerable part of the First Chapter is nothing more than a transcript from Sprengel, and presents a mere string of names and insulated facts. The history of a science or an art implies an inquiry into the sequence and connexions of facts, into the circumstances which have led particular individuals to discoveries and improvements, and into the causes which have impeded progress; along with an estimate of the reality and worth of pretended advances. Our author has scarcely entered in any degree on the business of an historian.

The First Chapter is disfigured by an incessant mis-printing of proper names, and even of other words; such as περατοειδής for κερατοειδής, tubes for tabes, scab for scale, Actius for Aëtius, Avenyoar for Avenzoar, Mallhæus de Gradibus for Matthæus de Gradibus, Plimpius for Plempius, Muck for Nuck, Novius for Hovius, Neister for Heister, Aribasius for Oribasius, &c. &c. The same thing occurs, more or less, in other parts of the work, where we find Veccà for Vaccà, Dzoudi for Dzondi, Woodhouse for Woolhouse, and, strangest change of all, Ammon transformed into Armenius!

Numerous omissions and errors in matters of more importance than orthography might be pointed out in our author's history of ophthalmology. Thus he tells us, that Winslow said, "that the iris has its convexity turned to the lens." (p. 18.) Winslow said not so. On the contrary, he stated that it was the crystalline which caused the iris to be convex anteriorly, as may be seen by referring to his paper in the 'Memoirs of the Academy of Sciences' for 1721.

Our author says, Le Cat termed the membrane on the inner surface of the sclerotic, *tunica fusca* (p. 19). No such term is used by Le Cat. He claimed, indeed, the discovery of such a membrane, and considered it as one of the continuations of the pia mater, while the choroid and iris formed another; but the name *lamina fusca* seems to have originated with Zinn.

Our author tells us, that Zinn "taught that the rete mirabile of the ciliary processes, which Lieberkuhn had skilfully injected, consists of very minute vessels." (p. 19.) The idea that these vessels formed a rete mirabile never entered Zinn's head. He speaks of the arteries of the ciliary processes as being above twenty for each, and forming a "pulcherrimum rete;" but this is quite a different matter from a rete mirabile.

"Rowley, professor at Oxford," says Mr. Haynes Walton, "proposed to operate while the patient was in the recumbent posture, to prevent the too ready exit of the aqueous humour." (p. 42.) Rowley was no professor at Oxford, nor anywhere else. He was, or pretended to be, physician to the St. Marylebone Infirmary, but performed operations. The position he recommended for extraction was rather semi-recumbent. A chair, with a pillow on it, was placed so that the light might fall in a proper direction for the operation. The patient was seated on a pillow on the floor, and leant his head back on the pillow placed on the chair, so that his face was horizontal. The operator seated himself before the patient, and, leaning forward, performed the extraction.

If Rowley was to be mentioned in a history of ophthalmology at all, he should have been characterized as perhaps the most impudent plagiarist that ever breathed; for he translated the whole of Plenck's 'Doctrina de Morbis Oculorum,' and without acknowledgment published it as his own, under the title of 'A Treatise on One Hundred and Eighteen Principal Diseases of the Eyes and Eyelids. London, 1790.'

We have seldom seen so many blunders in so small a space as those presented to us in the first chapter, which is really unworthy of the rest of Mr. Haynes Walton's work, and in a second edition ought either to be re-digested and re-written, or struck out.

In the Second Chapter, the author gives a succinct account of the introduction of chloroform as an anæsthetic agent, its utility, and the chief points to be attended to in its administration. He explains how the use of a proper inhaler insures the dilution of the vapour with a sufficient quantity of atmospheric air; so that its effects are produced with greater regularity, and thus the proper time for its discontinuance readily noticed. The time to commence an operation being the moment when voluntary motion is suspended, he remarks, that the proof of this is the loss of the sensibility of the conjunctiva; and that as long as there remains sensibility in that membrane, there will be contraction of the eyelids, when the globe of the eye is touched.

The chief objection to chloroform in eye-operations, is the vomiting and retching which it is apt to produce. When these occur, after any operation on the eye, they must be more or less detrimental, from the violent expiratory movement, and consequent determination to the head, with which they are accompanied; but after extraction of the cataract, or any other operation in which the cornea is largely opened, they involve the risk of totally destroying the eye by loss of the vitreous humour. We have always understood that vomiting was much more likely to ensue, if the chloroform was inhaled with the stomach full of food. But Mr. Haynes Walton quotes the opinion of Dr. Fleming, that the sickness is occasioned by exhibiting the chloroform on an empty stomach; and that on the morning of the operation, a little milk beaten up with an egg and some brandy or wine, should be given. In the extraction of a dislocated lens, when pain and irritability of the eye would render extraction in the ordinary way hazardous, from impossibility of steadying the eye except by undue pressure, and in the extraction of foreign bodies, and of opaque capsules from children, our author thinks the operator should run the risk of the vomiting, rather than forego the advantages to be derived from chloroform.

The Third Chapter contains a number of sensible remarks on eye-instruments in general. Mr. Haynes Walton recommends them to be light, the handles of wood, in place of ivory, and round and smooth, in place of facetted or cross-cut. The plan of illustrating each instrument by a figure, and of describing each apart from the operations, is good.

In the Fourth Chapter, our author quits the proper subject of his work, as announced in the title—the operative surgery of the eye—and favours us with an interesting account of the effects of burns and scalds, blows, wounds, and chemical injuries.

The application which he prefers in cases of burns and scalds of the eyelids is nicely-dressed cotton-wool, which is soft, light, cleanly, and easily changed by wetting any portion adhering to the sound skin, while that over the raw surface readily separates when surcharged with the moisture of the part. He thinks it better not to cut blisters, for the cure goes on more readily when the fluid is absorbed and the cuticle unbroken.

In ecchymosis, Mr. Haynes Walton questions whether any means, except gentle friction, can hasten the natural process of absorption. Poultices, then, of bryony root or Solomon's seal, infusions of rosemary or arnica, and even cold lotions, he dismisses as useless, in all ordinary cases of black-eye. It is only when extravasation is increasing, by an oozing from the circum-orbital vessels, that he enjoins pressure or cold, a smart purge, and perfect rest. He even speaks of reducing the heart's action by blood-letting; but he forbids leeches, incisions, and punctures.

Under the head of blows, the effects on the eye, when merely the surrounding bone has been struck, are well stated. Among other accidents mentioned, is the case of an engineer, who, in striking a bar of iron with a sledge-hammer, had a piece of the metal projected against his face, and his nasal bones fractured. Three days after, the conjunctiva of the left eye was slightly ecchymosed, the pupil dilated to a degree that left merely a narrow band of iris, and vision extinct. Cold lotions alone were used; improvement of sight was evident in a week; in four months, the patient could see bodies held laterally; and some months later, he could read large type. The pupil was less dilated, though it never recovered its natural size, and the iris remained slightly tremulous. In such injuries, our author trusts chiefly to rest, rather low diet, and abstinence from alcoholic drinks, and to cold-water cloths and opium externally. He doubts whether the change of colour in the iris, which not uncommonly follows a blow, is ever independent of inflammation; and inclines to think not. We have seen the iris become perfectly green from such a cause, without the least pain or redness. The change of colour in such a case, is probably the result of extravasation. In sub-conjunctival ecchymosis, we have seen the iris assume a greenish hue.

Traumatic inflammation of the eyeball, our author treats by general and local bloodletting, although he does not think that disease can be "bled out of the eye"; he gives mercury, and employs artificial dilatation of the pupil by a solution of the sulphate of atropia. Puncturing the cornea, under chloroform, when the eye feels tense, and the chambers seem preternaturally full, he rather recommends; but forbids the letting out of pus in the anterior chamber, or in the texture of the cornea. In cases of general suppuration of the globe, a free opening should of course be made for the evacuation of the matter.

On the subject of effusion of blood into the chambers of the eye, lacerations and detachments of the iris, and injuries of the lens, the reader will find a number of excellent remarks, illustrated by short references to cases. "A dislocated lens accompanied by an unbroken capsule, should be removed," says our author, "from the chambers of the eye at once—that is, as soon as there is proof, by opacity, that the capsule is present." Mr. Haynes Walton does not seem aware of the length of time the capsule and lens may, under such circumstances, remain transparent. Weeks, months, and, we suspect, years, may pass, before any decided opacity shows itself in the lens or capsule, provided the latter, inclosing the former, has been separated from its connexions, without any aperture by which the entrance of the aqueous humour is permitted. In such a case, the lens shows itself, in the anterior chamber, like a drop of water, of different specific gravity from the rest of the aqueous humour, and its margin seems surrounded by a narrow gilt-like ring, of a splendid yellow colour. In this state, we have extracted the lens and capsule, before they exhibited the least opacity, although for weeks they had lain in the anterior chamber.

Prolapsus of part of the iris, through a wound of the cornea, is a very annoying accident: it seems so simple to the unprofessional by-stander, and is known to the surgeon to be so difficult to remedy. Mr. Haynes Walton thinks it is the tightness with which the iris is held in the wound, that forms the difficulty; but we have always been of opinion, that could the little bag of iris, which protrudes, be emptied of its aqueous humour, it would often return immediately into its place. Puncturing it would do this, but then it would present ever after a false pupil. The plan, therefore, to be pursued, is, instantly after dropping the solution of atropine into *both* eyes, to bring the patient under the influence of chloroform, and, with a small curette, to press on the little bag of iris, so as to empty it of its contents. For this purpose, the curette should enter through the wound, carrying the bit of prolapsed iris before it. We have never known the rubbing of the cornea, through the medium of the upper lid, succeed in reducing a prolapsus of the iris.

The Fifth Chapter embraces foreign bodies on the surface of the eye, or within it, larvæ under the lids, and ossification of the ocular tissues.

The following is an instructive case:

"A gentleman, perceiving that something had entered his eye, and having in vain tried to remove it, applied to his usual medical attendant; nothing was found, but his suffering continued. On the following day, inflammation and pain rendered him unable to attend his duties. He was cupped, confined to a dark room, and salivated. Six weeks of misery were passed in implicit obedience to the rules of his adviser, who now became not only very anxious, but actually alarmed, and requested a consultation with some one more conversant with eye-diseases. This was granted; the upper lid was everted, and a portion of cigar-ash removed from about the centre of the cartilage, the usual seat of small particles under the lid. Relief was instantaneous, the symptoms declined rapidly, and the cloudy cornea recovered its transparency." (p. 101.)

The danger of destructive inflammation of the cornea, arising from scraping off a portion of its epithelium, as is often done by smiths and other mechanics, in removing *fires* from one another's eyes, ought, we think, to have been touched upon.

Numerous cases of much interest are detailed by our author in this chapter.

Abscess of the lids, symblepharon, anchyloblepharon, epicanthus, ptosis, trichiasis, entropium, entropium with trichiasis, ectropium, obstruction of the Meibomian ducts, and conjunctival calculi, form the subject of the Sixth Chapter, which is one of the best in the whole book, and will well repay the most careful perusal.

Most surgeons are aware of the unpromising results of operations in symblepharon. It is gratifying to learn, that in two cases Mr. Haynes Walton was successful. In the first,—

"The band which tied," says he, "the centre of the lid to the globe, encroached a little on the cornea, and was narrow, soft, and loose; I merely divided it twice, and each time it returned to its former state. I then removed a small central portion of what appeared to be the cicatrix of the band, and brought the edges of the divided conjunctiva, on the lid and on the globe, severally together by sutures; and the result was very gratifying. . . . .

"In the second, I was still more fortunate, for perfect success ensued. The patient, who was twenty-six years old, had, when a lad, accidentally received some quick-lime in his eye; and the usual process of adhesion of the lids to the globe, followed cicatrization of the conjunctival slough. . . . . The connecting medium, which seemed to consist entirely of conjunctiva, was long, and nearly isolated. Putting the lid on the stretch, I divided the band vertically through its entire thickness, carried the incision into the tissue below, and brought the edges of each side severally together by three sutures." (p. 123.)

In trichiasis, our author justly remarks, that where the lid is unhealthy, and the offending cilia, although few in number, are abortive, and grow directly inwards, extraction with the forceps should be laid aside, and some radical plan of treatment adopted. He recommends excision of a transverse slip of integument, not with scissors, as usually practised, but in a more methodical manner, with the knife. The lid being made tense, by the assistant drawing its external angle outwards with the one hand, and with the other raising the brow, if the upper lid is the one to be operated on, the skin is divided parallel and close to the margin of the lid, while another and a curved incision is made to meet the first at its inner and outer extremities. The flap is then dissected off, and the edges brought together by a sufficient number of stitches.

With respect to the operation of excising the bulbs of the cilia, Mr. Haynes Walton is of opinion, that it is impossible to perform it in an effectual manner, without bringing away a portion of the cartilage, and to a greater or less extent destroying the Meibomian glands, by cutting across their orifices. He subjoins a diagram, which will serve usefully to remind the surgeon, when about to perform excision of the bulbs, of the relative position of the parts concerned. He then describes Jäger's operation, in which a stripe of integument is removed with the knife, along with the bulbs or secreting organs of the cilia; but without mentioning that gentleman's name. Next, Vaccà's operation is described, not only without any reference to that surgeon, but in a way which may lead the reader to suppose it an improvement of Mr. Haynes Walton's own.

Our author seems rather favourable to the plan of transplanting cilia into lids upon which such operations as those now referred to have been performed; reminding us "that the bulb of the hair is an extra-vascular and extra-nervous organ, and in its normal position obtains its nutritive fluid by endosmosis." (p. 157.) It is true, that the hairs, like the cuticle, have no vascular connexion with the follicles by which they are secreted; but it is scarcely to be doubted, that into the bulb or hair-follicle there is projected a papilla, furnished with both nerves and capillaries. The extreme sensitiveness of the cilia shows that they are in connexion with nerves; an injection of the hair-follicles is among the most common anatomical objects prepared for the microscope. "If the follicles are perfect at their base," says our author, "there is no reason why the hairs [the transplanted hairs?] should not draw a parasitical life from the living tissues in which they are embedded." He seems to have forgotten, that in the operations in question the follicles have been excised, and totally rooted out. If they have not, the inverted cilia will be reproduced.

Under the name of Idiopathic Entropium, our author confounds two affections, which we consider essentially distinct — namely, acute or spasmodic inversion, which is scarcely ever seen except in the lower lid; and chronic or inflammatory inversion, which may affect either lid, or both together. The latter is often combined with trichiasis; the former never. The former affects, in general, old persons, in whom the integuments of the lower lid are relaxed and superabundant; the latter is met with chiefly in scrofulous subjects. In the acute or spasmodic variety, the inverted lid may, by slight traction, be made to assume completely its normal position, and it will retain this for perhaps some minutes, when, with a sudden twitch, it becomes again inverted. No traction brings the chronically inverted lid into its natural position; it may be dragged from contact with the eyeball, and its dwarfish cilia brought into view, but still its margin is turned abnormally inwards. The whole structures of the lid are changed, more or less, in the chronic variety; its natural pliability is lost, and has been so perhaps for months or years. In the acute, there seems almost no change of structure; the pliability of the lid is perfect, and often the patient has suffered from its malposition for only a few days.

Mr. Haynes Walton has an hypothesis respecting the cause of entropium.

He supposes it to depend on muscular action—on exorbitant muscular action, if we comprehend him right—first, of the fibres of the orbicularis placed nearest the edge of the lids; and secondly, of the musculus sacci lacrymalis of Duverney, or tensor tarsi of Horner. Here, our author ventures once more upon the historic walk, and, as in chapter first, betrays a considerable contempt for facts. He makes Duverney [not Duvernay, as he spells it] write and lecture in 1761, and say, "It is a small muscle. which I observed a long time ago. Horner, who has for a great many years claimed the discovery, which, as I have shown, was made and published before he was born, appears to be ignorant of its exact bearings." This must be spoken by Duverney's ghost, whom our author has raised for the purpose; for, while Horner is at this moment a professor in the University of Pennsylvania, Duverney was quietly consigned to the tomb in 1730. This shows the danger of claiming the discoveries of other people. It was not, however, John Guichard Duverney—author of the 'Treatise on the Ear,' and whose works were republished in 1761, under the title of 'Œuvres Anatomiques,'—who discovered the muscle of the lacrymal sac; but his cousin, as may be seen by referring to 'Schobinger de Fistula Lachrymali.'

Our author supposes Albinus to have "originated the name musculus ciliaris, thereby signifying a distinct muscle." Now Albinus, neither in his 'Historia Musculorum,' nor in his 'Tabulæ,' admits any distinction of the sphincter of the lids into two muscles—an external or orbital, and an internal or palpebral—as some of his successors do, and as was done long before his time by Vesalius, Columbus, Casserius, and Riolan, but describes the whole together, under the name of Orbicularis Palpebrarum. If the reader turns to page 268, of the 'Pentæstheseion' of Casserius, he will see represented the orbicularis palpebræ musculus major, and the two minores palpebrarum musculi,—these last, one for each lid, being what Riolan afterwards called the ciliary muscles.* Mr. Haynes Walton says that the part of the muscle situated on the edge of the lid, "is described as the thinnest portion of the orbicularis," but that on making dissections he found "that over the edges of the lids, for about the sixth of an inch, the muscle is thicker, perhaps twice as thick as over the remainder of them." This is generally known, and is thus stated by Zinn—"Ipsum limbum utriusque palpebræ investit fasciculus robustior, ex fibris rectioribus compositus, cartilagini tarso arctissime annexus, qui multis proprio nomine musculus ciliaris dicitur."

Our limits will not allow us to enter much farther into these anatomico-historical details, but we cannot quit the subject without stating, that Mr. Hulme's figure of Duverney's muscle, given by our author, is a complete caricature, which would entirely mislead the reader as to the connexions and strength of that small delicate portion of the orbicularis. Instead of a few thin fasciculi, running along the lacrymal canals, and terminating near the puncta, it represents two broad, thick bands of muscular fibres, sweeping along what seems to be the *inner* surface of the tarsi; while, in the explanation, it is stated, that the united tensor tarsi

* Ciliaris musculus cilium utriusque palpebræ orbiculatim cingit, ut palpebras inter se exquisitè claudat, cartilaginis tarsi latitudinem non excedit. Riolani Filii Anthropographia. Parisiis, 1618, p. 504.

and orbicularis pass on the *anterior* surface of the tarsal cartilages. How widely different the descriptions and figures of the tensor tarsi by Rosenmüller, Fischer, and Arnold! The first-mentioned thus describes it—"Musculus sacci lacrymalis, qui ab osse lacrymali incipit, sacci interiorem superficiem circumdat, et in ea parte, qua cum tarsis conjunctus est, sacci finitur. Teneràs quasdam fibras hujus musculi nonnunquam usque ad caniliculos lacrymales tendentes vidi."

Having satisfied himself that the cause of entropium is a faulty action of the tensor tarsi and ciliary muscle, it might have been expected that Mr. Haynes Walton would have tried the effect of extirpating the latter, without sacrificing any portion of the skin. Instead of this, he recommends the removal of the ciliaris, or at least of the marginal portion of it, *along with* as much skin of the lid as by a counter-tension shall overcome the abnormal position of the inverted lid. After the flap is cut away, the exposed surface is to be inspected, and if any muscular fibres have escaped, the forceps and knife must be reapplied. Three or four stitches bring the edges of the wound together, and are to be removed on the second or third day.

All other methods of treating inversion—such as excision of a transverse fold of mere skin, Ware's division of the lid by a vertical section, and Crampton's double section—our author condemns as ineffectual; and as for the plan of excising the cilia, which has been had recourse to by Mr. Wilde and others, he thinks that the large portion of the ciliary muscle and skin, along with part of the edge of the lid, which must generally be removed along with the cilia, is the cause of success. Amputation of the edge of the lid he pronounces worse than removing a thigh to cure popliteal aneurism; yet, in a chronic case, in which the conjunctiva of the upper lid was granular, the edge uncommonly thick and hard, the margin of the cartilage involved, and eversion not readily effected, he himself deemed it prudent to excise the edge of the tarsus. We should speak warily of an operation which sometimes we may find it necessary to practise.

Mr. Haynes Walton commences the subject of entropium by relating a very successful case, in which the lower lid of a child of four years old, was everted, in consequence of a cicatrice on the cheek. Whether the abscess which caused the cicatrice was connected with diseased bones is not mentioned; we presume it was not. The cicatrice was sparingly removed, and from either extremity of the small oval wound, two straight incisions were made. The surrounding skin was then separated from its attachments to a considerable extent, especially on the cheek, by which it admitted of being slipped towards the eye, and made to serve the place of that which had been destroyed. So extensively had the cellular tissue been destroyed by suppuration, that the lid could not be freed and replaced till the dissection had reached nearly to its margin. A portion of conjunctiva was next removed from between the lid and the globe, corresponding to the everted portion of the tarsus; and the operation was finished by drawing up the lid as far as possible, and fixing it by means of slips of plaster, passing circularly from the nose to the temple. Cicatrization was rapid, but some deformity remaining, owing to the curved cartilage, which could not be made to accommodate itself to the globe, after a few weeks, Mr. Haynes

Walton removed the bent portion, laying hold of it with the tenaculum forceps, and using the scalpel. Two sutures were used. Two excellent woodcuts illustrate the state of the eye before and after operation.

Other methods of operating for ectropium are explained, and especially that of borrowing a portion of skin from some neighbouring part, and either shifting it, or by reflexion transposing it, into the opened-up cicatrice, or into the place occupied by the cicatrice previous to its being extirpated.

The Seventh Chapter is devoted to the diseases of the excreting lacrymal organs.

When any part of the body is already known by an unexceptionable name, we must object to that name being changed for a new one. We object, therefore, to *lacrymal tube,* which Mr. Haynes Walton substitutes for nasal duct, and *head of lacrymal tube,* by which he chooses to designate the lacrymal sac. Such language is apt to lead to confusion. For instance, when he tells us that the common situation for an abscess of the sac to open, is "over the head of the tube," one might suppose he meant in that part of the lacrymal sac which surpasses the palpebral tendon, for this, the *finis cæcus,* is properly the head of the tube. Generally speaking, our author, by head of the tube, means the lacrymal sac; but in the explanation of fig. 69, he limits the term to that portion of it which is above the entrance of the canaliculi.

Our author regards obstruction of the nasal duct as generally of scrofulous origin. This view of the matter is supported by the following case:—

"A scrofulous youth was brought to me, with symptoms that left no doubt of the complete obstruction of the tube, but abscess had not supervened. The lower lid was partially everted, and a constant discharge of secretions from the lid and the tube had excoriated the cheek. Eighteen months in the West Indies established his health; and with the disappearance of glandular swellings about the neck, the tube regained its patent condition." (p. 213.)

The silver style is the local means recommended by Mr. Haynes Walton for re-establishing the course of the tears, in cases of obstructed nasal duct; and full and explicit directions are given for the introduction and management of this simple and useful instrument. He says, that when expense is not a consideration, a style, silver-gilt, or of solid gold, or of platinum, should be preferred. A silver-gilt one does not answer; as, we presume, from its exciting galvanic action, it gives constant uneasiness. One of gold or platinum will be better.

Our author, in reference to sounding the nasal duct from the nostril, as proposed by La Forest, Gensoul, Morgan, and others, states, that in many examinations that he had made of the opening of the nasal duct in the dead body, the aperture was not at first discoverable, but required to be searched for with a probe. Sometimes, he says, it is on a depression in the wall of the antrum, or on a projection. He notices, also, the varied direction of the aperture. The difficulty of the proceeding, then, of sounding from the nostril, caused by the anatomical intricacies of the part, and the injury likely to be inflicted in such attempts, are more than sufficient, he thinks, to banish the proposal from practice.

Caries of the orbit, which is treated of in the Eighth Chapter, as it is so often productive of ectropium, ought to have preceded lacrymal diseases.

The subject of nævus is minutely considered in the Ninth Chapter, this abnormal growth being distinguished as cutaneous, subcutaneous, and mixed. The danger is pointed out of mistaking the subcutaneous variety for a common fatty tumour. The mode of development, and the intimate structure of nævi, still remain obscure, even after all that Rokitansky, Paget, Birkett, and others have written on these points. When a nævus, however small, shows symptoms of increase, be its nature what it may, Mr. Haynes Walton unhesitatingly advises some treatment to be adopted. He discusses the several means which may be adopted; such as pressure, intense cold, searing by red-hot platinum wire, extirpation, vaccination, escharotics, injections, the ligature, the seton, the twisted suture, and the tying of the common carotid artery.

This chapter also includes the subjects of dilated and tortuous veins, aneurism by anastomosis, and true aneurism in the orbit.

The following case is interesting, and highly creditable to our author:

"A remarkably fine girl, two months old, was brought to me at the Central London Ophthalmic Hospital, in 1851, with a slight prominence of the right eye, discovered within a month after her birth. There was no indication of any particular disease, and, after a few visits, the infant was not again brought till she was four months old. At that time the eye was prominent, the lid swollen, the cheek puffy, and the conjunctiva thickly set with large bright-red vessels. Pressure on the eyeball lessened the protrusion for a few seconds, while crying rendered the eye more vascular, and caused great temporary protrusion. In a fortnight, there was an increase of all the symptoms: pulsation was not distinctly felt; at least, I could not satisfy myself of it; however, several surgeons declared that they felt it, and the stethoscope applied over the eye detected an arterial souffle, not heard at the other orbit. Those of my colleagues at St. Mary's Hospital, who kindly examined the case, agreed with me that there was an aneurism by anastomosis. Cold lotion had been constantly applied for three weeks without effect. It was not considered prudent to apply pressure, from the pain which it seemed to produce. . . . . .

"On the 5th of June, when the child was four months and three weeks old, with the assistance of Mr. Coulson, Mr. Browne of Belfast, and Dr. Taylor, I proceeded to tie the common carotid artery, Dr. Snow administering chloroform. The incision was made one inch and three-quarters long over the course of the artery. The undeveloped state of the muscles of the neck, and the adhesion of their surfaces, peculiar to infancy, rendered the use of the knife necessary for this separation: only a very small portion of the internal jugular vein was seen. The ligature was passed, but not tied till the effect of the chloroform had subsided. This was observed as a precautionary measure, but there was not the slightest perceptible effect on the brain when the circulation was checked. Only a few drops of blood were lost.

"As soon as the child had become insensible under the influence of chloroform, the protrusion of the eyeball was greatly lessened.

"June 6th.—The protrusion remains the same.

"7th.—The wound seems to have healed by the first intention. The protrusion of the eye is sensibly diminished; the surrounding integuments have a less swollen appearance. . . . . .

"June 10th.—The protrusion of the eyeball is gradually decreasing, and the child can now easily close the lids when asleep, which she could not do prior to the operation. . . . . .

"The sutures were taken out on the fourth day, and except where the ligature passed out, there was perfect union.

"Pressure was then applied by means of pads, retained by an elastic bandage round the head.

"The last time I saw the child was on the 1st of July, 1852, and the permanency of the cure is now, I think, placed beyond a doubt; the eye has returned nearly to its natural position, only the slightest prominence remains, and all the movements are perfect." (p. 258.)

The Tenth Chapter is devoted chiefly to gonorrhœal ophthalmia, and the treatment of chemosis by scarification. With a small curved bistoury, our author recommends numerous incisions from the edge of the cornea towards the lids.

Strabismus forms the subject of the Eleventh Chapter.

In the operation of dividing the rectus internus for convergent squint, our author uses a slightly bent hook, or probe fixed in a handle, and having with the scissors made a puncture through the conjunctiva and sub-conjunctival cellular tissue, opposite to the lower edge of the muscle, he thrusts the hook up under the muscle, and then, with the scissors, divides at once the muscle and the conjunctiva. In cases where the muscle is but loosely connected to the subjacent sclerotica, this plan may do, but where the connexion is intimate, as it often is, it will be difficult to get the hook introduced to the extent required; and in the hands of resolute and inexperienced operators, there is a danger of the hook being thrust through the sclerotica. There seems little or no advantage gained by poking in the dark in this manner.

Mr. Haynes Walton enumerates a few of the unfavourable effects of the operation; but on this most important and difficult part of the subject, he throws no new light.

Tumours of the lids and conjunctiva, pterygium, tumours involving the cornea and sclerotica, orbital tumours, enlargements of the lacrymal gland, and tumours connected with the gland, form the subject of the Twelfth Chapter.

Chalazion, or tarsal tumour, of which he gives two good woodcuts, Mr. Haynes Walton treats, we think, in a clumsy and inefficient manner. The lid being everted, he punctures the tumour with a scalpel or narrow knife, and then moves about the point of the instrument to excite inflammation, and thereby destroy the secreting surface. Usually no decrease in the size of the tumour follows for two or three weeks. If a month elapses, and there is still no diminution, the operation is to be repeated. Instead of this very unsatisfactory procedure, the plan is, on the inside of the lid, to divide the tumour throughout its entire length with the lancet, and then press out the whole contents. They generally form a reddish-yellow, semi-transparent mass, of gelatinous consistence, but fibrinous composition. The cavity closes immediately, and, generally, in a few days, no trace of the tumour remains. Any scraping of the inside of the cavity, or inserting of caustic into it, is worse than useless.

Two interesting cases are related by our author, the one of wart, and the other of melanosis of the conjunctiva.

Contrary to the opinion of Bonnet, but consistent, we think, with truth, our author regards Tenon's tunic of the eye as the same structure which was afterwards described by Dalrymple, Bonnet, and O'Ferrall. He adopts O'Ferrall's error in describing it as "extending backwards to the bottom or apex of the orbit, where," he says, "its consistence becomes less marked;" whereas it adheres to the optic nerve, as this penetrates into the sclerotica, and upon this adhesion its pathological effects very much depend. Effusion of sero-purulent fluid within the ocular capsule, prevents the motions of the eyeball, protrudes it, and gives it a feeling of extreme hardness, and an appearance as if it were greatly enlarged. Of the symptoms of tumours in the same situation, we are, as yet, in a great measure, ignorant. When the eyeball is protruded, but its motions free, the cause of pressure is certainly without the ocular capsule.

Our author has collected a number of interesting cases of orbital tumours of various kinds. The combination of encysted tumour with solid growth, and the degeneration of encysted into encephaloid tumour, both of which occasionally occur, do not seem to have come under his observation.

Protrusion of the eye, combined with anæmia and enlarged thyroid, rheumatic inflammation of the ocular capsule, abscess, and periostitis of the orbit, are treated of in the Tenth Chapter.

On anæmic exophthalmus, an affection to which attention has been so ably directed by Dr. Begbie, of Edinburgh, our author has been unable to throw any new light. Varicosity of the ophthalmic veins seems to us to be the most likely cause. Mr. Haynes Walton is more disposed to attribute the displacement to effusion into the ocular sheath than to any other cause yet suggested. That this is not the cause, appears to us to be evident, from the complete freedom of motion which the eyes enjoy in their protruded state. The whole subject of protrusion of the globe, and of orbital abscesses, is highly important, and is well illustrated by our author. Our limited space prevents us from following the subject further than to remark, that to produce prominence of the globe, with immobility, it is by no means necessary that there should be adhesion of the ocular sheath to the globe of the eye. We have seen this state of matters the result of effusion merely, and have witnessed instant removal of the protrusion, and restoration of the movement of the eyeball, by puncturing the ocular capsule.

Under the head of staphyloma, which forms the subject of the Fourteenth Chapter, our attention is directed to a new pathological fact—namely, the irritation which sometimes suddenly occurs in a staphylomatous eye from changes in the state or position of the lens. The following case is given in illustration:

> "A cook, thirty years old, was burnt in her eye by some boiling lard; a moderate-sized staphyloma followed, and remained for years without any inconvenience; then, on a sudden, and without any perceptible cause, intolerable pain ensued. The staphyloma was removed; and the lens, which was black, very hard, and apparently consisting of earthy matter, escaped. An analysis was not made, nor do I know the true cause of its change; but I have little doubt that it had been suddenly detached, or partially thrown from its position, in consequence of which it acted as a foreign body." (p. 383.)

Our author gives a good woodcut of choroid staphyloma; but in his speculations regarding this disease, he misses the cause of the weakened and protruding state of the sclerotica and choroid—namely, preternatural adhesion between the two membranes, in consequence of adhesive inflammation.

Between the state represented in fig. 109, and described as an increase in the size of the entire globe, and choroid staphyloma, as represented in fig. 108, there seems no good ground for distinction. The cornea is frequently enlarged and rendered semi-opaque, in cases of long-continued sclerotico-choroiditis, embracing the whole globe.

Conical cornea is shortly discussed in the Fifteenth Chapter. Mr. Tyrrell's plan of altering the shape and position of the pupil, removing it from behind the centre of the cornea, or that part which forms the apex of the cone, to near the margin, where the least change in form has occurred, Mr. Haynes Walton has tried several times without the slightest benefit. He altogether condemns the removal of the lens by solution, in this disease. Nor has he the least faith in the use of local applications.

In the Sixteenth Chapter we are furnished with a well-digested abstract of everything worth knowing, on the subject of shaving off opacities of the cornea with the knife:

"I must warn those," says our author, "who are unaware of the almost incredible manner in which the cornea is capable of clearing by the natural process, not to attempt the removal of an opaque surface, so long as there is still evidence of inflammation remaining; for, with few exceptions, so long as minute vessels traverse its substance, the power of nature may remove, or greatly reduce the opacity, and the powers of restoration may continue for months, and even years, after their disappearance." (p. 402.)

"On the two occasions that I attempted the removal of what I considered earthy deposit, the entire texture of the cornea proved to be pervaded, and I desisted when that was discovered. I have twice attempted to shave off circumscribed central opacities of the cornea, that were just large enough to interfere with vision, and found that their depth would not admit of it; in each case, the opacity was lessened by the slight subsequent inflammation.

"The practice of purulent inoculation, which has been proposed, is so dangerous, that it is not justifiable when any power of vision exists." (p. 405.)

These remarks are consonant with common sense. The three pages which follow, on the transplanting of corneæ from dogs and pigs (pigs are best, says Dieffenbach,) to the human eye, might have been spared. No reference is made to Reisinger, the inventor of the keratoplastic system; nor to Steinberg's mode of cutting out the cornea to be transplanted to the exact size of the cornea to be supplied, a thing of some importance, if such experiments are not to be abandoned as useless.

The Seventeenth Chapter, on cataract, occupies nearly a seventh part of Mr. Haynes Walton's work. The subject is, on the whole, ably treated, but without much of novelty.

The varieties and pathology of cataract are first considered; then, the cure by operation.

We cannot agree with our author, that the lens, in infancy, "is nearly spherical," (p. 414;) nor are we at all satisfied that the central capsulo-lenticular cataract, which sometimes appears after purulent ophthalmia in infants, "must arise from the capsule of the lens having been in contact with the cornea." (p. 422.) The cataract in question is observed where there has been no penetration of the cornea by ulceration, in which case such contact as is here supposed seems impossible; and as for the figure of the lens, we have repeatedly examined it immediately after it has escaped from the eye of infants, in whom the whole cornea has been destroyed by ulceration, and have recognised its lenticular double-convex form to vary by no means much from that of the adult lens. The diameter of the adult lens measures twice its axis; the lens, to be "nearly spherical" in the infant, would require the axis to be nearly equal to the diameter; and most assuredly this is not the case.

That there are cases in which the central half, or two-thirds, of the lenticular body is opaque, while the circumference is transparent, so that, by dilating the pupil, we enable the patient to see much better, does not admit of doubt; but that there is also a variety of cataract, in which the opaque lens is of small size, never having grown from birth, but continued of the size it then had, in which, on dilating the pupil, we bring into view a dark striated zonula surrounding the lens, we are perfectly satisfied. Mr. Haynes Walton combats Mr. Dalrymple's views on this point, contending that in such cases the lens is not deficient in volume, that the central part only is opaque, and the circumference transparent. He thus confounds two states materially different.

The directions our author gives for the employment of Purkinje's catoptrical test are not sufficiently explicit. Not merely must the pupil be dilated, but the candle with which the examination is made ought to be the only light in the room, and from the intrusion of this light into his own eyes the observer must be completely shaded. If any of these conditions fail, the examination will often prove imperfect and fruitless.

The following remarks, on what our author styles lenticular coloration, are worthy of attention, and show that much still remains to be investigated as to the nature and symptoms of cataract:

"It has long occurred to me whether the lenticular coloration of age is, in itself, ever intense enough to produce opacity of the lens, in fact—a cataract; I think that it may, for how, otherwise, is the total absence of all grayness to be accounted for in cataracts which are occasionally extracted? Indeed, so far as I can judge, no difference is discernible between them and the lens in old age, except in the former being rather more coloured. In confirmation of this, I would draw attention to the fact, that the coloration is often so apparent in life, and especially in dark races of men, that it may not at first sight be possible to say when cataract does or does not exist. I have lately seen two aged mulattoes, with this amber change so apparent, that the most experienced surgeon might have been deceived, and have pronounced cataract to be present, if either of these persons had complained of defective sight. During the last year, in the case of a woman of colour, an operation for what was supposed to be a cataract was proposed, by one, to whom, both as an author and as a practitioner, ophthalmic surgery in this country owes much of its advancement: the coloration was here so intense as to have deceived him, the defect of sight being due to presbyopia and vitiated secretion from the Meibomian glands, in consequence of chronic ophthalmia." (p. 415.)

"The paleness or light-yellowness that exists behind the pupil after middle life, is often thought to be symptomatic of amaurosis; it is, however, but the effect of the coloration of the lens, and is seen with or without defective nervous power: young persons with amaurosis never exhibit this appearance. It is this coloration that produces so much difficulty in diagnosis in adults. If a young person who is losing sight shows a dark pupil, we then at once declare that cataract is absent; but with impaired vision at the middle period of life, and in the aged, the question is not so readily determined: the natural colour of the pupil renders it difficult to discriminate the disease; and I do not hesitate to say, that from this it may be impossible, unless other symptoms exist, at once thoroughly to understand a case; hence it is that surgeons, most experienced in ophthalmic disease, fall into error, sometimes pronouncing cataract to be present when amaurosis exists, and *vice versâ.*" (p. 428.)

That the question of hardness or softness of a cataract is not always easily determinable, is well known. "Well; had I thought this cataract so soft, I should not have extracted it," is not an uncommon exclamation, when the surgeon picks up the lens after the operation is finished, and peels off the soft whitish hull from the firm amber-coloured kernel.

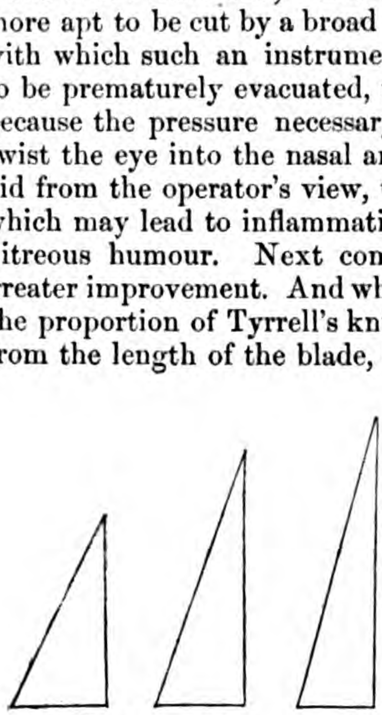
"The following anecdote," says our author, "illustrates the practical bearing of the matter. Some years since, a clergyman, forty years of age, consulted the late Mr. Tyrrell, for cataract. Mr. Tyrrell, who never used the knife when he could employ the needle for solution, determined in this instance to extract; his sudden death, however, prevented the operation. The patient then applied to a surgeon who always extracts when it is admissible, and whose success has probably never been surpassed; and we may fairly infer that he thought the cataract not hard enough to demand extraction, for he operated by solution. Here we have two highly practical men, both in extensive ophthalmic practice, differing on the question of consistence, and preferring different operations." (p. 420.)

Our author commences his account of this operation of extraction with the following statement:

"Beer's cataract-knife is that which has obtained the approbation of the majority of ophthalmic surgeons, the triangular blade peculiarly fitting it for fulfilling the desired object of cutting a flap in the cornea by one continuous movement. Mr. Tyrrell judiciously reduced its length five-sixteenths of an inch, allowing its breadth to remain unaltered. I have found it advantageous to diminish even his proportions. The annexed knife, so altered, measures, from the point to the shoulder, eight-tenths of an inch, and across the broadest part, four-tenths. When these dimensions are exceeded, it is uselessly large, frequently inconvenient, and, it may be, even dangerous, from its liability to wound the parts at the corner of the eye, when the narrowness of the palpebral commissures prevents the lids being sufficiently opened.

"The following diagram, which represents a cornea of full size, displays in the dotted lines the course the knife should take; and any breadth of instrument greater than is required to accomplish such an incision, is superfluous." (p. 442.)

In the last remark, that any *breadth* of knife greater than is required to accomplish a semi-circular incision of the cornea is superfluous, we perfectly agree; and it is plain, that unless the breadth equals the radius of the cornea, it will be too narrow. The *length* of the knife is a matter of little or no moment, because the incision is nowise affected, whether the blade measures, from its point to its shoulder, an inch or an ell. The extent of the angle at which the oblique cutting edge meets the straight back, is the point of importance; and one upon which Mr. Haynes Walton has fallen into the most extraordinary errors. Beer's knife measured from point to shoulder an inch and two-tenths, and its cutting edge met its back at an angle of 15°. With this instrument, the cornea is easily penetrated and counter-penetrated, and with very little pressure the incision is completed; the narrowness of the knife facilitating these steps of the operation, and preventing the eye-lid from being cut. With this knife, Beer operated, in the most beautiful manner, and with wonderful success, on thousands of eyes. Mr. Tyrrell did not allow the breadth of Beer's knife to remain unaltered. He reduced the length one-tenth of an inch, but increased its angle to 19°, the only effect of which was to render it more difficult and more dangerous to use: more difficult, because, the narrower the blade, the easier it is to transfix the cornea with it, and the less the force required to make it cut itself out; more dangerous, not merely because the eye-lid is more apt to be cut by a broad knife, but because, on account of the slowness with which such an instrument moves, the aqueous humour is more apt to be prematurely evacuated, than with one of well-adapted breadth; and because the pressure necessary to force a broad knife through, is apt to twist the eye into the nasal angle, whereby the inner edge of the cornea is hid from the operator's view, the eyeball being at the same time bruised, which may lead to inflammation, and even cause immediate bursting of the vitreous humour. Next comes our author, bent, in sooth, on some still greater improvement. And what does he do? He diminishes, he tells us, even the proportion of Tyrrell's knife. Yes; he snips off another couple of lines from the length of the blade, but increases the angle to not less than 27°! Here are the three knives, and the reader may make his choice at his leisure. Of course, our objections to Tyrrell's knife tell still more strongly against our author's. We hesitate not to state our belief, that no study nor practice can obviate the disadvantages attendant on the employment of such an instrument.



27° 19° 15°

The account given by our author of the several steps of the operation of extraction, is minute and judicious; as are also the directions about the after-treatment. He disapproves of the practice of binding up the eyes with a compress and bandage, as he thinks the pressure must be injurious, and the heat and discomfort inseparable from coverings, likely to lead to inflammation. As a protection to the eye from accident, he employs a very large, square, stiff shade, a little padded where it rests on the forehead; and which reaches from temple to temple and to the tip of the nose, sustaining it in position by elastic bands, one around, and two in a cross direction over the head. With the use of this contrivance, the eye cannot be accidentally struck. He disapproves of feeding the patient with slops, and allows him the usual diet at the usual times, only in less quantities.

In his description of the operation of reclination, Mr. Haynes Walton recommends a curved needle to be passed through the sclerotica, "about the sixteenth of an inch behind the cornea, in which position injury will not be inflicted," he says, "on the ciliary processes." (p. 470.) Now, the ciliary processes extend as far back from the edge of the cornea as one-eighth of an inch, and must therefore—along with a probably not less important structure, namely, the choroid muscle—be wounded, if the needle is introduced at the distance mentioned. Instead of the sixteenth of an inch, we should say the sixth was the proper place.

Our author further advises, that the needle be "directed inwards to the centre of the vitreous humour, the point carried forwards, and made to appear between the upper part of the cataract and the iris, when it is turned, and its concavity applied just above the centre of the cataract, which must first be tilted, and afterwards pressed backwards and downwards." In this way, he gives no directions respecting the posterior nor anterior capsule. They are left entirely to such fate as chance may direct. The effect of this negligence will of course generally be, that the cataract will rise as soon as the needle is disengaged. Not even then, are we directed to open the capsule; but are told that the reclination must be repeated till the cataract remains displaced; "and if, after several trials, it still re-ascend, as a last attempt the needle must be passed upwards and downwards, between its posterior part and the vitreous humour, to make a cavity for its reception." No wonder that operations for displacement are so generally unsuccessful as we believe them to be, when a plan of operation so utterly regardless of the structure of the parts concerned is adopted! It is not the healthy state of the vitreous humour, as our author hints, which offers the resistance requiring reiterated attempts to displace the cataract: it is the resistance of the capsule, and the firm adhesion of the suspensory ligament of the lens to the ciliary body. No attempt should be made to dislocate the lens, till a sufficient opening has been effected in the posterior capsule; the lens should then be reclined; and lastly, if the anterior capsule is opaque, it should be broken up. If it is transparent, as the natural boundary of the aqueous lens, and on the principle of damaging the internal textures of the organ as little as possible, and of inflicting no unnecessary injury, it should be left entire. Tilting away at the entire lenticular body, in repeated attempts at reclination, as our author advises, must often end in tearing the suspensory ligament from the choroid, and lead to destructive inflammation of the eyeball.

Mr. Haynes Walton's account of the anterior and posterior operations of division does not call for any particular remark. For the extraction of capsular cataracts, so often left after the lens is removed by absorption, he uses chiefly Charrière's canular forceps. Capsular cataracts have always been found more or less apt to follow the operations of extraction, displacement, and division, arising, in all probability, from inflammation, as is shown by their adhering so frequently to the uvea. Different modes of combating this sort of impediment have been had recourse to by different operators; some endeavouring to tear up or to displace the opaque membrane, and others extracting it, either through the sclerotica or through the cornea. Extraction through the sclerotica was practised by Freytag, with a needle-hook, *acu hamata;* while Earle, Middlemore, and others, used a small pair of forceps for the same purpose. Gibson extracted with a hook through a puncture in the cornea. We are of opinion that extraction of capsular cataracts is more safely performed through the cornea than through the sclerotica, but is more difficult of execution, especially if adhesions exist between the capsule and the uvea. Extraction through the sclerotica is sometimes followed by amaurosis, even when performed through a moderate aperture. A mere puncture may suffice, if the iris-hook of Schlagintweit be used, which, as it is now made, does not exceed the thirtieth of an inch in thickness. It is an instrument fully as much to be depended on, we think, as the canular forceps, more simple in its mechanism, and therefore not so easily deranged. Our author seems to prefer sharp-pointed canular forceps, which may, without any previous puncture, be passed through the cornea. We have not as yet been fortunate enough to meet with any sharp-pointed canular forceps, which could penetrate the cornea without excessive force. Great excellence of workmanship will indeed be required, to make the sharp blade sufficiently keen to enter the cornea readily; while the blunt and shorter one, to use our author's words, shall "have its edges so bevelled, that there shall not be any projecting angles, or any obtuseness, to impede penetration." (p. 491.) The utility of such an instrument is obvious; the aqueous humour being retained, facilitates the prehension of the opaque membrane. The whole subject of capsular secondary cataract is of great importance, and the reader will find much interesting information regarding it in the work before us. For some particular cases, our author employs a pair of forceps of his own invention, the one blade of which passes through the other, so that their extremities remain parallel at any degree of separation. The great objection to any forceps, however, except those closing by the advance of a canula, is the large incision which they require before they can be brought into operation within any part of the eye.

The chapter on cataract closes with some remarks on Mr. Tyrrell's operation of drilling capsulo-lenticular cataracts, combined with entire pupillary adhesion; and on cataract-glasses. These last Mr. Haynes Walton recommends should be of crystal, rather than glass, on the ground that the refractive power of the former being greater than that of the latter, the lenses need not be so thick. The refractive power of rock-crystal, however, is less than that of flint-glass, of which spectacles are commonly made.

In the Eighteenth Chapter, on entozoa and cysts in the eye, we observe Mr. Haynes Walton falls, in his first four figures of the cysticercus, into the usual mistake of representing it with its head up and its tail-vesicle down, a position in which the animal is never seen, in the eye. Its head being the heavier part, is always directed downwards. An interesting case is given, in which a cysticercus was extracted from an inflamed and amaurotic eye, from which the lens was absent, so that the hydatid could slip through the pupil into the posterior chamber.

Our author warns us against attempts at extracting cysts from within the eye, and recommends them to be merely punctured.

The Twentieth Chapter contains a short account of artificial eyes, and a figure of one of the old semi-ellipsoidal shape. No reference is made to the much boasted improvement of hemispherical ones.

The Twenty-first Chapter is devoted to malignant affections of the lacrymal gland, eyelids, and eyeball, including epithelioma, scirrhus, encephaloid, and melanosis. These several cancerous diseases are first described in general, and an account given of their microscopical characters; their origin, growth, and termination, are next considered; after which, encephaloid, melanosis, and scirrhus are described as they are found affecting the eyeball; malignant tumours in the orbit and eyelids follow, and the chapter ends with an estimate of the value of surgical operations in the cure of the cancerous diseases previously described. The whole chapter is excellently put together; and the views of disease and of treatment contain everything which the most recent researches of the pathologist, aided by the microscope, have been able to add to our knowledge on the subject.

Had our limits permitted, we should have been happy to have selected the account of the microscopic characters, and the estimate of the value of operations, as fair specimens of our author's sound pathological and practical views, as well as of his clear and effective manner of writing.

In the encephaloid disease of the eyeball which occurs in children, Mr. Haynes Walton states, that he can scarcely conceive any combination of circumstances that would warrant extirpation of the organ. The same observation applies, he thinks, to the rarer instances in which the disease appears in the adult; for though it may not always be so rapid in its progress, it has hitherto been found equally fatal in its results, and beyond the control of operative surgery. The highly unfavourable results of extirpation in cases of scirrhus of other organs, do not encourage to surgical interference when the eyeball is affected. As to melanosis, an early operation seems likely to be followed by prolonged life, and relief from suffering; but when protrusion and ulceration of the tumour have taken place, and still more, when there is reason to suspect that internal organs are affected, surgical interference will almost certainly precipitate the fatal termination. As a general rule, experience shows that surgical interference, in malignant tumours of the orbit, is injudicious. The same unfavourable results have been found to follow the excision of scirrhous growths and ulcerations from the eyelids, as from other parts of the body. Operations for epithelial cancer, when there is no affection of the lymphatic glands, and when every particle of the diseased structure can be removed, may sometimes be followed by a permanent cure. Such is a very condensed summary of our author's views on the practical question.

Two cases, related under the head of epithelial cancer, strikingly prove the malignity of this affection, and show the danger of being deceived by the term *epithelial* into any less suspicious view of its nature.

Were we to venture to decide the question, which is the best chapter in the work before us, we should say the Twenty-first, which treats of artificial pupil.

The conditions under which an operation may be undertaken, and those which contra-indicate it; the relative advantages of the several positions in the iris for an artificial pupil; the classification of the principal morbid states of the eye requiring such interference, with an indication of the most appropriate operations for each state, are all handled with clearness and correctness, and many practical points of importance strikingly illustrated by cases.

Our author insists on the great superiority of a central position for an artificial pupil over a lateral one; a fact which, though generally admitted, has not always been acted on, as it ought to have been. He tells us, that he has "ever preferred forming a pupil centrally, even though it should be by the side of a dense opacity of the cornea, or be somewhat shaded, to choosing the circumference of the iris, though there the cornea may be transparent." (p. 580.) When circumstances oblige him to form an artificial pupil towards the margin of the iris, and a choice is left, he prefers the inferior margin; but were retraction of the globe, or a prominent cheek, to interfere with the ready use of instruments, he would make it downwards and outwards.

Maunoir's scissors, iris-knives of three several sizes, the smallest rather broader than Adams', the middle one that of Beer, and the third somewhat broader than this; Tyrrell's hook, and two other small blunt hooks, and

Wilde's canular shears, are the instruments our author chiefly employs in forming artificial pupils. For the operation of separation, he uses a simple sharp hook, an instrument which, from its liability to let the iris loose after the separation has been begun, and to tear through the iris instead of separating it when the membrane is unsound, is inferior, we think, to Schlagintweit's hook, or even to the hook-forceps of Reisinger. The withdrawal of the simple hook, with the bit of separated iris, through the opening in the cornea, is also more difficult than that of the guarded hook of Schlagintweit.

The Twenty-second Chapter treats of extirpation of the eye, and concludes the treatise.

In the whole range of ophthalmological literature, we know of no work which, on the whole, better deserves a place in the library of the surgeon than the treatise of Mr. Haynes Walton. It is full of sound practical views, and shows the rapid advances which are being made in this department of the medical art. Most of the cases related have occurred to the author himself, and prove him to be an observing and able practitioner. His style is good, being perspicuous and unaffected. Any errors in the book are, in comparison with the mass of accurate information which it contains, but few and trivial, and will, no doubt, be corrected in a second edition, which, we believe, will speedily be called for. Most of the woodcuts are excellent, and show the applicability of wood-engraving, in the hands of a clever artist, to the representation even of objects so difficult to represent, as the eye and its diseases.

W. Mackenzie.

Review XI.

1. *Mémoire sur une Distinction Nouvelle de deux Formes de la Bronchite: précédé de quelques Considerations Générales sur l'Inflammation de la Membrane Muguese des Voies Aériennes.* Par J. H. S. Beau, Médecin de l'Hôpital St. Antoine, &c. ('Archivès Générales de Médecin, quatrième série, tom. lxvii. Sept. et Oct. 1848.')

Memoir on a New Distinction of Two Forms of Bronchitis, &c. By J. H. S. Beau.

2. *Mémoire sur quelques Parties de l'Histoire de la Bronchite et de la Broncho-pneumonie chez les Enfants.* ('Archives Générales,' Oct. 1851, et suivants.) *Même Sujet,* ('Gazette des Hôpitaux,' 1851.) *Mémoire sur la Broncho-pneumonie Vesiculaire, chez les Enfants,* ('Révue Médico-Chirurgicale de Paris,' 1852.) Par les Drs. Barthez et Rilliet, Médecins des Hôpitaux de Paris et de Géneve.

Memoirs on Bronchitis, Broncho-pneumonia, and Vesicular Broncho-pneumonia, as occurring in Infants. By Drs. Barthez and Rilliet.

3. *On the Pathological Anatomy of Bronchitis, and the Diseases of the Lung connected with Bronchial Obstruction.* By W. T. Gairdner, M.D., Fellow of the Royal College of Physicians, and Pathologist to the Royal Infirmary of Edinburgh.—*Edinburgh,* 1850.

It is now more than two years since we noticed the work of Dr. Fuchs, on the 'Bronchitis of Children.' We* had occasion, at that time, to remark upon the interest and importance of the questions which have arisen in regard to the different forms of consolidation of the lung connected with acute bronchitis in the infantile period; and we were disposed to accord to Dr. Fuchs the merit of a successful investigation of some of these questions. Our readers will recollect that Dr. Fuchs recognised a type of condensation, identical with the *lobular pneumonia* of many authors, and with the *état fœtal* of Legendre and Bailly, as being one of the characteristic pulmonary lesions of infantile bronchitis; and this form of disease he proposes, somewhat unnecessarily, to call by a new name, *apneumatosis*, in order to distinguish it from the congenital *atelectasis* of Jörg, the partially unexpanded lung of early infancy. In the work of Dr. Fuchs, the anatomy of this form of disease of the lung is described at considerable length, and three distinct stages of the affection are mentioned; and although the details of description may be objected to, in some instances, as not conveying very clear ideas of the author's meaning, yet there is no doubt that the fundamental proposition—the dependence of Legendre's *état fœtal* upon bronchitis, and especially upon the accumulation of mucus in the bronchi—is one calculated, if correct, to throw light upon infantile pathology in various directions in which the student had no little difficulty in groping his way to clear ideas. As we think that certain collateral researches, to which we shall presently advert, have left no doubt as to the general correctness of this doctrine, and as their application to pulmonary pathology has recently assumed quite a new importance, in relation to many acute and chronic diseases of the lung, both in infants and adults, we make no apology for recurring to the familiar subject of infantile "lobular pneumonia," and the allied forms of pulmonary condensation, as the starting point of the investigations which we propose to introduce to our readers.

The mere description, apart from theoretical interpretations, of a form of pulmonary disease, differing more or less from hepatization, and recognisable as that which has been since so successfully investigated by MM. Legendre and Bailly, under the name of *état fœtal*, is to be found in many treatises and monographs on the diseases of children. A type of pulmonary condensation, in which the affected parts are violet-coloured, less voluminous than natural, flaccid, tough, smooth on section, and devoid of granulations (*carnification, splenization,*)—covered by smooth and normal pleura, generally limited abruptly by the interlobular septa (*pneumonie lobulaire*)—and often seated at the anterior thin margin of the lung (*pneumonie marginale*)—has been so often adverted to, and independently described, that it is difficult to say to whom the merit of its distinction from the other forms of condensation ought to be assigned. It seems very clear, from a passage in the 'Auscultation Médiate,' that Laennec himself had not entirely overlooked the peculiarities of the lobular condensation. He describes it as occurring in portions the size of a filbert, amid crepitant tissue, and as resembling the

* It appears right to state, lest the use of the editorial "we" should cause any misapprehension, that this is the first contribution by the writer to this journal.

carnified lung of pleuritic effusion; but that he, in a great measure, misapprehended its frequency and importance, as well as its nature, cannot now be doubted by any one. He considered it as probably resulting from an imperfectly resolved inflammation of the pulmonary tissue, in its first stage; and, were it not that the passage in which his description occurs has been, for the most part, overlooked by subsequent writers, he might be supposed to have caused, by the weight and authority of his name, the error which enveloped the pathology of this lesion for so many years. As it was, the numerous descriptions of this form of condensation up to 1844, were, with few exceptions, vitiated and confused by the attempt to give it a position and classification among the different forms of pneumonia and broncho-pneumonia of children; and it was only when MM. Legendre and Bailly applied the convincing and simple test of insufflation of the lung, that the peculiarities of the so-called "lobular pneumonia," and its essential differences from all the well-known and recognised types of hepatization, became an irresistible conclusion. The experiment was not indeed new; but in this very circumstance lay no inconsiderable portion of its utility as an illustration of the pathology of lung. Those who were familiar with the researches of Jörg, published in 1832, had been long accustomed to regard the forms of pulmonary condensation which admitted of being brought back to their normal condition by insufflation, as a persistence of the fœtal unexpanded state of the organ. In the case of *new-born* children, indeed, in which alone the experiment had hitherto been tried, there were few, if any, who had disputed the correctness of this idea, and the distinction between the inflammatory and the non-inflammatory types of condensation was, in this country, and in Germany, perfectly well-established for the first periods of infantile life. In France, although the condition called by Jörg *atelectasis*, or imperfect expansion of the lung, was described with remarkable accuracy as early as 1821, in a thesis by Dugés,* yet neither his observations on this subject, nor the more complete researches of Jörg, seem to have been generally known and appreciated; and even MM. Legendre and Bailly appear to have worked out their researches upon the *état fœtal*, as a morbid state supervening after birth, without the advantage of knowing that a method similar to their own had fully established the occurrence of the same state as a congenital condition. The name of *état fœtal*, therefore, applied by them to the "lobular pneumonia" of other authors, has been justly regarded as open to objection, inasmuch as it is more applicable to the congenital than to the acquired condition.

These remarks may serve to illustrate the difficulties which environed this subject, and the extent to which these difficulties were opened up by the researches of Legendre and Bailly. There is no doubt that the pathology of infantile pulmonary disease received a most important impulse from the publication of their memoir, and that (in the words of Dr. West) they "have, by a simple experiment, thrown more light on the affections of the lungs in infancy and childhood, than all the writers of the previous ten years taken together." At the same time it will be seen, that they would have secured for their opinions a much more speedy reception, and

* See Memoir of Legendre and Bailly in Archives Générales de Méd., tom. lxiv. p 76.

would have effectually guarded them from being misapprehended, had they taken pains to appreciate the congenital conditions described by Jörg, and to connect the results obtained by that observer with their own. The two series of researches, indeed, were well calculated to lend each other mutual support; and we cannot but observe, that the views of the French authors as to the *état fœtal* have been less generally admitted in their own country than in England and Germany, principally, as we believe, because in France the researches of Jörg, and the truly congenital forms of condensation, are still, to a great extent, unknown or misunderstood.* To admit the persistence of the lung in the *état fœtal*, as a consequence of interruption to the great physiological changes taking place at birth, does not appear to any mind a very violent proposition; but to understand why the organ should return to that condition, after being once expanded, and that without any positive disease of its texture tending to occlude the air-cells, is not so easy; and MM. Legendre and Bailly, content with having demonstrated the fact, have left the explanation of it, in a great measure, to their successors in the inquiry. In passing to the examination of these more modern researches, we have thought it right to dwell for a moment upon the previous state of the question, and are now the more anxious to bear our testimony to the value of the researches of MM. Legendre and Bailly, because, in a former notice of them in this Journal, we expressed doubts of their accuracy on several points on which it has since been amply confirmed by the observations of others.

Regarding, then, the physical condition of the lung in the so-called "lobular pneumonia" as clearly established, inasmuch as it is proved, 1st, that this form of condensation has no connexion whatever with any of the ordinary types of inflammation or hepatization; 2nd, that it has the closest possible analogy, if not identity of character, with the unexpanded condition of the lung in the infant just born; we now proceed to the inquiry as to the true pathological character and rationale of this lesion. MM. Legendre and Bailly scarcely commit themselves to any

* In the latest treatise on the diseases of the infantile period, that of M. Bouchut (a work, too, professedly relating to the first two years of life), the observations of Jörg are completely ignored, and even his name is not once mentioned. The consequence of this omission is, that the key to the pulmonary pathology of the "*enfant nouveau-né*" is lost *ab initio*, and we find the author groping blindly among facts, the clear and manifest solution of which is now well known to almost all Europe. Thus, he says, "La *pneumonie* des enfants du premier âge est *d'autant plus fréquente qu'on se rapproche du moment de la naissance.* Elle est très commune chez les nouveaux-nés." (Traité Pratique des Maladies des Nouveaux-nés, &c., Paris, 1852, p. 363.) And again, "On rencontre les noyaux de *pneumonie lobulaire* engorgés au premier et au deuxième degré. . . . . *Quant au troisième degré, il est fort rare.*" (Ibid. p. 368.) It is difficult to imagine, in the busy capital of France, and in connexion with hospitals and societies, such an amount of scientific seclusion as is implied in the above statements. In a vain attempt to overthrow the conclusions of Legendre and Bailly, by showing the insufficiency of the insufflation test as a means of distinction between different forms of condensation, M. Bouchut correctly states that hepatized lungs can frequently be inflated. To this fact we can ourselves bear testimony, though we believe that M. Bouchut has overstated the fact, from not having accurately distinguished the cases of pure hepatization from the slighter forms of inflammatory lesion combined with collapse of the lung. He gains, however, nothing for his argument from this admission; for the real ground of distinction in cases of the *état fœtal*, as laid down by Legendre and Bailly, is not merely that the lung can be inflated, but that by inflation it can be brought back to an absolutely normal condition, which never can be the case when any morbid deposit is present in the air-cells (or in their walls, as is supposed by M. Bouchut to be the case in the lobular form of pneumonia). Our own observations on this subject will be shortly stated hereafter.

definite views on the subject, and merely indicate the circumstances which favour the development of the *état fœtal*, and with which it is commonly associated. These are—debility, congenital, or resulting from disease; prolonged dorsal decubitus; and obstruction of the bronchi by a thick and abundant mucus. Under these conditions the *état fœtal* most frequently occurs; but the direct agencies in the occlusion of the air-vesicles are regarded as being—1st, the natural contractility of the pulmonary air-vesicles; and, 2nd, the congestion of their walls, which principally affects the depending portions of the lung, and produces the peculiar form of condensation, called by Legendre and Bailly the *état fœtal congestionné*. How the lobular and marginal condensation affecting small and strictly limited patches at the anterior borders of the lung, is produced by causes so generalized in their action as the above, does not clearly appear; and while it is impossible not to regard the above explanation as very defective, we shall have occasion to see, hereafter, that the circumstances under which this lesion is developed in the lungs of infants have been, on the whole, very correctly appreciated by Legendre and Bailly.

Few authors who have written upon the subject of lobular pneumonia have less to retract than MM. Barthez and Rilliet; and they are quite entitled to claim the merit of having *described* accurately, even while under the influence of erroneous, or at least, imperfect ideas as to the anatomical characters of this disease. They have, as they remark, scarcely anything to change except in the department of anatomy. In their treatise on the diseases of children, they distinguished clearly between the lobar and the lobular pneumonia, considering them as diseases not merely different in situation, but in anatomical characters, causation, symptoms, results, and treatment. They regarded the lobular pneumonia as almost always a secondary disease, connected with bronchitis, and, therefore, properly distinguished by Seifert's name of broncho-pneumonia; while the lobar-pneumonia, or hepatization, was generally a primary affection of the pulmonary tissue. Moreover, they distinguished some varieties of this affection as identical in character with the carnified lung, described by Laennec as the result of pleuritic compression; thus arriving at an idea scarcely less accurate than that of Legendre and Bailly. They therefore readily adopt, in their later memoirs, the information furnished by the experiment of insufflation, and admit the normal state of the air-cells and the dilatability of the collapsed tissue in most instances. But in explaining the pathological nature of this lesion, they repudiate the ideas which appear, at first sight, naturally to spring from this admission. They refer the collapse of the pulmonary air-cells to a peculiar type of inflammation, and deny that the dilatability of the pulmonary tissue is any proof to the contrary. We are not quite sure that we altogether understand this doctrine, which is difficult to reconcile with our ideas of inflammation; but it is evident that MM. Barthez and Rilliet have been led to it by the observation of cases in which the affected lobules are not only collapsed and full of blood, but display a considerable amount of serous infiltration. In the absence of a rational explanation of the mode of production of the *état fœtal*, they ascribe this serous infiltration to a peculiar and specific inflammation, similar to that observed in the skin in some kinds of erythema, and in the exanthemata; a congestive inflammation not tending to the production of any further disorganization or effusion. How otherwise, they say, are we to explain the strictly limited and lobular character of the lesion; its occurrence in all parts of the lung, and not only at the depending portions; its occurrence in vigorous as well as debilitated infants; and its very frequent combination with inflammation of the bronchi? Do not all these circumstances point to a really active inflammatory congestion as the cause of this state of the lung, rather than to the passive congestion connected with constitutional debility, to which it is referred by Legendre and Bailly? In support of this opinion, MM. Barthez and Rilliet refer to cases of lobular pneumonia in which insufflation is very difficult and at times even impossible. This is, undoubtedly, their best objection to Legendre's theory, as it is quite certain that in these cases the walls of the air-cells must have undergone some textural change. But such cases are not the majority, at all events in the well-marked recent forms of the *état fœtal;* and it is to be remarked that the same resistance to insufflation has been observed in the true congenital form of pulmonary non-expansion or atelectasis, and has been, moreover, asserted to bear a relation to the long continuance of the lesion.* We cannot, therefore, concur with the theory which refers the lobular condensation of the infantile lung to a specific type of inflammation, any more than with Legendre's theory, which ascribes it to congestion; we think that our readers will agree with us in thinking that the difficulty is only removed a step further back by such an explanation; for why should a specific inflammation, however slight, and however peculiar in character, assume this accurately limited and lobular form? And, again, what idea can we form of an inflammation which leaves the delicate frame-work of the air-cells wholly unaltered? Such an "inflammation" can hardly be believed, in the present state of pathology, to be anything more than what it is called by MM. Legendre and Bailly, a mere congestion, with, perhaps, some œdematous infiltration. That such a condition exists in many cases of lobular condensation, we see no reason to doubt; but we confess that we are quite at a loss to find in it any adequate explanation of the fact of collapse of the air-cells subsequent to the establishment of respiration.

We have now to approach a different, and, as we think, a more satisfactory explanation of the forms of pulmonary condensation of which we have been treating. It is sufficiently evident *à priori*, that the occurrence of the *état fœtal*, or of the collapse of the pulmonary air-cells, in the distinctly limited lobular form in which it is so frequently observed, is not to be adequately accounted for by any influence acting through the general system, (such as constitutional debility,) or by a force such as the elasticity of the lung, which not being subject to variation, and being quite equably distributed, cannot well be conceived to affect certain pulmonary lobules to the exclusion of others. The theory of MM. Legendre and Bailly, is, as we have seen, (and as Barthez and Rilliet have also pointed out,) defective in attributing the *état fœtal* principally to the pulmonary elasticity, aided by everything which tends to obstruct the respiratory function. But although their rationale of the

* See Hasse on the Diseases of the Organs of Circulation and Respiration, article *Atelectasis*. Sydenham Society's translation, p. 250.

lesion is undoubtedly inadequate, we shall find that the observations of MM. Legendre and Bailly give a prominent position to one circumstance in connexion with the *état fœtal*, which has also attracted the notice of other observers. They state that the production of the *état fœtal* is *favoured* by the accumulation in the bronchi of thick mucus, and in proof of this opinion, they assert, that while they have met with this condition in eight cases, (in enfeebled infants,) "independently of all pulmonary inflammation," it has, in thirty-nine cases, been found connected, either with what they call "catarrhal pneumonia," (capillary bronchitis,) or with bronchial catarrh; and in twenty-seven of these thirty-nine cases, it was the pulmonary affection alone which caused death. These details certainly present strong evidence of an intimate connexion between collapse of the pulmonary air-cells and bronchial mucous accumulation.

This observation is not, however, peculiar to MM. Legendre and Bailly. Rilliet and Barthez, Fauvel, and other observers, have borne ample testimony to the connexion between pulmonary catarrh (particularly in the form called by the various names of bronchitis, suffocative catarrh, and peripneumona notha) and the lobular forms of pulmonary condensation. Scarcely less distinct is the evidence on the subject in the excellent work of Seifert.* This author, although writing before the French observers, to whom we owe the first careful and systematic description of the lobular forms of condensation, has nevertheless pointed out the connexion of infantile bronchitis with a peculiar form of "pneumonia," which the attentive and instructed reader will at once recognise as the "carnification" or "lobular pneumonia" of Rilliet and Barthez, and the *état fœtal* of Legendre and Bailly.† Nor are there wanting still earlier observations tending to the same conclusion. Dr. West has directed attention to a very remarkable passage in Dr. Alderson's paper on hooping-cough,‡ which shows a very thorough appreciation of the differences between lobular condensation and true hepatization; and in signalizing the former as the peculiar lesion of hooping-cough, Dr. Alderson will be admitted to have furnished an important testimony to its probable connexion with catarrhal affections. Moreover, Jörg has admitted, in relation to the unexpanded lung of new-born infants, the influence of pulmonary catarrh in maintaining and perpetuating this condition; although among the numerous causes to which he attributes atelectasis of the lung, the one above alluded to has by no means a prominent place. Finally, it may be observed, that in the description by Rokitansky§ of catarrhal pneumonia, the reader will not have much difficulty in recognising the forms of condensation of the lung to which we have been directing his attention.

From all these separate observations, converging, as it were, to a point, it follows, clearly enough, we think, that the *état fœtal*, or collapse of the air-cells, when occurring in a lung that has been once expanded, is, in all probability, a secondary lesion, and dependent, in the majority of instances,

* Die Bronchio-pneumonie der Neu-gebornen und Säuglinge. Berlin, 1837.

† Singularly enough, Seifert seems to have altogether overlooked the researches of Jörg, which could scarcely have failed, had he considered them carefully, to have modified his own views. Nor are we aware of any writer before Hasse who attempts the differential diagnosis of lobular pneumonia, as distinguished from the atelectasis of Jörg. Hasse's very acute remarks on this subject are well worthy of perusal.

‡ Medico-Chirurgical Transactions for 1830, pp. 90, 91.

§ Path. Anat., vol. iii. p. 106.

on a catarrhal condition of the bronchial tubes. This is, indeed, the most important conclusion arrived at by Dr. Fuchs, in the memoir formerly reviewed by us.* The condition of *apneumatosis* (we write it again, and for the last time, to express our earnest hope, that the mere sight of the intolerable mouthful will make every Saxon man among our readers do battle with fresh zeal against all such *sesquipedalia verba*) is described in this memoir as a quite ordinary and almost invariable concomitant of severe infantile bronchitis. The symptoms which it determines are described with very considerable minuteness, and, so far as we can judge, with accuracy; but in this portion of the work we find few additions to our knowledge. The main advance in the point of view assumed by Fuchs, as contrasted with that of Legendre and Bailly, consists in his having clearly kept before his eyes the fact of the connexion of collapse of the air-cells with pulmonary catarrh, and treated this connexion throughout as not accidental, or of secondary importance, but as one involving the theoretical view of the dependence of the pulmonary condensation mainly upon the obstruction of the bronchi by mucus. He gives many cases in illustration of the effects of infantile bronchitis; but by far the most valuable facts which he brings to bear on the subject are from the experiments of Mendelsohn and Traube, which scarcely leave anything to be desired. To these excellent investigators undoubtedly belongs the merit of clearly establishing the nature of the connexion between bronchitis and collapse of the pulmonary air-cells. We think it worth while to put our readers in possession of some of these experiments, as we are not aware that they have hitherto been noticed in any English journal. The experiments of Mendelsohn will be found in his excellent little work, 'Der Mechanismus der Respiration und Circulation" (p. 37); those of Traube, in his 'Beiträge zur experimentellen Pathologie und Physiologie, erstes Heft.'

Among the most striking of these experiments were the following. Tracheotomy being performed on a rabbit, a shot or slug (the size is not mentioned) was inserted into the trachea, and impacted into the left bronchus by means of a probe. The animal died in two days. "The right lung was large and emphysematous; the left collapsed; the lower lobe in great part red, void of air, as also the upper lobe in some portions, in the midst of which were emphysematous parts. The whole lung could be inflated from the trachea (of course, after removal of the obstacle)." In other instances, paper-balls and solutions of gum were employed, with results in a great measure the same. The condensed lung did not always yield completely to insufflation, and some other slighter differences were observed in the experiments; but the details are not given with the precision requisite for determining the nature of these differences. In one instance, it is stated that the juice from the condensed part "showed, under the microscope, almost no air, no blood (?), and many granulated, mostly round, bodies of different size:" the nature of these last we must leave our readers to discover. The above experiments are those of Mendelsohn.

Traube's experiments are nearly to the same purpose. A rabbit, having a paper plug inserted into the air passages, died in somewhat more than twenty-four hours.

* Die Bronchitis der Kinder. Leipzig, 1849.

"The right lung was, in its whole extent, dark-red, and uniformly solid to the touch; it had no appearance of air-vesicles on the surface. The lower lobe was completely distensible, and had, after insufflation, all the properties of the normal tissue. The upper lobe was left undistended for further examination of its physical condition Superficial incisions made perpendicularly into its substance showed a smooth glancing surface, from which, even upon pressure, no blood flowed out, except where considerable vessels were divided. Portions of the lung sank completely in water; there was no fluid (?) either in the trachea or the bronchi. The trachea was, before the lung was investigated, divided immediately above the bifurcation; the entrance to the right lung was found quite filled up by the plug; the left bronchus was also partially obstructed by its upper portion. The altered parts of the pulmonary texture yielded *only normal elements* [we presume to microscopic investigation]."*

The above experiments leave, as we think, no doubt as to the very considerable influence of bronchial obstruction in producing that physical condition of the pulmonary texture variously called collapse, état fœtal, atelectasis, lobular pneumonia, carnification, &c. (And here let us remark, parenthetically, that as the first of these names appears to have the merit of being intelligible, vernacular, and easily pronounced, while it has the further advantage of including all the others, and being unconnected with any theory or debatable position, we shall hereafter employ it alone, understanding by it the *simple* occlusion of the air-cells, whether congenital or acquired.) The mechanism of this lesion, as produced artificially in the experiments above related, may be open to discussion; but no doubt can exist as to the fact, that the existence of solid or fluid obstructions in the air-passages tends, in an eminent degree, to the production of pulmonary collapse, and that large portions of the lung may even be emptied completely of air in the course of a few hours, if the obstruction be considerable. Dr. Fuchs has gone, as it appears to us, very far out of his way to seek an explanation of this phenomenon. "How does it (the air) vanish?" he remarks. "Either it is received into the circulating blood, or it is in some other way absorbed, as gaseous substances are when they are extravasated into the cellular tissue." Had Dr. Fuchs been seeking a cause for the disappearance of some of those "vital spirits" or "humours," with which the old pathology abounded, but which were so attenuated and sublimated, that nobody ever succeeded in tracking them, he could not have been reduced to a more extraordinary hypothesis. We should, in this case, have scarcely looked for his adopting the simple view, that the "spirits" are expelled from the lung by the same track by which they are introduced into it; on the same principle that we should not expect a genuine ghost-seer to tell us, that a spirit from the unseen world had vanished by the door, if there was a possibility that it might have made its exit by the window, or the chimney, or have been "in some other way absorbed." We think that if Dr. Fuchs will reconsider his theory, he will find reason to commute it for a more natural one. It is to us sufficiently evident, that when the bronchi are plugged either with fluid or with solid substances, the obstruction is never absolutely complete; the air finds its way gradually out of the air-cells by the side of the obstruction; and it is prevented from returning, by a very intelligible mechanical condition. In such a series

* We have not access to the original; the above is taken from the work of Fuchs, where it has the appearance of an extract, but is not precisely indicated as such.

of diminishing tubes as the bronchi, an obstruction by a pellet of solid matter, or of viscid mucus, acts the part of the ball-valve of a syringe; the occlusion is tolerably perfect in the one direction, when the obstruction is driven onwards towards the narrower tubes by the force of inspiration; but the opposite, or expiratory, force tends constantly to dislodge the obstructing body, by pushing it towards the wider end of the tube, and hence the exit of air is always permitted while its entrance is constantly opposed.

We find, then, that theory and experiment concur in affording some explanation of the forms of condensation or collapse of the lung, which are so common in the infantile period, and the nature of which has been the subject of so much controversy. The connexion of collapse of the lung with bronchial obstruction appears well fitted, *à priori*, to account for its frequent occurrence in the lobular form; as the impaction of pellets of thick mucus in particular bronchial divisions furnishes the condition necessary for the production of this lesion. The congestive form of the *état fœtal* also, the "carnification" of Rilliet and Barthez, occurring, as it does, usually at the posterior parts of the lungs, and in large masses, would equally be explained by the known tendency of bronchial mucus to accumulate in the posterior bronchial divisions. Moreover, the very generally concurring testimony of authors as to the frequent coincidence of pulmonary collapse with bronchitis, furnishes an additional argument to this chain of pathological and experimental reasoning. We have seen that Rilliet and Barthez, Legendre and Bailly, Fauvel, Seifert, and Fuchs, are at one on this point; and we may add to these authorities the name of Dr. West, who has treated this subject in a very clear and satisfactory manner, and has both acknowledged and ably illustrated the influence of bronchitis in increasing the tendency to collapse of the lung.* Under these circumstances, it might almost be assumed, without more direct proof, that wherever collapse of the lung exists without other evident cause, it has its source in bronchial obstruction, were it not for three reasons—firstly, that up to this point, the mutual relation of these conditions has only been asserted in children below five years of age; secondly, that instances of pulmonary collapse have been recorded by Jörg, Legendre and Bailly, West, &c., apart from the appearances of bronchitis or distinct bronchial obstruction; thirdly, that neither in the child nor in the adult has it been shown, as a result of direct observation, that in the ordinary forms of bronchitis attended by mucous accumulation, obstructed bronchi are usually accompanied by collapse of the corresponding lobules. Nay, so far as recorded authority bears upon the question, the presumption in the case of the adult is in the opposite direction, since it is well known that a prevalent opinion, founded on a theory of emphysema by Laennec, has ascribed that affection of the lung to accumulation of mucus in the bronchi as its direct cause. The acknowledged frequency, indeed, of emphysema of the lung, or *distension* of the air-vesicles, as a complication of pulmonary catarrh in the child, as well as in the adult, would appear to neutralize, to a very considerable extent, the force of the evidence we have just been considering, as to the obstruction of the bronchi with *collapse* of those vesicles; and the other objections stated above to this doctrine might well be regarded

* Diseases of Infancy and Childhood, Lecture 14.

as fatal to its general application, if not subversive of it altogether. Here, however, we are enabled to call to our aid other observations bearing on the points just mentioned, and lately published by the author of this review in the 'Monthly Journal of Medical Science.' These researches, republished in the form indicated at the head of this article, maintain the connexion of collapse of the air-cells with bronchial obstruction, both in the child and in the adult; and from observations, chiefly on the latter class of subjects, it appears that the condition of collapse of the lung demands more consideration than it has yet received in relation to many of the most common pathological conditions of the organs of respiration at all periods of life.

It must have struck every one who has given attention to this subject as not a little curious, that so sharp a distinction should have been drawn between the affections of the lung in early childhood and those of the other periods of life. By almost all of the authors already mentioned (we might say *all*, with the exceptions presently to be mentioned), the peculiar and characteristic lesion of lobular pulmonary collapse is spoken of as an affection quite peculiar to the infantile period. Some of the earliest writers on the subject distinctly state, that of the two forms of pneumonia, the lobular and the lobar, the former *alone* is to be found in early infancy, the latter *alone* beyond that age. The latest authorities, in some instances at least, will be found to maintain the same opinion; and notwithstanding the distinct observations of Barthez and Rilliet, Legendre and Bailly, Friedleben, and others, as to the occurrence of *lobar* hepatization in young infants, Fuchs still asserts that "true pneumonia" does not appear to present itself in children below the age of five years; while, on the other hand, he describes the lobular collapse as a disease of infancy, and like almost all other authors, is silent as to its occurrence at other ages. The idea here suggested, that an absolute revolution in pulmonary pathology takes place, dating about the fifth year of life, would deserve more consideration than it has received, were it not for the suspicion which naturally arises, that imperfect or limited observation has more to do with the result than the organic differences of the infantile and adult lung. Indeed, few of the writers on children's diseases have professed to interpret the facts beyond their own special sphere of observation; and this circumstance, together with the praiseworthy diligence and minute accuracy with which the diseases of the infantile period have been studied during the last twenty years, has perhaps tended to give these diseases too much of a place apart and out of harmony with the general mass of pathological knowledge. In the present instance, the attempts to establish an analogy between the infantile and adult lesions of the lung, although not numerous, are sufficiently striking to deserve notice. Thus Rilliet and Barthez pointed out the resemblance between the infantile collapse and the "carnification" described by Laennec as the result of pleurisy. Seifert came somewhat nearer the mark, by showing that the lesions associated with broncho-pneumonia in infants, bear a strong resemblance to the "*peripneumonie des agonisans*" of Laennec, and the "*peripneumonie hypostatique*" of Piorry. Legendre and Bailly refer to the catarrhal affections of aged persons, in order to show "how much the diseases of the two extremes of life resemble each other;" and they admit, in general terms, a resemblance between the catarrhal pneumonia of infants and the "*peripneumonia notha*" of all ages. Finally, Dr. West enters somewhat more fully and satisfactorily into the subject; by a reference to the researches of MM. Hourmann and Dechambre, he establishes the reappearance of the phenomena of pulmonary collapse at the period of "second childhood;" and from the descriptions of typhoid fever by Louis, he surmises that "it would seem as if, in some diseases attended with much depression of the vital powers, this collapse of the lung were by no means unusual." The identity of the affection in the adult with that in the child is further proved by the experience of Dr. Baly, who communicated to Dr. West three cases of collapse of the lung in the adult, occurring in fever, with dysenteric symptoms; and the description given of one of these cases is so careful and exact, as to leave nothing wanting to the evidence, that in it the infantile collapse or lobular pneumonia was present in its most distinct form.

These observations indicate sufficiently clearly that, in some exceptional instances at least, collapse of the lung, even in its lobular form, is a disease of adult life. The occurrence of this lesion in old persons, in typhus fever, and in fatal dysentery, appears to Dr. West to indicate that a certain degree of bodily weakness, aided, in some instances, by bronchial obstruction, is the determining cause of its production. So far as these conclusions go, they are all affirmed by our own observations; which show that collapse of the lung, whether in the lobular or diffused form, is an exceedingly common lesion in the adult; "that in all essential characters it is the same in children and adults; that in both a certain degree of pulmonary collapse may be almost invariably found as a concomitant of fatal bronchitis; and that, in some cases, this state of the lung bears so obvious and undeniable a relation to obstruction of the tubes, as to lead to the almost unavoidable inference of the dependence of the former upon the latter." (pp. 19, 20.)

For the narrative of the personal observations on which these conclusions are founded, we beg to refer the reader to the memoir above referred to; merely observing, that these observations were made, in the first instance, almost entirely on adults, without reference to the facts or reasonings previously alluded to in this review; and that, therefore, they lend a valuable, because an independent, support to the theory which we have endeavoured above to deduce from the general history of this subject.

The following descriptions of collapse of the lung, as occurring in the adult in connexion with bronchitis, will no doubt be readily followed by those familiar with the descriptions of similar states in the infantile lung:

"*Bronchitic collapse of the lung* occurs under two distinct aspects: the *diffused* form, and the limited or *lobular* form. Of these, the latter variety is the more striking or characteristic, and has been, especially in the lungs of children, the subject of more discussion than the former; but the diffused form is by far the more common, and is, in fact, of very frequent occurrence, at least in its slighter degrees. Both forms present the same fundamental changes of the pulmonary tissue, which is usually of a dark violet colour externally, as seen beneath the pleura; and internally, of a more or less deep brownish red or mahogany tint. The colour, however, is by no means an invariable criterion, depending almost entirely on the amount of blood in the collapsed tissue. The affected parts are always more or less condensed: this condensation may amount to a mere diminution of the crepitation, or to a total absence of it, in which case portions are usually found to sink readily in water. These latter portions are both more flaccid and much less friable than the pulmonary tissue, when in a state of red hepatization; and they differ greatly from this lesion in the aspect of their section and the nature of the fluid it yields to the knife. In every variety of true pneumonic consolidation, in which the lung is completely void of air, the air-cells are occupied by a deposit, presenting to the naked eye (and still more distinctly to a power of 20 to 30 diameters) the well-known granular aspect of the hepatized lung. If the deposit is fluid or semi-fluid, it is capable of being pressed out, or scraped off, in the form of a thick, opaque, emulsion-like matter, of yellowish, orange, or grey colour; and in all cases it shows, under the microscope, abundant granular elements and cell structures, of the kinds usually found in inflammatory exudations in parenchymatous organs. In the collapsed lung, on the contrary, the section is comparatively smooth, having somewhat the appearance, as described by Laennec, of muscular flesh; it presents no trace of granulations, and yields, on pressure, or to the knife, only a semi-transparent bloody serosity, which, under the microscope, is seen to contain little or nothing besides blood-corpuscles, epithelium, and other portions of normal tissue, and, possibly, a small amount of pus, from the interior of the bronchi.

"In the diffused variety, the collapsed condition may be found affecting a more or less considerable portion of either or both lungs, usually at their posterior part, and passing quite gradually into the normal tissue; the supple, dense, tough feeling being exchanged for the normal spongy, elastic crepitation; and the violet, or deep purple colour, shading off into the usual hue of the surface. Even in this form of the lesion, however, a tendency of it at some points to be circumscribed by the interlobular divisions, may often be observed; this tendency being, so far as I have observed, quite characteristic of the bronchitic as opposed to the pneumonic consolidations.

"In the *lobular* forms of bronchitic collapse, which often occupy the anterior edges, as well as all other parts of the lung, the affected portions are everywhere accurately and abruptly marked off by the interlobular septa, the portions so limited being various in size and form, but always manifestly shrivelled and sunk in below the level of the surrounding parts. This is peculiarly manifest when they occupy the anterior edges. When they are scattered through the lung, they communicate to the fingers, in feeling the organ externally, much the same sensation as clustered tubercles in the midst of crepitant tissue. . . . . I have employed inflation in both the diffused and lobular form of collapse, as observed in adults, and as above described, with precisely the same results as those of Bailly and Legendre. I may state, however, that though this test is very useful in demonstrating the nature of the lesion, in a favourable case, to one not familiar with its character, I do not believe it to be applicable to the determination of the presence or absence of pneumonia in those mixed cases in which alone there is any difficulty. For I have observed that, on the one hand, the partially pneumonic lung may be inflated when the affection is recent, and combined, as it frequently is, with bronchitic collapse; and, on the other, that in the latter lesion, in its purest forms, complete inflation is often very difficult or impossible, after the collapsed state has been of some duration. In fact, the lung then begins to undergo a modification in its nutrition and structure, which ultimately leads to permanent atrophy." (pp. 12—14.)

We have already discussed so fully the evidence on the causation of pulmonary collapse, that it appears unnecessary to say more, at least on the subject of the influence of obstruction. We are desirous, however, of directing attention to the following general summary of these causes.

"In considering as a whole the causes which tend to produce bronchitic collapse (as revealed in the preceding investigation), they seem to resolve themselves into the following:—*Firstly*, the existence of mucus in the bronchi, which is more liable to produce obstruction according as it is thick and viscid; *secondly*, weakness, or inefficiency of the inspiratory power, however caused; *thirdly*, inability to cough and expectorate, and thus to remove the obstructing mucus. Of these conditions, the first must be considered as the exciting cause, the others as predisposing causes, co-operating with the first, but incapable without it of producing collapse." (p. 32.)

It will readily be understood from the above sentences, that in very debilitated subjects the most trivial mucous accumulations will suffice to produce collapse of the lung; and therefore that, under such circumstances, bronchitis is not necessarily present as its cause, because the mere accumulation of the natural secretion will lead to a sufficient amount of obstruction. Hence the extreme frequency of those forms of diffused pulmonary collapse which constitute the "hypostatic" pneumonia of M. Piorry, and the "*peripneumonie des agonisans*" of Laennec; and we believe it to be often, under similar conditions, in the asthenic forms of acute and chronic disease that the types of condensation occur which have been described under the names of "typhoid" or "latent" pneumonia.

"Again and again (we quote from the memoir above mentioned) has it occurred to me, under such circumstances, to open the bodies of persons in whom no suspicion of a respiratory affection existed during life, and in whom, nevertheless, condensation, abruptly lobular or diffused, has been found after death, affecting large portions of one or both lungs. On the other hand, I can remember instances in which a superfluous zeal, or *nimia diligentia*, in stethoscopic studies has detected the signs of these lesions, when not a single rational symptom existed to call for such examination; and very many cases, where the extent of the lesion was altogether out of proportion to the gravity of the symptoms that attended its accession."—(pp. 24, 25.)

The above remarks, it is worth while to observe, concur with the observations of M. Piorry on the hypostatic pneumonia, which he has found to occur at one period or other in the course of the greater number of severe diseases, and to be dissipated during the convalescence.

The reason of the great liability of the child to pulmonary collapse as a consequence of bronchial obstruction is very evident from the above considerations as to the cause of this lesion. In the first place the want of resistance in the bones of the infantile chest prevents the muscles from acting on individual portions of it with the force necessary, in some cases, to overcome bronchial obstruction; and accordingly deformity not unfrequently takes place in early life, mainly determined by this circumstance. But further, the very frequent existence of debility and imperfect nourishment in young children predisposes them very decidedly to the development of this lesion, even under slight obstructing causes; and it is well known that both the congenital and the acquired collapse are most readily developed under these conditions.

Having thus endeavoured to determine the relation of collapse of the lung to bronchitis, and to show its immense frequency and vast importance in pulmonary pathology, we shall proceed to lay before the reader some considerations as to the secondary organic results of this pathological condition. In this part of the subject, we shall be under the necessity of referring chiefly to our own personal researches; and we shall embrace what we have to say upon it here under three heads:

1. *Collapse of the Lung, on becoming permanent, gives rise to a peculiar*

form of Pulmonary Atrophy.—The lesion which we here refer to has nearly the same relation to the atrophy which surrounds retrograde tubercles, or which results from chronic pneumonia, as collapse of the lung itself bears to recent hepatization. In the atrophy from tubercular disease, the tissue is indurated, dark-coloured from carbonaceous deposit, and often charged with the remains of former exudation in the air-vesicles and their walls. In the atrophy from collapse of the lung none of these characters are present; the pulmonary tissue has simply disappeared, leaving a small amount of fibrous tissue, and sometimes a few specks of carbonaceous matter, in its place. The transition-stages between collapse, removable by inflation, and permanent atrophy, have been carefully observed in the bronchitic lung ('Memoir,' p. 67), and are also noticed as the result of congenital atelectasis by Hasse, (*loc. cit.*)

"In *simple atrophy of the lung*, the result of uncomplicated bronchitic collapse, the affected parts usually present somewhat different characters from other forms of pulmonary atrophy. They are, in fact, reduced to a lax fibrous or areolar texture, inclosing the remains of bronchi and vessels; perfectly flaccid, free from all induration or abnormal exudation, and very frequently, in the purest form of the lesion, free even from that excessive deposit of carbonaceous pigment, which is so apt to accompany all chronic affections of the lung. Such atrophied lobules will almost invariably be found, on examining the free anterior or lower margins of old emphysematous lungs; and, in more recent specimens of emphysema, the anatomist will generally be able to trace several of the stages which I have indicated above as intervening between collapse and atrophy. The atrophied lobules at the edge of the lung correspond to the indentations and grooves between the emphysematous parts. On examining them closely, there will often be found a thin lamina spread out between two emphysematous prominences, like the web of a frog's foot, and composed of the two pleural layers, enclosing the attenuated remains of the pulmonary tissue. Such portions are generally clearly and definitely marked off by the interlobular septa from the emphysematous lobules in their neighbourhood. In other instances, scarcely even this amount of tissue can be traced, and the two pleural layers may appear to be almost in contact over a small space, with little or no intervening substance. To any one who attentively studies a variety of such specimens, it will be apparent that simple atrophy of the lung, in its most complete form, is a lesion only to be distinguished by negative characters. The proper and special elements of the pulmonary tissue have disappeared; but they are not replaced (as in atrophy from other causes) by any adventitious structure, or even by the thickening or induration of the fibrous basis. For this reason, simple atrophy is sure to be overlooked, unless its traces be sought for in the manner I have described. In the centre of a lung, very many lobules may be entirely atrophied, and leave no visible or tangible evidence of their previous existence.

"Simple atrophy, like the lesion which gives rise to it, occurs in the lobular and the diffused form. The latter is chiefly found in the posterior portions of the lungs, near their root, among the great bronchi and the bronchial glands, which are often, in these cases, dark coloured from infiltrated carbon, even when the lungs are by no means remarkably so. In diffused simple atrophy, the lung is rarely entirely condensed, generally retaining a certain degree of crepitation, but being dense, tough, and fibrous; sometimes dark slate-coloured, at other times not so; and in the most marked and exaggerated examples, crossed in every direction by fibrous processes, or septa of considerable thickness and density, corresponding to numerous depressions and irregularities on the surface of the lung, which is usually, in these cases, very emphysematous in front, and over the surface generally. Such lungs will always be found, when a fresh section is inspected, with or without a lens, to present the most remarkable varieties in the size of the air-vesicles some of which are entirely obliterated, or very small, and others greatly expanded beyond the normal volume; the latter condition prevailing, of course, towards the anterior margins in the most emphysematous parts." (pp. 67, 68.)

The condition here described occupies, as might be expected from the great frequency of pulmonary collapse, a very important place among the chronic diseases of the lung. It is not unfrequently difficult to identify it, from the reasons stated in the above extract; and indeed its extensive prevalence at the root of the lung must always be, to some extent, a matter of inference rather than of direct observation. But any one who, having carefully observed one or two instances of the transition from pulmonary collapse into permanent atrophy, will carry the knowledge so acquired to the solution of chronic pulmonary lesions in general, will find a great amount of light shed on many otherwise inexplicable phenomena. The extreme frequency of depression, cicatrices, and slight indurations of the pulmonary tissue, has long been a matter of daily observation; and though it has been of late years customary to refer to these, somewhat vaguely and indiscriminately, as the results of pulmonary tubercle in an obsolete condition, yet there have not been wanting those who have expressed misgivings on the subject. The differences in the average frequency of these lesions, as given by different observers, is of itself a circumstance calculated to raise doubts as to the absolute correctness of their individual observations. Laennec, who first directed attention to these cicatrices as affording evidence of the frequent cure of tubercular affections, says, that they are observed "in almost all the phthisical subjects, and perhaps a quarter of the rest." No one will suspect Laennec of making a random statement upon a point of this magnitude; indeed, the evident care and minuteness of his descriptions of these cicatrices, his division of them into cartilaginous, fibrous, and cellular,—his correct appreciation of their relation to tubercular cavities in some instances, and concretions in others,—show that scarcely any important fact as to the progress and cure of tubercle had escaped his keen observation.

Our own observations, made specially with reference to this point, closely confirm those of the inventor of mediate auscultation; assuming, what we believe to be the case, that he had mentally disregarded those cicatrices which were not accompanied by collateral evidences of tubercular origin. We have found cicatrices, *presumably tubercular*, to exist in between 40 and 50 per cent. of the hospital patients examined by us, nearly a third of the whole number examined being cases of phthisis pulmonalis. Over and above this number, however, a large proportion of lungs contain more or less marked indications of partial atrophy, and often cicatrices and depressions of pervious parts of the surface. Sometimes we have even found calcareous concretions in considerable numbers, and so disposed, as to lead us to ascribe them to some other cause than tubercle. We cannot, with the evidence before us, doubt for a moment that many of these atrophic lesions have their origin in bronchitis and collapse of the lung; sometimes complicated with those local ulcerations which, to distinguish them from tubercular ulcerations, have been called "vacuoles" by the French pathologists, and by us "bronchial abscesses." The distinction between the tubercular and non-tubercular cicatrices is, however, we are ready to admit, often difficult, and not always possible. We find, accordingly, a considerable amount of variation in the results arrived at by those who have repeated Laennec's observations as to the cure of tubercle. M. Rogée, of Paris, finds evidence of healing tubercle in 51 per cent. of the bodies examined by him in hospitals; M. Boudet, on the other hand, gives 86 per cent.; Dr. Bennett, of Edinburgh, not more than 40 per cent., probably a smaller proportion even than that indicated by Laennec.

2. *Collapse of the Lung and Pulmonary Atrophy are the invariable accompaniments of Emphysema of the Lung; and the last-mentioned condition depends essentially on one or both of the former two as its cause.*—Although a great number of conflicting theories have been proposed to account for the development of pulmonary emphysema, not one of these has maintained for itself the position of a general law. The law of the production of emphysema can be said to be demonstrated, only when the conditions under which that lesion arises have been inductively sought and rationally connected, in such a manner as to leave no unexplained facts and no residual solutions. It is scarcely necessary to show that the non-mechanical explanations of emphysema (e. g., those which ascribe it to diminished tone or vital elasticity in the pulmonary texture) have no such demonstrative basis. The theory of Laennec, which ascribes emphysema to mucus in the bronchi and accumulation of air behind the obstruction, is vitiated by the ample proof which now exists that obstruction of the bronchi has precisely an opposite effect, giving rise to voiding of the air-vesicles and collapse of the lung; and it is, moreover, inconsistent with the observation, that emphysema usually occupies the anterior edges of the lung, while the accumulations of bronchial mucus generally take place at the back part of the organ. The idea that emphysema is dependent upon repeated forcible expirations, as in violent coughing, has never been supported by any considerable amount of direct proof, and is, as we believe, and have elsewhere endeavoured to prove in detail, totally untenable in a logical point of view; for the expiratory act is undoubtedly mechanically incapable of producing any other effect upon the air-vesicles than to empty and contract them.* The inspiration-theory of Dr. Williams† and others, which refers emphysema to the increase of the force with which air penetrates into the sound portions of lungs that are partially diseased, is in harmony with many facts; but it has never been put forward as a general law, and cannot be accepted as such; indeed, we shall immediately show that it is inconsistent with some of the facts to be explained, and insufficient for the solution of others. Lastly, the numerous eclectic theorists (as, for instance, Fuchs, Williams, &c.) who have embraced *several* of the above mutually exclusive speculations, merely show that they had not found their way to any really comprehensive solution of the difficulties connected with this subject.

Under these circumstances, we beg to assure our readers, that nothing but the most complete conviction of the accuracy of the views we are about to submit, could induce us to state our opinion more dogmatically than our predecessors in the inquiry: we feel, however, justified by an analysis of

* See the Memoir above quoted, p. 57; and a debate in the Medico-Chirurgical Society of Edinburgh, quoted in the Monthly Journal, vol. xiv. p. 471.

† Diseases of the Chest. Third edition.

observations extending over several years, and by all the consideration we have been able to give to the views of others, in stating—1st, that emphysema is a lesion of mechanical origin; 2ndly, that it is produced by the inspiration-force, and never during the expiration; 3rdly, that it is due *solely* and *exclusively* to the action of that force upon the previously sound air-vesicles, in lungs which have undergone, in other portions, atrophy or collapse (i. e., partial diminution of volume). This diminution of volume in a portion or portions of the lungs, we consider as the sole pathological condition necessary to the production of emphysema. We proceed to a short statement of the evidence of these propositions, or rather of the third of them, which includes the other two.

The first statement which we shall make tending to establish the connexion between emphysema and partial diminution of volume in the pulmonary texture, is, that emphysema never occurs unaccompanied by pulmonary collapse or by one or other form of pulmonary atrophy. We make this assertion as the result of direct personal experience, and are prepared, moreover, to show that it is consistent with general observation. In the memoir to which we have already alluded, p. 46 *et seq.*, there will be found an analysis of forty cases of emphysema, collected from a series of 502 miscellaneous cases of disease, and without any special reference to the present inquiry. Among these, two only are not noted as presenting some form of condensation of the pulmonary texture; all the others being affected either with collapse, lobular or diffused (often passing into simple atrophy), atrophy with induration, concretions, hepatization, or tubercle. That the two exceptional cases above mentioned are rendered so by imperfect observation, is made almost certain by internal evidence, as well as by the whole of our observations since this period, which have not furnished us with a single parallel instance. But further, an analysis of the cases of emphysema, in contrast with the miscellaneous cases, gives the following remarkable result:

	In Mixed cases.		In Emphysematous cases.
Hepatization of the lung . .	9·8 per cent.	......	10·0 per cent.
Tubercle of lung	20·0 „	......	20·0 „
Collapse, simple atrophy, &c. .	11·8 „	......	67·6 „
Atrophy with induration . .	7·5 „	......	25·0 „

From these numbers it must be at once evident that the atrophic lesions alone stand in any special relation to emphysema, while hepatization and tubercle are found among emphysematous in nearly the same proportions as among mixed cases. In fact, it results from a careful examination of the table given in the memoir, as well as from all our subsequent experience, that hepatization and tubercle are connected with emphysema only through the medium of their frequent association with the atrophic forms of pulmonary lesion.

It is therefore clear, that direct observation points, in a manner not to be mistaken, to partial atrophic disease as the invariable accompaniment of emphysema of the lung. But even had this not been so clearly shown in the manner above indicated, it might have been fairly enough argued from the mechanical conditions by which the lung is confined within certain limits of size. The emphysematous lung generally appears, as a whole, more voluminous than natural; but a moment's consideration will show that this is entirely from the fact of its not collapsing, like the normal organ, on being removed from the chest. The lung is, in fact, as was well known to Laennec, extremely restricted as to real enlargement by the bony case in which it is confined, and which does not admit of expansion beyond the capacity to which it is brought by a full inspiration. Were the emphysematous lung really increased in volume, as a whole, even to this amount, it is quite clear that there could be no respiratory movement; the thorax being maintained by the lung in a state of perpetual distension, instead of being itself the cause of the expansion of the lung. But as in most emphysematous lungs the air-cells are individually increased in size to a very marked extent, it is clear that in a case of very general or "universal" emphysema, such as is described by Louis and others, the thorax could not, by any conceivable amount of yielding of its parietes, give space for the hypertrophied organ, except upon the supposition that the enlargement of the emphysematous parts is accompanied by a nearly corresponding diminution of bulk in others. This conclusion becomes still more evident when exact measurement is applied to the emphysematous as compared with the healthy chest; we have, in fact, found, that so far from the chest being enlarged to any considerable extent in this form of disease, there is great reason to believe that it is usually smaller than natural, the arching of the front and the increase of the antero-posterior diameter being more than counteracted by a diminution in all the lateral diameters, particularly at the base of the chest.

We assert, therefore, both on the ground of personal observation and of inference from the mechanical conditions which prevent enlargement of the whole lung, that in emphysema the increase in size of the affected portions of the lung is always accompanied by diminution in the volume of others, frequently in the form of simple atrophy, or of atrophy with induration; but not rarely also in that of recent bronchitic collapse of the lung. In this last case we have usually found the emphysema to be also manifestly recent, as yet unaccompanied by disorganization of the air-cells, and it is under such conditions that the interlobular emphysema of Laennec is, according to our experience, most commonly produced.

But it will be asked—What is the proof that the emphysema is not the primary, and the atrophy the secondary lesion in these cases? We reply, that the variety in the forms of atrophy which are associated with emphysema demonstrates the contrary. We might, indeed, almost state the converse of the proposition maintained above, with respect to emphysema, and assert that atrophy of the lung, whatever its nature or cause, is, as a natural consequence, accompanied by emphysema. This statement would, however, be subject to numerous exceptions, chiefly in cases of debilitating disease or cachexia, in which, for reasons presently to be shown, emphysema frequently does not follow its usual antecedents. In the great majority of cases, however, in which chronic atrophy of the lung occurs in subjects not extremely exsanguine or emaciated, whether the source of the lesion be in bronchitis or in retrograde tubercle, emphysema is found to occur. In the latter class of cases its frequency has been noticed by almost all pathological writers of late years; and in these, as well as in the cases of bronchitis, it has been supposed that the emphysema bears an active part in the development of the atrophic lesions instead of being a secondary result of them. We are, on the other hand, decidedly of the latter opinion, inasmuch as the connexion of these two conditions admits of being clearly and rationally explained on this hypothesis alone.

And this leads us to the last branch of our argument—*the rationale of the production of emphysema in atrophic lungs.* Before stating the views to which we have been led upon this subject, we think it necessary to submit to the consideration of our readers one or two points in the natural history of emphysema, which we regard as important in relation to the present inquiry, and accordingly have made the subject of repeated observation and experiment. It has been often observed, but not so pointedly stated as it deserves to be, that *the emphysematous portions* of lungs presenting this lesion are almost invariably free from every diseased appearance, except the dilatation of the air-vesicles, and the consequent stretching and disorganization of their parietes. We have been unable, after repeated observation, to find the slightest ground in the physical condition of the lung for the theoretical remark that emphysema is preceded by an altered condition, or diminished resistance, in the walls of those air-vesicles in which it occurs. The observations of Mr. Rainey on the "fatty degeneration" of emphysematous air-vesicles constitute the only really important support which this idea has ever received; and we are able most positively to state, that so far from fatty or granular matter being a frequent or characteristically copious deposit in emphysematous parts, it is, in fact, an exceptional occurrence. That a few fatty granules should occasionally be present in emphysema no one can wonder, who is at all conversant with the frequency of this deposit in almost all the important organic forms of disease; but we recommend those who may have observed the fact announced by Mr. Rainey, and who may have felt disposed to acquiesce in his pathological conclusions, to look, in one or two instances, to the comparative abundance of fatty deposits in the emphysematous and non-emphysematous parts of the same lung, when they will soon find reason to abandon the hypothesis that fatty degeneration is in any way the cause of emphysema.

The next observation we have to record is, that the bronchi leading to emphysematous air-vesicles are usually quite free from obstruction. We cannot go further into the proof of this proposition, than to state that we have very many times performed the experiment of inflating lungs which were partially emphysematous, with the invariable effect of observing the portions so affected yield to the inflation instantaneously, while other parts, collapsed or even natural in appearance, followed the impulse of the air much more slowly, or not at all. These observations, which we believe to be perfectly conclusive upon the point in question, strike at the root of Laennec's theory of emphysema, even were the direct connexion of that lesion with obstructed bronchi not disproved by the whole of the evidence we have already adduced as to pulmonary collapse. The difficulty sometimes experienced in emptying the emphysematous bullæ by pressure, which had doubtless been observed by Laennec, and probably may have constituted the basis of his theory, is quite an exceptional phenomenon, and belongs, not to the early, but to the most advanced stages of the lesion, of which it cannot therefore be considered as in any degree the cause.

From the facts to which we have already directed attention, it may be safely assumed that emphysema is a lesion occurring from mechanical causes, those parts of atrophied and collapsed lungs to which air has the most free access; in other words, it is produced by atmospheric pressure in the comparatively sound portions of such lungs. A similar result, though not always on similar grounds, has been arrived at by most of those who have held the theory that emphysema is caused in inspiration. Some writers have held the strange and untenable doctrine, that emphysema is a physiological compensation for lost or obliterated pulmonary lobules. We need not, surely, take up the time of our readers in showing that an emphysematous lung is not functionally increased in value, and that it bears no analogy to the hypertrophy of a gland or a muscle under circumstances requiring an increased development of functional activity.

Having endeavoured to dispose of the preliminary difficulties and obscurities by which we have found the apprehension of the following views on the *genesis* of emphysema to be chiefly impeded, we may now recur to the statement of a theory which, in the memoir quoted at the head of this article, we have fully discussed in all its relations, with the view of showing that it harmonizes with all that is known of the conditions under which emphysema arises. Emphysema is, according to this theory, an *increase in volume* of those portions of the lung to which the air has access, to supply the place of *diminished volume* in those parts from which it is excluded. It is produced by the expansion of the chest in inspiration, and is dependent simply upon the normal expansion-force being exercised under the abnormal conditions to which we have alluded. It cannot be produced, in health, by *any* amount of inspiratory violence, because the lung admits of being readily and easily expanded, without straining any of its air-cells, to the full volume permitted by the expansion of the thoracic walls. It cannot even be produced in disease, except when the volume of the lung is directly diminished in relation to the space which it has to fill in inspiration. Emphysema, therefore, is never found in connexion merely with pleuritic effusion, or with hepatization, or with tubercle.* It cannot be produced where large cavities exist in the lung, even in connexion with atrophy, if they have very flaccid walls, and be distributed through all its lobes; for under such circumstances the cavities are expanded

* An intelligent and thoughtful critic, in the Dublin Quarterly Journal, to whom the author of this review is indebted for a valuable, because, evidently, a discriminating, favourable appreciation of his labours, objects to this proposition, and to the exclusiveness of this theory of emphysema; more, however, on theoretical grounds, than from any opposing observations. (See Dublin Quarterly Journal, vol. xiii. p. 160.) He argues that the *force* with which air presses into sound vesicles is increased by simple occlusion of a portion of the lung, apart from diminution of bulk. So, at least, we understand his algebraical argument. Leaving unquestioned the correctness of his formula, we think he will at once apprehend our objection to its application in the present instance, when we say that the question appears to us not one of *dynamics* at all. The impossibility of producing emphysema, in company with mere hepatization or other non-atrophic disease, arises not from the absence of a *force* sufficient to produce it (this we never maintained), but from *the insufficiency of the space within the thoracic cavity* to admit of, much less to create, an increased expansion of the air-vesicles under such circumstances. The lung is adapted by nature to fill the pleural cavity at its maximum of expansion, *and no more;* the thoracic walls are adapted by nature to expand the pleura and lung to their normal maximum, *and no more;* therefore, apart from the circumstances which we have maintained as the essential physical condition of emphysema, no air-vesicle of the lung can ever undergo preternatural distension from without; nay, even if distended from within, it would resist such distension, *because there would be no room for it to expand beyond the normal maximum.*

by the inspiration-force more readily than the air-vesicles can be forced beyond their normal maximum. Hence the comparative rarity of emphysema in connexion with rapidly advancing tubercle, while it is generally observed to be the accompaniment of the retrograde or contracting stages of that affection. Emphysema is, on the other hand, certain to be produced, wherever considerable atrophic lesions exist (especially if they are scattered and irregular in their distribution), provided the inspiration-force be exercised to the full extent; that is, provided the strength of the patient, and the physiological need of respiration, be not seriously impaired, as it often is in debilitating diseases; provided, also, the volume of the chest be not materially encroached upon by causes acting from without, as in the case of ascites or pleuritic effusion. All of these are circumstances which restrain or avert the development of emphysema of the lung, even under the conditions that would otherwise produce it.

"The theory here proposed has already been advanced by various writers, and with different degrees of precision of statement, to account for those cases of emphysema which are connected with the cicatrization of tubercular cavities and other kinds of pulmonary atrophy. It is obvious, however, that its true significance, and the extent of its application, cannot be understood, till it is clearly apprehended that all cases of considerable obstruction in bronchitis bring with them, *as a necessary consequence*, a certain amount of diminished volume in the obstructed parts of the lung; and, therefore, that the connexion of emphysema with bronchitis need present no difficulty to the pathologist, even when the latter affection has not been so violent or long-continued as to lead to any considerable amount of permanent and evident occlusion. That emphysema prevails in the opposite parts of the organ to those in which the direct effects of bronchitis are observed, becomes, in this point of view, one of the strongest evidences of its connexion with that affection. That in the great majority of cases, it is found in company with bronchitic collapse, or some lesion implying diminished size of the organ, amounts almost to demonstrative proof of the correctness of the theory here advanced." (pp. 61, 62.)

The production of emphysema in connexion with bronchitic collapse of the lung is best illustrated in recent cases. Collapse occurs chiefly at the back parts of the lungs, but especially towards the root, and *internally;* whereas emphysema is invariably developed *towards the surfaces*, and especially towards those surfaces most directly under the influence of the expansion of the most moveable portions of the thoracic parietes. The diaphragmatic, the sternal, the anterior costal surfaces of the lung are the seats of election of emphysema in such cases; the central and posterior portions are most frequently occupied by collapse and its result—permanent atrophy. In other words, the air-vesicles, on which the muscles of forced inspiration act directly, and especially those in contact with the most dilatable parts of the thoracic wall, are not the most frequent subjects of bronchitic collapse; their bronchi are usually maintained, even in atrophied lungs, freely pervious; and they are expanded beyond the normal proportions, under the powerful action of forced inspiration. Mucus accumulates, on the other hand, and collapse of the lung occurs, in those vesicles which are only secondarily brought under the influence of the dilating force, or which are in contact with the posterior portions of the thoracic wall, where little movement occurs.

The various deformities of the chest, produced in connexion with emphysema and bronchitic collapse, present many interesting illustrations of the views we have advanced; but we have not space for the discussion of this part of the subject, and must refer our readers for information to the work of Barthez and Rilliet, to the numerous papers of Dr. Sibson, to the little work of Dr. George A. Rees, on the "Atelectasis" of children, and to some of our own observations in the memoir formerly quoted.

3. *Collapse and Atrophy of the Lung determine, in a considerable number of cases, Dilatation and Hypertrophy of the Heart.*—In an early number of this journal, we shall devote an original communication to the investigation of the connexion of cardiac with pulmonary disease; and we shall then state our reasons for the belief that it is only the atrophic forms of disease of the lung which have, as a rule, the power of determining cardiac hypertrophy. In this place, we shall only state shortly, that we consider hypertrophy of the heart to be almost invariably a consequence of dilatation, or at least, over-distension, of its cavities; and it is, as we believe, chiefly in consequence of their tendency to produce dilatation, that pulmonary diseases act on the heart at all. To show how atrophic lesions of the lung produce dilatation of the heart would be to reproduce all the arguments we have adduced in the case of emphysema. It is sufficiently evident that the same inspiratory forces which produce the latter affection must tend also towards dilatation of the heart; all the more so, indeed, that this organ is, from its situation in front of the thorax, and resting on the diaphragm, directly in contact with all the most moveable portions of the thoracic wall. The greater and more general, therefore, the pulmonary collapse or atrophy in a given case of bronchitis, the more will the heart tend to dilatation; and the only resistance to this result will be found in the thickness and muscular power of its walls, which will constantly tend to counteract the expanding force of the thorax. This muscular resistance gives rise to hypertrophy, which, when considerable in amount, may for a time keep the dilating force in check, and limit both the extent and the consequences of the disease. The extreme and the most rapidly fatal cases of dilatation of the heart are, as Corvisart long ago pointed out, those in which the muscular resistance has been overcome, either from disorganization of the fibre, or from some other cause, and in which the "active" has been converted into the "passive" form.

In concluding, we must say a word or two as regards the paper of M. Beau, which is quoted at the head of this article. The distinction of catarrhal and asthmatic affections into *dry* and *humid*, is one of very old standing, and, according to the observations of almost all competent authorities, is both well founded and important. M. Beau is the latest expounder of this doctrine, and here, as in many other points, he seems to us merely to repeat, and often to weaken or mistake in repeating, the observations of his predecessors. He conceives himself to have made a great discovery in 1840, in first (?) distinguishing râles into "vibrants" and "bullaires." This distinction was, he tells us, "adoptée" (which, we suppose, is the French for stolen) by M. Raciborski, and has been erroneously attributed to him. If this claim to a new distinction be founded merely on the use of new words, we wash our hands of it, and leave M. Raciborski and M. Beau to settle the matter between them; but if it is pretended that there is really anything new in principle, we answer by simply going back to Laennec, and we would ask our readers, whether the modern writer has in any degree improved, either in true simplicity or in graphic force, upon the descriptions transmitted to us in the 'Auscultation Médiate.' We find there five râles distinguished: the *râle humide*, or *crepitation;* the *râle muqueux*, or *gurgling;* the *râle sec sonore*, or *snoring;* the *râle sibilant sec*, or *whistling;* the *râle crepitant sec à grosses bulles*, or *crackling.* The first of these is the râle of deposit in the pulmonary air-cells; the second is the type of M. Beau's *râle bullaire;* the third and fourth are, in like manner, the representatives of the *râle vibrant;* the fifth,* though heard most characteristically in nature, finds no place in M. Beau's philosophy; and all of these beautifully-descriptive terms have passed into our minds, accompanied by accurate and just observations upon each, before the daylight clearness of which, we do not hesitate to say, most of the more modern illuminations "pale their ineffectual fires." M. Beau may be assured that his "distinction" has no chance of a prominent place in the history of medicine; even though he founds upon it another "distinction *nouvelle*," that of bronchitis into *bronchite à râles vibrants* and *bronchite à râles bullaires*; a difference quite as well known to most other intelligent authors as to M. Beau, though not, perhaps, quite so ceremoniously ushered into a prominent position.

We have no desire, however, to undervalue the researches of M. Beau, which expound, in a new form, a fact and a doctrine well known, indeed, to previous inquirers (as he himself virtually admits), but still both correct and practical. M. Beau states that the "*bronchite à râles vibrants*" often exists in a chronic form, without much tendency to pass into the other and more formidable type of bronchitis, with moist râles and copious mucopurulent expectoration. This is, no doubt, quite true; and it is this form of bronchitis (so called) which constitutes the *catarrhe sec* of Laennec, the *asthma nervosum* or *siccum* of the older writers, the *emphyseme pulmonaire* of the modern French organo-pathologists. M. Beau admits the frequent concurrence of emphysema with this condition; and we think that he has seized a true point of view in combating the opinion which ascribes the whole of the phenomena of spasmodic asthma to this permanent organic lesion. He describes a variety of the bronchitis with vibrating râles, in which the tendency to dyspnœa, and the tendency to emphysematous disorganization exist during the paroxysm, but disappear completely in the intervals, as is shown by all the functions of health being completely restored. Such a state may, no doubt, pass into permanent disorganization of the lung by emphysema; but in its primary and simplest form it undoubtedly has no such complication, at least to any considerable extent. It is long known to popular experience as a disease of great frequency and of comparatively slight danger; it is even, in some cases, regarded by the subjects of it with a sort of grim and superstitious satisfaction, having, like its frequent companion, the gout, a traditionary character as a pledge of long life. Laennec, whose description of what he calls *catarrhe sec* is among the most graphic of his writings, says, that it infests, in some degree, nearly all the inhabitants of cold sea coasts, as well as those of moist

* Skoda admits this râle, which has been often denied, but considers it to be neither frequent nor important. We cannot help considering it to be both the one and the other.

valleys; and that even in the driest parts of France, the half of the most healthy adults present slight traces of this tendency. It is difficult to understand on what grounds so many able modern pathologists of France and Germany have contented themselves with ascribing this affection to pulmonary emphysema, without solving the question how the emphysema was produced, or even being haunted by the more evident difficulty, that emphysema severe enough to determine the paroxysms, would certainly not disappear or become latent in the intervals. In this country, we believe that comparatively few pathological authorities have embraced these opinions; and the great majority of practitioners certainly regard the asthmatic condition as the cause and not the effect of emphysema of the lung. This was also the opinion of Laennec; and we need not say that we think M. Beau has good grounds for maintaining it against the more modern views current among his countrymen. We are not surprised that he should have followed so illustrious and usually so safe a guide one step further, and have adopted, *in toto*, his theory of the mode of origin of emphysema in this affection.

But the question remains—What is the direct cause of the asthmatic dyspnœa, and of the emphysema which sometimes becomes its accompaniment in advanced cases? Here, again, we find French and English authorities commonly at variance, and this time M. Beau takes part with the great majority of his countrymen in denying the spasmodic character of the affection.

"This opinion," he says, "adopted by many conscientious physicians from respect for medical traditions, is no longer capable of being maintained, since auscultation and percussion have given us the means of seeing (so to speak) what occurs in the chest. It has, in fact, been ascertained, with the assistance of these two methods of inquiry, and in a manner the most positive, that there is no asthmatic dyspnœa without an obstruction of the bronchial tubes, which causes vibrating râles, and which, producing an obstacle to the exit of the inspired air, forces it to react on the vesicles and to dilate them."*

We had thought that the experiments of Williams, of Longet, and of Volkmann, which are, or ought to be, well known in France, might have saved the spasm-theory of asthma from being consigned so very coolly, as it is in the first sentence of the above paragraph, to the limbo of medical tradition; more especially as there never has been any doubt, even among the most hazy and "traditional" of the spasm-theorists, as to the existence of an obstruction in the bronchial tubes. The wheezing and "vibrating râles" were too evident a portion of the asthmatic paroxysm to be overlooked, even before auscultation and percussion were introduced. M. Beau has entirely missed the real point of the controversy, and has not offered the shadow of a proof on the real matter at issue—viz., whether the vibrating râles, the obstruction of the tubes, and the consequent dyspnœa, are caused by mucous secretions, as he himself maintains, by sudden inflammatory engorgement of the mucous membrane, as others have supposed, or by spasmodic narrowing of the tubes generally or locally, as is the common opinion in this country. The case which he details of M. V., although very interesting, proves nothing in regard to this question. Let us consider this subject for a moment.

On the part of the mucus-theorists, it is alleged that the paroxysm of

* Archives Générales, vol. lxxviii. p. 155.

asthma is almost always terminated by expectoration of a thick, semi-transparent mucus, and that its accumulation was in all probability the cause of the paroxysm. We admit the fact to be true, but doubt very much the correctness of the inference; at least it is certain that in ordinary bronchitis enormously greater accumulations of mucus take place with comparatively few signs of general obstruction. We think this position must be admitted by every unbiassed observer; and it is, in our opinion, fatal to this theory. Nor can we find more probability in the theory of inflammatory or congestive thickening of the bronchial mucous membrane. That such a lesion should become the source of most serious dyspnœa in ten minutes (an incident of frequent occurrence in the violent forms of asthma); that it should subside with almost equal rapidity; that it should almost never produce a directly fatal result by asphyxia, and very rarely issue in the expectoration of pus, while, on the other hand, far more severe forms of inflammatory bronchitis often produce comparatively little evident dyspnœa,—these are in our opinion ample reasons for rejecting the congestion-theory of asthma, and maintaining the spasmodic as probable, even had the power of the bronchial fibres to produce sudden and rapid obstruction not been positively ascertained. As our information at present stands, we must confess ourselves to be most unhesitating believers in the doctrine of spasm. We even go further, and think there is good ground for supposing partial spasm to be in all cases connected with bronchitis, especially in its early stages, and to be the chief cause of that narrowing of the tubes at particular points, which is the most probable mechanical condition producing the sonorous and sibilant râles.

We have been tempted into some speculations on the function of the bronchial muscular fibres in health, which we consider to be of considerable importance in explaining the phenomena of the asthmatic paroxysm, and generally of the respiratory irregularities in asthmatic individuals. We are inclined to believe that the bronchial fibres are of great functional importance in securing the extrusion of the excess of bronchial mucus, and that even in health they are endowed with a deobstruent function which is of the greatest importance to the normal play of the lungs. If this theory (of which the detailed statement will be found in the separate memoir, p. 39, *et seq*) be admitted as having any probable foundation, then the mere derangement of this deobstruent function may become (apart from all real excess of the bronchial secretion) a cause of the accumulation, or deficient excretion of bronchial mucus. Such a derangement we presume to exist invariably during the paroxysm of asthma, and frequently at other times in asthmatic persons. We are therefore strongly inclined to believe that the pearly, viscid sputa excreted by asthmatic sufferers at various times, and especially after the paroxysm, are not in any, even the slightest, degree, an inflammatory product, but merely a retained excretion, arising from irregularities of the deobstruent function of the tubes. The freedom of many habitually asthmatic persons from real attacks of bronchitis, together with many collateral facts to which we have elsewhere alluded, seems to us to place this view, not indeed upon a secure foundation, but on a somewhat higher and firmer ground than most of the other speculations to which the medical world has been addicted on this subject.

We must now bring this article to a conclusion; but would wish, before doing so, to revert for a moment to the primary phenomena of bronchial obstruction, in order to point out some considerations of great practical importance, though not so directly demonstrated by observation as those we have been occupied with. We have shown, by sufficiently ample evidence, that condensation of the lung of a peculiar type, must be considered one of the most direct and invariable results of bronchitis with mucous obstruction; that similar condensation occurs in a large proportion of debilitated individuals from a very minor amount of obstruction; that it is apt, in either case, to pass into permanent types of disease, determining atrophy and obliteration of some parts of the pulmonary tissue, while it leads to distension and rarefaction of others. It is almost superfluous to say to the modern physician, that all of these facts may be verified by him, as well at the bedside of the patient as at the dissecting-table; while the stethoscope, in following out the inquiries initiated by the scalpel, will at the same time certainly turn them directly to the advantage of suffering humanity. We can truly say, for ourselves, that there is not one of the general laws referred to in this paper which has not, in our own experience, been found to be of incalculable service in the clinical investigation of disease; and this has been the case, not in primary disease of the chest alone, but in a great multitude of constitutional and local affections in which the mechanical condition of the organs of respiration is of some value as an index to the general state of the patient. We have learnt to trace pulmonary collapse, not merely as a cause of death, but as a fleeting, temporary condition of immense frequency; and we have repeatedly been guarded from fallacy, in the estimation of physical phenomena, by having this fact ever before us in practice. We cannot in this place enter into what must necessarily be a somewhat complicated subject—the relation of the anatomical facts of which we have spoken to the diagnosis and treatment of the diseases of the chest; but we cannot deny ourselves the pleasure of stating, that while this inquiry has to us been fruitful in the exposure of error connected with this department of practical science, it has sent us back, with ever-renewed pleasure, to the real masters of observation, who, from the earliest ages to our own day, have been found to speak consistently with each other, because consistently with the unvarying course of nature. We have endeavoured in this article, in the more limited field of morbid anatomy, to do justice to the honest and correct, though often imperfect, statements of fact, which have been made in many different quarters, under varying and even opposite systematic views of the subject we have been treating; and we have done what we can, not perhaps, without error, but at least conscientiously, to eliminate from the whole a body of doctrine based upon secure foundations. The practical application of these doctrines we must, in the meanwhile, leave for the most part to those who have followed us intelligently through this complicated inquiry.

W. T. Gairdner.

REVIEW XII.

Die Sensorischen Functionen des Rückenmarks der Wirbelthiere, nebst einer neuen Lehre über die Leitungesetze der Reflexionen. Von EDUARD PFLÜGER.—*Berlin*, 1853. pp. 145.

The Sensorial Functions of the Spinal Cord in Vertebrate Animals, including a new Doctrine on the Laws of Conduction of Reflex Acts. By EDWARD PFLÜGER.

THIS essay is perfectly antagonistic, as well in style as in object, to the chapter On the Functions of the Nervous System with which Dr. Carpenter has enhanced the value of the last edition of his 'Human Physiology,' and which we shall take an early opportunity of reviewing. The author of the one would resuscitate the exploded doctrine, that the spinal cord, like the encephalon, is endowed with consciousness; the author of the other endeavours to show, that the encephalon, like the spinal cord, may act automatically and independently of consciousness, or, in other words, that the brain has a reflex function. So, in style, the one is a vituperating critic, the other, a calm philosopher; the one tries to demolish the facts and arguments of his opponents, the other, to propound lucidly his own. As the ancient Spartans exhibited their drunken helots to their children by way of warning against the vice of drunkenness, so M. Pflüger's vagaries may be used by way of warning against intemperance in criticism and prejudice in argument.

It has long been a familiar general fact, that adapted movements are made by every kind of vertebrate animal, not excepting man, after the head has been removed, or the communication with the brain cut off; and since the encephalon appeared to be the seat of volition and consciousness, it was difficult to explain them except on the hypothesis that the seat of the soul extended downwards. Unger first showed systematically the true nature of this class of acts—namely, that they depended upon the material organ, and its vis nervosa, and were in nowise to be attributed to the mind. The latest and most voluminous exponent of these views is Dr. Marshall Hall; and upon Dr. Marshall Hall, therefore, the vituperations of our modern Stahlian are more especially lavished. Extracts from his published works, and from those of Flourens, Valentin, Volkman, and others, are elaborately interpolated with notes of interrogation and exclamation—the shrugs, and sneers, and grimaces of typography—but with singularly small effect on the judgment of the reader. The writer, in the first place, gives an historical summary of the facts, commencing with Whytt and Haller, upon which the doctrine of reflex action is based, and argues therefrom that the adaptiveness of the acts of decapitated and vivisected animals demonstrates absolutely that they are the result of consciousness, or, more correctly, of volition. Now, it is fully acknowledged, that the purely automatic character of these movements is only a matter of inference. *We* can never state as a *fact* whether even another *man* feels or not, or is conscious, much less whether the lower animals; in every case it is only an *inference*, but in the majority the inference is on so sound a basis as to be almost irrefragable. If the separated portion of vivisected animals be endowed with consciousness, then consciousness is divisible—a conclusion which Pflüger accepts; but if adaptiveness be accepted as a demonstrative proof of the presence of mind in an organism, then no organism whatever, whether animal or vegetable, not even the minutest primordial cell, is unendowed with it—a conclusion Pflüger does not accept. An author who expressly asserts, and deliberately maintains, that consciousness may be divided into as many different parts as there are frusta in the divided spinal cord of an animal, and that the unity and indivisibility of the Ego is a mere hypothesis, has no difficulty in expressing his conviction that a *paraplectic* man is endowed with a cerebral consciousness and a spinal consciousness, although the patient may strenuously assert that his lower half is utterly without feeling. To these conclusions he is led simply by a play upon the word "consciousness." It is Life—"Life is motion. This motion (termed consciousness) is a part of the whole; this Life is a part of the great life of the world," &c. Dr. Carpenter fully meets all arguments of the kind advanced by Pflüger, and we have only dwelt upon these frivolities more than they deserve, simply to show how important it is to the attainment of sound conclusions in investigations of this kind, to hold fast to common sense and plain facts. These are, in truth, the sheet-anchor of the inquirer.

In addition to his attempted demonstration of the sensorial functions of the spinal cord, M. Pflüger gives his readers a general view of the doctrines and laws of reflex action, in which he adduces some propositions which he thinks new. These are, however, familiar to the student of these phenomena, for the new laws are simply obvious deductions from recorded pathological observations and experiments (or from vivisections not different from the recorded), and do not, therefore, require special notice.

PART SECOND.

Bibliographical Record.

ART. I.—*The Modern Practice of Physic.* By ROBERT THOMAS, M.D. Eleventh Edition, thoroughly revised, corrected, and to a considerable extent rewritten, by ALGERNON FRAMPTON, M.D. Cantab., Physician to the London Hospital. Vol. II.—*London.* 8vo, 1853.

THOMAS'S 'Practice of Medicine' was a well-known book in its day, and, as proved by the number of editions it has passed through, was one of the few medical works which commanded a regular sale beyond the profession. It was a practical work of no high pretensions, but easy to understand; and it was therefore consulted, with more or less benefit, not only by country practitioners who had no time to keep up with the science of the day, but by the host of non-licensed prescribers,—the druggists in large practice, and the clergymen in small. The author having died at a good old age, and the tenth edition having disappeared about the same time, the proprietors were naturally anxious not to lose altogether so profitable a concern, and they engaged the late Dr. Frampton to edit a new edition. Dr. Frampton appears to have aimed at preserving the practical character of the work, and at the same time at elevating, as far as could be done, its scientific tone. Retaining, therefore, the classification of Dr. Thomas, and many of his useful practical hints and directions, he appears to have rewritten most of the chapters. It is therefore, in reality, up to a certain point, almost a substantive work by Dr. Frampton. We regret to be obliged to say, up to a certain point, for the untimely death of the editor obliged his friends to complete the work; and Dr. Herbert Davies, Dr. Parker, Mr. Critchett, Mr. Wordsworth, Dr. Henry Powell, and Dr. Letheby, have each contributed various important chapters in the second volume.

The composite work thus produced is in many respects an useful one. The classification is unfortunate; and since Dr. Frampton modified so much, it is to be regretted he did not go one step farther. We could take exception at almost every step to the method in which various diseases are violently approximated, or as violently separated. This is, however, of comparatively little moment; if the matter be really good, we can easily overlook errors in arrangement.

The portion finished by Dr. Frampton, comprising the whole of the first volume, and part of the second, is characterized by much information, which is, however, neither very profound nor very well put together. We should say that Dr. Frampton had collected a great deal of matter for the work, and that he had not sufficiently digested what he got. He appears also to have been wanting in the power of selection; and, what rather surprises us in a physician to so large an establishment as the London Hospital, there are many points on which he does not seem to have been able to form an independent judgment, but has contented himself with heaping extracts together, and leaving the conclusions to flow from them, at the discretion of the reader. We must, however, except from this criticism all the therapeutical part of the work, which is written very carefully and very fully. This has evidently been the department of medicine most cultivated by Dr. Frampton, and in spite of the deficiencies in the pathology of the work, its richness in therapeutical instructions will cause it to be, as formerly, a favourite with the practitioner. As an example of what we mean, we may cite the chapter on Rheumatism. The *description* of the disease is meagre and unsatisfactory: it occupies only four pages, and the matter of these is not particularly good; the *treatment* of rheumatism, however, occupies eighteen pages, and twelve prescriptions, with English translations for the non-initiated, are given in foot-notes. Even here, however, we notice the want of arrangement and of selection which is so marked in other chapters,—still, as far as regards treatment, it is a good chapter. The same judgment may be passed on the previous chapter on Gout. In the section on Bright's Disease, Dr. Frampton has evidently feared to get out of the depth of the class of readers he had in view, and this important and difficult subject may be said to be merely touched. The opening chapters on Fever have evidently been written with greater care, but even these do not come up thoroughly to what is known.

On the whole, we may say that the work is characterized by industry, but not by originality; by good sense, but not by genius; by useful practical rules of treatment, but not by profound knowledge of disease. In saying, that although an improvement on the former editions, it aims at the same object, and in great measure realizes it, we believe that we shall have passed the proper judgment on this old friend in a new dress.

The contributions by Dr. Frampton's colleagues are well done, without being remarkable for any great merit. We must except, however, Dr. Parker's chapter on Insanity, and Dr. Letheby's on Poisons, which are both extremely good, the latter especially. The same character is throughout given to the book—viz., that of a good practical guide, without any pretension to length of detail or to depth of research.

ART. II.—*On the Growth of Plants in Closely-glazed Cases.* By N. B. WARD, F.R.S., F.L.S. Second Edition.—*London*, 1852. Fcap. 8vo, pp. 143.

MR. WARD has really done for the plant what John Howard accomplished for the wretched members of his own species—he has removed the difficulties, or rather lightened the burdens, of an unnatural mode of existence. He has, in fact, enabled us to cultivate the most delicate exotics of the vegetable kingdom, in all situations and under all circumstances, however discouraging; and he has furnished us with the means of becoming practically acquainted, at a very small cost, with the living flora of every country. To the botanist, the advantages which have been already derived from the application of these discoveries are incalculable, for they have taught him how he may safely transmit from place to place, however distant, plants of the most delicate conformation; and indeed, this application of Mr. Ward's principles has become so general, that there is not a civilized spot on the earth's surface which has not been more or less benefited thereby. But this is not all that can be said of it; for the contemplation and culture of Nature's works, in situations where, under ordinary circumstances, they could never have existed, is, at all times, a source of pleasure and of recreation; hence it happens that the possession of a little case filled with luxuriant vegetation, has often been the means of relieving the tedium of a monotonous life, of dispelling the gloom and sorrow of a sick chamber, of lightening the weight of affliction, and, in fact, of giving life and cheerfulness to the otherwise dull aspect of a very miserable dwelling. These considerations urge us to attach the greatest importance to the results of Mr. Ward's labours; and we may be excused for entering more fully into them than we are accustomed to do in this place.

The circumstance which first led our author to adopt the present mode of cultivating the growth of plants, is described in the Third Chapter of his work. He there states that the science of botany, and the writings of Linnæus, had been his recreation from his youth up; and that the earliest object of his ambition was to possess an old wall covered with ferns and mosses. To obtain this he built up some rock-work in the yard at the back of his dwelling-house, in Wellclose-square, and he therein planted a great number of British ferns, and several mosses, which he had collected in the woods near London, together with primroses, wood-sorrel, and other hardy flowering plants; but, in consequence of the smoky atmosphere of the locality, the plants soon began to decline, and they ultimately perished.

"When this attempt had been given up in despair, a fresh impetus was given to my pursuits, and I was led to reflect a little more deeply on the subject, in consequence of a simple incident which occurred in the summer of 1829. I had buried the chrysalis of a sphinx in some moist mould contained in a wide-mouth glass bottle, covered with a lid. In watching the bottle from day to day, I observed that the moisture which, during the heat of the day, arose from the mould, condensed on the surface of the glass, and returned whence it came; thus keeping the earth always in the same degree of humidity. About a week prior to the final change of the insect, a seedling fern and a grass made their appearance on the surface of the mould.

"I could not but be struck with the circumstance, of one of that very tribe of plants which I had for years fruitlessly attempted to cultivate, coming up *sponte suâ* in such a situation, and asked myself seriously what were the conditions necessary for its well-being? To this the reply was—*a moist atmosphere, free from soot or other extraneous particles; light; heat; moisture; periods of rest; and change of air.* All these my plant had; the *circulation of air* being obtained by the diffusive law already described." (p. 36.)

In this manner his little plants continued to flourish for a period of four years, when, by an accident, they were destroyed; but long before that, he had commenced a series of experiments in the cultivation of the more tender species of British fern. Among others he had chosen *Trichomenes speciosum* and *Hymenophyllum Tunbrigense,* both of which had hitherto baffled all the attempts of the most practical botanists to rear them; but they grew with surprising vigour in the case to which they were consigned by Mr. Ward; and our author has given an amusing account of the salutation which his plant received from Baron Fischer, the superintendent of the botanical establishments of the Emperor of Russia. When this botanist first saw the *Trichomenes* growing with health and vigour in one of Ward's cases, he took off his hat, made a low bow to the plant, and said, "You have been my master all the days of my life." The complete success of these experiments proved beyond doubt the applicability of his principles to the culture of the most delicate favourites of the vegetable world, and Mr. Ward soon had the pleasure of seeing his cases made the companions of the sick, and of hearing of the high esteem in which they were held by all classes of people. The consequence of this has been, that thousands of plants have been safely conveyed on shipboard from one climate to another, without being exposed to those dangers which in former times were their certain destruction.

ART. III.—*Urinary Deposits, their Diagnosis, Pathology, and Therapeutical Indications.* By GOLDING BIRD, A.M., M.D., F.R.S., &c.—*London*, 1853. 8vo.

FOUR editions of this work have been called for within ten years, a fact sufficiently indicative of its value. The present edition is enlarged, and in some respects much improved. A new and important chapter is added at the end of the work, on Blood-depuration by the Kidneys, which will be read with interest. Although it is not our custom to do more than notice any but the first edition of a work, the subject of urinary phenomena is so important, that we shall take an opportunity of discussing Dr. Golding Bird's treatise more at length than we are able to do at present.

ART. IV.—*On Perforating Ulcer of the Stomach, from Non-malignant Disease.* By EDWARDS CRISP, M.D. Part I.—*London*, 1852. 8vo, pp. 22.

THE author reprints a paper written in 1843, and adds to it such cases as have since occurred to him, or which he has found recorded in the English journals. Among 48 cases, he finds that 8 were in males, and 40 in females. The average of the former (in 6 cases) was 54 years; that of the latter (in 35 cases) being only 26·7 years, and 27 of the 35 being under 30 years of age. Adding these cases to others recorded in the previous memoir, we have:—Men 20, women 70. In 18 cases there were two ulcers. The descriptions of the ulcers and of the symptoms present nothing novel. In the treatment of the dyspeptic symptoms which frequently attend the ulceration, Dr. Crisp recommends iron, especially the sulphate and carbonate. Morphia and hydrocyanic acid are to be used when much pain is present.

ART. V.—*Observations and Reports on the Treatment of Intermittent Fever by Phosphate of Lime and Sulphur.* By Assistant-Surgeon A. BLACKLOCK, H.E.I.C.S.—*Madras*, 1852. pp. 103.

THIS book may be said to consist of two parts: 1st. An assertion, backed by cases, that intermittent fever is cured by a mixture of phosphate of lime and sulphur; and 2nd. An hypothesis to explain the action of the remedy. Of the latter part we need only say that it is lame and halting, and being supported by no well-ascertained facts, is scarcely worthy of criticism. Mr. Blacklock believes that intermittent fever is connected with a deficiency of phosphates in the nervous system and in the blood, but he has adduced no accurate experiments in support of this view, but has trusted to general reasoning. The only argument of serious importance is, that ague is more common among the Hindoo, whose diet is poor in phosphates, than among the Mahommedan sepoys, in whose food phosphates are abundant. It is stated, however, in a note from the Medical Board, at page 65, that the prevalence of fever among the Hindoos and the Mahommedans, is nearly the same—viz. 326 and 322 per 1000 respectively. Turning, then, from Mr. Blacklock's theory to his practice, we find that he recommends phosphate of lime to be mixed with one-sixth of sulphur, and to be given thrice daily in two-drachm doses, both during the paroxysm and the apyretic state. Occasionally, nitro-hydrochloric acid was combined with it, or calomel if the conjunctivæ were yellow. A white coated tongue was a contra-indication. In support of this practice, he narrates very shortly 13 cases of intermittent fever, in some of which the patient rapidly got well—in others not; 15 other cases are much better reported, by Dr. Anderson, in 8 of which the remedy was useful—in 7 failed altogether; 13 cases are reported by Mr. Drever, in which the benefit was more marked. Altogether, although the evidence is very insufficient for any satisfactory conclusions, we agree with the Medical Board of Madras, that the remedy may be useful in the milder forms of distinct intermittent, and that it is worthy of an extended trial. Mr. Blacklock is evidently a thoughtful and intelligent man, and if he would curb his tendency to loose and profitless speculation, and deal rigidly with matters of fact, he might do good service to therapeutics.

ART. VI.—*Rheumatism, Gout, and Neuralgia, as affecting the Head and Ear; with Remarks on some forms of Headache, in connexion with Deafness.* By WILLIAM HARVEY, Surgeon to the Royal Dispensary for Diseases of the Ear.—*London.* 8vo, pp. 291.

WE learn from the preface of this work, that the object of its publication "is to trace the relations existing between the ear and its appendages, and those gouty, rheumatic, and neuralgic disorders of the parts about the head and face, which often complicate, sometimes cause, and often protract indefinitely, that most afflictive condition, partial or total deafness." At another place the author writes: "Rheumatism affecting the structures of the ear, has not hitherto, as far as my researches have enabled me to ascertain the fact, been noticed in any medical work." (p. 9.) To this statement we must, however, demur. The subject has been treated in several works and essays, both English and foreign. Mr. Harvey himself, in one of his previous works—that written to expose the fallacy of curing deafness by excision of the tonsils—promises to translate the work of Dr. Lincke, of Leipsic, 'Handbuch der Theoretischen und Praktischen Ohrenheilkunde;' and consequently must be more or less acquainted with that author, who has given descriptions of rheumatism affecting the several parts of the ear, and who has placed rheumatic inflammations among the specific affections of the organ of hearing, in the nosology attached to the second volume of his very valuable work. (See vol. ii. p. 360, Otitis Rheumatica; and p. 367, Otitis Arthritica.) If Mr. Harvey will look into Dr. Martell Frank's 'Ohrenkrankheiten,' published at Erlangen in 1845, he will find a description of what is termed Rheumatic Otitis, at pp. 276 and 351; and also of Gouty Otitis, at p. 278. He will also find several notices of the same affections in the writings of Dr. Schmatz, of Dresden, a most industrious and careful compiler. Now, we have no desire to rake up foreign authorities against the writings of our countrymen, nor would we on the present occasion have alluded to these works, but for the author's statement as to his intention of producing an English translation of a book which described both these diseases twelve years ago.

It is, however, scarcely necessary to search for authorities, as Mr. Harvey's work being principally composed of quotations and references, he himself disproves the assertion set forth in the introductory passage alluded to, in almost every page of his book. Thus, at page 52, he inserts a long case from an essay published by Mr. Wilde, of Dublin, in 1847; and upon looking into the work from which that case is taken, we find—although we do not quite agree with Mr. Wilde as respects the views therein advanced —a separate and distinct definition of the affection, as "acute inflammation of the membrana tympani, accompanied by inflammation of the cavity of the tympanum, frequently of a rheumatic character." "There is also," says that author, when describing the character of the pain attendant upon those aural inflammations, the ordinary result of cold and exposure, "a feeling of fulness, and bursting within the organ frequently; with this there is pain and soreness over the side of the head, in the teeth, in the eye and temple, and in the superior lateral triangle of the neck; with occasionally stiffness and soreness of the upper portion of the mastoid muscle, and often flying rheumatic pains throughout the body, particularly in middle-aged persons, and those who have formerly suffered from rheumatic attacks." Mr. Harvey has not given any diagnostic marks beyond "wandering pains," and the many other unpleasant symptoms which gouty and rheumatic patients so frequently complain of, although he speaks rather affectedly of the inflammation attacking the "neurilemma of the nerves distributed to the cavity of the tympanum"!

The work is illustrated by cases collected from various sources, varying in value, and for the most part loosely put together. It is not a favourable specimen of English medical literature. It is too much made up, and yet too little digested, and therefore presents the common faults of verbosity and crudeness. Yet the author is an able practitioner, and must have considerable opportunities for original observation in aural pathology. If he will only use them well, we may have to review his next publication in a very different tone from that which we have been compelled to use towards the treatise before us.

ART. VII. — *A Practical Sketch of Low Inflammations.* By S. F. STATHAM, Assistant-Surgeon to University College Hospital.—*London,* 1852. 8vo, pp. 31.

MR. STATHAM believes that sufficient attention has not been paid to those forms of inflammation which are connected with some peculiarly depressed and vitiated condition of the system, and whose local characteristic is the rapid formation of purulent fluids. He discusses these forms briefly, but with vigour and some originality. He thinks that, practically, this recognition of "low inflammation" is of great importance, as antiphlogistic measures are hurtful, and stimulants necessary. On this point we cannot agree with Mr. Statham, and do not perceive that his cases are sufficient to warrant his rule of practice. We can only say, that as far as we have seen, a stimulant treatment of septic inflammation has been very unsatisfactory. Mr. Statham has had an important subject to treat, but has, we think, discussed it too hastily. We should be glad to see him handle it again, with the aid of more numerous and more fully recorded cases.

ART. VIII. — *The Dissector's Guide.* By ERASMUS WILSON, F.R.S.—*London,* Second Edition, 8vo, 1853. pp. 626.

THIS is an useful work, written by a man who knows what students want. It forms a kind of supplement to the author's well-known treatise—'The Anatomist's Vade Mecum.' This edition has been well revised, and has received some additional illustrations.

ART. IX. — *Hand-book of Natural Philosophy and Astronomy.* By DIONYSIUS LARDNER, D.C.L. Second Course.—1852. 8vo, pp. 456.

THE subjects of this volume are—heat, electricity, magnetism, and voltaic electricity. The plan of the work is to supply a compendious and yet accurate account of natural philosophy, for the use of students and of those who are immersed in the business of life, yet who wish to maintain their knowledge of what is going on. It is clearly written, full of details, and carefully illustrated.

ART. X.—*An Introduction to Clinical Medicine: Six Lectures on the Method of Examining Patients—Percussion, Auscultation, the Use of the Microscope, &c.* By J. H. BENNETT, M.D., F.R.S.E., &c. Second Edition.—*Edinburgh.* Small 8vo, pp. 134.

EASY and simple books of this class are extremely useful for students, and the one before us is one of the best we have seen. It errs, perhaps, on the side of too much simplicity; and the chapters on percussion, auscultation, and on diseases of the skin, are certainly too short. The lectures on the microscope are more complete. Nevertheless, had the book been larger, it would perhaps have not equally answered the intention of the author, which was, to provide his pupils with a simple guide in their clinical examinations. The rules laid down for the diagnosis of both pulmonary and cardiac affections appear to us too absolute; students must learn that for all rules there are exceptional cases, and if they do not do so in the wards of the hospital, they will acquire the knowledge afterwards, at the risk of having their confidence shaken in what may, after all, be in most cases a sound doctrine. Percussion and auscultation cannot be simplified beyond a certain point. A number of woodcuts are scattered through the volume, and add much to its utility. We recommend the work to all who require a plain and simple exposition of the chief methods of diagnosis.

ART. XI.—*Atlas of the Formation of the Human Body in the Earliest Stages of its Development, compiled from the researches of the late Professor Erdl, M.D.* By JOSEPH KAHN, M.D. (Vienna.)

As we learn from the preface to this work, it consists of plates copied from a certain portion of the Museum, executed under the superintendence of the late Professor Erdl, whose name is well known to the profession in Germany in connexion with microscopic embryology. The entire museum, which we have had an opportunity of examining, is a monument of industry and skill, and the plates before us bear testimony to the minute and exact character of this portion of it. How far the author is correct in anticipating many readers out of the profession, as he seems to do, or how much benefit the public would derive from a study of the Atlas, is another question, on which we have a pretty decided opinion.

The plates are preceded by a somewhat popularized description of the theory of impregnation, the development of the ovum, the changes produced by impregnation, the development of the embryo, and the changes produced in the gravid uterus. There is nothing new in these observations, nor anything which may not be found rather better described in several well-known works.

The plates, however, possess considerable value, and by means of them we think the student may obtain a more accurate notion of the changes consequent on impregnation, than from any other plates with which we are acquainted. As a work of art, they would be better were they less highly coloured, besides being more accurate. The first plate is of the external and internal organs of generation, is inserted for the especial benefit of non-professional persons, and (perhaps for that reason) is placed upside down. The second plate exhibits magnified views of the spermatozoa, the unimpregnated and impregnated ovum, and an embryo at the earliest period. Subsequent plates give the successive development of the embryo and its different parts with great distinctness. We are not quite sure that the relative proportion of organs is exactly preserved; but, notwithstanding, the plates do afford a great help to the junior embryologist. They will not, nor are they intended to, supersede the more elaborate and minute researches of Barry, Bischoff, Wagner, and others; but, as a prelude to more recondite studies, they are valuable, and as such we recommend them to the profession.

PART THIRD.

Original Communications.

Art. I.

Decennium Pathologicum; or, Contributions to the History of Chronic Disease, from the St. George's Hospital Records of Fatal Cases during Ten Years. By Thomas K. Chambers, Physician to St. Mary's Hospital, London.

Part II.*—Diseases of the Kidneys.

The zeal of accurate anatomists has within the last few years made us intimately acquainted with the various forms assumed by the products of disease. These forms are classified and arranged, and in the harmony which results we certainly gain clearer ideas of morbid processes. When Nature leaves her ordinary road, her wanderings are still to our conceptions governed by definite rule and order. But this mode of looking at her deviations with a naturalist's eye, has a tendency to beget an evil almost as much to be guarded against as the confusion we are escaping. Each morbid action is liable to occupy in our minds just the space it fills in the museum or the system; to be of importance as it is a striking type or proof of some law; and not in proportion to its consequences to mankind. The interest excited by its rarity may even bring it so near to our mind's eye, as to exclude the view of more important common objects which lie crowded at an unfair distance; just as in the cabinet of an entomologist an unique ephemera may be more prominent than a cabbage-butterfly or a honey-bee.

To give a just prominence to each diseased state, its frequency ought certainly to enter into the calculation. And so indeed it does, in a rough common-sense sort of manner, sufficient to make us fully sensible of the great want of more accurate data on this point in our pathological works of reference. Few things are more tantalizing than to read in such a book as that of Professor Rokitansky's for example, the statement, that such and such a morbid appearance is common, without a step towards the reckoning of "how common:" we are constantly wishing "to call up him who leaves half-told" such a tale as the comparative rarity of two lesions, without giving us the numerical value of the comparison, or at least his own experience of it. This feeling becomes still stronger when we enter on the suggestive subject of the union of different diseases. Statistical

* For Part I., see Medical Times and Gazette, July 24th, Aug. 7th, 14th, 28th, Oct. 9th, 23rd, Nov. 6th, 27th, 1852.

data alone can help us to know whether the combinations are accidental or essential; and in the want of statistical data, we naturally fear to work out a study which not only offers a real basis for the explanation of many phenomena, but opens out a path for still deeper investigations.

It is obvious that statistical deductions such as I have described above must not be derived from selected cases, neither must they rest on the frail foundation of præ-mortem diagnosis. A series of indiscriminate autopsies, where not only striking clinical cases have been examined, but as a rule all that die, must be the mine of our materials. From such a source alone can we have negative facts of even approximative accuracy; and without negative facts what are our conclusions worth?*

In the great hospitals of foreign parts a complete series of examinations is scarcely possible. In the vast *Allgemeine Krankenhaus*, at Vienna, to which the distinguished pathologist just mentioned is attached, we may believe Mr. Wilde when he tells us, that not half of the fatal cases are opened;† while in Paris, the inconvenience of the cellar at the Hôtel Dieu where the corpses are placed renders the study of morbid anatomy an impossibility to any but the most iron frames. At home, however, hospital governors, many of whose relatives and friends are students, endeavour to supply warmth and light and pretty fair ventilation, so as to leave no excuse for the neglect of this important branch of medicine; while it is made a matter of duty (that word of might to an English ear!) at most institutions, to open every dead body where legally possible. At St. George's hospital very accurate registers have been kept since 1840, and thus up to January 1st 1851 a continuous series has been made of 2161 autopsies in 2539 deaths; of which the 378 not examined were a perfectly promiscuous class, the reason of the omission being either the objections of friends, the sitting of a coroner's inquest, or some similar cause affecting all ages and diseases equally. It is clear that by comparing in these cases the number of times any pathological lesion was found with the number of times it was not found, either alone or in various relations, we gain an approximate idea of the chance of its separate or combined occurrence. And each similar collection of autopsies which is analyzed in the same way, will bring the approximation nearer to full truth.

I gave an example of the mode of working the rich mines of our hospital records in the 'Medical Times and Gazette' of last year, when I examined, through eight papers, some of the phenomena and complications of Tuberculosis.‡ In their preparation my chief sorrow was the necessity of confining myself to such a narrow field as one institution; and my sharpest spur to the labour was a hope of soon having like data contributed from other quarters, for the correction and confirmation of my

* The fallacy of partial statistics has been often exposed, and by none more ably than by Mr. Laing, in his Observations on Denmark, when remarking on the popular pious fraud of counting the criminals who cannot read, and attributing their vice to their ignorance. One might just as well, he argues, number those who have red hair, and set it down as the origin of all evil. But he omits to add, that the fallacy might become fair evidence if we could tell the numbers in the whole population who can or cannot read, and ascertain the amount of felony committed by each lot. The latter more perfect form will be the one aimed at in the statistics which follow.

† Austria, its Literary, Scientific, and Medical Institutions, by W. R. Wilde, p. 180, Edit. Dublin, 1843.

‡ An analysis of this series of papers appeared in the last number of this Review, Jan. 1853.

own. I now purpose to question the same registers concerning the history and complications of diseased kidneys; and by placing the whole of the subject, as far as I have worked it out, in one paper, to avoid the repetitions inevitable in serial publication. Here, as there, I would have the conclusions taken as provisional only; valid indeed against individual impressions or opinions unstrengthened by statistics, but open to correction by collating others similar, and taking a general view of the whole.

Inflammation of the Substance of the Kidneys.

Of acute inflammation of the substance of the kidneys there are recorded, in the 2161 autopsies, 45 instances. The causes of the inflammation were as follows:

Pyæmic infection in surgical patients, in 12 cases.
Suddenly checked chronic skin disease, in 1 case.
Injury to spine, in 5 cases.
The operation of lithotripsy, in 2 cases.
Neighbouring abscess, in 1 case.
Diseased bladders, urethra, prostates, &c., in 12 cases. (In one of these there was calculous matter in the kidney.)
The first "stage" of Bright's disease in 11 cases.
The presence of scrofulous tubercle, in 1 case.

The inflammation was characterized by congestion and redness of the parenchyma, either partial or universal; which redness is distinctly stated in 6 cases to have the dark-brown tinge, but usually the tinting is not very accurately described. In 12 cases, swelling and decided enlargement of the organs is noticed, along with congestion. The kidney, as a whole, is described as soft or flabby in 9 cases; but, sometimes, it appears that the congestion is so intense, or so much fibrin is thrown out, that a local hardening takes place, as happens in inflammation of the pulmonary tissue. This is recorded in one case of nephritis, consequent on an injury to the spine, not indeed included among the pyæmic cases above enumerated, but presenting many points of resemblance to them.*

When the death followed rapidly on the exciting cause of the idiopathic lesion, as, for example, in a case of scarlatina supervening after an operation for stone, where the child died with the eruption still out on her skin, the characteristic appearance was a scarlet congestion, with stellated veins, small spots of sanguineous ecchymosis, and enlargement of the Malpighian bodies. Where the inflammation was followed by continuity of tissue secondarily on a local cause, as in the instances of diseased bladders, injuries to the spinal cord and scarlatinous inflammation of the renal pelvis, the congestion was local and less intense.

In one of the cases of lesion to the spine only one infundibulum was affected, but usually the cortical structure seems to have been the part which the most clearly exhibited the inflamed condition.

In pyæmic or purulent infection of the blood, which, to my mind, is a

* Mr. Henry Lee, of King's College Hospital, informs me that in a donkey in which, for experiment, pyæmic infection had been artificially induced, "a great part of one kidney was more red than natural, and the affected portion rendered harder and firmer in a very remarkable degree. In this case there was no *livid* congestion of the kidney."

sort of type of an intense inflammatory state, the portions of the kidneys attacked, when described at length, are stated to have had a dark, reddish-brown colouring, which I suppose may be looked upon as a second stage of inflammation. In 6 cases there were small white spots, which in 3 are described as surrounded by a red areola; this is the third stage. In 5 instances they had broken up into complete "secondary abscesses," making a fourth and last stage. In one of these latter, though the corpse was opened twenty-nine hours after death, in cold weather, decomposition had so far advanced, that the abscesses were filled with fetid gas.

The difficulty with which the kidneys take on an inflammatory state, may be inferred from the few instances of the lesion which we have been able to extract from 2161 autopsies. A further proof of the same thesis may be found in their comparative resistance to the influence of pyæmic infection—comparative, I mean, when placed side by side with that possessed by the other great manufacturing viscera. While the kidneys suffered from this strong enemy to vital powers but 11 times, the lungs were inflamed by it 106, and had run on to abscess 33 times; and there were secondary abscesses in the liver 22 times.

Even when the pyæmia arose from lesions of the lower part of the urinary apparatus, the kidneys did not always suffer. This was instanced four times; once where the pyæmia followed lithotripsy, once where it followed lithotomy, once where it occurred in a case of diseased bladder, and once in a case of stricture. In these four instances, as I said, pyæmia occurred from lesion of the urinary organs, but yet the kidneys resisted the poison.

Another collateral evidence is the rarity of their inflammation from injury. There were, in the ten years, seven instances of violence experienced by the kidneys, and in none of these is inflammation described. In one of these, a "beautiful union by the first intention" (I quote Mr. Hewett's words) was found, within three weeks after the wound, and in spite of a general inflammatory condition of the patient from other injuries. In another, the hurt had been inflicted five days before death, yet no local reaction had taken place. The other instances prove less, because death occurred within thirty-six hours after the accident; indeed, the kidneys dwell in such a well-fortified place, that it is very uncommon to find a wound there without the rest of the body being so battered as to render life impossible.

In 33 other cases, there was pus contained in the substance of the kidneys, where the organs are either particularly described as pale in colour, or no mention is made of congestion or inflammation. Whether an acute stage of redness and swelling had necessarily preceded this it is impossible to say, and for that reason the cases are not clubbed together with the former set; but the similarity of the causes whence the lesions spring seems to me to render such a supposition extremely probable. These causes were as follows:

Diseased states of the lower parts of the urinary apparatus, calculi or chronic inflammation or cancer of the bladder, enlarged prostates, strictures of the urethra—in 23 cases.

Calculi in the kidney itself—in 3 cases.

Spinal disease—in 2 cases.

Disease of the uterus—in 1 case.
Bright's disease of the kidney—in 1 case.
Doubtful scrofulous degeneration—in 3 cases.

With the exception of pyæmic infection and scarlatina, which, from their poisonous effect on other organs, are so rapidly fatal as to exhibit to us inflammation of the kidneys in its acute stages, it will be seen that the causes of our two sets of cases are closely similar in proportionate frequency.

Inflammation of the Renal Pelvis.

Pyelitis, as this lesion has been called, occurred in 46 instances, 9 of which have been before mentioned in the list of acute inflammation of the substance, 3 in the list of pus in the substance without evidence of acute action, and 34 are independent cases. Of the first class,

1 was owing to pyæmic infection;
2 to injury to the spine;
1 to paralysis of obscure origin;
2 to disease of the lower parts of the urinary apparatus, and apparently communicated from them by continuity of tissue;
3 to the first form of Bright's disease, which will be hereafter discussed.

Of the second class—viz., where it was associated with a deposition of pus in the substance of the kidney, but no evidence of acute action, the 3 cases that occurred were due to disease of the lower parts of the urinary organs.

Of the third class, in which the pelvis was inflamed independently of the parenchyma,

12 cases had Bright's disease of the kidney in a chronic form;
15 had disease of the lower parts of the urinary organs;
1 had a calculus in the kidney;
5 had injury to the spine;
1 had softening of the brain.

One case among the first class, two cases among the third class, had earthy matter adhering to the mucous membrane, formed, apparently, secondarily to its inflammation. Two of these had injuries to the vertebral column, affecting the cord; and one had paralysis, very probably of spinal origin as well as the former. This deposition of solid matter as a consequence, not as a cause, of the disease, shows the early alkalescence of the urine in affections of the back-bone. It shows, too, that the fluid is secreted in a degenerated state from the uropoietic viscera, and not merely chemically altered by retention in the half-palsied bladder.

In two of the above-mentioned 46 cases of pyelitis, the secretion in the pelvis bore a fibrinous character; in all the others redness only and pus, as usual in mucous membranes, designated the inflammation.

Former Pyelitis.

The mucous membrane of the pelvis was thickened, showing the previous existence of inflammation which had passed away, in 5 other cases not included in the 46. In 3 of these there was diseased bladder; in one gonorrhœa (the patient having died of fever); and in the other, a chronic state of "cacœmia," finally fatal by pneumonia.

It is very remarkable how rare is the evidence of former renal inflammation, as well as of its existence at the time of disease; but how still more rare, nay, how almost unknown, are any of these conditions, except in cases where they are caused by previous lesions in other parts of equal severity, and so painful and dangerous as entirely to mask the acute state of the kidney: how rare, therefore, to find it suspected, before death, with just reason. I do not think this is fully appreciated by the bulk of the profession. Persons constantly come before us, both the sick seeking advice and the healthy proposing life insurance, who, on inquiry into their previous illnesses, say that they have been laid up with what their ordinary attendant called inflammation of the kidneys, and sometimes has not hesitated to act energetically on his diagnosis. I suspect we shall not err by considering the complaint, ninety-nine times in a hundred, to have been lumbago.

Bright's Disease of the Kidney.

The name here used to designate the peculiarities of structure which we classify under it, is the most convenient for statistical purposes. It is the simplest, shortest, and least open to theoretical objections; while at the same time there is a pleasure in feeling that by its employment we are helping to hand down a fellow-countryman to a deserved immortality. The division adopted will be a designed imitation of that employed by Dr. Frerichs in his 'Bright'sche Nierenkranheit und deren Behandlung,' Edit. 1851.

THE FIRST CLASS will contain those cases where the kidneys are found large, full of blood, deep purplish-red, and soft, like those affected with acute inflammation, sometimes slightly granular, but with no pale or yellow portions. There is usually, in the state of the pelvis, or in the exudation of fibrin, an evidence of an inflammatory condition. The urine is albuminous, containing blood-globules, and sometimes pus; and there is a disposition to fluid effusion under the skin, or into internal serous sacs.

As appendices to this class, will be noticed,

Appendix α.—Those cases where the general structure of the kidney is healthy, but spotted with points of extravasated blood, and associated with acute anasarca and albuminuria.

Appendix β.—Those cases where mere congestion of the kidney, however arising, and without alteration of structure, has been associated with albuminous urine.

THE SECOND CLASS will include

Sub-class A, where the kidneys are large and granular, but no mottling or yellow deposit is described.

Sub-class B, where the kidneys are large and mottled, or wholly filled with yellow deposit; in some of which the external surface is granular, but in most smooth.

Sub-class C, where the kidneys are large and coarse, or striated in texture.

Sub-class D, where the kidneys are large, and contain minute cysts, but are not otherwise of morbid appearance.

Sub-class E, where the kidneys are large and hard, and contain lumps of fibrin.

Sub-class F, where the kidneys are "large and congested" only.

The THIRD CLASS will contain all kidneys affected with atrophic degeneration, as exhibited in the following varieties: *Variety A*, a diminished and granular condition; *Variety B*, a granular condition, but normal size; *Variety C*, a mottled state, with diminution of size, either (x) granular, or (y) not granular on the surface; *Variety D*, the same without diminution of size; *Variety E*, a mottled and diminished state of the cortical structure in cases where the cavity of the organ is dilated; *Variety F*, a paleness, coarseness, and looseness of confused and atrophied textures; *Variety G*, atrophy of cortical structure, accompanied by redness or congestion; *Variety H*, the same with inflammation of the pelvis; *Variety I*, an atrophied and lobulated condition; *Variety J*, diminution with the development of cysts; *Variety K*, where no obvious abnormal condition, except "smallness and hardness," is exhibited.

The classification employed does not necessarily involve a decision on any part, as to whether it represents three forms or three periods of the lesion. Those who contend for the three period theory will make the first a state of hyperæmic action, necessary for the production of the exudative process, which swells the kidney out with foreign matter in the second class, while the third class exhibits the effects of this exudative process, first in the destruction of the natural tissue of the gland, and then in the gradual absorption of the exudation itself. My own opinion is, that such is likely enough the course of the disease in some cases; it may be taken as a typical consistent outline of its habits; such is the road which our fondness for system induces us to say it "*ought*" to observe. But I think that, in the majority of instances, the moral obligation is not recognised, that exudation will be developed without previous hyperæmia, and that atrophy will commence simultaneously with exudation. If, however, the stages are in individual instances duly gone through, it is not probable that their order of precedence is ever reversed; exudation does not lead to hyperæmia, nor atrophy to exudation. I have thought it right, at the risk of an accusation of egotism, to express my own opinion on this subject before it is elicited by the cases analyzed; but yet I trust that the dissent of other pathologists, or any future proof of the incorrectness of this opinion, will not render useless the facts arranged under its influence.

I have not demanded for the admission of cases into the lists, that the presence of albumen in the urine should during life have been proved. It would be illogical to have done so; for the present object is to compare, statistically, instances where Bright's disease of the kidney existed with those where it did not exist, so as to ascertain the peculiarities of the former. Now, to class the cases where albumen was found on one or two trials absent, still more to class cases where it was simply "not proven," as illustrations of those diatheses where the kidneys are free from the degeneration, would lead to results obviously most incorrect. That condition of urine must be examined separately, and a comparison of the conclusions then come to, with those I am now about to work out, will justify or vilify, as the case may be, my present plan. I mean to say, that if we find the complications, the habits, and the effects of albuminuria the same as the complications, the habits, and the effects of degenerated kidneys, their nature, their *latens processus* may be viewed as practically identical; but if different, then the connexion of the two remains an open question, so far as the present argument is concerned.

CLASS 1.—The first or inflammatory form of Bright's disease is unquestionably that which the least frequently appears in the dead-house. There are not found in the post-mortem books of St. George's, during the decennium, more than 7 cases which can by any means be referred to this category, and only 4 to the *Appendix a*, which are, apparently, all that Dr. Frerichs would refer to his first stadium. But under CLASS 2, there are 128; and under CLASS 3, as many as 315 cases included.

The proportions here are so very different from those given in the statistical list published in Dr. Frerichs' work, that the fact should not be passed over without notice. In a tabular view of 292 autopsies of cases of Bright's kidneys, he sets down 20 to the first stadium, 139 to the second, and 133 to the third. The different amounts that he assigns to the second and third will be spoken of afterwards, but the prominence given to the first set of cases in his list, and their infrequency in mine, should be explained here. It arises apparently from the difference of the sources whence the instances are taken. It will be seen that his table is a *resumé* of cases dispersed through various monographs by Drs. Bright, Barlow, Christison, Gregory, Rayer, &c., where they were originally placed, not as representing the whole, or any definite proportion of the whole, of the times that the lesions have occurred to the observer, but simply as picked examples of each form. Sometimes the very rarity of cases thus collected causes them to occupy a more prominent place in the writer's mind, and therefore a larger space in his book, than their frequency and consequent importance would justify; while, at the same time, his love of system makes him hail them with delight, as rare demonstrations of a state which he conceives every degenerated kidney must in life have gone through. We have probably in print every case of this first form which has come before each writer on the subject; while the other classes of Bright's disease pass daily under his eye, without engaging either his pen or his memory. On the other hand, the source of the statistics here collected is a continuous series of cases, not so striking indeed, nor so useful in giving an impressive picture of the disease to a youthful audience, but a more fair representation of real experience. Dr. Frerichs' numbers may indicate the space occupied in the observer's mind by the several forms, and those here derived from St. George's record the space occupied in actual nature.

As these instances are so few in number, they are not fit subjects for statistical deductions, but still may be usefully given in short, with references to the annual volumes of the post-mortem books, where they are to be found.

(1.) A man, aged 35, was attacked, 14 weeks before death, by rigors and inflammatory anasarca, with dark-bloody urine, which subsequently became purulent. He died of pulmonary asphyxia.—*Post-mortem:* The kidneys were found large, excessively congested, and slightly granular on their surface; the mucous membrane of their pelves and of the bladder inflamed. The heart was dilated and hypertrophied, and there were some vegetations on the mitral valve. (Year 1843, folio 123.)

(2.) A woman, aged 47, died of pneumonia and pleurisy, dating from 7 days before death. She had a few days previously been admitted for anasarca of an acute character, producing vesications of the cuticle. The urine was scanty, bloody, and albuminous.—*Post-mortem:* The kidneys were found highly congested, and very slightly granular. The heart was large, but was not opened, being wanted for other purposes. There was an organic lesion of the uterus, which, from its proximity, had affected the bladder. (Year 1844, folio 42.)

(3.) A man, aged 62, was admitted into the hospital moribund.—*Post-mortem:* The kidneys were found much congested and somewhat granular, and in the cortical part were spots of apparently recent fibrin. The heart was dilated and hypertrophied. (Year 1845, folio 148.)

(4.) A man, aged 36, died of gangrenous ulceration of the pharynx, which had supervened on tubercular disease of the lungs. There was no dropsy apparent after death; and the state of the urine during life was not noticed.—*Post-mortem:* The kidneys were large and much congested. The mucous membrane of the pelvis also was inflamed, and the substance rather granular. The heart was dilated, and the aorta atheromatous. (Year 1846, folio 85.)

(5.) A female died of asthenia from over-exertion in her journey to the hospital. She had anasarca of 4 weeks' duration, which had come on a fortnight after the disappearance of an eruption of scarlatina.—*Post-mortem:* The cortical part of both kidneys was much congested, and the pelves were inflamed and covered with a white opaque fluid. (Year 1849, folio 140.)

(6.) A man, aged 20, had dropsy for 6 weeks. The urine was albuminous, and loaded with blood-globules and occasionally with pus.—*Post-mortem:* The kidneys were large and much congested, but smooth. The heart was dilated, and there were fibrinous vegetations on the valves. (Year 1849, folio 259.)

(7.) A man, aged 44, had had dropsy several times before the occasion on which he was admitted with it into the hospital; it was then slight, but increased rapidly. There was much albumen and blood in the urine, and the patient died comatose.—*Post-mortem:* The kidneys were soft, of a dull-red colour, with the tubuli blocked up by casts. The heart was dilated and hypertrophied. (Year 1850, folio 214.)

On receiving these autopsies it will be observed, that except in one case, which was due to the action of the scarlatinous poison, in all instances where the heart was examined, it was found in some way diseased to such an extent as without doubt to interfere considerably with the circulation. It was diseased, too, in such a way as can hardly be supposed to have arisen in the short period during which dropsy and renal symptoms had manifested themselves. There would really seem to be no impropriety in calling these cases *inflammatory congestion of the kidney, connected with heart-affection.*

That they are truly called inflammatory seems likely from the resemblance of the appearances of the organ to what have been previously described on other data as the phenomena of its inflammation. Confirmatory of this are the state of the adjoining mucous membrane, the frequent invasion of the dropsy by rigors, the presence in the urine of the products of inflammation, sanguineous, fibrous, and purulent. All these phenomena are not described in each case, but some in one and some in another.

The last-named symptom, to wit, the excretion of blood, pus, and tube-casts, is apparently a true indication of sthenic inflammation, inasmuch as it occurs even without the presence of the uræmic or the dropsical diathesis. It was observed, for example, in one of the cases included in an earlier part of this paper, as an instance of inflammation of the kidneys, induced by pyæmia, where the purulent infection arose in a young man of 26, from an abscess in the neck. (Year 1849, folio 38.) The urine in this case was loaded with blood and casts of the tubes, and after death the kidneys were seen large, flabby, and congested, with white spots in them, surrounded by a red areola, like the early stage of secondary deposits in the lungs. Other instances might be cited where the same condition of urine was observed in inflammation of the kidneys, arising from diseased prostates urethras or bladders, but they would prove less, as the albuminuria might have been caused by previous chronic degeneration of the gland, which was evidently not the fact in this death by pyæmia.

It is on these grounds that I have included among the cases of inflammation of the renal substance, which were enumerated in a preceding section, both these instances of the first form of Bright's disease, and the rather more dubious conditions ranked under *Appendix a*, and as far as I can judge by their writings, such would have been the view taken of them by the best authors on the subject.

It is the inflammatory element which marks, to my mind, the difference between this congestion and that which occurs in healthy kidneys, and also in slightly degenerated kidneys as a concomitant of diseased heart. The last form of congestion, indeed, leads to the outpouring of albumen into the urine, and assists presumably in inducing dropsy, but does not eliminate the other acknowledged products of inflammation. The morbid states are allied so as to resemble one another in many particulars, and cases occur of which it is difficult to determine the assignment, from their occupying, in point of fact, the transition place between one and the other class. The difficulty is incidental to all natural classification of subjects; and as I think the important and real point to be the inflammatory state, I have made that the principle of difference on which this class is divided off.

The *Appendix a* which I have subjoined contains those cases where the kidneys are found of their normal dimensions and colours, throughout the general surface; but spotted with points of extravasated blood, and where there is during life a disposition to anasarca and albuminuria.

There was one instance of this appearance in a female of 37, who died after dropsy and albuminuria of eleven weeks' duration, connected with valvular disease and dilatation of the heart, and with pulmonary apoplexy (year 1849, folio 241); and there were three others, connected at a greater or less period of time with scarlatina. One was in a girl of 6 years old, who died of dropsy of a month's standing, which had come on a fortnight after the departure of scarlatina. (Year 1844, folio 191.) Here the kidney presented a healthy appearance saving the ecchymosed spots. The second was a girl of 3 years old, lately operated upon for vesical calculus, who died on the 5th day, of scarlatina. (Year 1849, folio 81.) The kidneys, of which I have a drawing, were palish and speckled with ecchymosis; the pelves, too, were inflamed, possibly in some measure owing to the recent operation. The third, a girl of 5, died of diffuse inflammation of the throat and pneumonia, accompanied by slight anasarca 3 weeks after the first invasion of scarlatina. (Year 1849, folio 234.) The kidneys were congested and ecchymosed.

It must be remarked that those cases alone are here alluded to where the general aspect of the parenchyma was healthy: the extravasation of blood into kidneys structurally altered will be afterwards separately noticed.

Scarlatina.—This would appear to be the most proper place to insert, parenthetically, the experience to be derived from the St. George's post-mortem books, respecting the effect of scarlatinous poison on the kidneys. There were 13 cases that died actually during the fever, of which 5 were not examined after death, and in two more, the state of the kidneys is not mentioned, being included, probably, among the other organs of healthy appearance. In one they are particularly stated to have been healthy. In two they were congested, in one large, in one pale and mottled with confusion of the tubular and cortical structures, included in CLASS 2 (year 1843, folio 126); in one speckled with spots of extravasated blood and with the pelves injected (mentioned as 2nd scarlatinous case under *Appendix a*).

Five died with dropsy after scarlatina;

One on the 4th day of the anasarca, with blood and casts of the tubes in the urine. (Year 1848, folio 188.) The kidneys were large and pale, but congested around the cones (included in CLASS 2).

One during the 3rd week after the first invasion of Scarlatina (mentioned as the 3rd case in *Appendix a*, as having congested and ecchymosed kidneys).

One in the 2nd week of the anasarca, which had come on 4 weeks after recovery from Scarlatina, where the urine contained albumen only and no blood (mentioned as the first scarlatinous case under *Appendix a*, as having natural-coloured pale kidneys with specks of ecchymosis in the cortical part).

One in the 4th week of the anasarca (mentioned as the 7th case under Class 1st, as having kidneys with congested cortex and inflamed pelves and infundibula).

One, a boy of 5, who died in the 7th week of the anasarca in consequence of effusion in the brain. The urine, during life, was at first scanty, and had contained blood and albumen, but was abundant and clear before death. The kidneys were found of perfectly normal appearance. (Year 1848, folio 237.)

It appears from these few cases that the lesions of the kidneys in dropsy after Scarlatina or in Scarlatina itself, are by no means invariable. Neither do the appearances at various periods of the disease, lie in the order which would indicate that they had passed through clearly marked stages of degeneration. The rule however would appear to be slight congestion with a tendency to hæmorrhage and to localized inflammation rather than to general inflammation of the whole gland, and (if we are to take the last case as a typical one) to a restoration of the organ to health, while the dropsy and its effects still remain.

The not unfrequent occurrence of large yellow kidneys, or of those affected with the mottled degeneration, in cases of scarlatinous anasarca, is an embarrassing fact. The first-mentioned case of dropsy after scarlatina is an instance of their being found thus diseased at a remarkably early period—so early indeed, that we can hardly conceive their being so affected by the acute complaint. It seems more rational to conclude that the renal lesion existed previous to the fever, was the co-cause of the dropsical symptoms which followed, and the immediate inducer of the fatal termination.

Appendix β.—Besides the scarlatinous dropsy mentioned above, there are 4 anasarcous cases recorded in the post-mortem books where the kidneys are described as normal in structure, though albumen had been found in the urine before death. In two (year 1848, folio 266, and year 1850, folio 58) these organs were congested, and the hearts were hypertrophied, with atheromatous valves. In two (year 1849, folios 239 and 240) there were cicatrices of cysts in the renal parenchyma, and also hypertrophy of the heart.

It is staking too much on fallible powers of observation, to assert that the kidneys in these 5 instances were really in a healthy condition; and still more dangerous if any theory or practice were built on such assertion. Yet it would have seemed unfair to omit mention of them, and so to give our statistics an unnatural completeness and finish.

It is remarked by Dr. Frerichs, with much judgment, though the concluding prophecy is perhaps too sanguine, that the instances of healthy kidneys being found in connexion with dropsy and albuminuria, "will go on decreasing, the more physicians habituate themselves to a critical examination of the morbid conditions of the kidneys—they will altogether disappear, when observers are in a position to seek for and recognise with the microscope fibrinous clots in the tubuli."*

Class 2.—This second class is designed to include all those cases in which the size of the organs appeared to have been augmented by addition to their substance, without apparent removal of the parenchyma by atrophy. There are found 128 cases in the 10 years probably referrible to this class, the appearances of the kidneys being described in the post-mortem books as is shown in the following table:

* Bright'sche Nierenkrankheit, p. 168.

Appearance of kidneys included under Class 2.	With albumen found in the urine.	With absence of albumen in the urine.	With the existence of albumen in the urine uncertain.	Totals.
Large and granular (*Sub-class A.*)	7	3	8	18
Large, mottled, or yellow (*Sub-class B.*)				
(*x.*) granular on surface	3	1	10	14
(*y.*) smooth on surface	31	1	56	88
Large, coarse, or striated (*Sub-class C.*) . .	3	...	...	3
Large and cysted (*Sub-class D.*)	2	1	...	3
Large, hard, and with lumps of fibrin (*Sub-class E.*)	1	...	...	1
Large and congested (*Sub-class F.*) . . .	...	...	1	1
Totals in Class 2	47	6	75	128

The chief object in this tabular conspectus of the appearances presented by the kidneys, is to exhibit the evidence on which the cases are included as instances of one kind of lesion. I am unwilling to set forth the sub-classes as different forms of the disease; indeed, I think it probable that several of them may be in fact only various modes of description adopted by various observers. The essential points appear to be, that in this class we have evidence, in the increase of size, that an addition to the substance of the kidneys is made by the deposition of a peculiar foreign matter—that in some cases (as in *Sub-class A*) this addition is not sufficient to prevent the free access of blood to the organ, so that the natural colour is kept up; but yet is sufficient to make that alteration in the texture known as "granulation," by some transformation, either of itself or of the renal parenchyma—that in others it is copious enough to obstruct the blood-vessels, and cause its own yellow colour to appear instead of the colour of the blood, and is deposited either universally or in irregular mottlings (as in *Sub-class B*)—that sometimes it is deposited in excentric-rayed striæ, following the direction of the tubuli (as in *Sub-class C*)—and that sometimes (as in the remainder) it has its appearance modified by unknown circumstances, so as to be described in less determinate words.

Class 3.—The third class will take in all the remaining instances of Bright's disease—those, namely, in which a common feature is atrophy; by which I mean the removal, from an organ which has already attained a certain degree of development, of its constituent elements, without a corresponding replacement of the same. There appear to be 313 cases in the decennium referrible to this class, the appearances of whose kidneys are subdivided in the table which follows.

Appearance of kidneys included under CLASS 3.	With albumen in the urine.	With absence of albumen in the urine.	With the existence of albumen in the urine uncertain.	Totals.
Small and granular (*Variety A.*) . . .	31	7	87	125
Natural size and granular (*Variety B.*) .	11	7	21	39
Small and mottled (*Variety C.*) (*x.*) granular	10	...	29	39
(*y.*) not granular	5	...	15	20
Natural size and mottled (*Variety D.*) (*x.*) granular	5	...	19	24
(*y.*) not granular	6	...	17	23
Dilatation of the cavity, with mottling and diminution of the cortical structure (*Variety E*)	1	...	7	8
Atrophy of cortical structure, with paleness and confusion of the structures, or with coarseness and looseness of the whole organ (*Variety F.*)	5	1	10	16
Atrophy of cortical structure, accompanied by redness or congestion (*Variety G.*) .	1	...	1	2
Atrophy of cortical structure, accompanied by inflamed pelves (*Variety H.*) . . .	1	...	...	1
Atrophy and lobular condition of cortical structure (*Variety I.*)	...	...	2	2
"Small," and with cysts in their texture *Variety J.*)	4	...	5	9
"Small and hard" (*Variety K.*) . . .	2	...	4	6
Too much decomposed for description . .	1	...	...	1
Totals in CLASS 3	83*	15	217	315

I have said before, that the principle of classification in the above list is the probable presence of atrophy in a greater or less degree. Where the organs are smaller than natural the existence of this morbid state is self-evident: where they are apparently of normal dimensions the case is little altered; for the addition of a certain quantity of morbid matter, as evinced by the changed colour and appearance, implies that the healthy texture must be removed if the size is retained. The chief varieties are

* There were also 3 cases of albuminuria not examined.

clearly the five first: many cases included in the others could doubtless be distributed amongst these, had the descriptions been always given in the same formula. At all events, I do not think we are in possession of such an intimate knowledge, generally distributed throughout the profession, concerning the minute anatomy of the rare forms of this lesion, as would enable us to separate, for statistical purposes, those less common varieties described by Prof. Rokitansky. I think it better, for the present, to keep all the atrophied kidneys together, though my own impression certainly is, that the lesions arise from various causes, and have essential differences.

I have not made, in the enumeration of appearances in either class, any allusion to microscopic phenomena. In the first place, they are not noted with sufficient frequency or harmony of nomenclature in the records I am analyzing; and secondly, if they were observed with care, I doubt whether they could be safely made a principle of division. In microscopic researches no count can be kept of what is, practically speaking, one of the most important elements of pathological classification—namely, the extent of distribution of morbid processes. The existence of a particular diseased state in the small piece or pieces which are submitted to the lens may be easily made evident; but whether this diseased state is an accidental local circumstance, or is spread through the organ in such amount as probably to interfere with its functions, can only be judged by the naked eye. To set down microscopic deviations from health as *the* disease, would lead to mistaken views. For example, the hearts of few chronic invalids can be examined without some part of the muscle exhibiting that granular condition which is found when the fibre is about to become oily, but to designate all these hearts as instances of fatty degeneration, and to suppose that they had any calculable influence on the duration of life, as distinct from the general diathesis, would surely be a serious error. Yet such, I think, would be the result risked by adopting microscopic appearances as a principle of pathological division.

Sexes and Ages of those affected.—The tables which follow are intended to exhibit the influence of the different circumstances governed by the time of life and the habits of the two sexes, on Bright's disease.

Actual Numbers of Cases of Bright's Disease in the Decennium.

AGES.	In 1st Class.		In 2nd Class.		In 3rd Class.		Totals in 3 Classes.		Totals
	Males	Females	Males	Females	Males	Females	Males	Females	Both sexes.
From birth to 15 inclusive	..	3	2	2	1	1	3	6	9
From 15 to 30 inclusive .	1	..	35	17	14	18	50	35	85
From 30 to 45 inclusive .	2	2	24	13	73	35	99	50	149
From 45 to 60 inclusive .	..	1	16	5	86	28	102	34	136
Above 60	1	..	9	2	28	17	38	19	57
Age unknown	..	1	2	1	10	4	12	6	18
Totals	4	7	88	40	212	103	304	150	454
Number of column . . .	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.

Per-centage Table.

Numbers of cases examined.	In 1st Class.		In 2nd Class.		In 3rd Class.		Totals in 3 Classes.		Totals
	Males	Females	Males	Females	Males	Females	Males	Females	Both sexes.
In 94 males, and in 60 females, from birth to 15, inclusive	..	5·0	2·1	3·3	1·0	1·6	3·1	10·0	5·8
In 377 males, and in 259 females, from 15 to 30 inclusive	0·2	..	9·2	6·5	3·7	6·9	13·2	13·5	13·3
In 472 males, and in 179 females, from 30 to 45 inclusive	0·4	1·1	5·0	7·2	15·4	19·5	20·5	27·9	22·8
In 299 males, and in 139 females, from 45 to 60 inclusive	..	0·7	5·3	3·5	28·7	20·1	34·1	24·4	31·0
In 109 males, and in 58 females, above 60 . .	0·9	..	8·2	3·4	25·6	29·3	34·8	32·7	34·1
In 74 males, and in 37 females, of unknown age	..	2·7	2·7	2·7	13·5	10·8	16·2	16·2	16·2
In total of 2161—viz., in 1425 males, 732 males, and 4 of unknown sex	0·2	0·9	6·1	5·4	14·8	14·0	21·3	20·4	21·0*
Number of column . .	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.

From these tables the following deductions may be legitimately drawn:

From the total of column IX. in the Per-centage Table.

Deduction 1.

More than one-fifth of those who die at St. George's, and therefore probably at all other metropolitan hospitals, (except, perhaps, the future Children's Hospital,) have these peculiar lesions of the kidneys.

It would be interesting to know whether this large number of instances of the lesion in question, is owing to any circumstances arising out of the metropolitan position of St. George's, or whether a rural population exhibits the same tendency to it. We might inquire further, whether any tribual or national peculiarities, or habits of particular classes, influence its production. A still more interesting question will be, whether or not the tendency is increasing amongst us; whether the human race, as it grows older, becomes more subject to a disease, the most common form of which, as we shall shortly see, is peculiarly one of declining years. The steady increase in intolerance of bleeding and of other depletory measures, exhibited by the sick, however afflicted, is a subject of daily observation. If

* In the 2161 cases examined, there were 4 where the sex has not been recorded; hence the per-centage of the total is somewhat under the per-centage of males and females added together.

we were to follow our grandfathers' prescriptions, few patients would survive. Is this change in man's constitution due to external circumstances only, to varied habits and manners, to any cyclical revolution of planetary and terrestrial phenomena as yet unobserved? or shall we seek for it in himself, and trace in visible anatomical differences the march of functional alterations? It is not impossible that the greater attention paid by morbid anatomists to the kidneys of late years, may have been fairly claimed by them on account of a greater tendency to degeneration; and that this tendency may have some relation of cause, or effect, or concomitant, to the well-marked intolerance of depletion. The answers to these questions lie in the far future.

From column IX. of the Per-centage Table.

Deduction 2.

The tendency to the atrophic forms advances steadily with advancing life.

We should not have been prepared for this deduction by the table of "Actual Numbers," but it is sufficiently clear from the "Per-centage Table," in all the columns where the numbers are high, and most so where they are the highest—viz. the last or general total.

From comparison of the Totals of the two Sexes.

Deduction 3.

The atrophic forms were one-twentieth more common among males than among females.

From columns VII. and VIII., line 4, Per-centage Table.

Deduction 4.

This excess of males arose mainly from their excess between the ages of 45 and 60 inclusive.

The greater tendency of males to Bright's disease has been often noticed, and is generally exaggerated, because the larger proportion of that sex who die in hospitals is not taken into account. One-twentieth is an excess that may very easily have arisen accidentally in the narrow field for statistics which a single institution affords. It is not so great as was to have been anticipated, when we call to mind how much more men are exposed than women to the external influences of cold and wet, and to temptation to intemperate habits. A suspicion is begotten, that we may err in attributing the anatomical lesions to the above popular causes: it may be that it is dropsy only which is due to them, and not diseased kidneys.

From columns III., IV., V., and VI. Per-centage table.

Deduction 5.

Though, as a whole, the tendency to Bright's disease advances with advancing life, yet an exception occurs in the period from 15 to 30 in the second class, where the tendency is greater than at any other quindecennial period in the same form.

This per-centage would have been still greater, if I had included in the 2nd class, those kidneys where the atrophy had made no perceptible alteration in the size of the organ. Of the 32 in the 3rd class belonging to this period, 20 had natural-sized mottled kidneys, in a few cases granular also. The evil of artificial classification is a separation like this of things which in nature are united or run one into the other by imperceptible steps, and the method of rendering the evil innocuous is to state it fairly and openly.

It may be observed, that the disposition to form, in large quantities, the peculiar yellow matter in the kidneys has so far an analogy to the tubercular diathesis, that it is most rife at the same period; but that the excessive tendency of males does not observe the same laws as it is found to do in that morbid condition. For whereas in Tuberculosis* the excess of males, beginning at the same time goes on augmenting till the last term of life; in the second class of Bright's disease, after being very marked at this second quindecennium, it is in the total by no means conspicuous. But the number of cases is too small for any practical remarks to arise out of this observation.

The difference in the proportionate numbers of the second and third classes contained in the list of Dr. Frerichs and those here exhibited, has been before alluded to. It is due, very possibly, to the arbitrary and artificial boundaries, which we each have set up for our own convenience, not running precisely in the same line. An instance of this occurs in the 20 cases mentioned a few sentences back; one does not know whether to refer them to his Stadium of Exsudation or Stadium of Atrophy, for in fact they are a transition from one to the other. The chief merit that I would claim for my classification is, that by the enumeration of what are taken as "Sub-classes" and "Varieties," all the faults of its artificial structure are made evident, and an opportunity for re-arrangement *ad libitum* afforded. But another reason for the disproportion of our statistics may be, that I have been obliged by my plan to take in as I best could a number of indeterminate and doubtful cases, which of course are simply passed over by the collectors of select cases, and counted nowhere. As a matter of fact, these usually bear a greater resemblance to the 3rd class, are included in it, and augment its bulk accordingly.

Incidental Anatomical Appearances in Bright's Kidneys.

Pyelitis.—Inflammation of the renal pelvis was seen in 3 cases of the first class; in 1 case of the second, where there was scrofulous deposit as well; in 11 cases of the third; but in all these the degeneration was not the sole cause of the mucous membrane becoming affected; for in 3 of them there was calculous matter in the kidney, in 1 a stone in the bladder, and in 1 old vesical and urethral disease.

Pus in Substance.—In 2 cases of the second class, in 4 of the third, there was pus in the substance of the kidneys; but in 4 of these there

* See Decennium Pathologicum, Part I., Tuberculosis, Medical Times, No. III., Aug. 14, 1852.

was other disease of the urinary apparatus to account for it, and 1 was a case of pyæmia. In 1 case of the second, and 1 of the third class, and in 1 otherwise healthy, there was pus contained in small cysts.

Effusion of Blood.—There have been 4 cases of ecchymosis included in Appendix *a* of Class I.; there were 1 in the second, and 3 in the third class; in two of the latter, the hæmorrhagic appearance was directly under the mucous membrane. In one other case of pale mottled kidneys of the natural size, there was a fibrous mass, which appeared to be an old clot. As similar fibrous masses were found in two instances where the kidneys were of healthy structure, and in one of the first-named cases, though albuminuria existed, no other evidence of Bright's degeneration appeared, I do not think we have any reason to associate blood-clots with that lesion as effect and cause.

Cysts.—Besides the 3 cases mentioned above of cysts containing pus, there was 1 instance of cysts containing calcareous matter. There were also the following number of instances observed of cysts filled with a mucous or serous fluid—viz., in the second class, 10; in the third, 67. These cysts are not peculiar to kidneys affected with Bright's disease; in 5 cases they were found without that degeneration, where the lower parts of the urinary apparatus presented an impediment to the easy passage of urine; and in 1 where the pressure of a malignant growth offered the same obstruction. In 30 they existed without any apparent disease at all of the uropoietic viscera.

Solid Urinary Deposits.—In 2 cases of the second class, in 5 cases of the third, there were calculi in the kidneys; in 5 cases of the third there was sand, in one of which, as before mentioned, it was in a cyst.

Tuberculosis of Kidney.—Tubercle, in the form of small round masses, was found 88 times in the kidneys in the 2161 cases examined during the ten years. In 15 only of these was it associated with degeneration. In one of the 15 the size was altered, the organs being mottled and large; 6 times they were mottled only; 8 times they were granular; on no occasion had they attained the advanced state of atrophy indicated by a reduction of bulk.* I think this observation is quite sufficient, at least to exempt tubercle in the kidney from any charge of being a cause of Bright's disease: their conjunction is evidently a mere coincidence.

Malignant Disease.—Malignant disease had affected the kidneys 9 times; of these, 2 had slight granulation of the surface, the rest were of healthy appearance.

Complications of Bright's Disease with other affections not of the Kidneys.

With Heart complaints.—In the following tables will be enumerated the number of times that cardiac lesions were found in combination with Bright's disease, compared with the frequency of their occurrence uncomplicated by that renal degeneration:

* See Decennium Pathologicum, Part I., Tuberculosis, in Medical Times, No. VI., Oct. 23, 1852.

TABLE A. — *Valvular Disease of Heart.*

Form of Valvular Disease.	With 1st Class of Bright's disease.	With 2nd Class.	With 3rd Class.	Total with Bright's disease.	Total without Bright's disease.
Heart healthy: recent fibrin on valves .	...	...	2*	2*	12
Heart healthy: valves thickened . . .	...	4	9	13	30
Heart healthy: chronic deposit on valves	...	5	18	23	46
Heart diseased: recent fibrin on valves .	..	5*	5*	10†	17†
Heart diseased: valves thickened . . .	1	6	40	47	65
Heart diseased: chronic deposit on valves	2	16	43	61	79
Total	3	35	115	153	247

TABLE B. — *Disease of Muscular Walls of Heart.*

Form of Disease.	With 1st Class of Bright's disease.	With 2nd Class.	With 3rd Class.	Total with Bright's disease.	Total without Bright's disease.
Hypertrophy	...	15	56	71	99
Hypertrophy with dilatation	3	22	56	81	76
Dilatation	2	20	51	73	188
"Enlarged," manner not specified. . .	1	...	4	5	19
Atrophy, or thinning without dilatation .	...	...	3	3	17
Excess of fat alone	...	1	1	2	13
Fatty degeneration visible to naked eye .	...	...	1	1	12
Total.	6	58	172	236	424

* One of each of these had also chronic disease of the valves, which must be remembered in casting the totals, else the same case will be counted twice.

† Two of each of these, ditto ditto.

TABLE C.—*General Totals.*

	With 1st Class of Bright's disease.	With 2nd Class.	With 3rd Class.	Total with Bright's disease.	Total without Bright's disease.
Total with diseased muscular walls of heart	6	58	172	236	424
Total with valvular disease, the walls being healthy	...	9	28	37	88
Total diseased hearts	6	67	200	273	512
Hearts probably healthy	4	59	113	176	1195
Not examined beyond the pericardium	1	2	2	5	(?)
General total	11	128	315	454	1707

454 + 1707 = 2161 total cases examined.

Deduction 1.

In 454 cases of Bright's disease were found 273, or 60 per cent. of diseased hearts.

In 1707 cases without Bright's disease were found 512, or 30 per cent. of diseased hearts.

Therefore, that a person with Bright's disease will have also diseased heart, is as 3 to 2, and that a person without Bright's disease will be so affected is as 3 to 7.

Deduction 2.

In 1371 cases of healthy hearts were found 176, or 12·8 per cent. of Bright's disease.

In 785 cases of diseased hearts were found 273, or 34·7 per cent. of Bright's disease.

Therefore, that a person without diseased heart will have also Bright's disease, is as 1 to 7, and that one with diseased heart will be so affected is as 1 to 2.

Corollary from Deductions 1 *and* 2.

That a person with Bright's disease will have also diseased heart is one-third more likely than not; and that a person with heart-disease will have Bright's disease is but half as likely as not;

And therefore, probably, Bright's disease is a frequent cause of cardiac affections, but cardiac affections are not a frequent cause of Bright's disease.

Deduction 3.

Where the muscular structure of the heart was morbidly altered:

In Bright's disease, of 236 cases, 116, or 49·1 per cent., had diseased valves.

Without Bright's disease, of 424 cases, 159, or 37·5 per cent., had diseased valves.

Therefore, the association of diseased hearts with valvular lesions is as 4 to 3, or one-fourth more common in cases of Bright's disease than in those free from it.

Deduction 4.

Where the muscular structure of the heart was natural in size and form:

In Bright's disease, of 213 cases, 37, or 17·3 per cent., had diseased valves.

Without Bright's disease, of 1283 cases, 88, or 6·9 per cent., had diseased valves.

Therefore, valvular lesions in healthy hearts are nearly 3 times as common where Bright's disease exists, than where it does not.

Deduction 5.

Recent fibrin on the valves was seen,

In the 454 cases of Bright's disease, 12 times, or in 2·6 per cent.

In the 1707 cases without Bright's disease, 29 times, or in 1·6 per cent.

Therefore, patients afflicted with Bright's disease are more prone to acute valvular inflammation than others.

Corollary from Deductions 3, 4, *and* 5.

As, therefore, in Bright's disease, valvular lesions are oftener found in both morbid and diseased hearts, and that both in a chronic and acute form, than in those who have not Bright's disease, we may infer that they are the consequence, more generally than the cause, of renal degeneration.

Complication of Bright's Disease with Tuberculosis of the Lungs.—In 11 cases of the first class, 2 had tubercle in the lungs.

In 128 cases of the second class, 32, or 25 per cent., had tubercle in the lungs.

In 315 cases of the third class, 52, or 16·5 per cent., had tubercle in the lungs.

In total 454 cases of Bright's disease, 86, or 18·9 per cent., had tubercle in the lungs.

In total 1707 cases without Bright's disease, 417,* or 24·4 per cent., had tubercle in the lungs.

* See Decennium Pathologicum, Part I., Tuberculosis, Medical Times, No. III., Aug. 14, 1852, where the total cases of tubercle in the lungs was seen to be 503, from which deduct 86 who had Bright's disease.

Deduction.—The atrophic forms of Bright's disease are one-third less likely to be complicated with pulmonary tubercle than those where a rapid copious deposit of the peculiar matter takes place, and these latter are about on a par with those free from renal degeneration.

The explanation of this is, not that Bright's disease confers any mysterious immunity from Tuberculosis, but simply, that the second-class kidneys are found generally at the same junior periods which are subject to tubercle; while the third class, like all atrophies, abounds in the later periods, which are also the least liable to the said pulmonary disease.

With Vomicæ.—In the 32 cases of pulmonary tubercle in the second class, 20, or 62·5 per cent., had vomicæ.
In the 52 cases in the third class, 31, or 59·6 per cent., had vomicæ.
In the 417 cases without Bright's disease, 289, or 69·3 per cent., had vomicæ.

Deduction.—The liability of tubercle to run into vomicæ is probably somewhat diminished by Bright's disease.

Art. II.

On the Mortality arising from the Use of the Forceps in Tedious Labours. By G. Hamilton, M.D., Falkirk.

About twenty years ago, immediately before settling in this locality, I received some practical instructions in the use of the forceps in protracted labours from Dr. Reid, of Edinburgh, who was known to the profession as an experienced and extremely dexterous manipulator in such cases; and I remember one of his remarks to have been, that, in a general practice, instrumental assistance might, he thought, be given with advantage much oftener than was then usual, especially when a certain amount of dexterity in the use of the forceps had been acquired. After entering upon the duties of the profession, I became convinced that Dr. Reid's views were correct; and upon talking over the subject with some intelligent medical friends, who had had a large amount of experience as accoucheurs, I found them much inclined to favour the same opinion. From that time until the present, my feeling has been, that similar views had gradually been gaining ground among intelligent general practitioners. Such being my impression, I was somewhat disappointed, in reading a review of Dr. Murphy's 'Principles and Practice of Midwifery,' in the 'British and Foreign Medico-Chirurgical Review' for last October, to find that a line of practice very different in this respect from what I have now for many years been accustomed to pursue, is still advocated. The fallacies of the statistics there employed to show the dangers connected with the use of the forceps, seem to me so obvious, and just views on the subject must possess so much interest for all who devote themselves to this branch of the profession, that I resolved to note the results of my own practice, in the last three hundred labour-cases attended by me, in order that the two series might be compared. I now furnish the data for instituting a comparison; and I shall give the means also, not only of comparing the two as a whole statistically, but in such detail as will enable a judgment to be formed of the *circumstances under which the results have been obtained;* for it appears to me, that the gross want of attention which we see paid to this element in such inquiries as the present, often renders these statistics not only worthless, but dangerous.

The question to be discussed is presented to us in a condensed form at page 422 of the 'Review' referred to, the general conclusions deducible from the statistical facts collected by Dr. Murphy being—1st. That in the forceps deliveries occurring in 78,892 midwifery cases, in the hands of British, French, and German practitioners, nearly 1 in every 4 of the children was still-born. 2nd. That, in protracted labours, "so far as the children are concerned, the proportion still-born is very much the same, whether the forceps be employed or not; the difference, if any, being in favour of leaving these cases to nature." 3rd. "That the use of instruments is to be discountenanced in all but exceptional cases of this kind, in which the habit of the patient is too feeble to admit of her enduring a protracted labour without risk of exhaustion." 4th. That Ramsbotham employed the forceps once in 729 cases, Joseph Clarke once in 742, Collins once in 684, Kilian once in 78, Carus once in 14, Siebold once in 9; and "Dr. Murphy's recommendation is to employ them only in cases of positive arrest," unless dangerous constitutional symptoms are present.

For comparison with the above, I shall now give the results obtained, both to mother and child, in my own 300 cases; and I may mention, that the few deductions I shall afterwards make on this subject are supported by a similar practice extending to at least a thousand cases. I have limited myself to the former number only because my notes regarding the earlier cases are less complete than I could wish. The numbers thus presented for statistical comparison, though limited, possess the obvious advantage over those collected from a variety of sources and attended by a variety of parties, that they were all, from the commencement, under my own guidance, in the same locality, and were all subjected to similar treatment. The population among whom I practise is generally robust and healthy, and is composed partly of the upper and middle classes, and partly of the working agricultural and manufacturing classes.

1st. There were 305 children, 5 of the labours having yielded twins.

Of these 305 children, 8 were dead at birth; 5 being putrid when born, and 1 having been destroyed at the commencement of labour, in a case of placental presentation. Of the other 2, 1 almost certainly died before the labour began. The remaining case formed a breech-presentation, in which considerable force was required to deliver the head. Setting aside, therefore, as seems fair, the first 7 cases, the mortality to the children at birth, from the labour process, was 1 in 298.

2nd. In the 300 cases the forceps were used 41 times—rather less than 1 in 7; and all the children were born alive.

3rd. Of the whole children born alive, 10 died within about a week; 6 having been delivered naturally and easily, and 4 with forceps. Of the former, 2 were certain to have died, of the latter 1, from being premature.

We have thus, of children that might have lived, 4 in 256, or 1 in 64, dead within a week, where the labour was natural; and 3 in 41, or about 1 in 14, where the forceps were used.

4th. The forceps were applied chiefly in first labours, and in females who had a particular form of pelvis. Thus, 10 primiparæ were so delivered; and among 7 other females they were used 17 times during the period included in the enumeration. If these 27 are deducted, we have the forceps used, in the remainder, in 1 in 21.

5th. The labour-process proved fatal to 3 mothers, or to 1 in 100, in all. Of these, 1 had a natural labour, and the other 2 were delivered with forceps. The mortality to mothers was thus 1 in 259 where labour was natural, and about 1 in 20 where the forceps were used.

6th. In none of the cases did any local injury to the parts occur from the use of the forceps.

The mortality to the children, from the labour-process, we thus find to be, under the treatment pursued by me, 1 in 298; while Dr. Clay gives the mortality to the children in the Dublin Lying-in Hospital, in 156,100 cases, as 1 in 17; Dr. Simpson, in the Edinburgh Maternity Hospital, as 64 in 1417, or 1 in 22; Dr. Lawrence, of Montrose, in a practice probably similar to my own, as 1 in 46.* We find, also, that the difference of mortality where the forceps were used is very great. In the article referred to, in the 'British and Foreign Medico-Chir. Review,' it ranges about 1 in 4 or 5; and Professor Simpson, in the Report of the Edinburgh Maternity Hospital referred to, states that he lost 2 in 3; while, in my practice, 41 consecutive forceps cases occur without one death. At the same time, we see that the mortality to the mothers has been very small. In only a few of the tables which I have consulted have I found it so low as in my practice—viz. 1 per cent. There must certainly, therefore, have been something very different from what usually occurs, either in the circumstances of the subjects of these cases, in the treatment employed, or in both. Perhaps the last supposition is the correct one; for—1st. Instead of the crowded, debilitated, and often rickety populations of large towns, from which these statistics are mostly drawn, I have stated that in the district in which I practise, the population is generally robust and healthy, while at the same time no puerperal epidemic prevailed in the neighbourhood during the period in which the cases occurred. 2nd. How far the treatment I have employed influenced the results, may be judged of from the following short sketch of the plan I usually pursue, in ordinary cases of cranial presentation.

Labour is usually divided into three stages. For the sake of convenience, however, in speaking of my practice, I shall divide it into two portions, the first terminating when the os uteri has been dilated to about double the size of a crown-piece.

I have for many years been in the habit, as a general rule, to which, however, a few exceptions have occurred, of interfering very little with the first half of the labour process. In protracted cases of this kind, should the pains not be very urgent, I quietly allow nature to take its course, for twelve, twenty-four, or even more hours, contenting myself with, in some cases, giving an opiate, and especially abstaining from all forcing measures.

* Edinburgh Monthly Medical Journal, Nos. 23, 29, 32.

I have not found, in my practice, the rule laid down by the late Professor Hamilton, that the first stage of labour should not be allowed to exceed twelve or fourteen hours, so urgent as was insisted on by him. With the precautions, which I shall afterwards mention, I have generally found that little more than annoyance to the mother results from a considerable extension of this rule. And here, I may remark, that it is of great consequence to keep in view, that delay in the first half of labour is by no means so dangerous as in the latter half; and hence, that an important exception must be taken to Dr. Simpson's statistical deduction, of the ratio of mortality increasing with the length of labour. *Cœteris paribus*, I have little doubt this rule would be found correct, were a sufficient number of cases accurately collated; but where cases are compared, in some of which the first half of labour was protracted, and the second short, while in others the reverse of this obtained, then the rule, it appears to me, is not correct, and is apt to mislead.

With the second half of labour I think commences the great danger from delay, and I therefore endeavour to shorten it as much as possible. My first efforts, in order to accomplish this, are directed towards clearing the head of the uterus. For this purpose, I rupture the membranes, if this has not already occurred, and support the anterior and lateral portions of the os uteri with my two fore-fingers during the pains, which are thus undoubtedly considerably increased in force and efficiency, until the uterus slips over the head. Where this cannot be easily effected with two fingers, I occasionally introduce the whole hand into the vagina, and push up the uterus all round the head. In two or three of the forceps cases I have given, I did not succeed in this procedure, although the head had got pretty well down in the pelvis; and in these I applied the instruments within the neck of the uterus, using the two fore-fingers of my left hand for pressing the uterus upwards as the forceps brought the head down.

The uterus having slipped over the head, my attention is next directed to the precise position which the ear next the symphysis pubis then occupies in relation to it. If the ear is exactly opposite the symphysis, or slightly to either side in the wrong direction for making the turn into the hollow of the sacrum, I rarely put off much time before applying the forceps, provided little or no alteration is taking place *in the position of the ear.* Under these circumstances, I do not allow myself to be deceived by the advance, or rather elongation and only apparent advance, of the head. When the ear does not indicate a revolution of the head, I feel assured that the labour will be a tedious and hazardous one, and generally within an hour, an hour and a half, or two hours, according as the severity of the pains and the nature of the case may seem to require it, I finish the labour by applying the forceps. Where the broad transverse form of the pelvis is decided, as in flat or squat-made females, where the ear is on the *wrong side* of the symphysis, where the pains are severe, but no progress is made, where the mother is a primipara, or where the first half of labour has been tedious, I feel that it is generally useless, and always hazardous, to lose much time, and I usually interfere at an early period. It will be observed that seven females furnish seventeen cases of application of the forceps. Now, all these have the broad, flat pelvis; and it is a common circumstance for such patients, who are aware, from previous experience, of the ease with which they can be relieved, to request that the forceps should be applied early, their own feelings making them sensible that no progress is taking place in the labour. In such cases, the pelves are often otherwise sufficiently roomy; and I have frequently little more to do than turn the head gently round, when the pains themselves effect the delivery in a few minutes. Some authors advise, when the head is placed transversely, that the fingers or hand should be used for the purpose of endeavouring to turn it round. In a very few such cases, since I have been in practice, where the pelves have been exceedingly large, or the children small, I have succeeded in doing this; but very generally I have found that no good can be effected with the mere fingers or hand in giving the head the proper turn, and I therefore never lose time in persevering in such attempts.

My endeavour, when using the forceps, always is, not simply to draw the head forwards, but also to make it perform the necessary revolution at the same time. Of the forty-one forceps deliveries which I have mentioned, speaking from recollection, I should say, that in at least eight-tenths the head was in the transverse position, or only slightly deviated from it. The rectification of this unfavourable position, therefore, I have found the most important point to which I have had to direct my attention in midwifery practice. When the child, even in an easy labour, is allowed to remain in this position above two hours, the danger to its life becomes imminent.

The remainder of the forceps cases was mostly of a more favourable character as to position, but required assistance, in consequence of the size of the child having been greater, or the pelvis less, than usual, or from the pains having been deficient in strength. The rapid deliveries which have been effected in this manner have almost removed, in my practice, one of the great sources of danger to the child, and of difficulty to the operator in applying the forceps. It is now several years since I have met with, among my own patients, a case of impaction from delay in delivery; and, except where there is a manifest disproportion between the size of the child and that of the pelvis, I seldom take this into account as one of the difficulties I have to contend with.

It has been stated, that this practice has yielded as its result 41 consecutive forceps deliveries without one still-born child; and, principally as a consequence of its application, there is presented the extraordinary circumstance, of no case of cranial presentation having been lost by the labour process in 298 consecutive births. Even this statement, however, does not give the full success which has attended it. I go back to my 318th labour, and 44th forceps case, before I meet with a still-born child; and again, even this does not give the risk to the child as a consequence of the application of the forceps, for the head, in the instance referred to, was so enormous that I never could get them fairly applied. I therefore turned the child, and tried, ineffectually, to deliver in this way. To accomplish delivery the perforator had to be used.

The necessary inference which must, I think, be drawn from what I have stated, is, not "that it may be laid down as a rule that nearly one-fourth of the children delivered by the forceps are lost," but rather, that, when applied in sufficient time, the increase in the amount of mortality caused by their use is very small indeed. I think it clearly follows from the statements made, that it is, in such instances as have been referred to, the delay in the second half of labour, and not the application of the forceps, which is usually fatal to the child. A mere inspection of the figures showing the ratio of cases in which the forceps have been used will demonstrate that very different results might be expected. We see them used in the ratio of 1 in 120 by Dr. Lawrence, Montrose; 1 in 472 by Simpson; 1 in 617 by Collins; 1 in 553 by Ramsbotham,* with very nearly the same ratio of mortality—viz., 1 in 4 or 5; while I have used them in 1 in 7 with no mortality, in 43 cases. We see, also, the result to the children upon the whole. Dr. Clay states Collins' mortality over all as 1 in 16; Dr. Simpson's, in the Report of the Edinburgh Maternity Hospital, is stated at 1 in 22; Dr. Lawrence gives 8 in 368, or 1 in 46; while I have 1 in 298. The favourable results obtained by me, I have already said, appear to me to have arisen from the second half of labour having been shortened; and my inference again therefore is, not that "the use of instruments is to be discouraged in all but exceptional cases;" but rather, that they should be used sooner and much oftener than is generally done.

That such should be the case is also supported by another and instructive view of this subject. During the period embraced by the 318 cases enumerated, I had occasion to use the forceps for a midwife twice, and for another party once, and the remarkable fact is brought out, that, when thus called to use them, my success was no better than that of others, for two out of the four children were still-born, and one was a case of impaction. The difference in result, however, is easily explained, when we examine into the attendant circumstances. The labour managed by the midwife commenced about 9 or 10 P.M; she was sent for about 5 A.M., and the woman continued from that time till noon in severe labour, when I was sent for, and found the head transverse. I delivered in a few minutes, and with the greatest ease, and yet the child was dead. In all probability had this case from the first been under my own care, I should have shortened the labour three or four hours. The other case was nearly similar. I have delivered with the forceps for this midwife, in all 14 times, and two of the children were still-born, and one of the mothers also died. I have noticed exactly the same circumstance in the practice of my assistants (who resided at a distance of several miles from me), when labours were allowed to be improperly protracted. Still-born children were constantly occurring; and the contrast with those labours which were entirely under my own care was so great as to have many years ago forced itself upon my attention.

I may observe, also, that it is certainly rather a startling circumstance to find in Professor Simpson's tables, constructed from Collins' practice, in 16,654 deliveries, only 24 forceps cases, while there are 74 perforations of the head.

It removes an important element of error in such inquiries as the present, that no supposable difference of dexterity in manipulation could, in my practice, have influenced the results.

* Edinburgh Monthly Medical Journal, Nos. 29 & 32. Dr. Lawrence does not state the forceps mortality, but only the general result. I have here quoted from Dr. Simpson's table, as I find the mortality given. In the extract from the British and Foreign Medical Review, however, Dr. Ramsbotham is stated to have used the forceps once in 729 cases, and Collins once in 684.

The mortality in my cases to the children born alive, but which died within a week after birth, was, of those born naturally, 6; and of these, 3 were premature. Of those delivered with the forceps, the mortality was 5; and of these, 1 was premature. Excluding the premature children, which could not have survived under any circumstances, the mortality was thus: in those born naturally, 1 in 86; in those delivered with instruments, 1 in 10. As the forceps deliveries were also, of course, those in which the labours were the most severe or protracted, the influence of the forceps in increasing the mortality was probably here, likewise, very small.

In 6 of the forceps cases chloroform was given. In 5 of these the deliveries were easy; and in all the recoveries were excellent. In 1 of the cases, however, the use of the chloroform was found to be objectionable, in consequence of its decreasing the strength of the pains. The position of the head barely allowed the forceps to obtain a proper hold when energetic pains occurred; and this could not be done when their force was somewhat lessened by the use of the chloroform. The chloroform was, in consequence, discontinued, strong pains returned, and the delivery was, with some difficulty, effected.

One consequence of pursuing the line of practice described has been, that, *for the accomplishment of labour*, I have latterly rarely used the secale cornutum. In these 300 cases I did not use it more than three or four times. In the first half of labour I have considered its use to be generally improper, having, as has been stated, usually left my patients at this period very much to themselves; and I have already remarked, that, with the precautions taken, I had seen no bad consequences follow. I do not, however, feel quite certain that this would have been the case had the ergot, or other means, been employed to force the uterus into premature action. In the latter half of labour I have also rarely had occasion to call in its assistance. In almost every instance where the ear has been placed opposite or near the symphysis pubis (and which I have said constituted about eight-tenths of the forceps cases), I have omitted its use, considering that the forceps supplied more certain and safe means of delivery. I am now, indeed, in the habit of using it almost solely either in the first portion of the second half of labour, where the case has been very protracted and the pains have become inefficient, or in cases where the head cannot easily be reached by the forceps.

The forceps I have for many years used are those invented by Dr. Zeigler, of Edinburgh, of the dimensions usually made by Young, of Edinburgh—viz., thirteen inches in length, over all. The ease and certainty with which these forceps lock, their workman-like proportions, and their straight blades (preventing, a good deal, I think, the risk of injury to the mother), all seem to me to render them the best instruments for short-forceps purposes which I have seen. I have used these, also, as long forceps, both with the blades equal and with a shorter blade; but in such cases I think they are barely of sufficient length, and the straight blades seem to me to have a disadvantage, from a high position of the head generally requiring a curved instrument to lay firm hold of it.

In my cases the maternal mortality was 1 per cent. in all, and 1 in 20 where the forceps were used. There can be no doubt that the abstract rule, that the mortality to the mother will be in the ratio of the severity of the labour, is to a great extent correct. The dangerous and uncertain development of the morbid puerperal diathesis, and the occurrence of puerperal epidemics, however, form such important elements in any calculations that can be made on this point, as to detract materially from the value of statistics. The safety of the child, it appears to me, is a far safer guide in forming an estimate of the comparative success of different modes of practice; and I think it may almost be adopted as an axiom, that, under similar circumstances, where many children are saved, comparatively few mothers will die.

In conclusion, I may remark, that the rules for my practical guidance in the management of labour which I have followed, are compounded of a limited application of two principles adopted by different authorities. With certain practitioners, I have, under ordinary circumstances, left the first half of labour very much to nature; while, to the latter half, I decidedly object to the application of this rule: on the contrary, in the treatment of this portion of labour I adopt the principle applied by Professor Simpson to labour as a whole, that the mortality will generally be in the ratio of the time it lasts; and I therefore adopt means for the purpose, as much as possible, of shortening it. The lives saved in my practice, compared with other mortalities which I have given, vary from nearly 2 to about $5\frac{1}{2}$ per cent. Anything approaching to this, even in a moderate private practice, would amount, in a series of years, to a large total; to the bulk of the population of a country the increase would be enormous.* The importance of the subject, therefore, can hardly be overrated. It is to be remembered, also, that the lives saved by our interference in this instance, unlike what is effected by medicine in many other cases, are mostly the best of the progeny; for it is generally the largest and most robust children that present the greatest difficulty in parturition.

If the line of practice which I have advocated recommend itself otherwise to the profession, it will not, I think, be esteemed a disadvantage that it greatly lessens the tædium of attendance to the practitioner.

Art. III.

On the Seat of Pulmonary Tubercle. By Edward H. Sieveking, M.D., F.R.C.P., Assistant-Physician to St. Mary's Hospital.

Morbid anatomists have hitherto failed in demonstrating with certainty the exact seat of pulmonary tubercle; and the statements of various observers with regard to the intra-vesicular or interstitial character of the deposit have been made more according to the theoretical bias by which they were influenced, than from actual observation. So close and accurate a pathologist as Hasse† admits that the exact seat of tubercles within the lungs has not yet been determined, in spite of the numerous researches hitherto made. Those in any way acquainted with the difficulties that interfere with the microscopic examination of the pulmonary parenchyma in its normal or morbid condition, will understand the cause of this. In health the amount of air-bubbles obscures our view, and the manipulation

* Taking the saving of life at 3 per cent., the numbers would have been, in the 156,000 in the Dublin Tables, 4680.

† Pathological Anatomy, Sydenham Society's edition, p. 328.

necessary to remove them alters the textures. In disease the air-bubbles still interfere; or where there is much congestion, or some material change, such as exudation, the confusion of the tissues and the opacity of the deposit create new impediments. In a great number of microscopic examinations of lungs containing tubercular deposit, we have been fortunate enough to obtain —we may say accidentally—sections which, examined by a low power, varying from 20 to 60 diameters, satisfactorily and distinctly exhibited the intra-vesicular character of the deposit. The most marked instance was the one delineated in the adjoining wood-cut (Fig. 1). It was taken from a child in whom an attack of acute tuberculization of the lungs supervened upon rheumatic heart-disease and central cerebral softening. Both lungs were studded equally from the apices to the bases with miliary tubercles of a translucent appearance, and of the size of an ordinary pin's head. Under a power of 20 diameters the termination of a minute bronchus was seen, from which, at regular intervals, small offsets were given off, terminating in the semi-opaque bluish tubercular nodule, encircled by the basement membrane of the vesicle. The brighter hue of the bronchule and its ultimate offset, with the sharp line of their coats, admitted of no doubt of their character, as little as of the continuity of their channel with what had been a pulmonary vesicle. The outline of the latter was obscured by what we have rarely failed to observe accompanying the deposition of tubercular matter, of whatever hue or character—a ring of exudation-corpuscles. The arrangement of the miliary deposit on the bronchule bore a close resemblance to that presented by a bunch of currants. The second illustration which we give (Fig. 2) was taken from a more advanced case of tubercular deposit in the lungs, occurring in an omnibus-driver aged 60, who had been ill six weeks, and occupying the greater part of both lungs. There was a large cavity in the right apex; the left apex presenting a remarkable freedom from deposit, though the remainder of this lung was extensively affected. There was

Fig. 1.

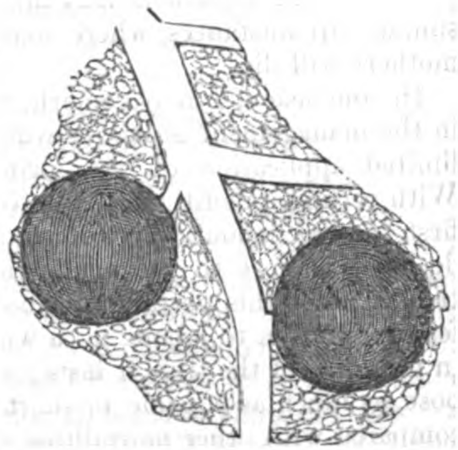
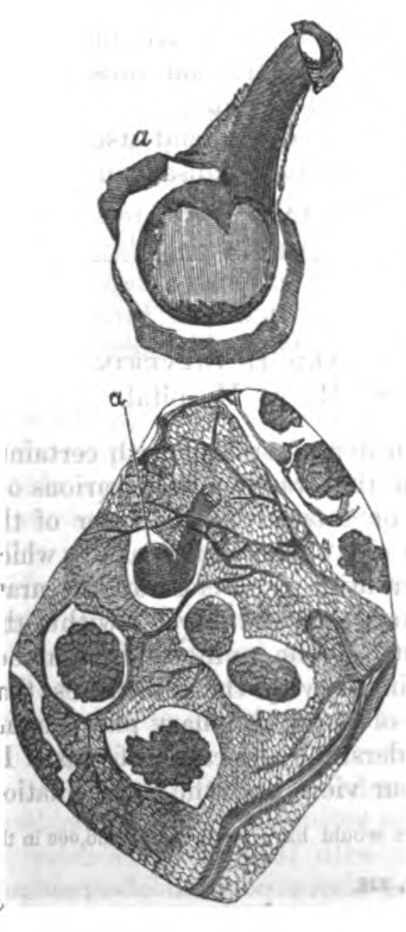
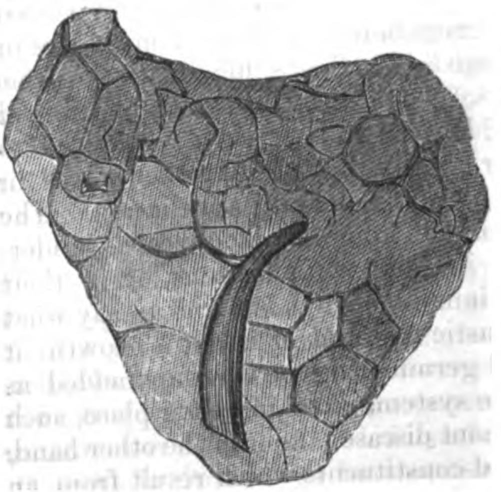


Fig. 2.



also fatty degeneration of the heart, atheromatous thickening of the aorta, and intensely fatty liver. At the first inspection, the tissue intervening between the tubercular matter—which belonged to the aggregated variety, and presented a grey and opaque appearance from retaining its crepitant character, and only exhibiting a florid hue,—was set down as not inflamed. Under the microscope it was found to be replete with exudation-corpuscles, showing the (inflammatory) molecular disintegration which was in active progress. A section was obtained, in which—besides numerous small tubercular deposits, more or less invested by exudation-corpuscles, and surrounded by the inter-vesicular textures, in which the bloodvessels were seen freely ramifying—one air-vessel was detected containing the tubercular matter. The opaque character of the contents distinguished it clearly from the normal tissues, while the sharp outline of the vesicle and of the bronchule, with its open mouth facing the observer, showed its relation to the air-tubes. The air-vesicle was noted to be surrounded with a light ring, like a halo, for which no explanation is offered. In the minutes of the observation it is stated: "I was fortunate in seeing the bronchule of one vesicle; the walls of the duct were seen continuous with the vesicle, which was lined by exudation-corpuscles and cells." In both the instances detailed, the tubercular matter filled (and undoubtedly also distended) the air-vesicles completely; the deposit ceased abruptly at the orifice of the terminal bronchule, and lay in the sheath formed by the basement-membrane of the vesicle, as a well-clipped bullet fills its mould.

Fig. 3.



The third illustration (Fig. 3) exhibits the changes produced by a more advanced degree of deposit than either of the former, while it also serves to prove the intra-vesicular seat of pulmonary tubercle. The specimen was taken from an individual whose lungs were the seat of extensive deposition of crude yellow tubercle. Fine sections exhibited a botryoidal arrangement, in which the bronchule leading to the diseased cells was patulous at its distal end, while as it approached the tubercular mass it became obliterated, and terminated in a mere cord drawn out to a point. The cells, from the mutual pressure exerted by the gradual increase of deposit, and the consequent obliteration of the interstitial parenchyma, had assumed a polygonal shape, in which, besides the tubercular matter, nothing but the vesicular coats remained to mark their individuality. Little was wanting to cause the destruction of these slight septa, and the whole would then be converted into one mass, subject to those ulterior metamorphoses that tubercle is liable to.

Our limits will not permit us to treat the various important considerations that suggest themselves in connexion with these matters more in detail at present; but it is hoped that the above positive observations will be acceptable to the profession, as much uncertainty yet prevails in our views, not only regarding pulmonary tubercle, but the actual healthy anatomy of the lungs. We cannot, however, dismiss the subject without observing, that while we are not prepared to assert that tubercular deposit in the lungs is never interstitial, we are inclined to believe that it is never primarily so; and our investigations have led us to believe, independently of any preconceived theory, that it is never effected without those local and molecular changes in the vascular system which are characteristic of inflammatory action, marked, on the one hand, by enlargement and congestion of the small vessels, on the other, by formation of exudative matter in the shape of aggregation corpuscles, or definite exudation-cells. But while we find these forms surrounding the tubercular deposit, they are in no way identical with it. Much confusion has arisen from using the various terms of microscopic nomenclature without a sufficient reference to definite characters, and we are of opinion that many of the views of distinguished observers, apparently at variance, with regard to the nature of tubercle, might be reconciled to one another, and to the real phenomena that present themselves. We must distinguish first between the crasis, or diathesis, or constitutional habit, that offers a tendency to tubercular deposit, and the local affection itself; secondly, in the analysis of the local affection, we have to separate the process and its accompanying phenomena, from its result—the deposit, effusion, or exudation of tubercular matter. We have above stated the process as we have observed it, to be one allied to inflammation; we have, in all stages of the deposit, from the most recent to those of older date in which a clear view was obtainable, seen the air-vesicle of the lung that was filled with tubercle, surrounded or invested by exudation-corpuscles, either mere aggregations of glistening molecules in a globular form, or advanced to the organization of cells, filled with the same molecules. When seen in the tissues, or detached from them and mixed up with tubercle, they preserve their identity, and no skill is required to recognise them; they differ from other objects that present themselves in microscopic pathology sufficiently to justify a definite appellation. The tubercular matter itself, after its elimination from the bloodvessels, undergoes a series of changes, which vary in their complications, or in their rapidity, according to the habit of the individual. It is easier to say what it is not than what it is: it is not a plastic material; it is not a growth; it is not the manifestation of a depraved germinating power, superadded as it were upon the normal energies of the system, or taking their place, such as we find to be the character of malignant disease; nor, on the other hand, is it identical with the effusions of blood-constituents which result from an exaltation of the normal energies and continue in possession of their vitality, by which they are susceptible of organization. The changes themselves bear a close resemblance, on the one hand, to crystallization; on the other, to chemical metamorphoses. In the most recent form we meet with tubercle as a finely granular blastema, in which there is a faint aggregation into circular forms. These forms next become more definite, exhibit a granulated surface, and predominate over the mere granular matter in which they are embedded. As the process of aggregation increases, the tubercle-corpuscles exert some mutual pressure, and their form is rendered slightly angular, while they vary in size from $\frac{1}{5000}$ to $\frac{3}{5000}$ of an inch; at the same time, there is an elimination of oily molecules, highly refracting particles, as they are commonly called. A chemical disintegration thus seems to manifest itself, and we have a new microscopic feature superadded upon those previously observed.

When the process of obsolescence ensues, the microscopic characters vary with the predominant features of the changes; the process of softening and parenchymatous fusion necessarily induces another series of changes; in neither, however, do we meet with anything like endogenous or independent development. The aplastic character of the tubercular product is maintained to the last. Epithelial forms and normal epithelium constantly occur in tubercular deposits; but never otherwise than as the cast-off tissue of the organ; and we are as little inclined to regard it as an essential constituent of the morbid product, as we should a portion of elastic fibre derived from the broken-up lung tissue which we meet with in a cavity. To this class we would refer all nucleated cells found in tubercle; and though we may not set down all non-nucleated corpuscles contained in morbid products as tubercular, we may, with Lebert, pronounce the tubercular corpuscle to be characterized by the absence of a nucleus. While the various products derived from the blood often pass into one another, both in regard to their chemical constitution, and the forms which they put on, observation teaches us the importance of recognising distinct types not as a matter of theoretic wisdom, but as a natural fact; and though we are not at liberty to expatiate more fully on the subject at present, we venture to conclude, that the local manifestations of the tubercular diathesis are not exempt from that general law.

Art. IV.

On the Elimination of Lead by Iodide of Potassium. By E. A. Parkes, M.D., Professor of Clinical Medicine in University College. (Supplement to a Memoir by M. Melsens, on the use of Iodide of Potassium in Mercurial and Saturnine Poisoning.)

In the memoir by M. Melsens, so ably translated by Dr. Budd, in the last number of this journal, the statement, that the compounds formed by the union of mercury and its salts with certain of the tissues, can be destroyed, and the metal be dissolved by iodide of potassium, and be eliminated through the kidneys, is proved not only by clinical testimony, but by actual chemical evidence of the presence of mercury in the urine. The elimination of lead in the same way is rendered highly probable by the solubility of the saturnine salts and compounds in iodide of potassium, and by the undoubted prophylactic and curative powers of iodide of potassium in cases of impending or actual lead-poisoning. M. Melsens did not, however, chemically prove that lead could be made to pass off by the urine in the same way as is undoubtedly the case with mercury, and he left, therefore, a gap in the chain of evidence for future observers to fill up. A case of saturnine paralysis has lately occurred to the writer, in which iodide of potassium appeared to cause the elimination of lead in the urine—a fact which seems to complete the argument of M. Melsens.

A painter, aged 38, was admitted into University College Hospital, in February, 1853; he had suffered for more than two years with paralysis of the extensors, and, in a less degree, of the flexors, of both fore-arms; there was a well-marked blue line along the edge of the gums. He had been incapable of work for eighteen months, and had therefore not been exposed for a long time to any fresh source of poisoning. He has been treated for two months very carefully, but ineffectually, in the Middlesex Hospital, and, among other means, by "sulphur baths."

Professor Williamson was so good as to undertake the examination of the urine for lead, in the Birkbeck Laboratory of University College. He was furnished with four specimens: 1st. The urine of February 2nd to 3rd, no medicine having been given. 2nd. The urine of February 3rd to 4th, no medicine having been given. 3rd. The urine of February 4th to 7th, 70 grains of iodide of potassium having been taken. 4th. The urine of February 7th to 10th, 90 additional grains of iodide of potassium having been taken.

Lead was not detected in the first two specimens of urine, but it was found in the urine passed after the employment of the iodide of potassium. I subjoin the report from the Birkbeck Laboratory.

"Four different portions of urine were received; two voided before the iodide was given, two voided afterwards.

"Equal portions of the urine, Nos. 1 and 2, were evaporated to dryness; the black mass which remained was calcined, and the fused salt was boiled with excess of chlorine water. This treatment was adopted in order to get evidence of lead from the insoluble sulphate. The solution with chlorine was tested carefully for lead, but none could be detected.

"The portions of urine voided after the medicine were treated as follows:

"About a pint of the urine was evaporated, and the organic matter destroyed by aqua regia, and the remaining salt fused, and boiled for some time with carbonate of soda. After having collected the precipitate and undissolved portion, it was well washed, and then treated with dilute nitric acid. The filtered solution was tested for lead with sulphuretted hydrogen, and it yielded a black precipitate of sulphide of lead. From the sulphide of lead from one of the urines, a distinct, though a very minute, metallic globule of lead was obtained.

"The quantities of lead present in the urines Nos. 3 and 4, seemed to be about equal, but too small for quantitative estimation.

"Birkbeck Laboratory, March 3rd, 1853."

It may possibly still be questioned whether lead might not have been detected before the use of the iodide, had the second and more delicate process been employed. This is, however, unlikely, for not only was the first process a very good one, but it can hardly be conceived that lead could have been passing off daily with the urine, before the employment of the iodide, without some improvement having taken place in the symptoms. So far from the symptoms having improved, they had been quite stationary for a long time, as usually observed in this obstinate form of paralysis. The compounds formed by lead with the tissues are well known to be extremely stable; and judging merely from the duration of the disease, the normal disintegration of the tissues appears in most cases quite insufficient to cause the elimination of the metal.

The iodide of potassium was administered in 10-grain doses, and on an empty stomach, in order to prevent decomposition by acids—a change which appears to destroy half its power. It was intended to combine galvanism with it, but unfortunately the patient, having behaved improperly, was obliged to be dismissed from the hospital. At the date of discharge no improvement in the symptoms was apparent.

The only other points determined in respect of the urine were the influence of the iodide on the water and sulphuric acid.

Effects of Iodide of Potassium on the Water and Sulphuric Acid.

Date.	Medicine.	Urine, quantity in 24 hours.	Reaction.	Sulphuric acid in urine in 24 hours.	Remarks.
Feb. 2, 3	None . . .	℥46	Acid.	23· 46	
„ 3, 4	„ ...	℥44	„	31· 90	
„ 4, 5	Haust. purgans, pot. iod., grs. x.	℥18	„	24·426	Several very loose stools, no urine with them; lithates.
„ 5, 6	Pot. iod., ʒss.	℥32	„	15·520	
„ 6, 7	„ ʒss.	℥54	„	26·784	
„ 7, 8	„ ʒss.	℥40	„	27· 08	Commenced to be iodized.
„ 8, 9	„ ʒss.	℥48	„	33·536	Strongly iodized.
„ 9, 10	„ ʒss.	...	...	...	Some urine lost by mistake, 39 oz. measured.
„ 10, 11	None . . .	℥33½	„	26·197	
„ 11, 12	„ ...	℥40	„	20·760	Effects of iodide passing off.
„ 12, 13	Haust. purgans	...	...	...	Some stools on the 12th, urine with them.
„ 13, 14	None . . .	℥54	...	30·076	Not iodized.
„ 14, 15	Pot. iod., ℈j. .	℥55½	„	30·305	Galvanism.
„ 15, 16	„ ℈ij. .	℥54	„	28·134	Galvanism.

The iodide, in doses of 30 grains in 24 hours, did not seem to have much effect on the water, and very little on the sulphuric acid. It is probable, indeed, that in most cases, this remedy has not the disintegrating and destructive effect of the alkaline salts of potash, although it possibly heightens this effect when combined with them.

The singular decrease in the quantity of the water on the 4th, 5th, and of the sulphuric acid on the 5th, 6th, is an interesting point, as it forms the fourth instance in which a striking decrease of sulphuric acid in the urine has been noticed after catharsis. The coincidence is worthy of inquiry, although it may turn out, on a more extended examination, to have been merely accidental.

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.

Part I. — Anatomical and Physiological Micrology.

The numerous and varied investigations which are being now prosecuted with such untiring energy and zeal in the various departments of medicine to which the microscope is applicable as a means of research, and the extreme difficulty of collating and comparing the results arrived at by the many observers of the several European and Trans-Atlantic schools, which lie scattered, in different languages, through the various scientific journals of the day, and in the Transactions and Reports of Academies and Societies, have led to the belief that it is highly desirable that the most important results of recent microscopic research should be brought into the compass of a retrospective report in the English language, which should appear at stated intervals. It is hoped that such a report will be, in some degree at least, useful to the advancement of medical science, and acceptable to the members of the profession, who will thus be enabled to keep their knowledge on a level with the discoveries of the day, in this department, which is, perhaps, now more extensively worked and more productive than any other portion of the entire domain of medical science.

In adopting the term Micrology, and in devoting to this subject a special section of this journal, we do not wish to be understood as implying that we in any way regard this class of subjects as different from those of general medical literature. We have always maintained that the microscope is only *one* of the many means of research at our command, and that to be used successfully it must be employed conjointly with the other aids to scientific inquiry. In placing certain classes of facts and observations in a special category under the term Micrology, we do so solely for the sake of convenience, and for the purpose of correlating inquiries which have great points of resemblance in the methods employed to pursue them, and in the results obtained thereby. Nothing, therefore, is farther from our thoughts than the wish to separate the minute structure of an organ or a tissue from its chemical properties; or, again, to separate these characters one or both from those of the physiological or pathological series, or all together from the therapeutical indications furnished in this order of induction alone, with anything like scientific precision and certainty. We adopt the term Micrology arbitrarily, but of necessity, and for the purpose of carrying out the great principle of the division of labour. So numerous and extensive, in fact, are the inquiries and observations in this department, that still further division is called for, and must be adopted, if we wish to confine our retrospect to anything like reasonable limits. The investigations going on in Vegetable and Animal Histology are naturally divided from each other. Moreover, we find that the memoirs on the minute structure of the lower animals are so numerous, that it would be impossible to combine them, except occasionally for the purpose of illustration, with the researches in human histology. And here again it is absolutely necessary to separate the pathological from the normal inquiries; we have therefore made arrangements for giving two reports annually, one on Normal Micrology, the other on Pathological. In each we hope to be able to give a succinct and satisfactory *résumé* of the advances which have been made in the preceding year. In commencing such a system of reports no ordinary difficulties present themselves; the amount of investigations of an important kind made within the last few years has been so great, that it becomes absolutely necessary to make selections, and in some cases to omit everything except a reference to the original communication, and a simple statement of its nature. It was also found necessary to limit our notices of memoirs to those which have appeared within the last year—a limit beyond which we have not gone, except in a few instances, for the purpose of completing a particular subject or giving a more clear view of an author's results. We feel confident that, with even great care on our part, many valuable essays have escaped our notice; but we trust that authors will kindly attribute this to its real cause—viz. the great extent of literary ground we had to traverse in our search through reports, transactions, and journals.

The intimate connexion of chemistry with micrological investigation is becoming every day more fully recognised; indeed, Lehmann has introduced the term Histo-Chemistry, to designate these necessarily allied methods of research. We need, therefore, make no apology for introducing accounts of the chemical constitution of tissues, when occasion may require it. It is, in fact, only by the combined employment of Chemistry and the Microscope that we can hope to obtain the solution of many important problems still unsettled.

Our Pathological part will appear in October of this year.

In collating the results of different microscopic observers, much perplexity is caused to the reader, by the different systems of measurement employed, the fractions of a line and a millimetre being constantly found in juxtaposition. It is greatly to be regretted that no successful measures have yet been adopted to obtain the recognition of a uniform standard. This would be a real boon to science; and we therefore urge upon microscopical observers the necessity of a combined movement to effect it. As editorial labours are greatly facilitated by working from *early* copies of papers, we will feel particularly obliged to any authors who will be kind enough to forward us copies of their essays. They may be addressed to the editor of this journal, or directly to ourselves, at Dublin. Continental papers will reach us readily if enclosed with exchange parcels of Reports and Transactions forwarded to the Royal Irish Academy.

HISTOGENESIS, OR DEVELOPMENT OF TISSUES.

"*Tissu Cellulaire Artificiel*" *of Melsens.*—The results of chemical investigation have made us familiar with the elementary composition of almost all the organized products which constitute the structures of animals and plants, and even in those instances in which our knowledge in this department has not yet reached the limits of scientific accuracy and precision, we may confidently anticipate that the labours of the numerous inquirers who are now actively engaged in prosecuting researches in organic chemistry, will, at no very distant period, fill many of the more important *lacunes* which may still be pointed out. On the other hand, histological inquiry has revealed to us the intimate structure and minute organization of every tissue, and with the aid of the microscope we can now, with perfect facility, study the ultimate form of every constituent element of the vegetable or animal, which possesses distinct morphic properties. What is still to be desired on this side of the general inquiry into the nature of organic bodies may be hopefully looked forward to; its accomplishment will no doubt, before long, reward the industry of the present generation of zealous and intelligent micrologists. But while chemistry thus informs us of the elementary composition of organic bodies, and their intimate structure is revealed to us by the microscope, we have yet much to learn with regard to the primary operations by which definite forms are assumed by the elementary tissues, at the moment of their formation. Here, indeed, exists a broad and deep *lacune*, which separates the results of the chemist and the micrologist. It is true, that bold speculations have been attempted to bridge this chasm; yet, notwithstanding what we owe to the splendid generalizations of Schleiden and Schwann, no portion of organic science is more incomplete, or presents so many undetermined problems.

Many considerations concur to establish the probability of the proposition, that the fluid state is the first in which the elements of a tissue exist previously to their undergoing a morphic determination, precipitation, histomorphosis, or whatever other analogous term may be used to denominate the change which occurs in the formation of a tissue of definite shape. The chemical properties of the fluid are familiar to us; so likewise are the forms which it assumes as a distinct tissue; but what those laws are, in obedience to which the latter condition results from the former, constitutes, I think, one of the most obscure, and at the same time intensely interesting questions which can be found in the whole domain of organic science.

Under the head of *histogenesis*—a term employed to designate the subject in question—many of the most obscure and difficult problems are included. Thus, for instance, we may inquire—what it is that determines the transudation of a blastema? what, the particular nature of this fluid, which cannot, in all probability, be identical in any two normal tissues, not to speak of those of pathological origin? And this latter question, it may be remarked in passing, bears a very great analogy to that which demands a solution of the obscure physiological problem, why the liver secretes sugar and bile, the kidney urine, &c. &c.

While science has still to deplore the extremely imperfect and unsatisfactory condition of this portion of its domain, we are yet not without indications of the direction in which research may be prosecuted. Thus we owe to Ascherson a knowledge of the interesting fact, that the contact of two homogeneous fluids, oil and albumen, results in the production of morphic elements. More recently still, the observations and experiments of M. Melsens have established the possibility of one of these fluids undergoing distinct and very remarkable histomorphosis. The experiments of this observer must be looked upon rather as a highly interesting exegetical study,—furnishing a valuable amount of inferential and collateral evidence, capable, to a certain extent, of application to the explanation of the changes which occur within the system,—than as a direct contribution to our knowledge of these operations.*

As will be seen, the results of this inquirer bear more particularly on the action of physical causes on a solution of albumen in combination with various salts, by which it is not precipitated. The solution was prepared as follows:—The white of an egg was mixed with its volume of water, and filtered; this constitutes the *normal solution of albumen*, with a specific gravity of about 1020. The filtered liquid is saturated with salts, which are added in excess, after which the fluid is filtered again, to separate the excess of salts; the fluid resulting from this second filtration may be denominated the *normal saturated solution of albumen*. The normal albumen saturated with chloride of sodium has a specific gravity of about 1200.

"My experiments have been made," says M. Melsens, "with almost all the salts which are without an apparent action on albumen, as well as with those which begin to precipitate it, but whose precipitates are soluble either in an excess of albumen or of the salt; for some salts of baryta, of lime, of magnesia, and of ammonia, &c., the albumen must be left in excess, for in saturating it with these salts it is precipitated, if they are added in excess; when we wish to examine the re-actions in this case, it is necessary to add, little by little, the solution of normal

* We may observe, however, that the action of acetic and other acids on the precipitation of albumen in the presence of neutral salts, had been already described fully by Parkes, (Medical Times, July, 1850.)

albumen, until we obtain the resolution of the precipitate first formed. I will not pronounce on the nature of the precipitates obtained; but it will appear evident that we must, in the generality of cases, admit that the albumen is precipitated in consequence of a particular physical disposition of the liquid; that if at times the precipitation does not occur immediately,—in dilute liquor, for example,—agitation may cause a troubled condition of the fluid, as occurs in precipitation, crystallization, solidification of water, of sulphate of soda, of phosphorus, &c. . . . . Tribasic phosphoric acid precipitates normal albumen saturated with salts; certain salts, amongst which are borax, phosphate of soda, acetate of soda, acetate of potash, form an exception to this rule; however, if the fluid be agitated with a glass rod, a troubled condition is slowly produced by the mechanical action."

These examples will be sufficient to show the nature of the results which have been arrived at by M. Melsens; similar experiments were made with a variety of other substances, such as corrosive sublimate, ether, alcohol, creasote, &c., but it is unnecessary to follow out their details. I shall therefore pass to the consideration of a series of experiments, which to many will appear still more conclusive, as to the action of physical causes on albumen.

"If," says M. Melsens, "after the experiments which precede, I am induced to believe that the particular physical constitution of the liquids plays some part in the precipitation of albumen, those which follow cannot leave the least doubt as to the action of agitation.

"Some very dilute solutions remain limpid until beaten with a glass rod, when they become troubled, and immediately parcels of fibres may be seen to form under the influence of agitation; under the microscope these appear as very distinct organized fibrous forms, which, by juxtaposition and felting together, constitute actual membranes. We have thus a phenomenon conformable to the production of mineral precipitates by the influence of agitation."

It is only necessary to notice briefly some of the remaining experiments. A current of air was passed through a solution of normal salified albumen, sufficiently dilute not to allow of the froth passing out of the vessel; this froth was seen to be transformed into a solid body, insoluble in ammonia, potash, water, or dilute acids. To obviate two objections which might be started to this experiment, air saturated with the vapour of water, and hydrogen purified by caustic potash and saturated with vapour, were successively employed. Lastly, to avoid all sources of error, a solution of albumen diluted with water was agitated *in vacuo*, by converting the vessel into a sort of water hammer, after expelling the air by heat and an air-pump, the orifice being subsequently hermetically sealed. The solution, perfectly limpid at first, became troubled after a few shakes, and a membrane was rapidly formed.

The solid body thus formed from a limpid solution of albumen by the simple effect of agitation, was subjected to microscopic examination by M. Gluge, from whose report the following extract is taken:

"The albumen of the white of egg, solidified by mechanical action, resembles false membranes, and even serous. It is presented to our view under the form of membranes covered with granulations of from $\frac{1}{2}$ to 1 millimetre in diameter, white, semi-transparent, about $\frac{1}{4}$ or $\frac{1}{2}$ millimetre thick, and sufficiently elastic. With a magnifying power of 300 we can distinguish an amorphous substance finely punctated, in which are found fibres, sometimes isolated, sometimes united in bundles, like the fibres of cellular tissue, more often easily isolated and elastic. Their diameter varies from $\frac{1}{800}$ to $\frac{1}{400}$ of a millimetre: more rarely there may be seen large and transparent fibres, analogous to those which are met with in fibrine. In the middle of these fibrous bundles may be observed granulations composed of little globules of $\frac{1}{400}$ to $\frac{1}{800}$ of a millimetre in size, and enclosing some bubbles of air. These globules are sometimes very regularly grouped, and then form rounded masses. The fibrous aspect of the solidified albumen differs from that which albumen possesses when transformed into pellicles, thin, opaque, and much less elastic, such as are obtained by the process of Ascherson. These last present folds, and not fibres fully developed like the former: they appear rather to be formed of very small granules."

I have myself had an opportunity of inspecting a very beautiful and large specimen of albumen membrane formed by the process of agitation, in the possession of M. Melsens. To the naked eye it presented the appearance of a membrane of a whitish colour, formed by the interlacement of parcels of fibres, which enclosed spaces of an irregular shape; it was tolerably tough, dense, and resistent.

M. Melsens was so kind as to present me with a small portion of the preparation, which I submitted to a most careful microscopic examination on my return to Dublin, and I had thus the satisfaction of verifying, in the most complete manner, the results of M. Gluge's investigations. As my examination was conducted with powers higher than those employed by this eminent micrologist, and as, consequently, some of the more minute details were better shown, I append a brief statement of the appearances seen in my examinations.

The specimen examined with a power of 420 diameters presented four distinct kinds of elements. Firstly, a granular base; secondly, fibres, which were flat, round, straight, curled, isolated, or interlaced in various ways; thirdly, spherical bodies of different sizes; and lastly, flat, scale-like particles, these being the least numerous constituents.

The granular base formed a very considerable proportion of the entire specimen, but did not appear to be uniformly disposed throughout it, as in some portions it formed nearly the entire mass, while in others it was almost altogether replaced by fibres.

The solidifying force would thus appear not to have acted with uniformity. To determine what modifications of it produced granular matter,—what fibres,—what again caused the formation of the little spherical bodies,—are questions of too delicate a nature to admit of ready solution. Could we arrive even at an approximate explanation, a great step would be achieved in the history of the obscure process of histogenesis.

The fibres constituted a very large element; the majority of them presented an extremely clear, decided, and *clean* outline. Many of them lay parallel to each other, and, as far as they could be traced, formed straight lines. Others, again, terminated in rapid curves; in one little parcel, which was very well shown, and which I had thus an opportunity of observing very attentively, they curled and interlaced with each other in a very graceful manner. In their general disposition, as well as in size and shape, they bore a very close resemblance to the fibres of the yellow elastic tissue, for which, I make no doubt, they would have been readily mistaken by any one casually looking at them through the microscope. Some masses of the fibres lay isolated; others were to be seen taking an origin in the granular base, where their commencement could not be well defined; others, again, lay in contact with the scale-like bodies, to be presently noticed. To me, however, the most interesting of all the structure observable in this preparation, are the spherical bodies. With the exception of the scales, they were the least numerous. I observed them in two situations, in most abundance on a square-shaped mass of the granular base, and in another portion in contact with fibres. The smallest bodies of a spherical shape, but not those to which I shall more particularly refer, were about the size of the ordinary oleo-albuminous granules, and closely resembled them in their optical properties. The *spherical bodies* were nearly uniform in size, grouped quite close to each other, and presented all nearly similar characters. They showed a dark border, their interior varied with the amount of light transmitted, but under all conditions of light, both as to intensity and obliquity, they presented a nucleus, which in all was of an elliptical shape, though the bodies themselves appeared as nearly as possible spherical. This nucleus was in length equal to about one-half the diameter of the sphere, and in breadth about one-eighth. What was the nature of these bodies? They were certainly not either spheres of oil or bubbles of air; there was not the slightest probability of the former substance being present; air-bubbles they also could not be; the specimen had been at rest in spirit for a very considerable time, while, as more positive evidence of their nature, I would adduce the peculiar nucleus, which in all was oblong, and did not disappear under any conditions of light. May we then regard them as nucleolated nuclei, or small nucleated cells? I confess that I can see no objection to this view; it is only to be regretted that it in no way throws light on the relation of the containing body to the contained, as to priority of origin, or necessary connexion of the one with the other. It is extremely interesting, however, to learn that albumen is of itself capable of undergoing a celluloid development. The last objects I shall notice, as observed in the preparation, are the scale-like bodies. There was only one situation in which I clearly recognised a group of them, though others existed scattered here and there. The group of scales was in close proximity to a parcel of fibres, and lay partly on a mass of the granular base. They appeared under the form of delicate laminæ, somewhat of a quadrangular figure, their borders irregular and serrated, and some few presented an imperfect nucleus; granules were likewise to be found on the surface of two or three. Were these of the nature of cells? This is a question which I cannot decide; it is possible they may be merely the result of a process of *flaking*, or of the fusion of granules precipitated together in small masses.*

Harting† has investigated the precipitate produced by mechanical means in albumen, and finds that in chemical composition it is different from the true connecting tissue; and he considers that the appearances are due to *flaking*, such as occurs in many fluids. M. Gluge's and my observations show, however, that this is not the case.

In connexion with the researches of M. Melsens, we may consider those of M. Panum on artificially produced morphic elements. We introduce also some interesting results of another class.

Artificial Cells, and Artificial Milk.—M. Panum‡ has been occupied with researches on the nature of the protein compound (casein of authors) which is thrown down as an amorphous granular mass, when blood-serum, previously rendered neutral in order to counteract the solvent force of its salts, is diluted with water. He has already published observations which lead him to think that this compound is identical, not with albuminate of soda, but with casein. He has obtained it in much greater quantity in the serum of the blood of women than in that of men, in greatest quantity in the serum of parturient women, and in small quantity in that of nurses. Thinking that the synthetic method might be brought to the aid of the analytic, to prove the nature of the substance in question, he conceived the idea of artificially forming with it, by the addition of the other necessary elements, some generally known products, such as milk and cheese, in which casein abounds. For this purpose he procured from six to eight pounds of tolerably clear serum of ox-blood, to which he added concentrated acetic acid (about six drops to the ounce), with a view to neutralize the salts; it was then treated with a large quantity of water. After being allowed to rest for some time, the compound already alluded to settled at the bottom of the vessel, and the supernatant fluid was poured off; by repeated additions of water, the substance was washed and freed from soluble albumen and other impurities, after which it was suspended in water. The fluid was now heated to 100° Fah., phosphate of soda was added until complete solution was effected, and then butter and sugar in the proportion in which they exist in milk, the whole being well shaken in a bottle. On cooling, this mixture assumed a milk-white colour, and had very much the taste of milk; it left, however, a weak but bitter after-taste.

* Académie Royale de Belgique, extrait du tome xviii., No. 7 des Bulletins. See, also, Lyons' Report on the 'Tissu Cellulaire Artificiel' of Melsens, Dublin Quarterly Journal of Medical Science, Feb. 1852, p. 237. We have reproduced the chief results of this memoir here, as they have been contested by Harting and Panum.

† Schmidt's Jahrbücher, No. 8, 1852.

‡ Virchow u. Reinhardt, Archiv., vol. iv. p. 155; translated from Bibliothek for Læger, July, 1850.

Under the microscope there was observed a large quantity of small spherules, having much resemblance to milk-corpuscles. On comparison with those of real milk, scarcely any difference could be seen, except that the artificial corpuscles were in general, though not in all instances, larger than the natural.

That these corpuscles possessed a membranous envelope, and were not merely drops or granules of butter, appears probable from the fact which was distinctly observed, especially in the larger ones, that the envelopes presented very evident though fine fissures; they were also not dissolved by ether, unless the membrane was previously removed by the action of acetic acid. Besides these small corpuscles, others of larger size, and exhibiting an interior formation, were found: these, says M. Panum, if met with in a living organism, would be regarded as nuclei with nucleoli, granular cell contents, &c. When sugar of milk was employed instead of ordinary sugar, the same structures presented themselves, but of a much smaller size. In attempting to follow out the supposed identity of the chief ingredient of the artificial with that of natural milk, M. Panum endeavoured to produce coagulation; but, even with the substitution of milk sugar in varying quantities, this could not be effected completely. The artificial fluid was readily drunk by two young cats, who were afterwards killed; in one, which was killed after it had drunk a very large quantity, the stomach was almost empty, but the chyliferous vessels were filled with chyle, though the animal had not had any other food for twenty-four hours previously. In the second only a small quantity of chyle was found in the vessels.

The production of these pseudo-cells has considerable interest in connexion with similar results arrived at by other processes. Panum remarks their analogy to those of the haptogen membrane of Ascherson. He has likewise observed cellular bodies to be formed by the union of chloroform and serum, which, if shaken together, form an emulsion, from which, on being allowed to rest, a copious sediment is thrown down. This, on microscopic examination, is seen to consist of small oval vesicles of about 0·001‴ in diameter, which consist internally of chloroform, and externally of a membranous envelope of coagulated albumen.

Cell-Development.—Donders* has investigated the elementary tissues, in reference to their morphological, chemical, and physiological properties. The following are his conclusions:—1. In plants and in animals an insoluble is formed from a soluble substance, which in virtue of its constitution takes the form of a cell-membrane. 2. The animal cellulose remains as such, or becomes thickened or is absorbed; it increases or diminishes with or without connexion with other cell-membranes; it becomes atrophied and forms fibres, which may again become thickened and grow: this atrophy takes place by a fibrous organization of the intercellular substance. 3. All animal cell-membranes, with their derivatives, have similar chemical and physical properties, and analogous chemical composition. 4. This animal cellulose exhibits strong resistance to most re-agents, has a slow change of substance, but neither contractility nor sensation.

Growth of Individual Cells.—Hugo Von Mohl† maintains that the innermost layers of cell-membrane are the youngest, while Harting and Mulder say that the outermost are the youngest. The latter authors are of opinion that the circumstance of thin, recently formed membrane being coloured blue by iodine and sulphuric acid, while in many full-grown cells only the inner layers manifest this re-action, while the outer are tinged yellow by these two substances, gives ground for the deduction that these outer layers have been formed subsequently to the others, and that the inmost layers of the full-grown cells are the same membranes which alone constituted the wall of the young cell. Mohl shows, by numerous careful

* Siebold und Kölliker, Zeitschrift. iii. 348; iv. 242. Schmidt's Jahrbücher, 12, 291. (We quote from the latter.)

† Botanische Zeitung, vol. v. p. 497; also Taylor's Scientific Memoirs, Part I. Nov. 1852; Part II. Feb. 1853. See, also, the recent work of Dr. Hermann Schacht, Ueber die Pflantzenzellen.

observations, that the walls of all the elementary organs of vegetables may be brought, by the action of caustic potash, or of nitric acid (though not by sulphuric acid, which was the re-agent employed by Mulder and Harting), into a condition in which they assume a blue colour with iodine; the only exceptions being the cuticle and perhaps the intercellular substance of some higher plants. Mohl therefore adheres to his already advanced views with regard to the endogenous deposit of the new materials in vegetable cells. We have introduced this illustration from vegetable cell physiology, as observations are more easily made on the growth of the vegetable than the animal cell. We may here refer to a memoir by Handfield Jones* on the development of fat-cells without previously existing nuclei. As the result of several investigations, he thinks that it must be admitted that the fat-vesicles are ordinarily developed without pre-existing cells or nuclei, the process seeming to consist in the gradual separation of oil from the blood, or rather from the exuded liquor sanguinis, whereby oil-drops are formed; these are at first minute, but afterwards enlarge by addition and coalescence, and soon become enclosed in an envelope of protein material, (whence derived?) This envelope is at first very feeble, perhaps scarcely organized, but afterwards acquires considerable strength, and sometimes persists after the absorption of the oily contents.

Cell-Development.—The extra-cellular development of cells, a condition admitted only by a few authorities in vegetable physiology, has been very extensively received in the domain of animal physiology and pathology, more especially since the promulgation of the cell-theory by Schwann. Remak has, however, from an early period, been strongly opposed to this doctrine, and has pointed out difficulties against its adoption, which are to be found, as he maintains, even in the writings of its chief supporters. In a recent communication on the subject,† this author cites many examples of admitted cell-development by segmentation—segmentation of the vitellus (Schwann), by formation of daughter-cells in the growth of organs, and the transformation of embryonal cells into tissues, epithelium, blood-cells, and muscular fibres (Reichert). He also calls attention to the absence of free nuclei in embryonal cartilage (Kölliker), and in the deeper layers of the epidermis (*idem*). J. Müller has likewise observed that endogenous cell-formation plays a large part in pathological anatomy. Remak himself considers the extra-cellular development of animal cells as improbable as the equivocal generation of organisms. As the result of a series of investigations undertaken to determine this question, he states that he has observed the propagation of the blood-corpuscles to take place by segmentation in the embryos of birds and mammalia; in the larvæ of frogs he has seen the striped muscular fibres to originate in the longitudinal division of cells. Observations since more extensively prosecuted have satisfied him that this is the general method of transformation of the embryonal cells into tissues. He gives the results of researches on the segmentation of the vitellus and the division of its cells. The cells he believes to pass subsequently into permanent tissues; thus the primary vessels are at first solid cylinders, consisting of embryonal cells united together, the external of which form the walls of the tube, while the central or axis cells pass into blood-corpuscles. He considers this theory applicable to pathological as well as normal histogenesis.

False Cellular Appearances.—Harting,‡ of Utrecht, calls the attention of microscopic observers to little bands or halos (coronæ) caused by diffraction and consequent luminous interferences in objects seen through the best microscopes. Thus, a bubble of air, which is well suited for making this observation, will appear as if surrounded by a thin membrane, and such errors have been often figured and described. Frequently not only one, but two or three, or even four, such bands may be seen, the margins of which sometimes exhibit prismatic colours.

* See London Medical Gazette, 1850. † Müller's Archiv., No. 1, p. 47, 1852.
‡ Edinburgh Monthly Journal, June, 1852, p. 579.

DEFINITE MORPHIC ELEMENTS.

Elements of Blood: Origin and Destination of the Blood-corpuscles.—Bennett* has investigated this highly interesting and important problem, about the solution of which two opinions have chiefly prevailed; one being, that the coloured corpuscles are formed from the colourless (by direct transformation, Paget); the other theory maintaining, that while such may be the case in fishes, reptiles, and birds, in mammals the coloured disc is merely the liberated nucleus of the colourless corpuscles (Wharton Jones). From his own researches Bennett inclines to the latter view; the paper contains the results of several observations connected with his researches on leucocythemia, which appear to him sufficient to establish this opinion. The following are his conclusions:

1. That the blood-corpuscles of vertebrate animals are originally formed in the lymphatic glandular system, and that the great majority of them, on joining the circulation, become coloured in a manner that is not yet explained. Hence the blood may be considered as a secretion from the lymphatic glands, although in the higher animals that secretion only becomes fully formed after it has received colour by exposure to oxygen in the lungs.
2. That in mammalia the lymphatic glandular system is composed of the spleen, thymus, thyroid, supra-renal, pituitary, pineal, and lymphatic glands.
3. That in fishes, reptiles, and birds, the coloured blood-corpuscles are nucleated cells, originating in these glands; but that in mammalia they are free nuclei, sometimes derived as such from the glands, at others developed within colourless cells.
4. That in certain hypertrophies of the lymphatic glands, their cell-elements are multiplied to an unusual extent, and under such circumstances find their way into the blood, and constitute an increase in the number of its colourless cells. This is leucocythemia.
5. That the solution of the blood-corpuscles, conjoined with the effete matter derived from the secondary digestion of the tissues, which is not converted into albumen, constitutes blood-fibrin.

Whatever importance may be attached to these conclusions as regards the adult, it must be borne in mind that blood-corpuscles are formed under conditions in which there can be no connexion shown with glands of any kind, as in the ovum; and again, as we learn from M. Lecanu's researches, that while fibrin is abundant in the serum, it exists only in the *envelopes* of the globules.

Micro-chemistry of Blood. — M. Lecanu has presented to the Institute a memoir on the blood,† in which he takes up the solution of some highly important questions—viz., the origin of the fibrine; the separation of the globules from the other constituents, and the determination of the chemical constitution of the globules. Having satisfied himself that a concentrated solution of sulphate of soda, which prevents the precipitation of fibrine, is without action on the globules, he received a quantity of blood into a solution of sulphate of soda, at a temperature of 12°, and marking 12° Baumé. The mixture was then filtered; the globules remained on the filter, while the serum passed through; from the latter, on the addition of eight or nine times its volume of water, the fibrine was precipitated in gelatinous filaments, scarcely a trace of it remaining in the filtered liquor. As it is subsequently shown that the globules contain but very little fibrine, and that only in their envelopes, it follows that this substance is contained in the serum chiefly. In order to obtain the globules perfectly free from serum, it is only necessary to allow them to remain on the filter, and wash them with the saturated solution of soda. When obtained thus, M. Lecanu finds that they consist of not less than eight different substances,—1. Hæmatosine; 2. Globuline; 3. A very small quantity of albumen; 4. A fibrinous matter, constituting their envelope; 5. An animal ex-

* Edinburgh Monthly Journal, March, 1852.

† Read to l'Académie des Sciences (de l'Institut), July 5, 1852; reported on by MM. Thenard, Dumas, and Andral.

tractive matter, soluble in ether and alcohol; 6. A fatty matter; 7. Various salts, amongst which are chlorides, phosphates, and alkaline carbonates; and 8. Water, which holds all these matters, with the exception of the envelope, in solution. Water, it is well known, breaks up the globules, leaving their envelopes isolated, and dissolving their contents; by boiling this solution, the globuline, hæmatosine, and albumen, are coagulated. Hæmatosine is soluble in alcohol and ether at ordinary temperatures, giving to the solution a beautiful red colour of blood, and by spontaneous evaporation forming small lamellæ of a metallic lustre, and an amethyst colour, exactly like the red silver of mineralogists. M. Lecanu believes in the presence of iron in the blood, but does not express himself definitely as to its particular mode of combination. (He suggests, with regard to hæmatosine, that there is reason to think that it would be an excellent substitute for the combinations of iron exhibited in chlorosis and other affections. The difficulty of procuring it in sufficient quantity is, however, considerable; the largest quantity he obtained being about 30 grains, from somewhat more than one pound and a half of ox blood.) With regard to albumen in the globules, the commissioners appear to think that as it is in such very small quantity in these little bodies, swimming in a highly albuminous fluid, its presence in them may be due to absorption or endosmose. The following contrast between the two chief constituents of the blood is highly interesting and valuable:—"It results," say the commissioners, "from these observations, that the animal matters which compose the serum are essentially different from those which compose the blood-corpuscles. The serum contains but albumen and fibrine: no globuline—no hæmatosine; the globules, on the contrary, contain hæmatosine and globuline, with a fibrinous matter, but no fibrine, and only a little albumen."

Nucleolated Red Corpuscles in Blood.—Mr. George Busk* has recently met with an example of this very rare condition. In his memoir on the Blood, in the 'Philosophical Transactions' (1845), Mr. Wharton Jones states that these bodies are common in the blood of the horse and elephant; but they appear to have occurred to his observation but once, and that doubtfully, in the blood of man. Mr. Busk's observation was confirmed by Mr. Huxley; only one corpuscle was seen, but in a very distinct and clearly defined manner. The nuclear portion of the corpuscle was rather smaller than most of the free blood-discs, but not so small as some of them, nor, apparently, much, if at all, below the mean size. On proceedings being taken to make an accurate measurement of it, it disappeared; no others could be found on prolonged investigation of the same blood, which was taken, about an hour after breakfast, from a young and vigorous man.

Blood-Corpuscle Nucleus.—Harting† advocates the existence of a nucleus in the human blood-corpuscle. When treated with a solution of corrosive sublimate, the corpuscles become remarkably altered in form; they contract about one-tenth of their size, assume a spherical shape, and at the border a small, generally round or oblong body passes out. This he considers to be the nucleus. This distinguished observer further remarks, that in the blood of the same animal much difference will be found in the comportment of the corpuscles to a solution of given strength, some being more and some less affected, apparently according to the manner in which they are presented to the reagent.

Absolute quantity of blood-corpuscles.—Quantitative Microscopical and Chemical analysis of the blood-corpuscles and blood fluids.—Under this head a memoir has appeared from the pen of Vierordt,‡ who proposes to estimate the quantity of the blood-corpuscles in a given volume of blood by actual numeration of them under certain conditions. The mode of procedure is as follows: a small quantity of blood is taken up by a capillary tube of $0 \cdot 1^{mm}$ diameter, and uniform in size throughout. The length of the blood column is measured under a low magnifying

* Microscopical Journal, No. 2, Jan. 1853, p. 145.
† Schmidt's Jahrbücher, No. 8, p. 147, 1852. ‡ Ibid., No. 4, p. 1, 1852.

power, and this quantity multiplied by the known diameter of the tube gives the total volume of the column. The blood is now allowed to run from the tube on a glass slide, and the last particles are washed from the tube with a solution of gum or albumen, with which the whole is now mixed, and uniformly spread on a glass slide in narrow striæ three to four inches long. With a finely pointed glass tube the corpuscles are distributed with as much uniformity as possible. The blood-streaks are then successively brought under the divisions of an ocular micrometer, and the number of corpuscles in each carefully counted. The smaller the diameter of the capillary tube, and the volume of blood employed, the less difficult will be the process of reckoning. Where we only wish for approximative results, a measured volume of blood may be mixed with a known quantity of a menstruum, such as gum or albumen solution, and a microscopic quantity of the mixture can be submitted to the microscope for the purpose of counting the corpuscles. In a subsequent communication* the author gives the results of nine measurements made with extreme care and delicacy of manipulation. The capillary tubes employed were $0{\cdot}8295^{mm}$ to $0{\cdot}8327^{mm}$ in diameter, and the quantity of blood operated on not more than $\frac{1}{500}$th of a cubic millimetre. The mean of the nine calculations gave, in a cubic millimetre, 5,174,400 blood-corpuscles; in a cubic line 59,396,100 corpuscles; the maximum in the cubic millimetre being 5,818,700, the minimum 4,597,800. These measurements were made during the winter, and when the author was suffering from indisposition. The process of counting is extremely laborious, Vierordt's idea of quickness being, that in summer and by good light a single enumeration may be completed in a week! The method of estimating the total quantity of blood-corpuscles in the body, by multiplying the number contained in a given volume, and ascertained by this process, into the total volume of the blood in the body, is open to several objections, and contains more than one source of fallacy. Thus the corpuscles vary much in diameter. (Lehmann has found great differences in the blood of the porta and the liver.) The several acts of micrometry and enumeration are of exceeding difficulty, and liable to many errors. But in addition to all these difficulties, which have been pointed out by Funke, in his report on the subject in the Jahrbücher (*loc. cit.*), we apprehend that the labour and truly iron patience required to carry out even a single numeration will be an almost fatal obstacle to its employment. We indulge hopes, however, that some simplification of the process may be devised, whereby its practical application to blood analysis may be, in a great measure, facilitated. In estimating the relative as well as actual quantity of the red corpuscles in various states of disease, such a method would be invaluable in clinical research. In a critical review of a work on leucocythemia,† without being acquainted with the researches of Vierordt, we had ourselves called attention to this great desideratum. Professor Bennett thinks the best method of estimating the relative proportion of the two kinds of corpuscles is to observe the spaces or meshes left between the rolls or aggregations of yellow corpuscles. In reference to this subject we ventured to make the following suggestions. "For the purpose of a more accurate estimate, we would suggest the use of an ocular micrometer, divided into squares of any convenient dimensions, with the aid of which we may ascertain, in a number of examples of normal blood, what is the natural numerical proportion between the red and white corpuscles, by counting both as they are placed under the squares in two or three opposite portions of the field selected indifferently; the average result of a few computations of this kind, we are of opinion, would be found not to be far from the truth. A similar proceeding could then be had recourse to, to estimate the proportion between the numbers of the white and red corpuscles in suspected cases of leucocythemia. Until some such process be brought into use generally, it is manifest that we can learn but little from such vague statements as are usually appended to reports of cases, "one-third as numerous," "greatly increased," &c., &c.‡ We do not

* Schmidt's Jahrbücher, No. 8, p. 147, 1852.

† See Bennett on Leucocythemia, reviewed in the Dublin Quarterly Journal, Aug. 1852, p. 204.

‡ Dublin Quarterly Journal, loc. cit.

venture to be too sanguine about the success of this process, but we have some hopes that it will be found useful.

Hepatic venous blood (*human*).—Lehmann has observed peculiarities in the hepatic blood of the horse, and Funke * has described the same characters as existing in that of the human liver. The red corpuscles are found to be somewhat smaller than the normal average, the central depression generally only slightly marked, and in many imperceptible; the corpuscles towards the borders of the field appear somewhat pointed or lenticular, but rarely two and never more than three are grouped like coins. Besides the coloured, numerous colourless cells are to be seen of various sizes, some very large, some single, some, especially the smaller ones, grouped together in twos, threes, or greater quantity. They are, for the most part, very pale, their surface only slightly granular; some completely resemble transparent vesicles, in which an excentric round nucleus is visible. In a considerable number fine dark highly refractive points are to be observed. This observation was made on blood from one of the large hepatic veins in an old woman five hours after death. From the spleen of the same individual, the

Splenic venous blood was examined by Funke. It contained elements similar to those described in the venous splenic blood of the horse, viz., small lenticular coloured cells, exhibiting only here and there the appearance of a central depression, and seldom united in *rolls*. The intermediate colourless corpuscles were of different sizes, generally small, partly single and isolated, partly united into large round or irregular heaps by an enveloping fine molecular mass. They are generally pale, but clearly granular, seldom exhibit a nucleus without the application of re-agents; but on the addition of acetic acid the majority display a single nucleus. He has also observed, as in the splenic venous blood of the horse, large round or oblong structures, having the appearance of coagula, and containing in their interior colourless blood-corpuscles and nuclei, and in a few cases, coloured corpuscles. He has not been able to establish the existence of a cell-membrane, and therefore doubts that these are to be regarded as "blood-corpuscle-holding cells." Granular cells were also visible.

Blood-corpuscle-holding Cells; Pigment-corpuscle-holding Cells. †—A very considerable amount of labour has been expended in the investigation of these structures by several micrological observers, and it is much to be regretted, with a great absence of uniformity in the results arrived at. Since the first discovery of the former bodies by Remak, two opinions chiefly have been entertained with regard to them. Kölliker (who describes them as round masses of blood-corpuscles which collect together, and subsequently acquire an envelop and nucleus) considers them as a stage in the solution of the blood-corpuscles; while, on the other hand, Gerlach, Weber, and others, regard them as formative elements. Remak, who gives the results of a very extensive inquiry, considers that both these theories are erroneous. In order to ascertain, if possible, the conditions under which these microscopic coagula occur, he examined a large number of animals, fishes, frogs, birds, and mammalia (dogs, swine, sheep, and oxen), in not one of which did he meet with a single example of these structures. From March to July, under different conditions of temperature, he examined eight perches, killed in different ways, with a similar negative result. At last, in the middle of July, he succeeded in finding them in a perch, but not until three-quarters of an hour after death, when the blood of the spleen presented the cells in question; though both it and other specimens of blood were unsuccessfully examined by vivisection. In other cases he was unsuccessful in his search for the cells, though made at a short period

* See Funke's Atlas, Plate IX., figs. 5 & 6; and letter-press descriptive thereof, p. 26.

† Müller's Archives, No. 2, p. 115, 1852. An article on this subject, by Mr. Wharton Jones, chiefly devoted to an exposition of Kölliker's views, appeared in the number of this journal for January, 1853; and as we now consider only those points not therein reviewed, we must refer our readers to it for further notice of these investigations.

after death, and also a day subsequently. The following are Remak's conclusions from his own researches. The belief in the existence of blood-corpuscle-holding cells, under normal conditions, in the spleen, liver, and other organs in the vertebrata, man inclusive, has arisen from mistaking for such structures, pigment-corpuscle-holding cells, and also round coagula enclosing blood-corpuscles. Neither the spleen nor any other organ can be considered as the locus of formation or of destruction of the blood-cells, so far as any observations yet made go. Pigment-corpuscle-holding cells occur so frequently in many full-grown fish and amphibia, that they must be regarded as normal elements of the blood. Investigations on the larvæ of frogs show, that when deprived of motion and nourishment, pigment-corpuscles are formed of the fat-corpuscles in the cells of the liver and spleen; and that during hybernation in the full-grown animal, a remarkable increase of the pigment formation takes place in the liver, at the expense of the fat. Lastly, blood-corpuscles holding fibrinous coagula form after death in the bloodvessels of the spleen and the kidneys in the tench (and probably also in other vertebrate animals). Their formation appears to stand in relation to the late occurrence of coagulation in the blood of the abdominal veins; but there is as yet, according to this author, no ground for believing that they occur during life in the vessels of the spleen or kidneys, or that they pass into the pigment-corpuscle-holding constituents of these organs.

Crystallization of Blood.—Funke* has prosecuted his researches on the crystallization of blood, and has ascertained *that all normal human venous blood may be crystallized.* This crystallization is brought about simply by the action of distilled water. A drop of blood is placed on a slide (the phenomenon will be better observed when the blood has been allowed to rest for a day; but the author states, that it can be produced in a fresh drop obtained from the hand by a needle), and after it has rested a few moments, a drop of distilled water is added, and the whole covered with a thin glass; in a short time, when the preparation is fairly coagulated, regular red-coloured crystals of different sizes and forms begin to appear; they are partly needles and columns, partly prisms, and partly rhomboid tables. The colour varies from a pale red to a deep crimson-red, the best examples of which are to be seen in the regular tetrahedra described by Lehmann and Kunde in the venous blood from the neck of the guinea-pig; these Funke states to be identical with the "albuminous crystals" of Reichert. Kunde has repeated the observations of Funke, and with similar results. Robin and Verdeil† have observed colourless tetrahedra in colourless blood-serum. These authors further observe, that many of the salts of the blood exhibit crystals of various forms, and have the faculty, during their passage into a crystalline state, of carrying with them any colouring matter which may be in their vicinity; and it is in this manner that they explain the formation of the coloured crystals of Virchow (hæmatoid crystals), and also those described by Funke. On the other hand, Funke, Kunde, Virchow, Reichert, and Lehmann, consider that a protein compound enters into their formation. Parkes has described and figured various forms of crystals as forming in putrefying human blood, with and without the addition of water.‡ A memoir by Virchow, on animal crystalline colouring materials, may be consulted with advantage.§ We have ourselves observed granular bodies and prismatic crystals in blood taken from the arm, twenty-six hours after being drawn.

PERMANENT TISSUES.

Dr. James Drummond‖ has instituted a series of inquiries into the mode of development of the tissues in the mammalian embryo. His researches embrace

* Schmidt's Jahrbücher, No. IX., p. 1. See also the very beautifully coloured lithographs, Pl. IX. of his Atlas of Pathological Chemistry. For his former papers see Henle und Pfeufer, B. ix.; also Dublin Journal of Medicine, May, 1852.

† Traité de Chimie Anatomique, vol. iii. p. 436. ‡ Medical Times and Gazette, June, 1852.

‡ Verhandlungen der Phys. Med. Ges., Wurzburg, vol. ii. p. 303, 1852. See also paper by Saunderson, in Edinburgh Monthly Journal, 1851–52.

§ Edinburgh Monthly Journal, Oct. 1852.

the consideration of, 1st, the morphological changes which the tissues undergo in the process of their formation; and 2nd, the chemical changes which the histogenetic or protein compounds undergo during this process. The author very justly observes, that the latter series of changes have not been sufficiently studied. He considers that with each morphological change we find a corresponding change in the chemical constitution of the structure; and, moreover, that whenever we find two or more tissues presenting the same morphological characters at any stage of their development, they have at this stage the same chemical constitution, however much they may differ in this respect afterwards. The memoir is chiefly devoted to

Morphology of white fibrous tissues, in its different stages of development. In a portion of blastema from the deep layer of the integument of an embryo calf, about two inches long, Dr. Drummond found—1st. Embryonic corpuscles in large numbers; they presented a rounded shape, granular surface, and measured from $\frac{1}{3000}$th to $\frac{1}{2500}$th of an inch in diameter; they were but slightly affected by acetic acid. 2nd. The same bodies, with a number of minute granules deposited around them. 3rd. Others, with the granular matter deposited in them so as to give a spindle shape. 4th. The last-named bodies, with the granular matter matted together so as to form a solid body. He concludes that Schwann's view with regard to the disappearance of the nucleus is incorrect, this body being visible in white fibrous tissue throughout the entire period of its existence in fœtal as well as in adult tissue.

Corresponding with the several phases of development just enumerated, the author of this paper has observed a passage from albuminous to other protein and gelatine-yielding compounds. The granules exhibit the following reactions.

They swell up and become gelatinous on the addition of weak acetic, tartaric, citric, and hydrochloric acids, by which, after a time, they are completely dissolved. They are insoluble in nitrate of potash. They are coloured orange-yellow by tincture of iodine, and greenish-yellow by chromic acid. Millon's solution of hyponitrate of mercury in nitric acid causes them to assume a blood-red colour. These re-agents have similar effects on plain and striated muscular fibre, as well as on the compounds of fibrine, and hence he concludes that the granules are identical with fibrine. At this stage, no gelatine exists in the white fibrous tissue. In the second stage, the reactions are the same. In the third stage, when the fibres are fully formed, gelatine can be extracted from them by boiling. Dr. Drummond recognises a second mode of formation of fibrous tissue, in which the blastema at once splits up into fibres. His researches are to be continued, and we anticipate many interesting results.

A memoir has been published by Engel, on the growth and development of animal cells and fibres, and the characters of their nuclei.*

Bone, Cartilage, and Connecting-tissue Corpuscles,—identity of these elements.—Virchow† has succeeded in isolating the bone-corpuscles and their canaliculi from the intermediate substance, by maceration in hydrochloric acid. Dry fragments, as well as fresh moist spicula from the medullary canal and thin layers from the cortical substance, were macerated in concentrated hydrochloric acid, either directly or after being boiled for some time. As the result of the action of this acid there remained only a pale, homogeneous, lightly striated mass, in which no corpuscles could be observed. He therefore concludes, that the bone-corpuscles, as well as the bone-canals, must be regarded as parts having a different chemical composition from the osseous basement structure. Donders and Kölliker had already obtained results somewhat similar. The former had observed, that on macerating bone for a considerable time in dilute hydrochloric acid, and then placing it in potash for five hours, and lastly in water, the osseous tissue dissolved, but the corpuscles remained, and nuclei, some of which were surrounded with cells, became visible in them. Kölliker likewise observed round or oblong nuclei in the corpuscles of bones similarly treated, but he doubts the existence of cells.

* Sitzungsbericht d. Kais. Akad. d. Wiss., vol. vii. Part I. p. 57. Vienna, 1851.

† Verhand. d. Phys. Med. Ges. Wurzburg, vol. ii. p. 150, 1852.

Branched and anastomosing cells, similar to the corpuscles of bone, had been found by observers in the cartilage of the invertebrata (by Bergmann in Loligo, Quekett in Sepia Officinalis). Virchow and Quekett had also described them in Enchondroma. More recently, Virchow has ascertained that they exist in normal cartilage. This author is convinced that the so-called cartilage-corpuscles are actual cells, lying either in a cavity in the basement-structure, or in a cell-cavity with a wall of double outline, and that they possess a membrane, granular contents, and a nucleus often provided with nucleoli. Near the line of ossification in growing cartilage, as well as in the young callus cartilage of fractures, these cells are extremely large, clear, and round; while in the neighbourhood of joints they are very small, pressed together, and dark. On the addition of water they shrink, and sometimes form such peculiar branched bodies, that they may be readily mistaken for branched cells. The best examples of the transition from round or oval cartilage-cells to those of stellate form, are to be found in situations where the fibrous passes into the hyaline cartilage, as in the invertebral substance. Here, in a section, all the stages may be observed between the hyaline cartilage, with round or oval perfect cells, and the fibrous, with its oblong, caudate, club-shaped, and stellate elements; boiling, or treatment with acetic acid, will render these appearances more distinct, by making the fibrous structure more transparent. It is exceedingly difficult to demonstrate the presence of actual cells in the small cavities of cartilage near the surface; the membrane of the cell appears as if it had melted into the intercellular substance, leaving only cell contents and a nucleus. Virchow states, however, that by continued action of acetic acid, or by maceration in hydrochloric acid, the entire cell, with contents and nucleus, becomes visible in the cavity; but without these preliminary steps, erroneous impressions may be received as to the relation of the cell to the intercellular substance.

Areolar, or Connecting-tissue Corpuscles.—Henle described interstitial and enveloping nucleated fibres in areolar tissue—the first, coursing along in the interspaces or on the borders; the second, spirally coiled round the parcel. Both these forms he considered to take their origin in cytoblasts, or nuclei, which became prolonged and grew endways. He studied their development partly in fully formed tissue, partly in embryonal; in the latter he found, at first, in a homogeneous substance, nuclei thickly pressed together, and arranged in longitudinal rows, which subsequently, by elongation and growing together, constituted nucleated fibres. This exposition was generally received, until Reichert stated that there was nothing determinately known about the formation of these "spiral fibres." Schwann described the embryonal areolar tissue as a gelatinous homogeneous mass, which is dissolved by boiling, leaving behind cells, which now become apparent, not being acted on by heat. The cells are partly round, partly caudate; the homogeneous substance is regarded by Schwann as a blastema, out of which the cells are formed. Reichert, on the contrary, regards it as purely an intercellular substance, from which the later connecting tissue is directly formed. Originally only cells exist, between which there subsequently appears an intermediate substance, with which their envelope becomes blended; the complete as well as the young connecting tissue being homogeneous. He denies altogether the existence of fibres, and of caudate or stellate cells. Virchow agrees with Reichert as to the persistence of the gelatinous intercellular substance, and its transformation into proper connecting tissue; but by careful treatment with reagents he has obtained further results. By boiling, by treatment with acetic acid, by hardening in pyroligneous acid, and by maceration in concentrated hydrochloric acid, he has found that in the connecting tissue, as in bone and cartilage, the intercellular substance may be separated from the cells which exhibit the greatest resistance to these reagents, and in this respect constitute a special group. In connecting tissue thus treated, cells will be obtained which are either isolated or anastomosing; round, oval, caudate, stellate or branched, interstitial or enveloping; *thus, then, the bundles of the connecting tissue are only striæ of the intercellular substance separated by the cells.*

These cells possess generally one, seldom two, nuclei; in transverse sections the nucleus appears round; in longitudinal sections, oblong or pointed; they may also have a spiral appearance. He has not seen any example of the direct anastomosis or union of these cells, though their prolongations, or branches, freely unite. Cell-contents are rarely to be met with, with the exception of a few fat granules which lie near the nucleus. The prolongations of the cells are sometimes large, sometimes exceedingly fine threads; the longer the finer.

Virchow states, that when the eye has been long practised to recognise these structures in specimens prepared as above, it will readily detect them in fresh sections; it is necessary to examine, not so much the bundles themselves as the objects situated between them. This examination may be most easily made in the ligaments, sinews, periosteum, cornea, also in the Pacchionian corpuscles. In the situations where these nucleated fibres have become apparent on boiling, or by the action of acid, there may also be observed fine tubes and canals; they form single or parallel interstitial channels, sometimes a thick network with stellate knots, but always a regularly arranged system of tubes or lacunæ. The structure is sometimes so like that of a bone section, that it is possible to mistake it for such; there are the same oblong or oval corpuscles, with branched or anastomosing prolongations, as are found in bone. Virchow considers that the ossification (?) of such a tissue takes place simply by the deposit of calcareous matter, the connecting-tissue-corpuscles passing into bone-corpuscles.

Quekett has already indicated the occurrence of what he terms nucleated fibre-forming cells in areolar tissue (in a fibrous tumour), and Kölliker has made many observations of cells in the connecting tissue of sinews, ligaments, and synovial prolongations. E. H. Weber and Kolrausch have found cells in the semilunar cartilages of the knee-joint, which, on section, present all stages of transition, from simple cartilage to bone-cartilage: thus, in one place, there will be found round nucleated cells, in a homogeneous intercellular substance; in another, long anastomosing, branched, angular, or stellate cells.

Virchow believes that these cell-fibres and cell-stars constitute a great system of anastomosing vessels and spaces throughout the connecting tissue, which serve apparently for its nutrition, allowing the nutrient fluid to be conveyed quickly and uniformly throughout the substance of the tissue, the nuclei serving as the regulating, the cells as the conducting, apparatus. He remarks on the probable sources of error in Lessing's recorded observations of a system of fine lymphatic vessels, which that author seeks to substitute for the bone canals and corpuscles. In a letter to the editor of the Archives,* Reichert considers the disputed points in the structure of the connecting tissue, and takes a critical survey of Virchow's observations. He regards as one of the most difficult problems to be resolved in Microscopic Anatomy, the determination of the question, whether the cartilage-corpuscles, the bone cells, the spiral fibres, and the corpuscles of mucous tissue, are to be considered as cells in their complete integrity, or only as the remains of cells which have undergone a fusion-process. He is of opinion that Virchow's observations on this point are not conclusive. He has often seen the cartilage-cell, under pressure, appear as a tenacious soft mass; and in the connecting tissue he has observed the corpuscles to undergo most remarkable changes of form, under conditions in which it was almost impossible to succeed in demonstrating them as cells. In all such cases, even when it cannot be demonstrated by the microscope, he thinks that we must assume that a cell membrane exists. Reichert considers Virchow's observations on the spiral fibres "as a most valuable contribution to our knowledge of the connecting tissue structures." He has himself prosecuted researches on these points, and finds that the spindle-shaped cells become transformed into spiral fibres, while the intermediate intercellular or basic substance forms the fibrillar matter. He considers, therefore, that the spiral-fibre—which name he prefers to that of nucleus-fibre, the nucleus, as Virchow observes, being often separated from the fibre when the latter becomes a long, thin, cylindrical body—must take its place henceforth amongst the independent histological elements.

* Müller's Archives, No. 4, p. 521, 1852.

Remak* also has investigated the elementary composition of connecting tissue and cartilage. In some respects his opinions agree with those of Reichert, Virchow, and Donders. He regards the *basic fibres* (stützfasern), as he terms the fibrous network and the spiral fibres, as equivalent to cells. Carrying out his views of intra-cellular cell-genesis, he considers the areolar-tissue parcels, like the hyaline base of cartilage, as the parietal substance of the original cells, which have themselves passed into the basic fibres. The embryonal cells which form the hyaline base of cartilage he describes as possessing a double envelop. In the process of cell-genesis by division, only the inner of the two membranes is engaged, the outer remaining in the relation of a mother-cell to the included daughter-cells resulting from the division. This outer membrane receives deposits of cartilaginous substance in layers on its internal substance; in this manner a cartilage-vesicle is formed, within which there may be one or more primary cells; and in the latter, division may take place in a similar way. The cartilaginous layers thus formed uniting together, constitute the intercellular substance, denominated by Remak parietal substance, from its relation to the individual cells. In some cartilages the cells do not unite together, but exhibit a free outline throughout the entire period of their existence.

On the structure of cartilages the reader may consult a paper by Prof. Redfern, on the phenomena presented in their growth and reparation.†

Kölliker‡ agrees with Virchow in considering that the so-called nucleus-fibres are developed as cells, and not as nuclei; but he dissents from the view which assigns to all the spindle-shaped cells the office of forming the nucleus-fibres, and denies any cell-development to the connecting tissue itself. He is of opinion that only a small proportion of the spindle-shaped cells stand in relation to the fibres, this portion of them being distinguishable from those of the connecting tissue by their shortness, dark outline, their fine, thread-like ends, and their elongated nuclei. These cells are to be found in embryos of four months, and even earlier, in situations where nucleus-fibres afterwards appear. On the other hand, in many places, as in the perimysium, the outer skin, the mucous membrane, fascia, and fibrous membrane, every trace of their original cell-composition becomes lost, only nucleus-fibres remaining, except in a few situations where the original cell-enlargements may be recognised, as occurs in the fasciæ and ligaments and in the cornea. Kölliker had already pointed out, that where in the fully-developed condition elastic tissue appears, in the young only nucleus-fibres exist. According to this author, the connecting tissue is developed under two forms, the solid and the areolar, and in somewhat a different way for each. The areolar, as it occurs in the sub-cutaneous and sub-mucous tissue, in the large cavities around the viscera, first appears in the embryo as a transparent, areolar, gelatinous mass, which consists of spindle-shaped or stellate anastomosing cells, and a semi-fluid, clear jelly, contained in the intercellular meshes, and including a small number of round cells of no definite character. He regards this structure as identical with the mucous tissue of Virchow.

The solid form of connecting tissue found in sinews, ligaments, &c., is developed, according to Kölliker, from spindle-shaped cells. He concludes, from his own researches, that the connecting tissue cannot be classed as identical with the cartilage tissue, or bone.

Mucus-Tissue.§—Besides the mucus, and the striated, nucleated network, Virchow has found a third element in mucus-tissues—namely, round, granular, nucleated cells, like those which occur in cartilage and connecting tissue, having an envelop which resists the action of water, acetic acid, &c., and viscid, coagulable, and contractible contents. These cells are of different sizes, and present appearances which render it probable that they are propagated by division. He considers that it will be more suitable to call this tissue *mucus-tissue*, even should

* Muller's Archives, 1, p. 62, 1852. † Edinburgh Monthly Journal, 1851.
‡ Verhandlungen d. Wurzb. Phys.-Med. Ges., Band iii. heft 1, s. 1, 1852.
§ Verhand. d. Phys.-Med. Ges. Wurzburg, vol. ii. p. 561, 1852.

further investigation prove it to be similar to the connecting tissue. To this class of tissue belong that of the umbilical cord, that of the chorion, and a great number of structures which have been denominated *colloid tumours.*

Development of Bone.—M. Charles Robin* has published a memoir on the development of bone. In some respects his results are interesting and important in connexion with the researches of the foregoing observers. The first mode of bone-formation which he describes is ***that by substitution;*** in which, as will be seen, ossification takes place without the intervention of cells or vessels. Portions of cartilage will be found in which no special point of ossification has yet become apparent, but in which only a central spot may be observed somewhat less transparent than the remaining parts. This on further examination may be proved to be a granular, opaque deposit in the basic substance of the cartilage, which gives the re-actions of phosphate and carbonate of lime with hydrochloric acid. This deposit is to be recognised some days before bloodvessels appear; it extends to the surface and to the ends of the bones in striæ, without rendering the cartilage opaque thereby. The granulations which compose this deposit have dark borders, and a clear yellowish centre; the granular condition, however, soon disappears, and the deposit has a homogeneous structure. The formation of the bone-capillaries is subsequent to that of the deposit. (This statement is reiterated by M. Robin.)

The osteo-blasts appear simultaneously with this deposit. The cartilage spaces begin to contract, the included cells become atrophied, and their borders appear irregular and dark; they have a diameter of $0{\cdot}018$ to $0{\cdot}025^{mm}$; on the borders of the osteo-blasts appear indentations, single or bifid: these are the commencements of the ramifications.

The second mode of bone-development described by M. Robin is ***that by invasion.*** As in the first form, a granular matter is deposited, which is seen in the interspaces between the cartilage cavities. (This mode seems to us identical with the first.) In the third form, bone is developed without any pre-existing cartilage or blastema. M. Robin has found this mode of development only in certain parts of some of the bones of the cranium.

Capillaries of Bone.—The first appearance of capillaries in bone is to be found at about the tenth or eleventh week of fœtal life, while ossification takes place a week or two earlier. The first entrance of vessels into the bone substance has not been observed; this author, however, supposes that we may conclude, from analogy, that the vascular canal commences at the point of contact of one of the vessels of the bone-membrane, and that by continued and progressive absorption of the osseous structure, it burrows deeper and deeper. He is quite opposed to the opinion that the vessels are formed by a communication being established between the cartilage spaces, or that they result from solution and absorption of the unossified cartilage. Towards the middle of fœtal life the vessels pass beyond the limits of the newly-formed bone, as their formation takes place now more rapidly than that of the osseous tissue; they run in canals in the substance of the cartilage, at right angles to the line of ossification. The diameter of the canals varies between $0{\cdot}08^{mm}$ and $0{\cdot}30^{mm}$; they contain one or more vessels, furnished with all the coats; Kölliker says he has observed the muscular coat. The elements of the medulla of bone are an amorphous connecting tissue, with molecular granules, medullary cells, and free nuclei, polynucleated plates (Robin), fat-vesicles and vessels. Robin denies the existence of a medullary membrane.

Medullary Cells and Polynucleated Plates.—M. Verneil† has made some investigations of the structure of bone, and in all respects confirms the statements of M. Robin, with regard to the medullary cells and polynucleated plates described by M. Robin. The former are spherical or polyhedral cells, found as normal

* See Schmidt's Jahrbücher, No. 1, p. 9, 1852; also Gaz. Med. de Paris, Nos. 19, 20, 23, 1851.

† Gaz. Med. de Paris, No. 26, 1852. For M. Robin's original communication, see Gaz. Med., No. 51, 1849; also Lyons' Retrospect of Histology, Dublin Quarterly Journal of Med., Aug. 1850.

elements in the medullary tissue, having a diameter of 0·015mm to 0·018mm. The latter are large plates or flattened lamellæ, sometimes polygonal, sometimes irregularly spherical, with a diameter of 0·050mm to 0·080mm; they are finely granular, and have each a nucleus or nuclei varying from six to ten, which are imbedded in the substance of the plate, and have a diameter of 0·005mm, and a length of 0·009mm.

See a memoir on the development of bone, by Engel.*

MUSCULAR SYSTEM.

Structure of Muscle.—A very long and elaborate memoir has appeared from the pen of Dr. Martin Barry,† on the spiral structure of muscle, and the muscularity of cilia. These papers originally appeared in 'Müller's Archives' (1850), and as from their extent, and the peculiar nature of this author's views, his researches do not admit of being condensed, we must refer those anxious to become acquainted with them to the originals themselves.

Dr. Dobie‡ has investigated the structure of muscle, and his researches have been made the subject of a report to the Edinburgh Physiological Society, from which we make the following extract. The primitive fibrilla presents the appearance of a cylinder bounded by two distinct margins, and consisting, apparently, of a series of dark and light particles arranged alternately, each light particle being crossed by a dark line. On the outer margin is seen a distinct white band or space, corresponding to what is termed by Harting, the band of diffraction. If this be regarded not as an optical phenomenon, but as a portion of the primitive fibril itself, the dark particles would seem to be surrounded on every side by a white space, and to present somewhat the appearance of a nucleus, while the space itself would represent the cell-wall. The reporters consider, however, that Dr. Dobie's researches are opposed to this view.

Non-striated Muscles.—Much attention has of late been devoted to the study of the non-striated, organic, or flat muscles, or, as they are now termed, Contractile Fibre-Cells, as they occur in different parts of the body. On this, as on other points of minute anatomy, we owe much to the labours of Kölliker,§ who has investigated them with much care. The following is a summary‖ of the situations in which they have been found.

Intestinal canal, in the muscular tunic, and in the papillæ, Brücke; *gall-bladder,* Kölliker; *urinary organs,* in the calyces and pelvis of the kidneys, in the ureters, the muscular coat of the bladder, in the trigon, Arnold; *sexual organs, in the male,* in the tunica communis, epididymis, vasa deferentia, and ejaculatoria, vesiculæ seminales, prostate and urethra: *in the female,* in the Fallopian tubes, in the uterus; *middle arterial coat,* this, according to Henle and Valentin, is very similar to the flat muscles of the intestinal canal; Kölliker and Eylandt confirm the existence of fibre-cells in it; *venous membrane,* in that of all veins, with the exception of those of the placenta, the meninges, the brain, the bones, and the corpora cavernosa; *lymphatics,* observed by Kölliker only; *glands,* in the spleen, Kölliker; in the liver, Meyer; in single glands, sometimes found, sometimes not; *in excretory ducts,* however, they are almost always present; *in the trachea; in the lung-vesicles,* Moleschott; *in the iris,* Valentin, Brücke, Kölliker; *outer skin,* tunica dartos, nipple, and areola mammæ; *in the hair-roots,* Eylandt, Kölliker. Walther has investigated the muscular fibres in the intestine, the bladder, and the middle arterial-coat, and thinks that he has succeeded in demonstrating a perimysium, consisting of a finely-granular hyaline membrane, which is seen after

* Sitz. Bericht d. Kais. Akad. Wien, vol. vii. part 4, 1851.

† Philosophical Magazine, Nos. 23 & 24, 1852; Muller's Archives, Heft 4, 1850.

‡ Monthly Journal of Medicial Science, June, 1852.

§ Kölliker's memoir appeared in the Zeitschrift f. Wiss. Zoologie, vol. i. p. 48.

‖ See a paper by Walther on this subject, Schmidt's Jahrbücher, No. 2, part 1, 1852. Consult also Lehmann's Physiological Chemistry, vol. iii. p. 64, &c.

long exposure of the fibres to the action of very dilute hydrochloric acid, which dissolves the fibrine.

Muscular Fibres of Areola and Nipple.—Kölliker has described a circular arrangement of the muscular fibres in the nipple and areola. M. Augustus Mercier* states that he has been long familiar with these structures, and their existence in the situation in question; but his views differ from those of Kölliker with regard to the disposition of the fibres. He believes that they are arranged in semi-ellipses, and that they form, on each side of the nipple, a plane, directed from above downwards, and from without inwards, in the same line as the fibres of the great pectoral. These planes of fibres are capable of compressing the base of the nipple, as the orbicularis acts in closing the lids, and as sphincters generally close their orifices. When the fibres are in action, it is easy, says this observer, to see the skin folded in the same direction. They have not only the effect of destroying the balance naturally existing between the afflux and reflux of the blood, but they are also of use in retaining the milk in the ampullary dilatations of the lactiferous ducts.

Muscular System of Mucous Membrane of Stomach and Intestines.—Brücke† has described a muscular layer situated, in the stomach, below the pepsin glands, in the small intestine below the follicles of Lieberkühn, and in the large intestine below the glandulæ tubulatæ; it consists of an external layer of longitudinal, and an internal of circular fibres. On the inner surface of the latter will be found an irregular mesh-work of these fibres, which surround the basis of the above-named glands, and pass between them to reach the surface of the mucous membrane, where they are only covered by the epithelium, basement-membrane, and capillary vessels; and it is to this mesh-work and its fibres that the villi owe their contractile powers. Van der Byl has confirmed these observations.

Contractility of the Gall-bladder.—Kölliker, who first described a delicate layer of organic muscular fibres in the walls of the gall-bladder, was, however, unsuccessful in his attempts to produce contractions in this organ by the application of an electro-magnetic apparatus, though the experiment was made only fifty minutes post-mortem in the body of a man who had been hung. Brücke‡ repeated the experiments on a dog narcotized by injection of opium into the jugular vein. The spiral of a Neef's electro-magnetic apparatus was brought into contact with the walls of the gall-bladder, when a slow but decided contraction was observed to take place. The experiment was several times repeated with similar results.

Contractile Fibre-cells of the Iris.—Mr. Joseph Lister§ has published investigations, the results of which confirm the opinions of Kölliker. The cells present a long, flat, or ribbon-like form, and there appears to be a tendency to a transverse arrangement of the included granules; this, however, is not a constant appearance, as in some cells a longitudinal arrangement may be met with. Kölliker describes a sphincter and dilator pupillæ; the former being visible without much difficulty in the eye of the white rabbit, or the blue iris in man, from which the uvea has been removed; it is about a quarter of a line broad in man, exactly forming the pupillary margin, and situated somewhat nearer the posterior surface of the iris. The dilator is the most difficult of examination. In all important respects, Mr. Lister's observations agree with those of Kölliker.

MUCOUS MEMBRANE—EPITHELIUM.

Structure of the Bronchio-Pulmonary Mucous Membrane.—In some preliminary observations on the pathology of the bronchial membrane, Dr. C. Black‖ states the result of his examination of the normal membrane. This he describes

* Gaz. Med. de Paris, No. 1, 1852.
† Edinburgh Monthly Journal, April, 1851.
‡ Schmidt's Jahrbücher, No. 1, p. 16, 1852.
§ Microscopical Journal, No. 1, Oct. 1852, p. 9.
‖ Edinburgh Monthly Journal, January, 1853.

as consisting of an epithelium, a basement or germinal membrane, on which the epithelium rests; and thirdly, of a layer of simple fibrous tissue, vessels, and nerves, subjacent to the basement-membrane. The epithelial cells are oval, polygonal, nucleated, and have their free surface set with delicate cilia. He believes that the epithelium (contrary to the opinion of some histologists) is prolonged into the ultimate pulmonary cells. If a very thin slice of pulmonary tissue be taken from the surface of the lung, and macerated for a short time in distilled water to decolorize it, and be then examined under the microscope, such cell will be found to have a perfect lining of epithelium.

The basement-membrane is spread out in a thin structureless layer, which is continued into the ultimate air-cells, constituting a part of their walls. Its thickness in the larger bronchi measures from $\frac{1}{18000}$th to $\frac{1}{20000}$th of an inch. It is found studded at equidistant points with small projections, which he regards as the germinal nuclei of epithelial cells. He regards the composition of the basement-membrane as similar to that of the blood-plasma after coagulation of the latter; that its existence is temporary, and that it is being continually disintegrated at its epithelial surface for the maintenance of cell-growth. The third layer, or the fibrine, consists of white and yellow fibriuous elements. The section of the bronchial membrane, microscopically considered, consists of definite, isolated, granular cells, floating in a viscid homogeneous fluid, which latter is the contents of the cells that have already attained their full growth, and have liquified in the natural process of decay. As an accidental, yet almost invariable, constituent of the bronchio-pulmonary secretion, will be found shreds or débris of the cell-walls. The granular cells or mucus-corpuscles (so called), when they have attained their full development, measure $\frac{1}{1500}$th to $\frac{1}{2500}$th of an inch. In this condition they are isolated, present a dark, well-defined outline, and are more prominent than pus-cells, with which they are frequently confounded.

Uterine Mucous Membrane.—Kilian* maintains the existence of a mucous membrane in the human uterus; it is more developed in the adult than in the young, and has a very considerable thickness; it possesses utricular glands, and is clothed with an epithelium, which, in the neck and the neighbourhood of the vaginal orifice, is furnished with vibratile cells. He is further of opinion, that the ciliated epithelium exists in the body of the uterus also, of which he has assured himself by examination of the uterine membrane in a rabbit, after the commencement of gestation. (It is not stated distinctly that cilia were observed in the human uterus.)

The augmentation in volume of the gravid uterus depends on the increase and multiplication of its elements. It is composed of fibres of gelatinous appearance, lightly striated in the longitudinal direction, often granular, and strongly adherent to each other, with distinct nuclei. These fibres grow during the entire period of gestation, the nucleus finally becoming changed into a fibrous filament. The division of the fibres into fine parallel fibrils, as described by Henle, this author thinks the result of preparation. Kilian recognises two such muscular layers. He has also described papillæ on the neck of the uterus. In studying the changes in the uterus after expulsion of the fœtus, he has observed the muscular fibres less coherent, filled with fatty granules, and remarkable for the extreme paleness of their nuclei.†

Mucous Membrane and Epithelium of Larynx.—Rheiner ‡ has investigated the epithelial covering of the larynx. Taking the free border of the epiglottis, the aryteno-epiglottidean folds, and the arytenoid cartilages as the limits between the larynx and the mouth and pharynx, he finds that in adults the pavement epi-

* Gaz. Med., No. 23, 1851, (from Henle und Pfeufer's Zeitschrift.)

† Compare herewith the researches of M. Retzius of Stockholm on the changes of the uterine tissue post partum.

‡ Verhand. d. Phys.-Med. Gess. zu Würzburg, Band i. Heft 2, 222, 1852.

thelium of these cavities is prolonged over the line named into the larynx, to the extent of 2''' to 3''' (nearly to the superior chorda vocalis), forming a margin whose covering differs in no respect from that of the higher parts. Below this line ciliated cells begin to appear more or less clearly. Henle has observed the entire under surface of the epiglottis furnished with cilia in the fœtus, and for some months of infantile life Rheiner has found the condition of these parts different from that in the adult. The large flat cells of the latter are replaced by a smaller variety, the epithelium, however, retaining its tesselated character, and its cells having the same relative dimensions; it constitutes, in fact, the variety described by Henle as "transition-epithelium." The ventricles of the larynx are found lined by a ciliated epithelium, which is interrupted at the free border of the inferior vocal chord by large flat cells, which form a band some lines in breadth, and are similar in size and structure to those of the epithelial lining of the mouth; further examination showed that both were continuous in the interval between the arytenoid cartilages; the superior and inferior portions of the ciliated epithelium thus come to be separated by a prolongation of the pavement-cells. From the first tracheal ring downwards the bronchial membrane is always completely invested with ciliated epithelium, and in many cases the cilia are to be found much nearer to the inferior vocal chord. Rheiner has examined the arrangement of the epithelium in the dog and rabbit; he found it in the former disposed as follows: from above downwards, the under surface of the epiglottis to the superior vocal chord inclusive, exhibited pavement epithelium; the ventricles, ciliated epithelium; the inferior vocal chord, flat epithelium, and from this down, ciliated epithelium: in the rabbit, first, a broad band of flattened cells; middle and base of epiglottis, ciliated cells; superior vocal chord, flat cells, and from this down same arrangement as in dog.

Vibratile Cilia.—M. Biermer* has studied the ciliary motion on the respiratory membrane in man, rabbits, and dogs. In a phthisical subject twelve hours after death, powdered charcoal sprinkled on the tracheal membrane was observed to be moved in a constant direction towards the larynx, from below upwards (the trachea being held vertically). This motion was visible to the naked eye, but was much more distinctly seen with a lens. On the next day vibratile cilia were still to be recognised, and in another case they were found fifty-two hours after death. Observations on the rabbit and dog confirmed the direction of the current in the trachea and bronchi of the second order. The rapidity of the movement was about two to three lines per minute. We have, ourselves, seen a beautiful example of ciliary motion which persisted for some hours, and was most distinct and easily recognised in a cell from the surface of a nasal polypus.

Hitherto observations have not been made of ciliated epithelium in the glands of the mammalia, though it had been seen in the kidneys of fishes and reptiles, and in the Wolfian bodies; Gerlach thought he saw cilia once in the kidney of a bird. Lately, however, Leydig † has succeeded in detecting ciliary motion in the uterine glands of the pig; it was first discovered accidentally, by Dr. Nylander, in a section to which no water had been added. The uterine glands in the pig form canals about 0·024''' to 0·05''' broad, and 6''' long, much coiled and looped, and terminating in large gland knots; each gland consists of a fibrous membrane and a ciliated epithelial layer. The ciliated cells are found throughout the entire length of the canals to their blind terminations; the motion is very energetic, and continues for a very considerable time. The author concludes that it is highly probable that ciliary motion exists in the uterine glands of other mammalia, the human subject included.

Dittrich, Gerlach, and Hertz,‡ made minute and careful search for ciliary motion on the walls of the cerebral ventricles, in subjects after decapitation; but though in one case the examination was made only twenty-four minutes after death, no cilia

* Verhand. d. Phys.-Med. Ges. zu Wurzb., i. 14.
† Muller's Archives, No. 4, p. 375, 1852.
‡ Edinburgh Monthly Journal, Jan. 1852.

could be detected. They therefore conclude that it is exceedingly doubtful that ciliary epithelium exists in these cavities in the adult. Purkinje and Valentin's observations, it must be borne in mind, were made chiefly on the human embryo.

Dermoid Layer of Membrana Tympani.—Under this name Mr. Toynbee has described a lamina very thin and transparent, which he states to exist between the epidermis and the radiating fibres of the membrana. Examined by the microscope, its structure is found to resemble areolar tissue. When injected, it may be observed to have numerous vessels ramifying through it, which form an elaborate plexus; it is also abundantly supplied with nerves.*

Middle Arterial Coat.—Mr. Drummond † has found that the middle coat of the arteries is composed of fibres arranged spirally, and has demonstrated this disposition in the fœtal calf. In the full-grown human fœtus, however, this author finds that he is unable to uncoil the artery in the above manner. In the healthy adult human artery he has demonstrated a distinctly spiral arrangement of the whole fibres of the external coat.

Corpora Amylacea.—Under this name Virchow ‡ has described certain bodies found (in the human subject) chiefly in the brain and spinal marrow, in part mingled with the proper nerve elements, and under conditions which do not admit of their development or composition being accurately ascertained. They are generally of microscopic size, and it is not easy to determine whether they exist in or around cells. Amyloid bodies occur in other parts of the body, but of manifestly different composition; these may be of considerable size, and then come under the category of "concentric spherical coagulated structures," formerly described by Virchow. The small concentric "colloid bodies" first seen by Kohlrausch in the kidney, and afterwards by Virchow, in small cysts on the serous surface of the female sexual organs, are the nearest approach to the bodies under consideration. The prostatic concretions are also somewhat similar in form. These consist of a semi-solid substance which exhibits the reactions of a protein compound, and which, on further investigation, Virchow has found to be a peculiar insoluble protein body, mingled with the seminal fluid, and which gives to the latter the appearance of a soft coagulum. Under the microscope this presents amorphous homogeneous colloid masses, more quickly soluble in acetic acid than any known protein substance, the solution being precipitated by ferrocyanide of iron. This substance unites with the proper seminal matter, and forms concretions, which on section present the appearance of concentric layers. An analogy may be traced with the concretions of other secretory and excretory apparatus, from those of the pancreas to those of the kidney and bladder. In connexion with this subject, it is necessary to call the attention of observers to the concentric bodies formed in the crystallization of the salts of various animal fluids. Then the carbonates, sulphates, and oxalates of lime, phosphate of soda, as also cystine and leucine, may present more or less perfect concentric arrangements. The carbonate of lime, however, gives the most delicate, and at the same time, perfect example of this appearance: we have ourselves repeatedly met with such forms.§

MICROSCOPIC ANATOMY OF THE NERVES.

Stellate Cells of the Retina.—In some cartilaginous and osseous fishes, H. Müller‖ has observed a layer of stellate cells, which he believes anastomose with each other. Kölliker observes, in reference to this communication, that if it be

* Phil. Trans., 1851; Monthly Journal, Nov. 1851.

† Edin. Phys. Soc. Monthly Journal, June, 1852, p. 574.

‡ Verhand. d. Phys.-Med. Ges., Wurzburg, vol. ii. p. 51, 1852.

§ See Robin and Verdeil's Atlas to their Traité de Chimie Anatomique.

‖ Verhand. d. Phys.-Med. Ges. zu Würzburg, vol. ii. p. 216, 1852.

confirmed by further research that these anastomosing cells are actually nerve-cells, it will be the first established instance of anastomosing stellate nerve-cells.

Nerves of Bone.—Kölliker* has made very extensive researches on the distribution of nerves in bone. He finds that filaments accompany the vessels, and are distributed with them, not only in the medullary surfaces of the long bones, but also in the spongy substance of the epiphyses, and even in the compact substance of the shafts. In the very centre of the cortical substance alone has he failed to detect nerves. In the short bones, the bodies of the vertebræ have been found very rich in nerves; in the scapula, the ilia, the sternum, and the cranium, they can also be demonstrated. They are derived from those of the cerebral and spinal systems, the sympathetic having apparently no share in furnishing them. It is not ascertained how they terminate.

Microscopic Ganglia.—So far back as 1838, Remak† observed minute ganglia in the substance of the heart of man and mammalia, on the finest branches of the pneumogastric. In 1840-41 he found similar ganglia on the walls of the larynx and bronchi; and he has lately assured himself of the existence of similar ganglia on the gastric branches of the pneumogastric, in frogs, birds, and mammals; but he has not been able to find them on the œsophagus. He has detected them in the posterior wall of the bladder, and in the gravid uterus. He has also detected ganglia on the glosso-pharyngeal, in the tongue of man and mammalia. Kölliker has confirmed this observation, as also the absence of such bodies on the terminal branches of the lingual and hypoglossal nerves.

Reichert‡ has studied the course, division, and termination of the nerve-fibres in the cutaneous muscle of the frog, with great care. We can only refer to this memoir.

Nerves of the Heart.—Cloetta§ has repeated, with the aid of the microscope, the investigations of R. Lee on the nerves of the heart, with the result of confirming the chief observations of that author. He could not, however, satisfy himself of the existence of the *fascia cordis.* As the nerves cross the vessels, many of them present flat enlargements, having a considerable resemblance to ganglia, and actually described as such by Lee. Cloetta, however, only regards them as nerve-expansions, as he could not see any ganglionic cells in them under the microscope. He confirms the statement of Lee as to the greater richness in nerves of the left ventricle than the right. Microscopic examination shows an abundant supply of nerves in the endocardium.

Nerves of the Uterus.—Herschfeld‖ has satisfied himself that nerves exist in the neck as well as in the body of the uterus; they are derived both from the cerebro-spinal and the sympathetic systems.

Corpuscula Tactus.¶—Our knowledge of the anatomy of the cutaneous nerves has been enriched by the discovery made by MM. Rudolph Wagner and G. Meissner, of certain hitherto undescribed bodies, which exist in some of the papillæ of the cutis, in particular situations of the body, and with which peripheral nerve-filaments are found to be connected. M. Wagner considers the papillæ of the cutis as divisible into two classes—those which contain nerves, and those which contain vessels: in the former the corpuscles are to be found. These little bodies are round, oval, or oblong, bear some resemblance to a fir cone, measure about $\frac{1}{40}'''$ in length, and

* See Microscopische Anatomie, vol. ii. part. ii. p. 337 *et seq.*

† See a short paper by Remak, Müller's Archiv., i. p. 58, 1852.

‡ Schmidt, i. 16, 1852.

§ Verhand. d. Phys.-Med. Gess. Würzburg, vol. i. p. 64, 1852.

‖ Gazette Medicale, No. 44, 1852.

¶ The first account of the corpuscula tactus is to be found in the Reports of the George Augustus University, and of the Royal Society of Sciences of Göttingen, Feb. 2, 1852.

$\frac{1}{80}'''$ in breadth, and are imbedded in the papillæ near their termination; a nerve-filament, with distinct double outline, is observed to separate from the cuticular plexus, and, after running through the papillæ, to enter the base of the corpuscle. The existence of these bodies has been since confirmed by many observers, but very great differences of opinion prevail as to their histological composition, and their mode of connexion with, and relation to, the nerve-filaments which are observed to enter the papillæ in which they are found. Their discoverers first described them under the name of "tästkörperchen" (corpuscula tactus), and considered them to consist of a system of superimposed laminæ, with long spindle-shaped, transversely-placed nuclei interposed, the nerve-filaments being supposed to terminate in the corpuscles after subdivision. A different structure, however, is indicated and described in connexion with some lithographic illustrations which accompany a subsequent paper by R. Wagner.* In these figures the corpuscles are seen imbedded in the papillæ, with the nerves entering their base, and remaining visible for a variable distance within the body. On the surface the corpuscles present sharp, transverse, dark-bordered striæ, two striæ being generally united together, and running parallel to each other. Small dark points like nuclei lie scattered more or less thickly on and between the striæ. It is but just to add, that M. Wagner considers the actual structure of the corpuscles as still an open question.

Kölliker† has published the results of his investigations into the structure of the papillæ of the cutis and the corpuscula tactus. He considers the papillæ to consist, exclusive of nerves and vessels, of a gelatinous tissue, sometimes homogeneous, sometimes distinctly fibrillar, which it is impossible to distinguish from connecting tissue (bindegewebe); of fine elastic fibres in various stages of development, spindle-shaped cells (areolar-tissue nuclei of Virchow), a cellular net, and isolated fine elastic fibres, and a fine net. These elements are usually so disposed, according to Kölliker, in most of the papillæ, that one can distinguish a cortical layer and an axoid or central portion. In some papillæ the fibre-elements take a longitudinal course, and the connecting tissue is distinctly fibrillar; the superficial portion is clear and homogeneous, and in many places is separated from the outer layer by transverse fibrous elements. He considers *Wagner's corpuscles* to consist of these fibrous elements in an undeveloped form and closely united together. He regards the corpuscles, in fact, as nothing but the *axis* already described by himself, and represented in fig. 4 of his 'Microscopic Anatomy,' and denominates them *axis-bodies* (*axenkörper*). He considers them to be formed of a homogeneous connecting tissue, best seen in a transverse section, and of an outer layer of undeveloped elastic tissue, which appears in the form of spindle-shaped cells united together, and more or less split into fibres, with long nuclei; the corpuscles being thus, in his opinion, no peculiarly-developed structures. According to his researches, they exist in the surface of the hand, the red borders of the lips, and the point of the tongue, only in small quantity in the back of the hand and the sole of the foot, and are absent in the toes, the breast, the back, the glans penis, and the nymphæ. Contrary to the opinion of Wagner, Kölliker states that a vascular loop can exist in the same papillæ with a corpuscle; and he suggests, with great probability, that when the nerves are invisible, it may be owing to the presence of pale, marrowless nerve-tubes, such as he has described in the skin of the mouse, and which occur likewise in man. Kölliker has generally observed two, sometimes—as occurs in the finger-pulps especially—four, dark-bordered tubes, surrounded by a neurilemma (which has escaped the notice of former observers); they appear as fine nerve-filaments of 0·006''' to 0·012''' in diameter, are tortuous, and run through the axis of the papilla to the base of the axis-body, when they become lost to sight. If, however, a very fresh preparation be examined after treatment by acetic acid, they will be seen to run, either isolated or together, outside the

* Müller's Archives, Heft 4, p. 493, 1852.

† Zeitschrift f. Wiss. Zoologie, v. C. Th. v. Siebold und A. Kölliker, Band iv. Heft 1, 1852.

corpuscles to the end of the papilla, their neurilemma becoming very fine, and at last disappearing altogether. With regard to the actual mode of termination, Kölliker has observed loops in at least six cases with great distinctness; in one instance, in the lips, he met with a beautiful nerve-knot.

From our own researches, made with considerable care, we are disposed to consider Wagner's original descriptions of the corpuscula tactus as the most accurate: his representations appear to us more like the actual structures than any we have seen. We cannot speak with any degree of certainty from personal observation, as to the disposition of the entering nerve-filament. In some instances, (none of our preparations was injected,) we thought we were enabled to recognise vessels in the base of some of the papillæ which presented corpuscles at their apices; we therefore incline to Gerlach's views on this point, and cannot agree with Wagner in considering the vascular and nervous elements incompatible constituents of the same papilla. Examination of Kölliker's figures has not satisfied us that these structures were accurately known and correctly described before the appearance of the joint researches of Wagner and Meissner, to whom, therefore, we think the merit of their discovery fairly belongs.

Professor Nuhn,* of Heidelberg, has also investigated the structure of these corpuscles. He estimates the proportion of the nerveless papillæ to those containing the corpuscles as about 8 to 5 — 3. With regard to their structure, his opinions coincide very nearly with those of Kölliker. He has observed the nerve-tubes under several conditions of relation to the bodies, being in some cases tortuous, in others distinctly looped; sometimes wound spirally round the corpuscle, and disappearing at its end near the point of the papillæ. In several instances he has observed and figured loops, which he is satisfied were not vascular, as he has been able to trace them back to the nerves under the papillæ. The disposition of the loops to the corpuscles was very various: in some cases they lay at the lower end of the body, in others they reached its apex; sometimes they lay at the side, sometimes in the middle, sometimes in the interior; but generally on the surface. In single papillæ containing corpuscles Nuhn has not observed vessels, except in one instance; but in the compound papillæ, vascular loops are readily seen,—there may be two loops, one on either side of the corpuscle.

In general, according to this author, the compound papillæ contain a corpuscle and a single vascular loop, or a pair of loops with an interposed corpuscle; exceptions, however, will be found. Thus, there may be no vessels, but in their place two corpuscles; and again, there may be vessels and no corpuscle. Here and there papillæ may be found, in which no trace of either nerves or vessels appear; in such cases, he considers it most probable that nerve-filaments do exist, but that they have become invisible from the absence of the central axis. He was unable to trace the further course of some tubes which he had distinctly seen to enter the papillæ. (41 well-executed figures accompany this paper.)

J. Gerlach† has also published a memoir on the structure of the corpuscula tactus. This observer divides the papillæ of the cutis into two groups — the *hill-shaped*, and the *sugar-loaf-shaped;* the former occur chiefly in the skin of the face, they have a broader base and are less high than the latter, which are best seen in the point of the finger. Numerous transition forms are met with between these types. Thus, lateral projections and splitting at the apex will be seen; contiguous papillæ will be found united, and this gives rise to the distinction of single and compound papillæ. The histological elements of the papillæ described by Gerlach, are chiefly connecting tissue, firmly fibrillated in a direction corresponding to the long axis of the papillæ; likewise short elastic fibres, which are principally to be found in the central parts of the papillæ. The border often presents a serrated appearance. Gerlach describes a vascular loop as a never-absent constituent of the papillæ; it is to be found coursing through the central part of the papillæ, and presents an in-going and out-going vessel; the transverse diameter

* Illustrirte Medicinische Zeitung, Zweiter Band, Heft 8, p. 80, 1852. † Ibid., p. 87.

of these vessels is 0·004‴, they belong to the medium-sized capillaries, and present a simple capillary structure. Within the same papillæ they may be often seen to cross each other; and this may occur more than once. These papillary vessels are not to be regarded either as arteries or veins: they are strictly capillaries, and are in direct relation with the capillaries of the cutis. Gerlach says that he can by no means assent to the opinions of Wagner, who regards the papillæ which possess corpuscles as wholly destitute of capillaries. After numerous injections of the skin, he has arrived at the conclusion that all the papillæ contain vascular loops. He describes the corpuscles as lying near the apex of the papillæ, while the capillary loop does not ascend so high, but ends before the commencement of the corpuscle; or the latter may occupy a lateral enlargement of the papilla, while the capillary loop runs to its end. The form of the corpuscles is round, oval, or cylindrical; their size varies, the mean being about 0·02‴ long, and 0·008‴ to 0·01‴ broad. Two small corpuscles may be found occasionally lying together. In shape and structure Gerlach likens them to an oval axis on which a thread has been wound in closely-set spirals, the thread having about 0.005‴ diameter, with a dark double outline, and a light stripe of homogeneous substance interposed. In his account of the relation of the nerve-filaments to the corpuscles, he states that one or two dark-bordered primitive fibres, of 0·001‴ in transverse diameter, may be observed to enter the base of the corpuscle after a tortuous course in the papilla; nerve-fibres may be seen to enter the side of the corpuscle about its middle or inferior third. In two cases he has observed a dichotomous division of the primitive fibre in the corpuscle, and he considers *that the fibres resulting from this division—which have always a dark border, and are only of a diameter of* 0·0005‴*—form the corpuscula tactus of R. Wagner, by surrounding, in the manner above described, a part of the axis-substance of the papilla with closely-set spiral coils.* The termination of the spiral fibres is to be looked for at the upper end of the corpuscle, and he is of opinion that they end in loops; a figure is given in which this termination is plainly visible. Gerlach, therefore, considers that Wagner and Kölliker are both partly right in their views of the relation of the nerves to the corpuscles: Wagner so far as he describes the nerves as remaining in the corpuscle, and Kölliker in stating that they terminate in loops. Gerlach regards the corpuscules as special organs, neither ranking them so high, in a histological point of view, as Wagner, nor so low as Kölliker, who considers them only as a peculiar arrangement of the elastic and connecting-tissue of the centre of the papilla. Gerlach also remarks a great similarity between the spirally-coiled nerve-fibre and the inductive coil of an electro-magnetic apparatus. (10 partly-coloured figures accompany Gerlach's paper.)

Regeneration of Nerves.—Dr. Augustus Waller has been engaged in a very interesting series of investigations on the regeneration of nerves.* While previous observers were contented with examining the nerve-tubes at the point of section, or in the cicatrix, this author has pursued the investigation to the peripheral ends; and has arrived at the interesting and unexpected result, *that the old fibres of a divided nerve never recover their original functions, and that reproduction of a nerve takes place not only in the cicatrix itself, but throughout the terminal ramifications.* The vagus of a dog having been divided, was examined after twelve days, when it was found that the inferior segment was completely disorganized, the fibres being all converted into black or irregular and opaque parcels, and the membranous tubes destroyed. At the end of a month the condition was different: almost all the disorganized substance had been removed; new fibres were found in place of the old, possessing all the characters of young fibres, and being very difficult of recognition, owing to their grey colour, intimate

* Philosophical Transactions, 1850; various papers in the Comptes Rendus of the Académie des Sciences, 1851, 1852.

adherence, and want of double contour; but on the addition of organic acids—concentrated acetic especially—they were readily recognised as embryonic fibres. The disorganized nerve presents nothing similar, there being only an amorphous tissue, which dissolves readily in acetic acid, without any residue. The areolar tissue which surrounds the nerves presents nuclei, which, however, it is easy to distinguish from those of nerve-fibres,—they are shorter, thicker, irregularly scattered on the surface of the membrane, showing no approach to parallelism, and the tissue itself does not separate into cylindrical fibres. The gelatinous fibres of Remak, which present the same structure and reactions as the young nerve-fibres, do not exist in appreciable quantity in the vagus before its distribution to the œsophagus, and hence cannot be a source of error in these observations. The author thinks that the neurilemma plays an important part in the regeneration of nerve-fibres: it remains intact during the changes just described. The results of section applied to the sympathetic fibres show that regeneration takes place in them in a similar manner. The following remarkable results were observed with regard to nerves in connexion with ganglia: the roots of a spinal nerve were laid bare, and cut above the ganglion, in such a way as to leave a portion of them in connexion with it; the animal was again examined after twelve days, when it was found that the sensitive part of the root attached to the superior part of the ganglion was altogether disorganized, in the same manner as when a nerve is cut in its peripheral portion. The nerve, followed into the ganglion, exhibited its branches disorganized, subdividing in the body, and mixing with fibres altogether normal, and appearing to terminate in a collection of ganglionic structures equally altered. All the fibres which passed out of the ganglion preserved their normal condition, the state of the fibres being found the same, after a month or more, as at first. The regeneration of the superior fibres between the ganglion and the spinal marrow takes place in the ordinary manner. The motor fibres were completely altered and disorganized to their extremities. These observations appear to be decidedly opposed to the views of Valentin, who has advanced the opinion that the ganglia are not centres of innervation, but only a mechanical means for the arrangement of the fibres.

After sections of the chorda tympani,—which was effected by passing a cutting instrument into the tympanal cavity, and turning it in different directions,—the inferior portion was found, at the end of from ten to twenty days, in the cat, dog, and rabbit, to be completely disorganized. In all these animals only about twelve or twenty normal tubes were to be found in the midst of the disorganized textures. These tubes appeared to the author to come from the lingual nerve, and to follow an ascending course in the chorda tympani.

GLANDULAR ORGANS.

Glands of the Buccal Cavity.—An extensive and elaborate memoir on this subject has been published by Kölliker.* We purpose to notice only such portions as relate to microscopic anatomy. The saccular glands are to be found either isolated, as at the root of the tongue, or grouped together as in the isthmus faucium, and the tonsils. Each gland preserves an external thick fibrous envelope, and is lined internally by a prolongation of the epithelium of the mouth. The fibrous layer is only $0{\cdot}01'''$ thick, consists of the ordinary connecting tissue, with nucleated fibres interspersed, and passes down to unite with the connecting tissue of the deepest mucous layer. The proper wall of the gland is situated within this fibrous envelope, and is much softer, of a whitish-red colour, and consists of two distinct layers. Imbedded in the wall of the gland are numerous follicles and vessels. The follicles measure about $\frac{1}{10}'''$ to $\frac{1}{4}'''$, are of a round or oblong shape, and much resemble the capsules of Peyer's and the solitary glands, and the vesicles of

* Verhand. d. Ges. zu. Würzburg, vol. ii. p. 169, &c., 1852. See, also, Kölliker's Microscopische Anatomie, vol. ii. part 2, p. 33, &c.

the spleen and lymphatic glands, and like these structures, possess an envelope and contents. The former consists of homogeneous tissue without nucleus fibres; the contents consist of fluid and solid portions; the fluid being in very small quantity, and having an acid reaction; the solid portion will be found to consist chiefly of small cells and nuclei. Nerves and vessels, the latter in abundance, may be observed on the gland-walls. Kölliker considers the tonsils as nothing more than an aggregate of such glands (from ten to twenty in number). The epithelium of the mouth can be traced into them through the apertures on their surface. Their secretions are also similar. This is contrary to the opinion of Henle, who regards the tonsils as examples of glands "en grappe." Kölliker, however, has confirmed his opinions by examination of the tonsils in the swine.

On the subject of glands, the following papers have recently appeared: by Heyfelder* on the Structure of Glands; Sappey† on the Origin of the Lymphatics of Glands; Brücke‡ on Peyer's Glands; Bernard§ on Structure and Functions of Glands. This author only recognises two divisions of glands, those "en grappe," and those "en crypte."

Glandular Organs of Stomach.—Ecker‖ describes the glands of the stomach, from numerous observations of his own on fresh and normal subjects. In the middle of the stomach are to be found only cylindrical glands of $\frac{1}{2}$ to $\frac{1}{3}'''$ in length, and $\frac{1}{50}'''$ in diameter, which for the most part run a straight course in the mucous membrane, and only towards the end become globular; their contents are large, round, or angular cells of 0·017 to 0·020^{mm}, with nucleus and granular contents. The glands of the cardiac portion are similar, only they are more generally branched and enlarged at their ends, and often contain highly-refractive fat-granules. He affirms the existence of the glands "en grappe" at the pyloric portion, which Frerichs and Kölliker deny.

An interesting paper on the comparative anatomy of the gastric glands by Molin deserves perusal; it is also well illustrated with lithographs.¶

Brücke** has published a memoir on the structure and development of Peyer's glands.

Wedl†† has investigated the glands "en grappe" found on the biliary ducts. They are more difficult of detection in man than in the horse. In the human subject, he has found them partly round, partly oval or oblong. In the ductus choledochus, the oval glands are $1\frac{1}{3}$ millimetre in diameter, the long ones $1\frac{1}{2}^{mm}$. He regards them as analogous to Brunner's duodenal glands.

Professor Frey, of Zürich, has succeeded in injecting the patches of Peyer's glands in the rabbit. Kölliker‡‡ has repeated his observations, and confirms his results. He found a quantity of vessels, often considerable, coursing through the nuclei and cells of the follicles, in a manner similar to the course of the vessels in the granular layer of the cerebellum. These capillaries were generally very fine, 0·0025 to 0·004'''; some, however, of 0·006 to 0·008'''. In the solitary follicles also he found vessels similarly disposed.

We regret that our limits oblige us to pass over two memoirs on the mammary glands, one by Luschka,§§ the other by Lauger;‖‖ they are of considerable extent, and do not readily admit of condensation.

No structures are more difficult of investigation than those of the glandular organs. We have yet much to learn with regard to the minute anatomy of glands.

Malpighian Bodies of Kidney.—In a communication on the relation of the

* Acad. d. Sciences, Juin, 1852. Gaz. Med., No 25, 1852. † Gaz. Med., No. 27, 1852.
‡ Denkschriften der Kais. Akad. d. Wiss. zu Wien, vol. ii. 1851. § Acad. d. Sciences, 1852.
‖ Henle und Pfeufer's Zeitschrift, N. F. ii. 3. Schmidt's Jahrbücher, 12, 301, 1852.
¶ Denkschriften der Kais. Akad. d. Wiss. zu Wien, vol. iii. 1852. ** Ibid., vol. ii. 1851.
†† Sitzungsbericht der Kais. Akad., 1850.
‡‡ Verhand. d. Phys.-Med. Gess. Würzburg, vol. ii. 1852.
§§ Müller's Archives, iv. 402, 1852. ‖‖ Denkschriften, d. K. Akad., Wien, vol. iii. 1852.

Malpighian bodies of the kidney to the uriniferous tubes, the author, Dr. J. Marcusen,* states, that his observations lead him to confirm the opinions of Bowman.

Structure of the Liver.—Kölliker† considers the liver to be distinguished from all other glands, as first pointed out by Henle, by the existence of a parenchyma composed of cells with membranous walls, distinct from their contents; these cells are placed in juxtaposition, and occupy the spaces between the meshes of the capillary vessels; but he has not been able to discover any evident connexion or continuity between this cellular parenchyma, which he believes constitutes the secretory apparatus of the organ, and the first appearance of the biliary canals with distinct walls.

In a memoir‡ presented to the "Académie des Sciences," M. Lereboullet advances similar views. He considers each hepatic lobule as constituting in itself a small liver, composed of secreting cells, afferent and efferent capillary vessels. He has not been able to trace the envelope of the capsule of Glisson in the human hepatic lobules. The cells contain a spherical nucleus, with a variable number of small punctiform transparent nucleoli, grey or yellow granulations, scattered through the cell, or accumulated in little heaps (biliary granules), and very small fatty vesicles in the midst of these granules. The nucleus may be absent, and the biliary granules indistinct. In the fœtal liver of the mammalia, as in those of mollusca and crustacea, there exist two kinds of cells, fatty cells in considerable abundance, and endogenous biliary cells of smaller size. M. Lereboullet regards the fatty cells (taking into account their development in the livers of fishes) as the first stage of the biliary cells. The biliary cells are arranged in longitudinal series, radiating to the centre of the lobule. These chains of cells do not form tubes, as believed by E. H. Weber; the cells remaining distinct and without communication. Two adjacent rows of cells form canaliculi, into which injection may be forced; they are, therefore, mechanically produced, and have no proper walls. The author has observed two sets of capillaries, the intra-lobular and peri-lobular, derived from the vena porta; and he considers that the biliary secretion takes place not only at the circumference, but also at the centre of the lobules, the blood of the hepatic artery having but a very small share in its production.

Dr. C. Handfield Jones§ has investigated the structure of the liver with great success, and his name is intimately associated with this subject by his able papers in the 'Philosophical Transactions.' His researches embrace an anatomical survey of the structure of this organ, in the various classes of animals. Our limits, however, prevent us from going into the comparative anatomy of the subject; we therefore make only such brief extracts as may serve to illustrate the most important disputed points. In recapitulating his observations on the liver of the invertebrata, he remarks, that the tubular or follicular type of arrangement is that which generally prevails, and to which a tendency is almost invariably manifested. He considers it probable, however, that a more or less considerable portion of the gland is in a condition which may be termed parenchymatous, the secreting structure being interstitially situated, and not in connexion with any excretory duct. In the next great division of the animal scale, the hepatic apparatus is constantly distinguishable into a tubular and parenchymatous portion; but the latter now preponderates immensely; and the office of the tubular structure is confined to serving as an excretory duct. Dr. Jones observes, that in the liver of fishes, the fibrous tissue, corresponding to the capsule of Glisson, is in comparatively small quantity; and as the separation of the elementary parts is thus facilitated, he has been enabled to make observations on the condition of the ultimate hepatic ducts. The following is the mode of proceeding adopted. A main branch of the duct is gently separated from the surrounding tissue, and by traction and

* Schmidt, Jahrbücher, 10, 14, 1852. † Microscopische Anatomie, vol. ii. part 2, p. 213.

‡ This memoir was presented to the Academy, Feb. 27th, 1851, and obtained the Prix Portal. See Comptes Rendus, No. 2, 1852. Some interesting observations are appended on fatty degeneration of the liver, to which we will revert in our Pathological Section.

§ Philosophical Transactions for 1852. Dr. Jones's last essay was read Jan. 17th, 1852. We have referred to his paper in the Transactions of 1849, in order to give a connected view of his researches.

laceration of the parenchyma, it is isolated with a multitude of its ramifications, and after careful washing, is removed to a glass slide. It is now spread out, covered with a thin glass, and may be conveniently examined with the microscope. Many of the smaller ramifications will be found to have sustained no injury; they have a diameter of $\frac{1}{1000}$th to $\frac{1}{500}$th of an inch, run a very long course, and give off but few branches at distant intervals; they taper slowly towards their extremities, which are found in various conditions, sometimes undoubtedly closed, with a defined, rounded margin, formed by homogeneous membrane; this he considers to be a rare form, the structure generally becoming less distinct towards the extremity, and the duct seeming to cease gradually. Similar appearances were observed in reptiles and birds, but with more difficulty in their preparation. The mode of termination of the hepatic ducts has been long a vexed question, and the most opposite opinions have been maintained by different authors. Thus the liver has been classed amongst the glands "en grappe;" its ducts have by others been thought to form a reticular network. Much of this perplexity, doubtless, depended on the fallacious appearances produced by injections; a method of investigation at one time too exclusively relied on, and which, when taken *per se*, abounds in sources of error and deception. The researches of Kiernan made an era in this department of anatomical investigation, and those of Henle led the way to some just and accurate views.

Amongst recent observers, Professor Retzius* is of opinion that the hepatic ducts form close perilobular or alveolar networks in the sheath of Glisson's capsule, from which are given off minute lobular networks interwoven with the portal-hepatic plexuses, and constituting with them the substance of the lobules. Dr. Leidy's account is similar; he describes the cells as lying within the tubes, which have walls of basement-membrane, and are more than double the diameter of the secreting cells. Dr. Handfield Jones differs from those above cited, as also from Guillot, Müller, Weber, and Kronenberg, who all, more or less, corroborate the original description of Kiernan. He admits that from injections he has obtained results confirmatory (*pro tanto*) of the views of these authors. Recent dissections, however, have only served to confirm the opinion he had previously advanced, (*vide supra.*) His investigations were repeated on fishes, reptiles, birds, and mammals; and with uniform results. As formerly, the examination of the hepatic structure in the fish was attended with little difficulty; he was also more successful in his investigation of the hepatic structure of reptiles, and he entertains no doubt that it is formed on a plan exactly similar to that in fish. In the frog, the parenchyma presents no appearance of any lobular division; it is often of a very dark colour, owing to the presence of a black, pigmentary matter; it consists of delicate, large, feebly-formed nucleated cells, which have no well-marked envelope, and almost appear like masses of granulo-amorphous matter; there are also numerous free nuclei and much diffluent granular, oily, and black pigmentary matter. In the frog, also, he was able to examine the ducts very successfully; the smallest appeared to be like those in the snake, small cylinders of granulous substance imbedding nuclei, and not usually containing vesicles; those of larger size are usually full of pellucid contents, in which the outlines of exceedingly delicate cells are visible, which exhibit nuclei only on the addition of nitric acid.

In human livers also he has succeeded in observing the ducts in a very satisfactory manner, and recommends the following as the best mode for obtaining a view of their course and relation. A thin section is made at one or two lines' depth from the surface of the organ; this section is treated with acetic acid, and moderately compressed; if the liver be tolerably free from oily matter, it will now become much more transparent, and on careful examination of the portal canals, spaces, and fissures, some ducts will be found, whose course can be traced. In this way, says the author, it is manifest the ducts are examined *in situ*, and if the observer has previously acquainted himself with their appearance, by dissecting them out as they lie in the Glissonian sheaths, he will have no difficulty in recognising and

* We quote from Dr. Handfield Jones; see Phil. Trans., 1853.

following them, though much difficulty may be experienced in determining their actual mode of origin or termination. The termination by distinctly closed extremities he is satisfied of having actually observed himself, as well as their slight amount of ramification, and the little intimate relation they appear to hold to the parenchyma in which they run. In many fissures not a trace of them can be seen, and, observes Dr. Jones, in those where they do exist, it is manifestly impossible that they can come into relation with any more than a very small portion of the parenchyma. How then, he asks, if all the cells secrete bile, could this make its way into the ducts? The ducts as they run in the fissures and canals are in the closest relation to the portal vein branches, and are surrounded by the terminal plexus of the hepatic artery. Kölliker expresses himself with considerable uncertainty as to the mode of termination of the hepatic ducts, and their connexion with the parenchymatous cells; his observations, however, as well as the illustration appended thereto,* go to support the opinion of Jones. Kölliker says (*loc. cit.*), "without going further into the question of the distribution of the ciliary ducts, I will only remark that in microscopic preparations made with care, the interlobular ducts of Kiernan may be observed with readiness in the interspaces between the hepatic islets, and it may be seen that they are formed on the usual type of all efferent ducts." The smallest measure $\frac{1}{100}'''$ in diameter, have a clear lumen (central cavity) of 0·0033''', and consist of the usual pavement epithelium in a single layer, the cells of which are readily distinguishable from the hepatic cells by their small size, (0·004''' to 0·005''',) their pale contents, and the smallness of their nucleus. An outer fibrous envelope was occasionally to be observed in some of the smaller ducts, under favourable circumstances, but in the larger canals of 0·04''' to 0.05''' it was always present, and their epithelium was more cylindrical. Kölliker has not observed any direct connexion of the finest canals with the parenchymatous cells, though he has frequently sought for it; this he regards as a blank to be much regretted in our knowledge of the minute anatomy of the organ; we trust that further research, and the prosecution of Dr. H. Jones's method in the hands of other inquirers will confirm the results obtained by him.

Development of the Liver.—The most recent researches make it more than probable that the opinions hitherto received with regard to the mode of development of the liver were in some important particulars quite erroneous. It was very generally believed that the rudimentary liver originated by an offset from the embryonic intestinal tube. Reichert, however, described the liver of the embryo frog as being formed by the anterior mass of the yolk, which is contained in the abdominal cavity, becoming isolated from the rest of that substance, and constituting an independent body. Handfield Jones also has observed similar appearances. The order of development observed by him in the frog was, that a portion of the common yolk-substance contained in the abdomen was set apart for the development of the liver, this occurrence taking place at the same time that the intestine is beginning to be formed; the first rudiment of the efferent apparatus is the gall-bladder, the ducts being subsequently formed, both being at first in a solid condition. In the chick he has observed the intestine to be formed by the constriction of the central transparent portion of the germinal membrane, which seems to be a homogeneous membranous expanse, not composed of cells, and covered only with oil-drops, while the rest of the germinal membrane, with which it is continuous, is covered by adherent yolk-cells, and overspread with ramifications of the omphalo-mesaraic vessels. When the constriction of the germinal membrane takes place, two tracts of oily matter appear, which pass the one backward, the other forward; the latter runs towards a quantity of blastema situated behind the heart, which is the rudiment of the liver; and which, up to the 9th or 10th day, has no connexion by ducts with the intestine. About the 11th day the parenchyma of the liver was found to consist of nuclei, cells, amorphous, and abundant oily matter; here and there bright yellow particles, which were doubtless biliary matter. The

* Microscopische Anatomie, fig. 250, vol. ii. part 2, p. 220. This figure is, however, stated to be half a plan, (*halb schematisch.*)

development of the pancreas was found to be very similar, and the author believes the same plan will be found to be followed in all glands; these organs, first consisting of blastematous matter, imbedding nuclei and myriads of granular globules, clustered over with oily molecules. This view is opposed to that of several distinguished physiologists. Bischoff mentions that the mammalian liver originates by an intestinal protrusion.

Functions of the Hepatic Cells.—The prevalent opinion amongst physiologists is, that the bile is actually secreted by the hepatic cells, and liberated by their dehiscence in the duct. Though having long received this doctrine unquestioned, Handfield Jones* is now disposed to doubt it, as he has failed to satisfy himself of the presence of bile in the cells as a normal and usual condition. According to his observations, the cells are generally pale, granulous, nucleated bodies, with a more or less distinct envelope, and with few or many, as the case may be, large oil-drops, amid the soft albuminous mass. Not unfrequently a distinct yellow fluid is seen infiltrating the granular contents, or several yellow, highly-refracting drops are observed in the same situation; they indicate that bile is present, but he states that they are seldom visible, except when the liver is in a state of so-called biliary congestion. The cells, when massed together and viewed by transmitted light, very commonly, he admits, perhaps always, produce a reddish-yellow tint; but this he considers to differ from that of bile, and to be probably owing to some diffused hæmatin. Having communicated with some observers on this subject, Mr. Bowman replied, that he believes "that the bile does exist in those hepatic cells which lie nearest the surface of the lobules; . . . that the cells lie in series extending from the hepatic venous to the portal venous surface; and that there is an onward march of the cells from the former to the latter." Mr. Simon states, "in perfectly normal conditions, amongst vertebrate animals, bile *cannot*, to the best of my belief, be demonstrated to exist in the hepatic cells, and I have for a long while strongly inclined to the belief that normally it never lies within them." Mr. Paget says, "I quite agree with you, that in the healthy livers of men the cells do not contain any coloured material, but I think I have seen their contents partially coloured yellow in other cases than those of congestion." Our author is of opinion, that in *perfectly healthy states* of the mammalian liver, bile does not exist in the cells, that it is not in any case necessarily formed there, but that this may be always effected by the ducts. The entire of this portion of the memoir requires to be studied in full: the following extract contains the most important conclusions of the author:—"It is clearly proved that *sugar* is made in the liver, that it is not found in the blood entering the vena porta, but that it exists in very large quantity in the blood passing out by the hepatic vein; the substance, also, of the liver, the parenchyma, contains abundance of sugar, as I have repeatedly observed. These facts show that one, and probably the chief, function of the hepatic cells is to elaborate sugar from the material intended to be employed in respiration; and that having done so, they allow this product to be returned into the circulating fluid, where, perhaps, it undergoes further changes before it terminates in carbonic acid. The parenchyma of the liver thus resembles closely a ductless gland, such as the supra-renal capsule, allowing its product to return to the blood from which it had been elaborated; it may be also inferred as not improbable, that as the elaboration of sugar is certainly one purpose fulfilled by it, so it is more likely that the bile-secreting function properly belongs to another apparatus associated with it—that of the excretory ducts." These investigations must, in the minds of most observers, create great doubt as to the theory of the action of the biliary cells hitherto received; but even should the justness of these views be fully confirmed by further researches, we have a new difficulty to encounter—viz., the source of the biliary secretion; for we are of opinion that so highly complex a fluid can scarcely be formed by the action of tubes and vessels.

Our limits now compel us to close this first part of our Annals. We purpose to resume our labours in October, with the subject of Pathological Micrology.

* Op. cit.

ANATOMY, PHYSIOLOGY, AND ORGANIC CHEMISTRY.*

On the Composition of Human Milk in Health and Disease. By MM. Vernois and A. Becquerel.

Looking at the contradictory reports of various analyses of milk, MM. Vernois and A. Becquerel have entered into an elaborate investigation of the entire subject. They have especially chosen 89 uniform and complete analyses to deduce certain deductions from. The following is their account of the composition of this fluid:

	In Health.	In Acute Disease.	In Chronic Disease.
Water	889·08	884·91	885·50
Solid parts	110·92	115·09	114·50
Sugar	43·64	33·10	43·37
Caseum and extractive	39·24	50·40	37·66
Butter	26·66	29·86	32·57
Salts (by incineration)	1·38	1·73	1·50
Density	1032·67	1031·20	1031·47

There are more solid parts in the milk of nurses aged from 15 to 20, than in those of from 35 to 40. The quantity of butter is notably increased during the colostral period. Gestation does not induce alteration in the composition of the milk at first, but at a later period it increases the proportion of solid parts. Menstruation diminishes the density, the weight of the water and of the sugar. It increases the weight of the solid portions, especially the caseum. Insufficient aliment renders the milk too watery, the effect falling especially on the butter and caseum. An excess of butter or caseum always accompanies an ill state of health of the nursling. There are certain women whose milk, independently of any special cause, always contains an excess of butter or caseum.

In both acute and chronic disease the water diminishes and the solid parts increase; but there the analogy between these two classes ceases. In acute disease, the sugar considerably diminishes, while the three other elements are increased, the caseum alone nearly repairing what is lost by the sugar. In chronic disease, the butter and salts are increased, the sugar remains stationary, and the caseum diminishes. Thus, in acute diseases, we have loss on a respiratory element, and excess in a nutritive element; and in the chronic, loss on the nutritive element, and increase of the respiratory element. In phthisis, without diarrhœa or emaciation, there is little sensible modification; but these being present, there is considerable diminution in the weight of butter. In syphilis the density is extraordinarily raised; the butter diminishes, and the salts disproportionately increase.—*Gazette Médicale*, 1853, No. 5.

On Elective Elimination by the Salivary and other Secretions. By M. Cl. Bernard.

In this paper M. Bernard calls attention to the fact, that some of the secretions rapidly eliminate certain substances, while other substances, equally soluble, are either eliminated much more slowly, or not at all. He relates the results of a series of experiments, in which iodide of potassium, iodide of iron, lactate of iron, cane and grape sugar, and yellow prussiate of potass, were injected into the veins, and the various secretions then tested for their presence.

Of these, *iodide of potassium* appeared, at latest, in from 30 to 40 seconds in the saliva, and was also rapidly observed in the tears and pancreatic juice. It required more than an hour to become detectible in the urine or the bile; and if

* Some of the abstracts of Physiological Papers are kept back, in order to be inserted in the Physiological Record which will appear in July.

injected in very small quantities, was not found in these at all. Introduced into the stomach, and especially fasting, it was found in the saliva in $1\frac{1}{2}$ minute. The *yellow prussiate of potass* was not discernible in the saliva, while in 7 minutes it was found in the urine and abundantly eliminated; the serum of the blood also exhibiting a notable quantity in an hour and a half. It was also found in the bile, while, although it was thus circulating in the blood, no traces of it could be found in the pancreatic juice. *Grape and cane sugar* never passed into the saliva or pancreatic fluid, while it was manifested in the bile and urine, though less rapidly than the prussiate. As various authors state they have detected sugar in the saliva in *diabetes*, the author examined that of several such patients under M. Rayer's care. In none was sugar detected, although the bronchial mucus and sputa evidently contained it. The *mammary gland*, which in the normal state contains the sugar of milk in its secretion, refused passage to grape or cane sugar, even when these substances existed in large quantities in the blood. A saturated solution of *lactate of iron*, thrown into the veins, never gives rise to iron in the saliva; but when the iron is injected as an *iodide*, it obtains admission into the saliva, both the iron and the iodine being then detectible.

This expulsion of certain salts by this or that secretion, is not the only peculiarity the history of elimination presents. Some substances are eliminated rapidly and completely, while others remain within the tissues for a more or less long period. It is well known that certain of these, as mercury, antimony, and arsenic, become localized in certain organs—e. g., the liver—and are then gradually eliminated: but it has not been noted that others, as the iodide of potassium, which are perfectly soluble, and remain soluble in the economy, wherein they circulate without enduring any accident, may remain for a certain time in the substance of the organs. Two or three weeks after iodide of potassium had been introduced into the stomach of several dogs, and long after its supposed entire elimination by the urine, in which it had ceased to appear, it was found in the saliva and gastric juice. If, however, purgatives were employed after administering the iodine, it ceased to be detectible in a few days in any of the secretions.—*Archives Générales*, N. S., vol. i. p. 5.

Comparative Observations on the Amount of Substance excreted in 24 hours with the Urine. By Dr. SCHERER.

SCHERER has employed in these observations the method lately used by Liebig for determining the amount of urea and other components of the urine.

Tables are given of the amount passed daily of water, solids, urea, extractives, uric acid, mucus, and inorganic salts, in two children and two adults. The chief result is, that the so-called extractive matters are in much greater abundance in the urine of adults; in these they constitute 30 per cent. of the solids of the urine; in children only 10·6 per cent. The inorganic salts are in greater amount in children (36 per cent. of the solid residue) than in adults (30 per cent. of the solids); the urea is more abundant in children (53 per cent. of the solids) than in adults (38 per cent.) For every pound weight of the body,* the children lost in 24 hours 13·096 grains (avoir.) of solids through the urine; the adults only 9·528 grains. The children lost rather more than ℥viss of water; the adults only ℥iv. The children lost for every pound weight of the body, 4·741 of inorganic salts, 6·996 of urea, and 1·667 of extractives; the adults lost 2·903 of inorganic salts, 3·691 of urea, and 4·741 of extractives. It follows from these facts, that for an equal weight of the body the children eliminated by the kidneys more water, more solids generally, more urea and salts, than adults; but less extractives (i. e., in the tables, extractives, uric acid and mucus,—in fact, the constituents of the urine, minus salts and urea). The nutritive changes in the bodies of children proceed in fact more rapidly than in adults, and the daily elimination of used materials is therefore greater.

* The pound referred to is apparently the *pfund bayer*, which corresponds to 1·29 avoir.

It appears, from the experiments of Scharling (Liebig's 'Annalen,' Bd. xlv.) that the amount of carbonic acid excreted from the lungs and skin in adults and children has a similar relation, being greater in the latter than in the former by 1·88 to 1 for every pound weight.

In 24 hours the urine of one of the two adults, who weighed 112 pounds (bayer.), contained 28·725 grains of sulphuric acid. It is not stated in which adult this result was found. One was rheumatic, but without pyrexia; the other healthy.

In the urine of a madman who refused food the same excess of extractives was found; and this case proves that the extractive matter does not, by further oxidation, pass into urea, carbonic acid, or ammonia. In this patient the inorganic matters sank to 15 per cent. of the solid residue. In this patient taking no food the solids of the urine were simply the used portions of the tissue; it becomes of interest to compare the urinary constituents with those in the adults. For every pound weight of the body the starving madman lost, in relation to the healthy adults, 1 part of solids to 2·4 parts, or less than one half; 1 part of urea to 2·3 parts; 1 part of salts to 5 parts; 1 part of extractive matter to 1·7 parts;—so that the extractive matters preserved their preponderance when no food was admitted, and are therefore a true excrementitial product.

Scherer appears to believe that the amount passed by the madman represented the eliminated tissue-products, and that passed by the healthy adult in excess of this represented the surplus food; but to this view obvious objections may be advanced.—*Würzburg Verhandl.*, Band iii. Heft 2, pp. 180.

PATHOLOGY AND PRACTICE OF MEDICINE.

On Epilepsy. By Dr. EVERTS and VAN LEEUWEN.

THE aim of the authors is to estimate the effects produced by sex, age, time of day, of year, phases of the moon, and atmospheric conditions, on the attacks of epilepsy. The first point only is discussed in this communication. The particulars are arranged in tables, and are drawn from a close observation of 17 epileptic men, and 13 epileptic women, in a lunatic asylum. The result is, that the attacks of the men were more frequent by day (from 6 A.M. to 6 P.M.); those of the women by night (from 6 P.M. to 6 A.M.) Of 13 women, only one had more attacks by day than by night: 9 had most attacks in the first part of the night, especially from 9 to 12 o'clock; by far the greater number of attacks were in the six hours from 9 to 3 o'clock. Among the 17 men, in 6 the number of attacks was very small; in 2 they were equal day and night; in 4 the night-attacks outnumbered those of the day; in 5 the day-attacks were most numerous. The increase in the number of the attacks from 9 P.M. to 3 A.M., observed among the women, was never noted among the men.—*Henle's Zeitschrift*, Band iii. Heft 1, p. 1.

On the Specific Gravity of Cerebral Substance. By Dr. BUCKNILL.

THESE important observations were published almost simultaneously with those recorded by Dr. Sankey in our last number. The specific gravity was estimated much in the same way, by means of a solution of sulphate of magnesia; but the grey and white substances were taken together. The specific gravity of healthy cerebral substance is estimated as 1046. The cerebellum has a higher specific gravity than the cerebrum. In eight cases of *general paralysis*, the average specific gravity of the cerebrum was much diminished, being only 1039·5; the highest, 1042; the lowest, 1038; the average specific gravity of the cerebellum was 1042; the highest, 1045; the lowest, 1037. The diminished specific gravity is said to be owing either to fat or to the effusion of serum, combined or not with atrophy of the nerve substance. A table of 30 chronic cases is given, in which, besides the specific gravity of the cerebrum and cerebellum, the weight of the brain and the capacity of the cranial cavity for water (at 60° Fah.) is stated. Some very interesting results are thus brought out. Thus in the "diseases accompanied by low specific gravity, the absolute weight of the brain, as compared with the capacity of the cranium, is diminished to a greater extent than can be accounted for by loss of specific weight; and, on the other hand, in epilepsy, apoplexy, and cerebritis, the weight of the brain, as compared with the capacity of the cranium, exceeds the standard of health." In convulsions and coma terminating life, the specific gravity appears, *cæteris paribus*, higher than when syncope and asthenia immediately precede death.

The author thinks that his observations already establish the existence of two forms of atrophy—viz., a positive shrinking, and a relative or interstitial atrophy, where inert molecules replace the active cerebral material.—*Lancet*, Dec. 25th, 1852.

Sarcinæ in the Lungs. By Dr. Zenker.

The sarcinæ of Goodsir are most frequently found in the matters vomited, or discharged by stool, or found in the stomach and intestines after death. Heller has noticed them in the urine in three cases; and although some doubt was expressed as to these observations, Hughes Bennett has lately also detected them in the urine. Virchow has found them in a gangrenous portion of lung. Dr. Zenker has found them also in the lung, although the case differs materially from that related by Virchow.

In a patient dying with encephaloid disease of the lungs and abdomen, in the lower lobes of both lungs, both in parts collapsed and in air-holding parts, were numerous small yellow patches, made up chiefly of sarcinæ, which were contained also in the serum pressed out of the lung. The yellow patches showed also fatty drops and cells, portions of plants, and yellow masses, which gave the colour to the part. The sarcinæ and similar vegetable débris were found also in the stomach. The author therefore believes that the sarcinæ were, in reality, derived from the stomach, although he does not conceive that they entered the lung after death. On the contrary, he believes that a portion of vomited fluid was drawn into the lung during the last days of life. No vomiting, however, was at this time noticed.

In Virchow's case, where sarcinæ were found in a single point, such an interpretation is hardly justifiable. Indeed, from the immense quantity of sarcinæ found at one point only in the lung, an actual growth of them in that situation can hardly be denied.

In Dr. Zenker's cases, the presence of sarcinæ gave rise to no dyspeptic symptoms, and the author coincides with Virchow and Frerichs as to the perfect innocuousness of the sarcinæ ventriculi.—*Henle's Zeitschrift*, Band iii. Heft 1, p. 117.

Phlebitis of the Vena Porta. By M. Leudet.

M. Leudet relates an interesting case of this affection. It occurred in a woman æt. 18, who was seized with shivering and prostration on the 2nd Oct., and died on the 26th, with symptoms much resembling those of purulent infection. The peritoneum was found much injected, and between the layers of the mesentery abundance of pus existed in communication with enlarged venous ramifications, which were distended with pus to the trunk of the vena porta, at the level of its entrance into the liver, at which point it was partially plugged by a firm, yellow coagulum. The intra-hepatic ramifications all contained pus, but no coagula. The vena porta and its ramifications were firm and resistent, gaping upon incision, the lining membrane, too, having lost its smoothness. The splenic, renal, and pelvic veins, and the venæ cavæ, were healthy. The liver was large, and an infinite number of small abscesses were diffused throughout its entire extent. The hepatic veins were healthy.

In reviewing the various cases on record, M. Leudet found many of them too insufficiently detailed to contribute to the general picture of the disease. Both the adhesive and suppurative forms have been described; but a mere increase of consistency of the parietes, altered colour of the lining membrane, and other appearances, have been noted, which would not now be received as characteristic of phlebitis. All the portions of the vena porta are not equally liable to inflammation, pus being most frequently found in the mesenteric branches, the trunk, and the intra-hepatic ramifications. Only one instance of the splenic being affected is on record. In some rare cases, the phlebitis has been confined to the intra-hepatic ramifications; but some of these may be examples of rupture and hepatic abscess into the venous canals. The venous parietes are thickened, and gape like arteries after incision. The colour of the vein is usually of a dullish-grey, the internal membrane being often thickened; but the injection mentioned by the older pathologists is not usually observed. Although soft, whitish coagula may be found adhering to a part of the circumference of the vessel, the caliber is never completely obstructed. The *liver* usually undergoes increase of size, and is often the seat of very numerous abscesses, surrounded by a blackish areola, bearing a great analogy to those termed metastatic, varying in size from a pea to an egg, and communicating directly with the inflamed veins. The spleen is rarely affected beyond enlargement. Waller has met with abscess of the kidney; and Henoch states that secondary abscesses of the lungs, joints, and brain, are not rare; but he gives no cases.

The affection is usually rather sudden in its access; but in some cases epigastric pain has long been complained of. This is, indeed, the most common initial symptom. Several authors regard meteorism as an early and important one; but dilatation of the subcutaneous abdominal veins is by no means of constant occurrence. Marked shivering usually attends the onset of the disease, or is, at all events, present during its course, coming on quite irregularly several times in the day, and that when there is no enlargement of the spleen or abscess of the liver. The profuse sweats mentioned by some are often absent. More or less icterus usually comes in the course of the affection; other symptoms, as thirst and vomiting, are more uncertain, as are others which come on later, as coma, delirium, and prostration. This last, however, often appears at the very first, and always at some period. Amongst the leading symptoms must, then, be placed the epigastric pain, meteorism, irregular shivering, and the enlargement of liver and spleen. The *duration* of the disease is very variable, cases being recorded which have continued from 8 to 60 days. In the majority of cases the patients were aged between 24 and 40; the prior condition of health presenting nothing of special note. The causes are quite obscure, the disease in most cases seeming to arise spontaneously. —*Arch. Gén.* N.S. 1, pp. 145—159.

Report on Thirty-eight Cases of Pneumonia. By Dr. RUBACH.

AMONG 230 patients admitted into an hospital for railway workmen, there were 38 cases of pneumonia. Of these, 5 died from pneumonia, or 13·37 per cent.; and one from subsequent arachnitis. The pneumonia was on the right side in 18, on the left in 20; in 3, the upper lobe was attacked; in 4, it spread from above downwards; in 8, from below upwards. In 14 patients, on admission, there was bronchial respiration; in 17, bronchial respiration and crepitation; in 4, "inflammatory crepitation," which did not pass on to bronchial respiration, but passed into subcrepitant and mucous rhonchi. In 6 cases the pneumonia was pure; in 32 there was conjoint pleurisy, with pleural friction; in 7 cases bronchial catarrh had, for a long time, preceded the pneumonia; in 10 cases bronchial catarrh was left behind. In 12 cases, after the commencement of convalescence, there was œdema of the feet; in 3 cases œdema came on at the height of the disease. 16 patients were delirious; 4 were icteric, and in three of these cases the pneumonia was on the *left* side; in one case Bright's disease came on suddenly, at the commencement of resolution.

The commencement of the disease was, in the majority of cases, by shivering, and the patients could name the exact hour. In 36 cases the expectoration was characteristic, and in 7 there were fibrinous coagula.

Percussion seemed to give better evidence than auscultation; dulness being perceptible when the auscultatory signs were very obscure. In some cases pleuritic effusion occurred, as friction-sound was first heard, then lost, and then again heard. The exudation in the lungs and pleura was often absorbed with extraordinary rapidity, but the latter appeared to disappear soonest. The "redux crepitans rhonchus" was often not heard, or the coarser moist rhonchi concealed it.

The average duration in hospital was $16\frac{2}{3}$ days.

In 18 cases the temperature, pulse, and respirations were accurately noted twice a-day: in the morning (remission time) and in the evening (exacerbation time). Tables are given of all these cases. In all the cases the change at commencing resolution, from the fever heat to the normal temperature, was sudden; it occurred in 9 on the 5th day; in 5 on the 7th; in 1 on the 9th; in 1 on the 11th; and in 1 on the 13th day,—so that Traube's statement* about critical days is so far confirmed. In one case, however, the temperature lowered on the 8th day. Sweat occurred on the same day as the decrease of temperature in 14 cases; the urine was scanty, dark-coloured, and acid, during the disease; was often very copious and pale during resolution. In one case, during resolution, albumen was present; in another case, hydrops Brightii came on. The pulse fell with the temperature, as already stated by Traube. The number of respirations stood in no relation to the pulse.

The temperature seemed to stand in closer connexion to the pyrexia than to the local exudation: thus the exudation first poured may commence to retrograde, while the temperature is still high; the inflammation is then going on in some other part of the lung. When successive parts of the lung were thus attacked, the retrogression of the exudation took place in the same order. The statement of Traube, that the inflammation ceases to spread when the decrease of temperature commences on a critical day, appears so far confirmed by these cases.

In the treatment, general bleeding was not used; cupping was employed when the pain in the side was very severe. Warm fomentations after the cupping, or without it, gave great relief; blisters were avoided, as they distressed the patient, and their good effects did not ensue till hours after their application. In 4 cases tartar emetic was used; in the other cases, nitrate of soda was employed, and afterwards senega and muriate of ammonia. — *Würzburg Verhandl.*, Band iii. Heft 2, p. 163.

The Syphilitic Affection of the Liver. By Dr. Böhmer.

Gubler stated, four years ago, that some children, born with syphilis, were affected with a "fibro-plastic disorganization" of the liver; and Depaul, the discoverer of the alleged syphilitic lung-affection, affirms that he has observed something similar. Dittrich has, more lately, also declared that the liver is infected in utero in some syphilitic fœtuses. The author having met with an instance of this kind, which was declared by Dittrich himself to be an instance of the congenital syphilitic affection of the liver, narrates the case, and appends some remarks in opposition to this presumed syphilitic origin. He also states, that in order to investigate this subject, he examined the bodies of all the syphilitic children who died at the Viennese Foundling Hospital. He was, however, unable to find this hepatic affection in any other instance. In the case referred to, there was exudation on the surface of the liver, uniting it to the diaphragm, and forming a pseudo-mem-

* Vide p. 38 *et seq.*

brane of areolar tissue, which passed also into the liver, but did not especially implicate the portal canals.

The author then passes on to narrate shortly 14 cases in adults, communicated to him by Dr. Sigmund, the director of the syphilitic hospital at Vienna, in which there was liver affection. Unfortunately, it is not stated to what number of deaths these 14 cases corresponded, but it is said that this was considerable. In these 14 syphilitic adults the liver was in the states described as "granular liver," "cirrhosis," "hepatitis adhæsiva," "pylephlebitis," and with fibro-cellular coatings of the surface. In all these cases, Dr. Böhmer considers that there was nothing *specific*—i. e., nothing that does not present itself daily in the livers of men who are not syphilitic. He enters into a long polemic on this point, and discusses, in connexion with it, the various pathological conditions of the liver comprised under the terms cirrhosis, pylephlebitis, and adhesive hepatitis.—*Henle's Zeitschrift*, Band iii. Heft 1, p. 88.

SURGERY.

Subconjunctival Ophthalmia. By F. A. von Ammon.

To the already numerous subdivisions of Ophthalmia, M. Von Ammon, in this paper, adds another. It is an inflammation of the tissue situated between the conjunctiva and sclerotica—an interstitial inflammation, which may extend to either of these two membranes. It is usually of partial extent only. At the inner or outer side of the globe, in the direction of the internal or external rectus, a slight rounded or ovoid tumefaction, perhaps one-third of an inch in extent, is observed; which, of slow formation, has usually only excited the patient's attention from its becoming the subject of some accidental irritation. The redness is usually dull, and the conjunctiva is often so raised by serosity as to be mistaken for an excrescence. The pain is not usually severe, but is increased in strong light, or by exciting diet, and pressure through the eyelid causes tenderness. In some very rare cases the swelling is noticed in the direction of the superior or inferior rectus. Occasionally, too, the whole subconjunctival surface is affected, giving the appearance of ecchymosis, but with more transparency. Sometimes both eyes suffer simultaneously. The newly-formed subconjunctival vessels are easily distinguished by a glass from those of the conjunctiva. When the portion of the subconjunctival membrane in the vicinity of the cornea is the part only affected, a very characteristic falciform appearance is produced by the exudation and hypertrophy that take place—small ulcers, also, after a long period, often being produced. Eventually, the conjunctiva, at its junction with the cornea, becomes thinned and wasted by the diminution of its vessels.

The affection is very tedious in its progress, lasting for months, and has a great tendency to relapse. A sero-adipose exudation takes place, which rarely goes on to suppuration. The secretion is usually readily resorbed, but is sometimes discharged by ulceration. Thinning of the conjunctiva and sclerotica usually result, the latter membrane taking on a more blue appearance than in the normal state. This atrophy of these membranes seems to be the special termination of this interstitial ophthalmia. The eye long retains a great sensibility—a kind of fear of motion and a sensation of cold are experienced; and the sight seems uncertain. The affection is especially met with in persons in whom the venous system predominates; those who have abused alcoholic fluids; in persons predisposed to adiposis, with rheumatic complications; in the scrofulous; during pregnancy; and during anomalous menstruation. Cold has almost always been the occasional cause.

When neglected, as is usually the case, it acquires great tenacity, rarely terminating in suppuration, but by slow retrocession, accompanied by partial atrophy of the adjoining membranes. For its treatment, purgatives and resolvents are especially indicated, owing to the stasis of the venous abdominal circulation which is usually present; neutral salts and senna, and occasionally senega, being very useful,—drastics being avoided. In other cases, two or three drachms of sulphate of potass in almond emulsion is very useful. Collyria and stimulating applications, however weak, are mischievous, especially at an early stage. Advantage arises from suspending before the eye, at night, a little bag, containing two or three drachms of belladonna powder, and an ounce of linseed meal. Derivatives are of no use, but pediluvia act as calmants. The peculiarity of the patient's constitution has always to be borne in mind. After the cure, the eye must be used very cautiously; and as long as it remains feeble and dry, the dropping vinum opii into it is a good practice. When disorganization of the membranes has taken place, a weak solution of nitrate of silver is better.—*Annales d'Oculistique*, xxviii. pp. 67—75.

On the Operation for Phimosis. By M. MAISONNEUVE.

M. MAISONNEUVE observes, that simple as is the operation for phimosis, there are few others so defective in their results, that by the longitudinal incision leaving inconvenient angular flaps, and the modification of the Jewish circumcision leaving the entire glans exposed. On the examination of persons suffering from congenital phimosis, it will be found that the constricted portion of the prepuce forms a circle of only moderate breadth, beyond which the skin retains all its laxity. If this circle be removed, the glans can expand itself during erection, while, as the prepuce has undergone but a slight loss of substance, it still covers the glans when quiescent. Drawing the prepuce forwards, M. Maisonneuve removes a circular slice with the knife, which includes all the constricted portion, and it at once retracts, leaving the glans exposed, and only covered with the preputial mucous membrane. In the second stage, the mucous membrane is divided along the dorsum to the extent of a centimetre, and the angles of the incision snipped off. Lastly, the edges of the prepuce are united with those of the mucous membrane by means of *serres-fines*. A supple and mobile prepuce covers the glans, and readily slides back during erection.—*Gaz. des Hôp.*, 1853, No. 13.

MIDWIFERY, &c.

The Alteration by the Forceps of the Position of the Child's Head.
By Professor SCANZONI.

IN his work on Midwifery, and in a former number of the 'Verhandlungen der Phys. Med. Gesellschaft,' in Würzburg, Professor Scanzoni recommended that the forceps should be used, not merely to deliver, but to alter the position of the head. This doctrine being lately attacked by Moser, as "erroneous and dangerous in the highest degree," Scanzoni reiterates his assertion in the present paper. According to his views, the turning of the head round the perpendicular axis is indicated in those cases in which the delivery is retarded to such an extent as to endanger the life of the mother or the child, or of both, in consequence of the normal rotation of the child's head taking place too slowly or not at all. It is further indicated in those cases in which another circumstance requires the extraction of the child by the forceps, if it is to be expected that the extraction may be performed in a shorter time and with less danger after previously having turned the head, which may always be supposed, if, after the head has far descended into the cavity of the pelvis, the sagittal suture or the longitudinal line of the face stands in the oblique diameter, or if the forehead—the calvaria or the face presenting—is directed towards the anterior wall of the pelvis. As indispensable conditions for the admissibility of the proposed operation, Scanzoni considers, that the exact position of the head has been previously ascertained, and that the head stands in the lower half of the cavity of the pelvis. In performing the operation he thinks it necessary that both blades, with their points directed towards the anterior wall of the pelvis, should be, as much as possible, applied to the lateral surface of the head; and that, by turning the instrument round its longitudinal axis, in presentations of the calvaria, the occiput,—in presentations of the face, the chin—is turned towards the anterior wall of the pelvis. If the sagittal suture or the longitudinal line of the face is parallel with the transverse diameter of the pelvis,—or if, in calvaria-presentations, the occiput—in face-presentations, the chin—stands at the anterior end of one of the oblique diameters,—a single application and turning of the instrument is sufficient to move the occiput or the chin to the anterior wall of the pelvis. But if the forehead—either in calvaria or in face-presentations—be in front, the forceps ought to be applied twice: at first, the points directed towards the forehead; later, with the points towards the occiput or chin (according to calvaria or face-presentation being concerned).

Scanzoni does, however, not claim the merit of having first proposed this operation, as it had been performed already by Smellie, Baudelocque, Capuron, Ritgen, Osiander, Naegele, Kiwisch, and others. — *Würzburg Verhandl.*, Band iii. Heft 2, p. 237.

Apparatus for Premature Infants.

Dr. Channing, in an account he is publishing of his visits to various European hospitals, describes an apparatus he saw in use at the Maternité of St. Petersburgh, the use of which might be advantageously extended. "It was a species of cradle, without rockers, into which infants are placed when prematurely born, or when imperfectly developed, and for whom a steadier and higher temperature is required than that of the atmosphere. It is made of brass, and is everywhere double; a space being left between the two plates comprising it, with openings, into which warm water may be poured, and others for drawing it off when cool. A soft bed, and a properly arranged canopy, when needed, complete the apparatus."—*Boston Medical Journal*, xlvii. p. 307.

On Extraction of the Child after Turning. By Dr. Pröbsting.

Dr. Pröbsting enters his protest against the modern recommendation of leaving the completion of the delivery to nature, after version has been accomplished for malposition—declaring that the old practice of finishing the delivery forthwith saved more children, and was more favourable to the mother's recovery. We scarcely know to what cases he alludes, when he says that in cases so left, delivery is not, as a general rule, accomplished for several hours after the turning has been performed. Certain it is, that, severely as he criticises English obstetrical practice, which he maintains is much behind the German, no practitioner in this country would think of leaving these cases so long, but would be guided as to the period of interference by the condition of the mother, and the amount of vigour in the funicular pulsation. Dr. Pröbsting gives us a tabular view of 30 cases of uncomplicated turning he has met with. In 6 of these the expulsion of the children was left to nature, and in all they were still-born; while of the 24 others, for whom extraction was resorted to immediately, 2 were born putrid, 9 still-born, and 13 living.—*Neue Zeitschrift für Geburtskunde*, Band xxxiii. p. 20.

Effects of Hospitals on Mortality of Children. By Dr. Bailly.

Dr. Bailly, in relation to M. Hervieux's paper upon the effect of prolonged horizontal posture in the production of the mortality of Foundling Hospitals, refers to the well-known essay, published in the 'Archives' by himself and Dr. Legendre, while *internes*, ten years since, at the Children's Hospital, in which it is shown that the pneumonias so fatal to young children in that establishment are due to the stasis of blood, consequent upon prolonged decubitus. Since that period he has been engaged in provincial practice, and all that he has observed confirms by contrast the statements then made. In vain has he sought for those numerous examples of bronchial catarrh generating fatal pneumonia, or of children dying with their skins indurated and their lungs gorged with blood. The question is, indeed, whether such diseases be not entirely generated in hospitals, and whether much that has been written in valuable works on hospital practice, is of any use whatever in private practice. In the most wretched cabins in the country, where the exquisite cleanliness and able medical attendance of the capital are wholly wanting, no such mortality prevails, because the children still breathe fresh air, and are frequently raised from the recumbent posture. Just in the same way, the success that attends surgical operations is immensely greater in the country than in the Paris hospitals. Frequent as are cases of traumatic erysipelas, purulent resorption, and gangrene in the latter, Dr. Bailly has never met with a case of either in the country—the wounds readily cicatrizing, without giving rise to prolonged suppuration and troublesome complications. However advantageous large children's hospitals may be on the score of economy, and for teaching disease, they are very destructive to infantile life, and would be advantageously replaced by small establishments situated in the environs.—*L'Union Médicale,* 1852, No. 149.

MATERIA MEDICA.

Hyposulphite of Soda and Silver. By M. Delioux.

M. Delioux (Bull. de Thérap., xliii. pp. 289, 401) states that two years' trial of this substance at the Marine Hospital at Rochefort, leads him to the conclusion, that in some cases it may advantageously be substituted for the nitrate, as a much milder preparation. It is formed by pouring a solution of hyposulphite of soda over oxide of silver, recently precipitated by potass, until the oxide is entirely dissolved. Evaporation furnishes minute crystals of the hyposulphite of soda and silver, which are dried in a mild heat, sheltered from light. It is insoluble in alcohol, and very soluble in water, the solution being more slowly decomposed by light than that of the nitrate. Kept away from the light, its transparency may be indefinitely preserved. When quite pure, it does not discolour the epidermis or linen. It is less astringent and irritating than the nitrate, and may in several cases thus be advantageously substituted for it. It is especially in urethral discharges it has been found useful, solutions of 1 or two parts to 30 of water exciting less irritation than solutions of the nitrate of half the strength. He, however, usually employs it only in the proportion of $\frac{1}{2}$ to 1 part to the 100 of water. He has not used it internally.

On the Solution of Calculi. By H. Bence Jones, M.D., F.R.S.

In 1848, the author first attempted to dissolve urinary calculi by placing them between the electrodes of a galvanic battery, and immersing them in a solution of nitre, the object being to decompose the nitrate of potash by the galvanic action, and by means of the active agents thus liberated to act on the calculus chemically. An effect was produced on the uric acid in contact with the negative pole, but no very decided results were obtained. In the summer of 1852, under more favourable circumstances, the investigation was resumed. A very considerable portion of a piece of uric acid was dissolved in a strong solution of nitre, when the nitre was decomposed on the surface of the calculus, by means of a battery, consisting of ten pair of Grove's plates. Larger calculi, more dilute solutions of nitre kept at the temperature of the body, and batteries of different powers, were tried. The results with uric-acid calculi may be thus arranged.

Experiment.	Duration.	Strength & Temp. of Solution.		Power of Battery.		Result.	
1st.	4 hours.	Saturated.	212°	10 pr. of Grove's.		½ gr.	dissolved.
2nd............	6h. 5m.	¼ Nitre, ¾ Water	109°	5	„	11 gr.	„
3rd............	6h. 10m.	„	101°	10	„	14 gr.	„
4th............	6h. 20m.	„	100°	10	„	16 gr.	„
5th............	6h. 45m.	„	106°	10	„	12 gr.	„
6th............	3h. 17m.	„	98°	20	„	27¾ gr.	„
7th............	6h. 30m.	⅛ Nitre, ⅞ Water	98°	20	„	22 gr.	„

The loss was determined by drying the calculi, before and after the experiment, in a water-bath. The size of the electrodes used was much smaller than the calculi employed. Considerably larger electrodes might be introduced into the bladder.

Calculi, consisting of oxalate of lime, were also tried in the same and in different solutions.

Experiment.	Duration.	Strength & Temp. of Solution.		Power of Battery.		Result.	
1st............	7 hours.	¼ Nitre, ¾ Water	90°	5 pr. of Grove's.		½ gr.	dissolved.
2nd	7 hours.	„	104°	10	„	2 gr.	„
3rd	6h. 15m.	In Sulphate of Soda	101°	10	„	2 gr.	„
4th............	5h. 45m.	In common Salt	102°	10	„	1 gr.	„
5th............	6h. 10m.	¼ Nitre, ¾ Water	108°	20	„	6 gr.	„
6th............	3h. 19m.	¼ Nitre, ¾ Water, with Phosphate of Soda	110°	20	„	1 gr.	„
7th............	3h. 15m.	Same with Bichromate of Potash	111°	20	„	2 gr.	„
8th............	3h. 17m.	½ Nitre, ½ Water	110°	20	„	2½ gr.	„
9th............	2h. 50m.	¼ Nitre, ¾ Water]	92°	20	„	2½ gr.	„
10th............	3h.	¼ Nitre, ¾ Water	100°	40	„	5 gr.	„

Hence, oxalate-of-lime calculi can be only very slowly acted on in a solution of nitrate of potash, which, when decomposed by galvanic agency, acts energetically on uric-acid calculi. The action is at least four times as slow as on uric-acid calculi. In other experiments, in which the calculi consisted of oxalates mixed with urates, and oxalates with phosphates, it was found that they dissolved more rapidly than when they consisted of oxalate of lime alone.

Phosphatic calculi were also treated in the same way.

Experiment.	Duration.	Strength & Temp. of Solution.		Power of Battery.		Result.	
1st. A piece of hard phosphate of lime ...	7h. 15m.	¼ Nitre, ¾ Water	102°	10 pr. of Grove's.		15 gr.	dissolved.
2nd. Fusible...	1h. 13m.	„	96°	20	„	31 gr.	„
3rd. Fusible...	3h. 23m.	⅛ Nitre, ⅞ Water	98°	20	„	67 gr.	„
4th. Phosphate of lime ...	6h. 30m.	„	98°	10	„	20 gr.	„

In these calculi the action was almost confined to the positive electrode. The effect on carbonate of lime, in the form of marble, was also an object of experiment.

Experiment.	Duration.	Strength & Temp. of Solution.		Power of Battery.		Result.	
1st	5h. 30m.	¼ Nitre, ¾ Water	104°	10 pr. of Grove's.		27½ gr.	dissolved.
2nd	6h. 30m.	In Sulphate of Soda	101°	10	„	4½ gr.	„

Thus, then, by effecting the decomposition of dilute solutions of nitrate of potash on the surface of calculi, from two to nine grains of uric-acid calculus can be dissolved in an hour, whilst in the same time, from two to twenty-five grains of phosphatic calculi can be dissolved. With oxalate-of-lime calculi, with more rapid decomposition of the solution, from a quarter of a grain to a grain an hour is the utmost that can be dissolved.

The author finds that M. Dumas and others have attempted to act on calculi by means of electricity; but he considers that the principle of his experiments differs from that of others, inasmuch as he uses the galvanic action only for the purpose of liberating chemical agents on the surface of the calculi. Instead of applying mechanical force or electrical force to destroy the stone, he puts on chemical force, which he directs and controls by means of galvanic action. It is, then, certain, that by this means acid and alkaline calculi can be dissolved with considerable rapidity out of the bladder. It remains now to overcome the mechanical difficulties, in making an instrument—1st, which will conduct the electricity to the surface of the stone in the bladder; 2ndly, which will set up no chemical action on mucous membrane of the bladder; and 3rdly, which will admit of the injection, or of a current of dilute solution of nitrate of potash, to pass in and out of the bladder.—*Royal Society.*

Tartarized Sulphate of Quinine. By Dr. Galamini.

Dr. Galamini (Bull. delle Sc. Med., xxi. p. 99) speaks in strong terms of praise of the febrifuge power of sulphate of quinine when combined with equal parts of tartaric acid — a much smaller quantity of the alkaloid so administered sufficing. During an epidemic of ague, it was given in 43 cases, in 31 of which it speedily effected a cure. In 21 of these, half a scruple sufficed, while in 10 others, it required more continued use. In most of the cases, there was hyperæmia of the brain or bronchial membrane, enlarged spleen, or gastro-biliary derangement, requiring the preliminary employment of bleeding or purgatives. Of the 12 other cases, 5 had relapses; in 3 no effect was produced; and in 4, the above-named irritative symptoms returned.

THERAPEUTICAL RECORD.

Anchylosis.—In partial anchylosis of the knee-joint, Mr. Hancock (Lancet, Jan. 29) has met with great success from forcible extension, while the patient is under the influence of chloroform. Only in one of six cases did inflammatory symptoms follow.

Ascites.—Teissier (Gazette des Hôp., 1852, p. 120) relates 3 cases of ascites treated by iodine injections, composed of 60 to 70 grains of iodide of potassium, 7 to 10 drachms of tincture of iodine, in 6 ounces of water. The iodine was rapidly absorbed and excreted through the kidneys.

M. Ore (Bull. de Thér., Sept. 1852) has used iodine injections in 5 cases: two were cured; three died, but not from the injection. The first effects were sinking, pallor of face, lowering of pulse, and severe pain; in ten minutes these symptoms went off; subsequently, there was heat, pain, fever, some meteorism, colic, and sleeplessness. No important peritonitis, however, ever appeared to come on. The strength used was one part of the tincture to three of a vehicle. The remedy is contra-indicated in ascites, dependent upon liver or heart affection, or when there is kidney-disease. When the ascites is from peritonitis, or follows ague, the injection is to be used.

Bubos.—M. Bonnafont (L'Union Médicale, 1852, No. 155) describes a plan of treating suppurating bubos, by which he says much time is saved, and unsightly scars are avoided. When the bubo is quite ripe, it is pierced in its most tense part, and in the direction of the ileo-pubic axis, by a seton needle, carrying four threads. The two apertures of entrance and exit of the needle must be formed beyond the perimeter of the phlegmasia of the skin. The pus is gently but effectually evacuated by pressure, and then compression is applied by means of pieces of agaric or a dossil, occupying all the space between the apertures without obstructing them. To these scraped lint is applied, and the whole is supported by a spica bandage. In the evening, somewhat forcible compression is again to be applied, so as to empty the abscess; and the pad is to be again applied. This is to be repeated twice daily until complete cure. Care is to be taken not to draw out the threads unless free discharge is secured, and their ends should be tied together. The medium time a bubo so treated requires for cure is twenty days, although some are cured in ten; and the points made by the seton leave hardly any marks. When the bubo has been opened before the entire gland had suppurated, the case is sometimes more tedious, a clear or sanguinolent fluid continuing to ooze. In such cases, vinous injections, combined with compression, have been found useful.

Cancer.—M. Devay (Gaz. Méd., 1852, No. 52), of the Hôtel Dieu Lyon, has long been engaged in investigating the therapeutical properties of conium in cancer, being of opinion that Storck's experiments should be resumed with the aid of the improved chemical knowledge of the present period. He finds the best preparations to be an extract and balsam, containing 1 per cent. of conicine, made from the seeds of the plant, gathered when at maturity, of full weight, and of an ash-grey colour. As the result of his researches he states,—1. That an ointment, applied externally, in chronic enlargements of scrofulous glands, possesses a resolvent power greater than that of any other substance. 2. In engorgements of the uterus, or inflammatory hypertrophy of the organ—so frequently complicating its prolapsus or deviation—this medicine, employed internally and externally, is of great service. 3. In cancerous affections it exerts remarkable calming effects, and in some cases even cures seem to have resulted from its employment, especially in the atrophied form of scirrhus. Its use is less satisfactory in soft and rapidly increasing tumours, but the progress of some of these has seemed to be retarded. In other cases, it has diminished the size of secondary tumours, rendering the primary ones more amenable to surgical operation. As a means of assuaging suffering, whether used topically or taken internally, it is invariably preferred by the patients to opium and all other narcotics.

M. Manec, surgeon to the Salpêtrière, has just obtained a recompence of 2000 francs from the Académie des Sciences (Gaz. Méd., 1853, No. 10), for the perseverance he has shown in investigating the action of Frère Cômes Arsenical Paste in more than 150 cases of cancer, in some of which he obtained unhoped-for results. His experience leads him to these conclusions:—1. That the arsenical paste penetrates the cancerous tissue by a sort of special action which is limited to it. This action is not simply escharotic, for beneath the superficial, blackish layer, which the caustic has immediately disorganized, the subjacent morbid tissue seems struck with death, though it may retain its proper texture, and almost its ordinary appearance. Later, the cancerous mass is separated by the eliminatory inflammation which is set up around its limits. The same paste, which extends its action more than six centimetres deep in a cancer of close texture, when applied to superficial gnawing ulcers, usually only destroys the morbid texture, however superficial this may be, and respects the sound parts. 2. The absorption of arsenic is proportionate to the extent of surface to which it is applied; and as long as this does not exceed a two-franc piece in size, there is no danger from this source. A large surface should only be attacked by successive applications. 3. Arsenic which is absorbed is chiefly eliminated by the kidneys, during a space of time of not less than five, and not more than eight days, as amply demonstrated by Pelouze. Thus, if we allow nine or ten days to intervene between successive applications, all danger from absorption may be avoided.

M. Gozzi (Bull. delle Sc. Med., xx. p. 231) strongly recommends the following caustic for the destruction of cancerous growths:—Corrosive subl., ℈j; caustic potass, ʒss; arsenic and cerussa, āā, gr. vj.—to be made into a paste with starch and white of egg. While using this or other caustics, emollient poultices, ointments, &c., should be avoided, as diminishing their effects, unless the irritation produced by their application has been excessive. M. Gozzi objects to the usual plan of destroying the tumour, layer by layer, from the apex to the base, the latter becoming very indurated after these repeated applications, and offers great obstacles to the approximation of surrounding granulations and their cicatrization. He prefers applying the caustic laterally, in the direction where the tumour seems most inclined to separate.

M. E. Cazenave (L'Union Médicale, ix.) speaks very highly of a caustic formed by pouring hydrated sulphuric acid on powdered saffron.

Convulsions, Puerperal.—Mr. Bolton (Lancet, Jan. 29) relates a case in which severe puerperal convulsions, coming on immediately before labour, and unchecked by depletion, were completely arrested by the inhalation of chloroform.

Dr. Holst (Neue Zt. für Geburtsk., vol. xxxii. p. 85), in a bad case of puerperal convulsions, attended with great rigidity of the os uteri, threw up warm water against the os uteri for six minutes, as in Kiwisch's method for bringing on labour. The os speedily dilated, and labour was completed by the forceps.

Croup.—Mr. Smith (Med. Times & Gaz., March 5) relates 4 cases of croup in which *tracheotomy* was performed. All the patients were in extremity, and in all the result was fatal.

M. Guersant (Bull. de Thérap., xlii. 293) details one case, and refers to another, in which tracheotomy was performed twice in the same child. M. Guersant has now operated in croup 150 times. The earlier cases were less successful than the later: 13 of the last 40 private cases, and 13 of the last 31 hospital cases, recovered, or at the rate of 36·62 per cent. In 3 cases there was faulty deglutition after the operation, and food escaped through the tracheal opening. In such a case the child must be fed through the œsophageal tube passed in through the nares.

Diabetes Mellitus.—Dr. Hanekroth (Schmidt's Jahrb., 1853, p. 173) recommends a mixture of sulphate of iron ʒjss., tinct. cinch. c., aq. menth., āā. ʒvj.; 20 to 30 drops every 2 hours. In two cases there was perfect recovery.

Mr. Sampson (Lancet, Jan. 8) states that the permanganate of potash (grs. ij. —v. in solution thrice daily) has a marked effect in reducing the quantity of urine in cases of obstinate dyspepsia, and in diabetes mellitus. In a case of the latter disease, the quantity of urine fell from 10—12 pints to its normal amount, but still contained sugar!

Diarrhœa.—Mr. North (Med. Times & Gaz., Feb. 12), in noticing the employment of *dilute sulphuric acid* (in ʒss. doses every 2 or 3 hours), states that it is in the serous forms, especially when attended with cramps, that it is most useful.

Dyspepsia.—Dr. Bennett (Ed. Monthly Journ., Feb. 1853), in a lecture on Dyspepsia, after insisting on the necessity of seeing that there is no excess in eating and drinking, that the food is properly masticated, and that proper rest is taken after food, remarks that the sense of load or weight is best relieved by acids, especially the hydrochloric. Acid eructations and cardialgia are best relieved by alkalies and bitter tonics. In cases in which fatty matters do not appear to be digested, liq. potassæ is recommended. When the flow of bile appears deficient, mild mercurials and rhubarb is the best treatment.

Epilepsy.—Dr. Marshall Hall, in a series of articles (Lancet, Jan. and Feb.), brings forward evidence to show the advantage of *tracheotomy* in preventing the severest attacks of epilepsy, by rendering impossible the *laryngismus*, on the occurrence of which depends the violence of the fits.

Dr. Tyler Smith (Lancet, March 5) has performed tracheotomy in a case of "uterine epilepsy." The operation appeared useful, but the ultimate issue of the case is not recorded.

Dr. Abbot (Amer. Journ. of Med. Sc., Jan. 1853) gave the *cotyledon umbilicus* (grs. v. night and morning) to a man aged 50, who had been subject to epilepsy every two or three weeks for five or six years. The fits entirely ceased, but the patient began to totter in gait, and had attacks of partial loss of consciousness. The cotyledon was discontinued, and the fits returned.

M. Moreau, physician to the Bicêtre, has been induced, by the recommendation of M. Herpin, to try the oxide of zinc. His results do not support M. Herpin's statements.

Erysipelas.—Dr. Creighton (Edin. Month. Jour., Dec. 1852) speaks highly of the *tinct. ferri sesquichlorid.*, in doses of ♏x. to ♏xv. every 2 or 4 hours. It appears, however, that ant. pot. tart. (gr. $\frac{1}{4}$) was also given, so that the inference appears doubtful.

Fever, Intermittent.—Dr. Pepper (Amer. Jour. of Med. Sc., Jan. 1853) has tried *bebeerine* and *cinchonia*, in ague. The former succeeded in 2 cases, failed in 2; the latter seemed more useful.

Fever, Continued.—Dr. Barclay (Med. Times & Gaz., Jan. 8) records the effect of large doses (10 grains every 4 hours) of *sulphate of quinine*, in 18 cases of continued fever (chiefly typhoid?) In 5, there was marked depression; in 2 the pulse became slow, without depression; in 11 no physiological effect was produced. The average duration of the fever was not shortened by the treatment, and cinchonism in fever is considered not to be "more speedy, safe, and effectual" than other methods.

Dr. Dundas (Med. Times & Gaz., Jan. 29) denies the accuracy of Dr. Barclay's inferences, states that "no prostration of the vital powers" is produced by quinine, and relates a case of typhoid (?) fever in which the remedy was useful.

Dr. Douglas (Amer. Jour. of Med. Sc., Jan. 1853, p. 282) states that he has never seen the least good follow the use of large doses of quinine in *typhoid fever*.

Fistula in Ano.—M. Alquié (Gaz. des Hôpitaux, No. 48) states that he has found it a very beneficial practice to cauterize the lips of the wound by means of nitrate of silver, after the incision of fistula, premature adhesion being prevented, without the necessity of interposing tents, lint, or other material. The nitrate should be applied, but only to the lips, twenty-four hours after the operation, and repeated first every, and then every other morning. He was led to the practice by observing the condition of wounds when touched by the nitrate. The pellicle or superficial eschar is eliminated in a day or two; the surface so covered being unsuited to contract adhesions. This covering also enables the wound to tolerate the presence of the intestinal discharges which pass over it. Most persons complain little of the pain caused by the application; but in some cases it is severe.

Fractures, Compound.—M. Trastour details in a series of papers (Archives Générales, vols. xxix. and xxx.) numerous cases of compound fracture he has witnessed in M. Chassaignac's wards, illustrative of the favourable results that have followed their treatment by "occlusion." As our readers are aware,* this consists in the immediate application of a cuirass of adhesive plaster, which is restrained *in situ* for several days, all surrounding inflammation being kept down by leeching, if necessary. M. Trastour's report is highly favourable; under this plan the wound far more rapidly heals, pain and traumatic fever are much diminished, as is the chance of the occurrence of nervous delirium, tetanus, erysipelas, and purulent infection. In cases in which it may be doubtful whether amputation will be required, it enables us to wait with safety for the decision; and brings these traumatic cases in nearer relation to the cases in which amputation is performed for disease, and in which its results are so much more satisfactory. Since he has adopted this practice, M. Chassaignac never amputates for traumatic injuries of the fingers, however violent the injury may have been. Even when re-union does not take place, very much longer and better stumps result from leaving the case to nature.

Fractures of the lower end of the Radius.—M. Robert (L'Union Médicale, 1853, Nos. 6 & 7) observes that the various plans of treating fracture of the lower end of the radius with antero-posterior displacement, are either insufficient when the amount of pressure is slight, or dangerous when it is great, in consequence of the possible inflammation and gangrene of the skin of the wrist. Moreover, long-continued pressure gives rise to slow inflammatory action and adhesion among the synovial sheaths, leading to stiffness of the wrists and fingers, and an inability to employ the limb for many months after the accident. The following is M. Robert's own plan of treatment:—The fracture need not be reduced; but the patient, lying in bed, stretches out his arm horizontally and parallel to the trunk, and puts it on its palmar surface, upon a pad well filled with oat-chaff.

* British and Foreign Medico-Chirurgical Review, vol. v. p. 262.

This cushion terminates by a thick lower edge, which corresponds to three fingers' breadth above the palmar fold—i. e., a little above the fracture, so that the hand hangs pendant over this border, the fingers being prevented coming in contact with the bed. When inflammation exists, leeches, fomentations, &c., are applied to the exposed part. At the end of the eighth or tenth day, the patient may get up, supporting the arm in a sling, and still allowing the hand to hang down; or, indeed, he may be up from the first, resting his arm on a table. M. Robert has thus treated from fifty to sixty cases, in all of which rapid consolidation, with absence of deformity, has resulted. The greatest advantage, however, attendant upon the plan, is the freedom of the synovial sheaths from adhesion, so that the patient can at once resume his occupations.

Furunculus.—In the furunculoid epidemic lately prevalent, various means are proposed by different medical men; nitro-hydrochloric acid, alterative doses of mercury, chlorate of potash, quinine, and iron, are among the chief measures recommended.

Galvanism.—Mr. Springfellow exhibited to the Medico-Chirurgical Society (Lancet, March 5) a new form of galvanic-battery, resembling Pulvermacher's, but more portable. For a full description, see report in Lancet.

Heart-Disease.—M. Beau (Archiv. Gen., Fev. 1853, p. 181), in a long paper on heart-affections, in which several original and disputable views are advocated, recommends *digitalis,* not as a sedative, but as a *stimulant,* and states that it acts by relieving the imperfect action of the heart, or what he calls the state of *a-systolia,* in many cases of hypertrophy. In a case of this kind the heart beats rapidly, perhaps 120 times per minute, but the pulse is feeble and unequal. After the use of digitalis, the action falls perhaps to 60 per minute; but the beats are much stronger, and are more regular; at the same time, the face ceases to be injected, and the dyspnœa diminishes. M. Beau believes that the digitalis *augments* the contractility of the ventricle, and thus produces these effects. He calls digitalis "the cardiac cinchona." He gives 20 centigrammes of the leaves infused in a cupful of water, every morning, on an empty stomach. The diet must be good.

Intestinal Obstructions.—Mr. B. Phillips (Lancet, Jan. 1) advises, in intestinal obstruction, that the drastic purgatives, such as croton oil, should not be given at an early period; but one or two full doses of calomel and opium (8 to 10 grs. of calomel to 2 grs. of opium) should be first given, and large emollient enemata be thrown up every six or eight hours. If these means fail, Mr. Phillips pushes mercury to salivation, mercurial inunction as well as administration by the mouth, being employed.

Neuralgia.—Cazenave (Rev. Med. Chir. in Med. Times and Gazette, Feb. 15) recommends in facial neuralgia, an ointment composed of chloroform 20 parts, prussiate of potash 10 parts, and lard 60 parts; a piece the size of a walnut to be rubbed over the painful part. An oiled-silk cap is then to be worn for some hours.

Ovarian Cysts.—M. Duplay (Archiv. Gen., Feb. 1853) relates a case in which an ovarian tumour was tapped, 16½ litres of fluid drawn off, and 250 grammes of a fluid (composition, water, 100 parts; alcohol, 50; iodine, 5; iodide of potassium, 5) were thrown into the cyst, and then made to issue out again through the canula, by pressure; all but 2 grammes were discharged. Some sharp pain and fever followed; but in three days all unfavourable symptoms disappeared, and the patient up to the date of report (exact time not given) had continued well.

Palate, Fissure of.—Dr. J. Mason Warren describes (Amer. Jour. of Med. Sc., Jan. 1853) a new forceps to be used in the operation for cleft palate; the instrument has a double curve, one anterior, the other lateral; and the posterior jaw of the forceps is longer than the other. The instrument has double teeth.

Phlegmon.—Polli (Gaz. Tosc. 1, 1852) confirms the statements of Bellini, as to the abortive cure of subinflammatory swellings of the skin and subcutaneous cellular tissue by the application of a few drops of liquor ammoniæ. In syphilitic bubo this method is also useful.

Puerperal Miasmata.—Dr. Busch, Director of the Berlin Midwifery Clinique, (Neue Zeitsch. für Geburtsk., vol. xxxii. p. 313), after remarking upon the great difficulty there exists in keeping a lying-in hospital free from puerperal fever, relates the result of an experiment he tried at Berlin. During February and March, 1851, after an epidemic of influenza, one of puerperal fever prevailed extensively amidst all classes at Berlin, the hospital suffering severely during the latter month. It was evacuated, thoroughly cleaned and ventilated for six weeks, and re-opened in May, when, however, every woman admitted became affected soon after delivery. Reflecting upon the influence of hot, dry air in destroying contagious fomites, Dr. Busch had stoves introduced into the wards, and all the bedding, utensils, &c., were exposed to a temperature of from 150° to 170° Fah. during two days. On patients being re-admitted, no more cases occurred, although the disease still prevailed in Berlin. In December, 1851, four women were seized with the disease in one apartment, one of them dying. A heat of 170° Fah. was resorted to, and no extension of the epidemic occurred. To the time of writing, June, 1852, no recurrence had taken place.

Rheumatism.—Dr. Bennett (Edin. Monthly Journal, Dec. 1852) relates six cases of acute rheumatism in which *nitrate of potash* was used. In all the medicine was useful; in two the benefit was very marked.

Sarcinæ Ventriculi.—Dr. Hassall (Med. Times and Gazette, Jan. 29) relates a severe case in which *sulphite of soda* (as recommended by Dr. Jenner) was used with great benefit; infusion of *quassia* and *bicarbonate of potash* were also employed, and seemed useful.

Dr. Bennett (Edin. Mon. Jour., Feb. 1853, p. 168) refers to a case in which the sulphite of soda was ineffectual.

Scarlatina.—Dr. Gillespie (Edin. Mon. Jour., March, 1853) recommends the repeated use of the warm bath, mercurial laxatives on first admission, afterwards small repeated doses of a diaphoretic, containing antimonial wine, spirit of ether, nitrate and liquor ammoniæ acetatis; and the application of a strong solution of nitrate of silver to the throat.

Scurvy.—Dr. Hammond (Amer. Jour. of Med. Sc., Jan. 1853) recommends strongly, from experience of twelve cases in New Mexico, the use of the *salts of potash,* especially the bitartrate. The remedy was first employed in consequence of Garrod's recommendation.

Stomatitis Ulcerosa.—Dr. Mackenzie recommends sponging with the dilute nitric acid of the Pharmacopœia, and giving internally carbonate of ammonia, and citrate of iron.

Syphilis.—Professor Gamberini (Bulletino delle Sc. Med., xxi. p. 253) confirms the favourable opinion expressed by Dr. Daveris,* of the *iodide of sodium,* and believes, from an experience of 116 cases, that it may be advantageously substituted for the iodide of potassium. The dose was at first ʒj., raised gradually to ʒij. per diem.

Testis, Enlargement of.—Mr. Hutchinson describes (Med. Times and Gazette, Feb. 26) an *air-compressor* of the testis. It consists of a double bag; the testicle is placed inside, and air is forced into the cavity of the bag. The apparatus is made by Mr. Fergusson, of Giltspur-street.

Tracheotomy.—Mr. Henry Thompson (Lancet, March 5) describes and figures a new instrument for performing tracheotomy, the peculiarity of which is, that it opens the trachea without cutting through the cartilaginous rings; an opening is made between two rings, and widened by dilatation. A case is related in which the operation was performed. The instrument is made by Weiss.

* See British and Foreign Medico-Chirurgical Review, vol. ix. p. 560.

Ulcers.—Mr. Hancock (Lancet, Feb. 12) has employed to phagedænic ulcers undiluted *chloroform*, as a local application, with benefit. The pain is severe, but not so much so as from the use of strong nitric acid; the effect is equally useful.

Melicher (Wien Zeitsch., viii. p. 6) recommends a solution of *gutta percha in chloroform* for the purpose of covering scrofulous and indolent ulcers, and protecting them from the action of the air.

Variola.—Dr. Paterson records (Edin. Mon. Jour., Dec. 1852) two cases of small-pox, in which mercurial ointment mixed with starch, being spread on the surface for four or five days, profuse salivation followed.

Vomiting.—Dr. Inman (Med. Times and Gazette, March 5) recommends chloroform (five drops with water) by the mouth in sympathetic vomiting. It is less nauseous than creasote.

STATISTICS.

Statistics of the Deaf and Dumb in Ireland.

MR. WILDE has published a very interesting paper on the deaf and dumb in Ireland, as shown in the late Census. In all Ireland, 4485 deaf mutes were returned; or (allowing for disturbing causes) 1 in every 1500 inhabitants. The proportion in all Europe is 1 in every 1593 persons. In Ireland it varies in different parts:—1 in 1794, in Leinster; 1 in 1689, in Connaught; 1 in 1487, in Ulster; 1 in 1469, in Munster. It is more common in the rural than in the civic portions; and in the hilly than the flat districts. Of the whole number, 4151 were deaf and dumb; and of these, 2349 were males, and 1802 females,—or 100 men to 76·61 females. 334 were dumb but not deaf. Deaf-and-dumb persons do not directly engender deaf-and-dumb children: thus, in 77 cases, one parent, and in 5 instances both parents, were deaf and dumb; only two mute children resulted. Yet muteism is often manifest in several members of a family derived from a common, though not a deaf-and-dumb stock. Thus, 2512 families had one mute child; 287, two mutes; 127, three mutes; 32, four mutes; 8, five mutes; 3, six mutes; 1, seven mutes; 1, eight mutes. On inquiry, also, it is found that hereditary predisposition from ancestors could be traced in 281 cases; in 149 of which the taint descended through the father's side, and in 132 through the mother's.

Six cases are recorded of deaf, dumb, and blind persons.

Of the deaf and dumb, 744 were educated.

In respect of race, 1198 were Irish, 352 English, and 121 Scotch. As to mortality, out of 291 recorded deaths, 72 were from epidemic, and 135 from sporadic diseases—and of these there were no less than 77 cases of consumption, or 1 to 2·81 of the entire number of specified causes.

An interesting American table of the deaf-and-dumb and blind persons, as compared to the entire population in the United States, is annexed, which we extract.

It appears that there are 9422 white mutes in America, in a white population of 19,371,591; and 96 coloured mutes among a free coloured population of 251,205.

	Deaf & Dumb.	Blind.	Insane.	Idiotic.
Whites . . .	1 in 2073	1 in 2455	1 in 1295	1 in 1384
Free Coloured .	„ 2956	„ 867	„ 1355	„ 983
Slaves	„ 6552	„ 2646	„ 11,011	„ 3081
Total Slaves and Free Coloured	„ 5730	„ 2131	„ 5936	„ 2461

Journal of the Statistical Society, March, 1853.

BOOKS RECEIVED FOR REVIEW.

A Synopsis of the Molluscæ of Great Britain. By W.E. Leach, M.D. London, 1852. 8vo, pp. 376.

On the Growth of Plants in Closely-glazed Cases. By N. B. Ward, F.R.S. London, 1852. 8vo, pp. 143.

Principles of the Anatomy and Physiology of the Vegetable Cell. By Hugo Von Mohl. Translated by Arthur Henfrey, F.R.S. London, 1852. 8vo, pp. 158.

Syphilitic Diseases, their Pathology, Diagnosis, and Treatment, including Experimental Researches on Inoculation, &c. By John C. Egan, M.D., M.R.I.A. London, 1853. 8vo, pp. 339.

On the Pathology and Treatment of Hysteria. By Robert Brudenell Carter, M.R.C.S.E. London. 8vo, pp. 161.

The Modern Practice of Physic. By Robert Thomas, M.D. Eleventh Edition, thoroughly revised by Algernon Frampton, M.D. Cantab. London. 8vo. Vols. I. and II.

The Half-yearly Abstract of the Medical Sciences. Edited by W. H. Ranking, M.D. Cantab., and C. B. Radcliffe, M.D. Lond. Vol. XVI., July to December, 1852. London. 8vo, pp. 388.

The Retrospect of Medicine. Edited by W. Braithwaite. Vol. XXVI., July to December, 1852. London. 8vo, pp. 448.

On Perforating Ulcer of the Stomach, from Non-malignant Disease. By Edwards Crisp, M.D. London, 1852. 8vo, pp. 22.

A Treatise on Operative Ophthalmic Surgery. By H. Haynes Walton, F.R.C.S. Eng., Surgeon to the Central London Ophthalmic Hospital, &c. London, 1853. 8vo, pp. 628.

Observations and Reports on the Treatment of Intermittent Fever by Phosphate of Lime and Sulphur. By Assistant-Surgeon A. Blacklock. Madras, 1852. 8vo, pp. 103.

A Treatise on Auscultation and Percussion. By Dr. Joseph Skoda. Translated from the Fourth Edition, by W. O. Markham, M.D., Assistant-Physician to St. Mary's Hospital. London, 1853. 8vo, pp. 346.

A Treatise on Removable and Mitigable Causes of Death, and on the Leading Principles of Public Hygiène. By Norman Cheevers. M.D., Bengal Medical Service. Calcutta, 1852. Vol. I. pp. 239, with an Appendix.

Atlas of the Formation of the Human Body in the Earliest Stages of its Development; compiled from the Researches of the late Professor, Dr. M. P. Eral. By Joseph Kahn, M.D. (Vienna.) Illustrated by 60 Figures, contained in 13 Plates. London, 1852. Small folio, pp. 29.

Observations on Syphilis, and on Inoculation. By John Crouch Christophers, M.R.C.S. London, 1853. 8vo, pp. 74.

Six Lectures on Materia Medica and its Relations to the Animal Œconomy. By John Spurgin, M.D. London, 1853. 8vo, pp. 204.

Précis Théorique et Pratique sur les Diathèses. Par P. Baumes, M.D. Paris, 1853. 8vo, pp. 448.

Traité des Fistules Vesico-Uterines, &c. Par A. J. Jobert (de Lamballe), M.D., Chirurgien de l'Hôtel-Dieu. Paris, 1852. pp. 420.

Contributions to Obstetric Pathology and Practice. By James Y. Simpson, M.D., Professor of Midwifery in the University of Edinburgh. Edinburgh, 1853. 8vo, pp. 80.

Report on Bethlehem Hospital of the Commissioners in Lunacy to the Right Honourable Sir George Grey, Bart., M.P., &c. &c.; together with Notes of the Evidence. Printed by order of the House of Commons, 14 December, 1852.

An Account of Bethlehem Hospital, abridged from the Report of the late Charity Commissioners. Second Edition, with some Information respecting Bridewell. London, 1853. pp. 47.

Die Krankheiten des Anges. Von Dr. Fred. Arlt. Bd. 11, Hälfte 1. Prag, 1853. pp. 144.

Results of the System of Separate Confinement, as administered at the Pentonville Prison. By John T. Burt, B.A., Assistant-Chaplain. London, 1852. pp. 285.

Sanitary Measures and their Results; being a Sequel to the History of Cholera in Exeter in 1852. By Thomas Shapter, M.D., Physician to the Devon Hospital. London, 1853. 8vo, pp. 32.

The Dissector's Manual of Practical and Surgical Anatomy. By Erasmus Wilson, F.R.S. Second Edition. London, 1853. pp. 626.

Urinary Deposits, their Diagnosis, Pathology, and Therapeutical Indications. By Golding Bird, A M., M.D., F.R.S. Fourth Edition. London, 1853. pp. 473.

The Diseases of Women, and Ovarian Menstruation, &c. By Edward John Tilt, M.D., Senior Physician to the Farringdon General Dispensary, &c. Second Edition. 1853. 8vo, pp. 276.

On the Local Application of a Benumbing or Congealing Temperature in Inflammatory, Painful, and Malignant Diseases. By James Arnott, M.D. London. 1852. pp. 16.

On Near Sight, Aged Sight, Impaired Vision, and the Means of Assisting Sight. By William White Cooper, F.R.C.S.E., Ophthalmic Surgeon to St. Mary's Hospital. Second Edition. London, 1853. pp. 320.

Hysterical and Nervous Affections of Women. By William John Anderson, F.R.C.S., late District-Accoucheur to St. Mary's Hospital. London, 1853. 8vo, pp. 39.

Lectures on the Nature and Treatment of Fever. By D. J. Corrigan, M.D., T.C.D., M.R.I.A., Physician in Ordinary to the Queen in Ireland, &c. Dublin, 1853. 8vo, pp. 104.

A Dictionary of Medical Science. By Robley Dunglison, M.D., LL.D., Professor of the Institutes of Medicine in Jefferson Medical College, Philadelphia. Ninth Edition. 1853. pp. 927.

INDEX TO VOL. XI.

OF THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

END OF VOL. XI.

Savill & Edwards, Printers, 4, Chandos-street, Covent-garden.