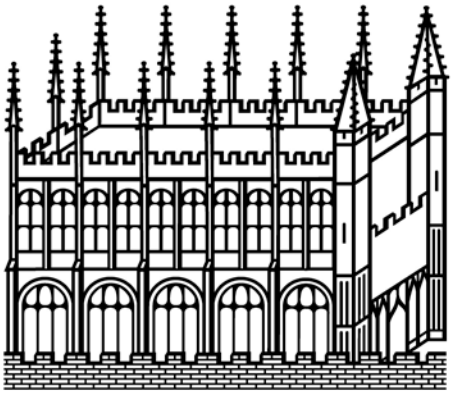

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JANUARY, 1852.

PART FIRST.

Analytical and Critical Reviews.

ART. I.

1. *Lectures on Tumours, delivered in the Theatre of the Royal College of Surgeons of England for the year* 1851. By JAMES PAGET, F.R.S., &c., Professor of Anatomy and Surgery to the College. (From the 'Medical Gazette.') 8vo, pp. 88.
2. *Ueber die Cyste. Zur Anatomie des Kropfes. Denkschr. der K. Akademie der Wissenschaften.* Von CARL ROKITANSKY.—*Wien*, 1849.

On Cysts; and on the Anatomy of Bronchocele. By CARL ROKITANSKY.

OUR readers need not again be told, why we consider Mr. Paget's Lectures at the College of Surgeons to be among the most valuable contributions which our profession receives to its stock of information during the successive years of their delivery; and it is scarcely necessary that we should add any other reason for singling them out for special notice, from amongst the multitude of scholastic prelections which fill so large a part of the pages of our weekly periodicals. Were there no other ground, this would be a sufficient one; that they are addressed, not to an audience of youths in their pupilage, but to an assemblage of men far advanced in the knowledge and practice of their profession—men from whose qualifications it may be expected, that they will not receive what is advanced with the blind confidence of mere learners, but that whilst ready to profit by the results of the special attention which the lecturer has given to his subject, they will criticize his doctrines and possibly canvass the correctness of his facts. We can scarcely conceive any greater stimulus than this, to the exertion of the lecturer's best powers in the labour of careful preparation, that he may do justice alike to his subject and to himself; and it is not saying too much of Mr. Paget to affirm, that there is probably no individual in our profession, by whom such a stimulus would be more rightly appreciated, or who would more worthily exert the rare abilities with which he has been endowed, in doing justice to the task which he has undertaken.

The subjects of Mr. Paget's successive courses become year by year more directly *practical;* and those who have followed him through the series, will now, we think, be fully prepared to admit the excellence of the plan on which they have been arranged. They have bridged over the hiatus between normal and morbid anatomy, between physiological and pathological science; and they are now carrying us on to the discussion of some of the most important questions of surgical practice, with all the advantage derivable from the most comprehensive survey of what, for want of a better term to replace the hackneyed phrase, we must call the *philosophy* of the subject.

There is certainly no department of surgical pathology which has made more rapid advances within the last twenty years, than that which relates to the subject of morbid growths; and this is doubtless due in the main to our improved acquaintance with the general physiology of nutrition, and to the application of that new power of structural analysis with which we are furnished by the microscope. We might not unprofitably enter into a detailed contrast between our present amount of information upon this subject, vague and imperfect as it confessedly is in many departments, and that which prevailed (we will say) when Dr. Carswell's work gave the most complete and advanced view of it which the *then* state of knowledge admitted. But such a comparison would keep us too long from our immediate object, which is, to place before our readers the leading points that stand out in most prominent relief in the very comprehensive survey taken by Mr. Paget—a survey which includes, we feel it safe to affirm, everything of note that has been yet contributed by others, together with a large amount of original matter furnished by his own observations and inquiries.

Amongst those recent contributions to our knowledge of tumours which have most advanced our scientific acquaintance with them, must undoubtedly be ranked the essay of Professor Rokitansky on *Cysts;* whose researches, with those of Simon, Lebert, Birkett, and others, have introduced a new and most important "idea" into the study of these growths—an idea which is pregnant with deep meaning, and which will prove, we feel assured, most fertile in its applications, both as regards science and practice. On this account, we shall specially dwell on that department of Mr. Paget's course which embraces this subject; and his development of Rokitansky's views is so complete, that we shall have very little to add to it from the original fount.

We need scarcely labour to impress upon our readers the value which a thorough complete elucidation of the whole subject of Tumours will have for the surgeon. There is no topic on which the opinions of the profession are more divided, than in regard to the line of demarcation between the "innocent" and the "malignant," and consequently with respect to the propriety of operative interference with those growths which are near its borders. A striking example of this discrepancy is furnished by the replies to the question of operating for cancer, furnished to the American Medical Association by a number of the most distinguished experienced surgeons of the Union, of which a notice was contained in our last number (p. 463). It is obvious that the value of any answer to this question must depend mainly upon the accuracy of the diagnosis of the cases on which it is founded; so that, even if there were no other reason, this alone would be amply sufficient to show the necessity for the most careful study

of the whole subject, with all the appliances which modern science can afford for its elucidation.

Mr. Paget applies himself, in the first instance, to the *definition* of the class of objects on whose consideration he is about to enter; and the pains-taking mode which he has obviously felt himself obliged to adopt for clearly marking them out, is one among the many illustrations continually presenting themselves to the thoughtful mind, of the difficulty—not to say impossibility—of giving *verbal* expression to *ideas* which are very distinctly impressed on our consciousness. We all know what a *tumour* is, and should seldom have any hesitation in pronouncing of any particular morbid growth with whose structure we might be acquainted, whether it should rank in that category, or not; and yet it is by no means so easy to say *why* we should do so. In fact, Mr. Paget's definition of a tumour somewhat reminds us of Dr. Johnson's celebrated definition of network—as "Anything reticulated or decussated, at equal distances, with interstices between the intersections;"—and this from no fault of Mr. Paget's, but from the inherent necessities of the case.

The class of diseases which includes tumours may be reckoned as a part of that great division named hypertrophies or overgrowths, all whose members consist in additions to the organized materials of the body, and appear to be expressions of a morbid excess of its formative force; but whilst in ordinary hypertrophy the excess of growth takes place, so to speak, on the normal pattern, and frequently has reference to some special requirement of the system, (as when an hypertrophy of the heart adapts the organ to an increased difficulty in propelling the blood through the lungs,) in the development of a tumour there is a departure from that pattern or type, and this without any purpose that can be discerned in regard to the general economy. Of this contrast we have a good example in the case of a fibrous tumour growing into the cavity of the uterus.

"Such a tumour may resemble in its tissues the substance of the uterus itself, having well-formed muscular and fibrous tissues; and, so far as the structures formed in excess are concerned, we might regard the tumour as the result of an hypertrophy not essentially different from that which, at the same time and rate, may take place in the uterine walls around it. But an essential difference is in this—the uterus, in its growth around the tumour, maintains a normal type, though excited to its growth, if we may so speak, by an abnormal stimulus: it exactly imitates, in vascularity and muscular development, the pregnant uterus, and may even acquire the like power; and at length, by contractions, like those of parturition, may expel the tumour, spontaneously separated. But the tumour imitates in its growth no natural shape or construction: the longer it continues the greater is its deformity. Neither may we overlook the contrast in respect of purpose, or adaptation to the general welfare of the body, which is as manifest in the increase of the uterus as it is improbable in that of the tumour." (p. 2.)

Two very distinct sets of characters, be it observed, are involved in the preceding description—namely, the *anatomical*, and the *physiological*,—those which have reference to structure and arrangement, and those which have reference to growth and action. These it will be found of great importance to keep apart.

Mr. Paget then proceeds to compare tumours with the morbid products of inflammatory and other perversions of the ordinary nutritive operations; and he draws between them the following distinctions:—

"1st. The accumulation of lymph in inflammation appears chiefly due to the morbid state of the parts at or adjacent to the seat of exudation. We have, I think, no evidence that the lymph of inflammation increases by any inherent force—any attraction of self-organizing matter, or any multiplication of its own cells; but the increase of all, or nearly all, tumours, is 'of themselves:' they grow as parts of the body, but by their own inherent force, and depend on the surrounding parts for little more than the supply of blood, from which they may appropriate materials. A tumour, therefore, as a general rule, increases constantly; an inflammatory exudation generally increases only so long as the disease in the adjacent parts continues.

"2nd. The materials severally produced in excess, in these two cases, have different capacities of development. The inflammatory exudation, in whatever part it lies, has scarcely more than the single capacity to form fibro-cellular or fibrous tissue: the material that begins or is added to a tumour may, indeed, assume either of these forms, but it may assume any one of several other forms.

"But, 3rdly, the most striking contrast is in the events subsequent to this first organizing of the two materials. The later course of organized inflammatory exudations, like that of the organized material for repair after injuries, is usually one of constant approximation to a healthy state. As newly-formed parts, they gradually assimilate themselves to the shape and purpose, if not to the tissue, of the parts among which they lie; or they are apt to waste, degenerate, and be removed. Their progress is ever towards a better state; so that, in the whole course of exudative inflammatory diseases, some can see nothing but an 'effort of nature' to avert or repair some greater evil.* It is very different with the class of diseases to which tumours belong: it is in their very nature to proceed to further and further deviation from the proper type of the body. The structure of tumours may, indeed, be like that of some of the natural parts; it may be identical with that of the part in which they lie: in this they may be homologous; but, considered in their life, they are not so; for, commonly, they are growing while the tissues far and near around them are only maintaining their integrity, or are even degenerating, or yielding themselves to the anormal growth." (p. 4.)

It is, as Mr. Paget justly observes, "in the consideration of this activity and partial independence of the life of tumours, and of the diseases allied to them, that we shall ever discern their true nature;" in other words, it is in their physiological rather than in their anatomical characters, that their peculiarity essentially consists. Thus a tumour may be entirely made up of tissues in all respects similar to the normal components of the body, such as fat, cartilage, bone, or simple fibrous structure, and its only anatomical peculiarity may be the *position* of the new growth; yet physiologically, its independence is just as great when its textures are most similar, as it is when they are most dissimilar, to those of the body in general. Of this a remarkable example is afforded by a case mentioned by Mr. Paget as having occurred at St. George's Hospital, in which fatty tumours of considerable size were found in the mesentery of a patient, from whom, in the extremest emaciation of phthisis, nearly all the natural fat was removed; thus showing that the adipose tissue of the tumours was placed, in some way or other, beyond the pale of that operation by which the oleaginous contents of the ordinary fat-cells are taken back into the current of the circulation, when their combustion is needed to maintain the heat of the body. Hence, says Mr. Paget, "it is not in the likeness or unlikeness to the natural tissues, that we can express the true nature

* There are, indeed, cases in which organized lymph and scars continue to grow; but these are quite exceptional, and are to be regarded as diseases of the same class as tumours, peculiar only in respect of the materials in which they are manifested.

of tumours: as dead masses, or as growths achieved, they may be called like or unlike the rest of a part; but as things growing they are all unlike it. It is, therefore, not enough to think of them as hypertrophies or overgrowths: they must be considered as parts overgrowing, and as overgrowing with appearance of inherent power, irrespective of the growing or maintenance of the rest of the body, discordant from its normal type, and with no seeming purpose." It is no real objection to this view, to say that tumours may cease to grow, and that some kinds of them seem to have a natural limit to their size, almost as constant as that of the proper organs of the body; for whilst their peculiar independence *of growth* is of course manifested only during the period of increase, their independence of action is also plainly demonstrated by the fact of such limitation, and also by the occurrence of degenerative or absorptive processes, which have no relation to the actions going on in the body at large.

But although tumours may thus be usually marked out as distinct from other abnormal growths, yet there are, as in most other groups of natural objects, connecting forms, which can scarcely be referred with propriety to either side of the line of separation. Such are those glandular tumours which are made up of the proper substance of the gland with which they are connected, as the mammary, the prostate, or the thyroid; and which, though frequently encysted, are sometimes met with as outlying parts of the gland itself. Of these Mr. Paget remarks—

"Clearly these are all essentially the same kind of disease: yet, to call them all 'tumours' would be to do as much violence to the conventional use of terms which have become not merely the expressions, but the guides, of our thoughts. The best course seems to be, to make an arbitrary division of this group. In accordance, then, with the arbitration of custom, we may assign the name of tumours to such examples of these morbid growths or growing parts as, 1st, are isolated from the surrounding parts by distinct investing layers of tissue; or, 2ndly, are continuous with the natural parts, but are abruptly circumscribed in the greater part of their extent; or, 3rdly, are formed of new materials infiltrated and growing in the interstices of natural parts." (p. 6.)

There is another class of objects to which tumours come into close relation—namely, the excessive or supernumerary parts, which are not unfrequently formed during fœtal life, and which cannot be ascribed with any probability to the "fusion" of germs. As we remarked in our last number (p. 523), there seems a strong physiological probability that a large proportion of the cases of *monstrosity by excess,* are to be explained rather on the hypothesis of an unusual developmental power of a single germ, than on that of the union and partial fusion of two;* and thus, all hypertrophies and tumours may be looked upon, in one sense, as expressions of this power. In the formation of a supernumerary part, this has been sufficient, not merely to produce the tissues, and to develop them according to a regular morphological type, but to impart to this

* We are glad to find our views on this subject confirmed by the high authority of Professor Vrolik, who has come to the conclusion, "that the whole class of monsters by excess owe their origin to different degrees of one common fault, and consequently that the explanation of their origin ought to be the same for all; that no kind of fusion can account for the production of supernumerary individual organs, the rest of the body being single; but that it is not impossible that excess of power in the ovum, which all admit can alone explain the lower degrees of duplicity, may, in proportionately higher degrees (perhaps by the formation of two primitive grooves) produce the more complete double monsters, or even two such separate individuals as are sometimes found within a single amnion."—Cyclopædia of Anatomy and Physiology, vol. iv. p. 976.

type a separate and independent existence; thus producing an additional finger or thumb on each hand, or a double pair of arms or legs, or even a double head or trunk. In the hypertrophy of a regular or normal part, the new tissues are still developed according to a regular morphological type, but they have not the power of individualizing themselves (so to speak), and are so incorporated with the normal elements as to augment the size of the existing organ. In the formation of a tumour, on the other hand, the morphological nisus is altogether wanting; and whilst the tissues themselves are perfectly formed, and have a marked power of independent growth, the mass composed of them is altogether amorphous, its configuration being usually determined rather by the physical conditions under which it is produced, than by any peculiar tendencies of its own. But further, in many of the large class of tumours distinguished as malignant, the development of tissue has not gone to the extent of producing any one of the normal histological species; and herein (as in other particulars presently to be noted) the grade of vital power would seem to be still lower. Thus, whilst the tumour may on the one side resemble the hypertrophied organ in its intimate structure, but differs from it in its greater independence, so does it resemble the supernumerary part in independence, whilst deficient in morphological completeness. Yet even here it may be difficult to draw the line, especially in those cases where the abnormal growths are formed of some normal tissue, and the tendency to them is symmetrical, as in the following example, which we cite from the Seventh Lecture:

"A boy, six years old, was in St. Bartholomew's Hospital, four years ago, who had symmetrical tumours on the lower ends of his radii, on his humeri, his scapulæ, his fifth and sixth ribs, his fibulæ, and internal malleoli. On each of these bones, on each side, he had one tumour: and the only deviations from symmetry were, that he had an unmatched tumour on the ulnar side of the first phalanx of his right fore-finger, and that each of the tumours on the right side was rather larger than its fellow on the left.

"I saw this child's father, a healthy labouring man, forty years old, who had as many or even more tumours of the same kind as his son, but of which only a few were in the same positions. All these tumours had existed from his earliest childhood; they were symmetrically placed, and had ceased to grow when he attained his full stature, and since that time had undergone no apparent change. None of this man's direct ancestors, nor any other of his children, had similar growths, but four cousins, one female and three male, children of his mother's sister's, had as many of them as himself.

"The swelling on the little boy's fore-finger was an inconvenience to him, and at his parents' request Mr. Lloyd removed the finger. It consisted of an outgrowth or projection of healthy-looking cancellous bone, full of medulla, and coated with a thin layer of compact tissue; its substance being regularly continuous with those of the phalanx itself." (p. 78.)

Although, however, in such instances as these, the line cannot be drawn without artifice between Tumours and Monstrosities by excess, yet, as Mr. Paget remarks, there are in the majority of cases sufficient characters of distinction between them:

"For, 1st, the congenital excesses of development present a more complex structure, and are more conformed to the plan and construction of the body, than anything that can be reasonably called a tumour. And if it be said that this higher organization is no more than is consistent with the period of formation, which is in embryo-life, when the force of development is greatest; then, 2ndly,

we may note this difference—that the congenital excesses are limited for their increase to the period of natural growth of the body. They commonly cease to grow when or before the body has attained its full stature; they usually conform to its methods and times of development, growth, and decay." (p. 5.)

Having thus defined Tumours in general, Mr. Paget proceeds to subdivide this class of growths; and institutes, as his primary groups, the two sub-classes of *innocent* and *malignant*—using these terms, not as being free from objection, but as implying a more natural and a less untrue division than any yet invented to replace them. This division is certainly eminently practical, because it involves the great question on which all operative interference must turn. There may, or may not, be obvious and well-marked structural differences between these two classes, which shall give to the distinction between them a definite *anatomical* basis; but even if this be completely substantiated, the practical value of such anatomical distinction entirely depends upon the *physiological indicia* it affords, with regard to the probable course of the morbid growth, and to its tendency to reappear if extirpated. Thus we shall suppose that the diagnostic characters of each form of tumour were clearly made out, and that a particular question of operation were under discussion, that question might be satisfactorily decided by the microscopic examination of the smallest portion of the morbid growth; but although the surgeon might have immediately before his mind nothing but the question of malignancy or non-malignancy, and might adopt the rule of operating in the latter case, and of not operating in the former, yet, if he were to ask himself, or be asked by another, the *rationale* of that rule, he would pass at once from the anatomical to the physiological characters, from the structure of the mass to its growth, and to the constitutional tendencies with which it is connected.

It is not possible to draw a definite line of demarcation between malignant and non-malignant tumours, any more than between tumours in general and other morbid growths. All that we can do is to set up a distinct type or standard of each, round which we may arrange, in more or less close proximity, the several forms that present themselves; some of these on the borders of the two groups bearing a close conformity to each other. The distinctions have been so well laid down by Mr. Paget, that we shall quote his enumeration of them in full. The following characters may generally be considered diagnostic of malignancy; whilst their absence, or the existence of their opposites, may be counted among the general characters of the innocent tumours:

"1st. The intimate structure of malignant tumours is, usually, not like that of any of the fully developed natural parts of the body, nor like that which is formed in a natural process of repair or degeneration. Many of the cells of cancers, for example, may be somewhat like gland-cells, or like epithelium-cells; yet a practised eye would easily distinguish them, even singly. And much more plainly their grouping distinguishes them: they are heaped together disorderly, and seldom have any lobular or laminar arrangement, such as exists in the natural glands and epithelia, or in the innocent glandular or epithelial or epidermal tumours. These innocent tumours are really imitations, so far as their structure is concerned, of the natural parts; and the existence of such imitations makes the diversity—the heterology, as it is called—of the malignant tumours appear more evident.

"Still, this rule of dissimilarity of structure in malignant tumours is only general. The other properties of malignancy may be sometimes observed in tumours that have, apparently, the same structure as those that are generally

innocent. I shall have to refer to cases of fibrous tumours which, in every respect of structure, were like fibrous tumours of the uterus, and yet returned after removal, and ulcerated, with infection of adjacent parts, and appeared in internal organs. These, with some others, must be regarded as malignant, though in structure resembling innocent tumours and natural tissues. On the other hand, there are some innocent cartilaginous tumours with structures that do not exist in any of our natural tissues. The two sets of cases, though both be exceptional, supply sufficient grounds for not preferring such terms as 'homologous' and 'heterologous' before 'innocent' and 'malignant,' if they are meant, as they commonly are, to apply to the structure of the several growths.

"2nd. Malignant growths may have the character of infiltrations—i. e., their elementary structures may be inserted, infiltrated, or diffused in the interspaces and cavities of the tissues in which they lie. Thus, in its early state, a malignant tumour may comprise, with its own proper elements, those of the tissue or organ in which it is formed; and it is only in its later life that the elements of the tissue or organ disappear from it, gradually degenerating and being absorbed, or possibly yielding themselves as materials for its growth.

"Thus a hard cancer of the mammary gland includes in its mass a part, or even the whole, of the gland itself, as if there were only a conversion of the gland-tissue: and one may find within the very substance of the cancer the remains of the lactiferous tubes involved in it, and, with the microscope, may trace in it the fibro-cellular tissue that separated the gland lobes, and the degenerate elements of the epithelial contents of the tubes and acini. But among all these lie the proper cells of the cancerous growth, and these usually increase while the original structures of the gland decrease. So, too, in medullary cancerous disease of the uterus, the uterus itself, or part of it, is in the tumour, and gradually wastes while the medullary matter diffused or infiltrated in it is growing.

"The malignant growths may, I say, thus appear as infiltrations; but they are not always so. Thus, though the hard cancer of the breast is, commonly or always, an infiltration of cancerous substance in and among the proper structures of the gland; yet the hard cancer of the bones is usually a distinct tumour, such as has no mixture of bone in it, but may be enucleated from the cavity or shell of bone in which it lies. So, too, while the medullary cancer of the uterus plainly consists in an infiltration or insertion of new material in the substance of the organ, that of the breast is usually a separate tumour, and altogether discontinuous from the surrounding parts.

"Many other instances of similar contrast might be cited; still the fact that their elementary structures may be thus infiltrated in the tissues they affect, is a characteristic feature of malignant tumours. I think it is never imitated in cases of innocent tumours.

"3rd. It is, also, generally characteristic of malignant tumours that they have a peculiar tendency to ulcerate, their ulceration being commonly preceded by softening. One can, indeed, in this particular, only observe a graduated difference between the innocent and the malignant diseases; for certain innocent tumours, if they grow very rapidly, are apt very rapidly to decay; and then may suppurate and discharge their pus and débris with foul and dangerous ulceration. Thus the quickly-growing cartilaginous tumours may imitate, in these respects, malignant growths; so may large fibrous tumours when they soften and decay. Or, again, when an innocent tumour grows more rapidly than the parts over it can yield, they may waste and ulcerate, and allow it to protrude; and it may now itself ulcerate, and look very like malignant disease. This may be seen in the protruding fibrous tumours that ulcerate and bleed; or, in a more striking manner, in the protruding vascular growths that have sprung up in the cystic tumours of the breast. Or, once more, the characters of readiness to ulcerate may be imitated by innocent tumours after injuries, or in exposure to continued irritation; for they resist these things with less force than the similar natural parts do. Hence sloughing and ulcerating fibrous, erectile, and other tumours, have been often thought cancerous, and so described.

"The respective tendencies to ulcerate can, therefore, be counted only as constituting differences of degree between the innocent and the malignant tumours. We may speak of a liability in the one case, of a proneness in the other: in the innocent, the ulceration has the marks of disease in the growth; in the malignant, it appears rather as a consequence of natural degeneration or molecular death.

"4th. The softening that often precedes the ulceration of malignant growths can hardly be considered separately from the minute account of their structure. I therefore pass it by, and proceed to their fourth distinctive character, which is to be noticed in the modes of their ulceration.

"This is, that the ulcer, which forms in, or succeeds, a malignant growth, has no apparent disposition to heal; but a morbid substance, like that of which the original growth was composed, forms the walls or boundaries of the ulcer; and as this substance passes through the same process of ulceration which the primary growth passed through, so the malignant ulcer spreads and makes its way through tissues of all kinds.

"In contrast with this character of malignant growths, it is observable that beneath and around an ordinary ulcer of the natural tissues, or of an innocent tumour, we find the proper tissues unchanged; or, perhaps, infiltrated and succulent with recent lymph, or the materials for repair; or somewhat indurated with lymph already organized. The base and margins of a cancerous ulcer are themselves also cancerous; those of a common ulcer are infiltrated with only reparative or inflammatory material. In like manner, if ulceration extend through an innocent growth, it may destroy it all; and no similar growth will form in the adjacent parts, replacing that which has been destroyed: but, in the ulceration of cancer, while the cancerous matter is being constantly discharged, by sloughing or ulceration, from the surface, new matter of the same kind, and in more abundance, is being formed at some distance from the surface; so that in a section through an ulcerated cancer, one does not arrive at healthy tissues till after passing through a stratum of cancer.

"5th. Malignant tumours are, again, characterized by this,—that they not only enlarge, but apparently multiply or propagate themselves; so that, after one has existed for some time, or has been extirpated, others like it grow either in widening circles round its seat, or in parts more remote.

"Mere multiplicity is not a distinctive character of malignant diseases; for many innocent tumours may be found in the same person. But in the conditions and circumstances of the multiplicity there are characteristic differences. Thus, when many innocent tumours exist in the same person, they are commonly, or always, all in one tissue. Thus a man may have a hundred fatty tumours, but they shall all be in his subcutaneous fat: many fibrous tumours may exist in the same uterus, but it is so rare, that we may call it chance, if one be found in any other part of the same patient: so, many cartilaginous tumours may be in the bones of the hands and feet, but to these, or to these and the adjacent bones, they are limited.

"There is no such limitation in the cases of multiplicity of malignant tumours. They tend especially to affect the lymphatics connected with the part in which they first arise; but they are not limited to these. The breast, the lymphatics, the skin and muscles, the liver, the lungs, may be all, and at once, the seats of tumours. Indeed (and here is the chief contrast), it is more common to find the many malignant tumours scattered through several organs or tissues, than to find them limited to one.

"Moreover, if there be a multiplicity of innocent tumours, they have generally had a contemporary origin, and all seem to make (at least for a time) a commensurate progress. But the more ordinary course of malignant tumours is, that one first appears, and then, after a clear interval of progress in it, others appear; and these are followed by others, which, with an accelerating succession, spring up in distant parts.

"6th. A sixth distinctive character of malignant tumours is, that in their multiplication, as well as in their progress of ulceration, there is scarcely a tissue or an organ which they may not invade.

"In regard to their multiplicity, I have just illustrated their contrast in this point with the innocent tumours; and a similar contrast is as obvious in the characters of the ulcers. It is seldom that a common ulcer extends, without sloughing, from the tissues it has first affected into any other; rather, as a new tissue is approached, it is thickened and indurated, as if to resist the progress of the ulcer. But before a cancerous ulcer the tissues in succession all give way, becoming first infiltrated, and then, layer after layer, degenerating and ulcerating away with the cancerous matter.

"One may see this very well in bones. Specimens are to be found in nearly all Museums, of tibiæ (for example) on the front surfaces of which new bone is formed, in a circumscribed round or oval layer, a line or two in thickness. This bone, which is compact, hard, smooth, and closely united with the shaft beneath it, was formed under an old ulcer of the integuments of the shin. But, on the other side, specimens are found, which show that when a cancerous ulcer reaches bone, at once the bone clears away before it; and a cavity abrupt, jagged, eaten-out edges, tells the rapid work of destruction. Neither are specimens rare, showing the progressive destruction of more various tissues; such as a cancer of the scalp making way by ulceration through the pericranium, skull, and dura mater, and then penetrating deeply into the brain; or one in the integuments of the shin going right through the tibia, and deep into the muscles of the calf." (pp. 6—9.)

The value of these characters is not in the least degree impaired by the fact, that each of them may be absent in tumours having all the other features of malignancy, and that certain of them may be occasionally observed in tumours which in other respects are non-malignant. Such exceptions present themselves in all really natural arrangements of any objects whatever; and it shows a great ignorance of the principles of classification, to build up objections upon them. As well might we say that there is no natural division between birds and mammals, because there are birds without wings, and mammals that possess them;—a bird whose feathers are little more than branching hairs, a mammal whose mouth is furnished with a horny bill like a bird's, and so on. Our fundamental idea of the distinctness of the two classes is not based on these or any other single characters, but on the combination of several; and so it is with regard to the classification of tumours, of which the great majority may be placed without hesitation in one or other of the groups which have been thus adopted, whilst the remainder may probably have definite places assigned to them so soon as their history shall have become better known.

With regard to the differences of origin of the malignant and non-malignant tumours respectively, Mr. Paget thinks it unwise to speculate far; but he considers that we are justified in coming to this general conclusion—namely, that malignant tumours are to be looked upon as local manifestations of some disordered state of the blood, incorporating in their tissues peculiar morbid materials which have accumulated in that fluid, and which their growth may tend to increase. On the other hand, we can only regard the innocent tumours as the result of some inexplicable error of nutrition in the part that they affect, and as being only in the same measure dependent on the state of the blood as are the natural tissues, which require, and may be favoured by, the presence of their appropriate materials of nutrition. The occurrence of tumours after injuries naturally suggests that they may be due to the organization of effused blood, or of some inflammatory exudation, or of the material of repair. But he urges the following objections to such a view:

"1. It is an almost infinitely small proportion of injuries that are followed by the growth of tumours. 2. In a large majority of cases of tumour no injury or previous local disease is assigned, even by the patients, as the cause of the growth. In 100 cases, taken indiscriminately from those I have lately recorded, no local cause whatever could be assigned for the growth of 75 tumours, of which 35 were innocent and 40 malignant; of the remaining 25, referred by the patients to previous injury or disease of the part, 10 were innocent and 15 malignant tumours. 3. Blood extravasated, and the products of the inflammatory and reparative processes are not indifferent materials, such as would pursue this or that direction of development, according to chance, or some imaginary influence exercised on them. They have a proper tendency to assume the form of fibro-cellular, fibrous, or osseous tissue. They do not become, when their history can be traced, either fatty, or perfectly cartilaginous, or glandular tissue, such as we find in tumours. 4. No intermediate conditions have been yet found between blood or lymph and a tumour. And, lastly, all the facts relating to injuries, as favouring or determining the growth of tumours, are explicable on the supposition that the injury impairs for a time the nutrition of a part, and diminishes its power of excluding abnormal methods of nutrition." (pp. 9, 10.)

Putting aside for the present the malignant tumours, Mr. Paget next proceeds to the classification of those which are ranked as innocent; and in the arrangement which he adopts we find no great departure from the usual system. He makes some very appropriate remarks in reply to the objections of those who consider all attempts at classification as futile, in consequence of the occurrence of osculant or connecting forms, such as either exhibit characters *intermediate* between those of two well-marked species, or present such characters *in combination;* and, with reference to the difficulty presented by the diversity in the characters of tumours which must be ranked as examples of one and the same species, he makes the following observations, to which we would specially direct our readers' attention:

"This diversity of characters is, indeed, the great difficulty with which the pathology of tumours has to contend; but the diversity is not to be called inconstancy: it is due to the fact that each tumour has, like each natural tissue, its phases of development, of degenerations, and of diseases. Now we have scarcely yet begun the study of the variations to which, in each of these phases, the several tumours are liable. We may have learned, for example, the general characters of cartilaginous tumours, as they grow in the most favourable conditions; but how little do we know of the various aspects these may present when they fail of due development, or fall into various diseases, or variously degenerate! Yet all these changes have to be studied in the history of every tumour; and it would be as reasonable to charge any natural tissue with inconstancy, because it is altered in development and disease, as to hold that the similar diversity of tumours is an objection to their classification according to their structure.

"However, while I put this aside as an objection against classification, let me not be thought to underrate it as a difficulty; it is the great difficulty with which we have to contend. The work we have to do is not only to distinguish each kind of tumour from all other kinds, but, and in order to this end, to distinguish, as I may say, each kind from itself, by learning in each all the changes occurring in the various stages of its life. The difficulty of such a task cannot be exaggerated, while we consider the rarity of the objects to be studied; but it must be overcome before we can cease to speak of 'anomalous tumours,' and of 'strange distempered masses,' or, which is more important, before we can, even after the removal of a tumour, speak with any certainty of the issue of a case." (p. 11.)

The first subdivision of the innocent tumours is into the *Cystic* and the *Solid.* The two groups, however, are connected by intermediate forms;

for whilst, on the one hand, a solid growth may spring from the inner wall of a cyst, may enlarge more rapidly than the cyst itself, and may thus come to occupy its cavity completely,—on the other, cysts may be formed within solid tumours, and may, by their disproportionate increase, reduce the solid structure to scarcely more than a congeries of cysts, or to one great cyst.

I. CYSTIC TUMOURS may (1) be *simple* or *barren*, containing a mere serous or other fluid, without any trace of organization; and they may be described as serous, synovial, mucous, sanguineous, colloid, seminal, &c., according to the nature of that fluid. Or (2) they may contain organized substances, and may be named as a group *proliferous;* its several members being described according to their contents, as glandular, cutaneous, sebaceous, epithelial, dental, and the like. Some of these, we may remark, approach very closely to "monstrosities by inclusion," which can scarcely be accounted for in any other rational mode than by the hypothesis of their gemmiparous production as abnormal offsets from the parent structure at an early period of its development, and must be considered as imperfect attempts at the production of another individual. Such is the case more especially with certain ovarian cysts.

II. Of SOLID TUMOURS eight specific divisions may be formed, according to the resemblance of their component structure to that of the natural tissues: these are—1. *Fatty;* 2. *Fibro-cellular;* 3. *Fibrous;* 4. *Fibroid* or *Fibro-plastic;* 5. *Cartilaginous;* 6. *Osseous;* 7. *Glandular;* 8. *Vascular* or *Erectile.* Under each of these may be arranged certain varieties, composed of instances which in some uniform manner deviate, without quite departing, from the usual characters,—as the fibro-cystic, fibro-calcareous, and other varieties of the fibrous tumours. And in each assumed species or group, a distinction must be made according to their modes of growth and of connexion with surrounding parts; some among them being only intermediately connected with those parts by a layer of tissue which at once separates and combines them, so as to admit of being enucleated or shelled out from the matrix; whilst others are connected with the adjacent parts by continuity of similar tissue, and thus appear as growths, not in, but of, the parts. Of the former we have a characteristic example in an ordinary fatty tumour; of the latter, in a polypus of the nose or of the uterus, or in epulis. The former are the proper or typical tumours, or "discontinuous hypertrophies;" the latter are rather outgrowths, or "continuous hypertrophies." Here, as elsewhere, however, no definite line of demarcation can be drawn; for although, as in the cases just cited, the distinctions are strongly marked, yet when we come to the tumours of bone and periosteum, and to the erectile tumours, we find that they vanish so completely as to be no longer recognisable.

Commencing his more detailed survey with the *Simple* or *Barren Cysts,* we find Mr. Paget assigning to them three modes of origin. It is freely admitted by Mr. Paget, however, that although we may clearly recognise these three modes as occurring in particular cases, yet that it would not be possible to found any classification upon them; since it appears that either method may lead to a similar result, so that in the more advanced states it will be generally found impossible to say in what manner they originate,—whether by growth of parts once normally formed, or by transformation of elementary and rudimental structures:

"1st. Some are formed by the enlargement and fusion of the spaces or areolæ in fibro-cellular or areolar or other tissues. In these spaces fluids collect and accumulate; the tissue becomes rarefied; and, gradually, the boundaries of the spaces are levelled down and walled in, till a perfect sac or cyst is formed, the walls of which continue to secrete. Thus are produced the bursæ over the patella, and others; and to this we may refer, at least in some cases, the formation of cysts in tumours, and, perhaps, in other parts.

"2ndly. Some cysts are formed by dilatation and growth of natural ducts or sacculi; as are those sebaceous or epidermal cysts which, formed by enlarged hair-follicles, have permanent openings. Such, also, are certain cysts containing milk, that are formed of enlarged portions of lactiferous tubes; such the ovarian cysts formed by distended and overgrown Graafian vesicles; and such appear to be certain cysts formed of dilated portions of bloodvessels shut off from the main streams.

"3rdly. Many—and perhaps the great majority of cysts, such as those of the kidney, the choroid plexuses, the chorion, and the thyroid gland—are formed by the enormous growth of new-formed elementary structures having the characters of cells or nuclei, which pursue a morbid course from their origin, or from a very early period of their development." (p. 12.)

The first two of these modes have long been known and admitted. The third, however, has only been fully understood since the minute structure and the normal history of growth of glandular organs have been elucidated by microscopic investigation; and the discovery of the method in which this perversion manifests itself may be regarded as one of the most interesting examples of the mutual assistance yielded by the conjunction of physiological and pathological inquiry. We are indebted to Mr. Simon for the first sure step in this path; his observations on the origin of cysts in the kidney, as a consequence of sub-acute inflammation, (Medico-Chirurgical Transactions, vol. xxx.,) having been fully confirmed by Rokitansky and other eminent pathologists. There is nothing peculiarly distinctive about such cysts, when they are once fully formed. Their membranous walls usually consist of several layers of fine well-formed areolar tissue, whose fibres are commonly mingled with nuclei or nucleus-fibres; and they are in general rather firmly connected with the adjacent parts, from which they derive the bloodvessels that copiously ramify upon them, so that the cysts cannot easily be removed entire. They are usually, also, lined with epithelium, which seems to be always of the tesselated form, and may consist of either nuclei or nucleated cells; but this, according to Rokitansky, may be wanting in the larger cysts, whose inner layer is a nucleated, structureless, or striated blastema, externally splitting into fibres in the direction of the long axis of the oval nuclei which it contains.

We cannot give a more concise or complete account of Rokitansky's investigations on this mode of cyst-origin, than Mr. Paget has prepared to our hand; and we shall therefore employ his excellent description:—

"In a portion of such a granular and cystic kidney, nests, as Rokitansky calls them, of delicate vesicles, from a size just visible to that of a millet-seed, may be seen imbedded in a reddish-grey or whitish substance. These differ in size alone from the larger cysts to which one's attention would be sooner attracted; and, on the other side, it is simply in size that they differ from many much smaller. For if a portion of such a nest be examined with the microscope, one finds, together with the débris of the kidney, variously diseased it may be, a vast number of vesicles or cysts that were invisible to the naked eye. The most striking of these have a wall consisting of layers of fibres scattered over with curved nuclei, and are filled with granulated nuclei, or, more rarely, with round or polyhedral cells, some

of which may contain a molecular or granular pigmental matter. In many of these cysts, the nuclei or cells are reduced to an epithelial lining of the cyst; and in some even this is absent, and the barren cyst is filled with a clear or opaline adhesive fluid.

"From the size just visible to the naked eye, such cysts vary down to one-five-hundredth of an inch in diameter; and together with these are cysts whose walls (though their contents are like those of the others) consist of a structureless hyaline membrane: and these lie in a stroma which is equally simple, but seems to develop itself gradually into a fibrous structure circumscribing the cysts.

"Moreover, one finds, in the same specimens, structures of the most various sizes, which, except in size, agree completely with the last-mentioned structureless vesicles, and show every grade of size down to that which is just larger than a nucleus. The smallest of these contain a clear fluid, or are slightly granulated: in the larger there is a central nucleus, and to this are added a second, a third, and a fourth nucleus, and so on till there appear several which fill up the commensurately enlarged vesicle. Now in such a nucleus seems to lie the nucleus of the history of development of these autogenous cysts, not in the kidney alone, but in any part in which they may occur. A nucleus grows to be a cyst, whether a simple or barren one, or one that has an endogenous production of nuclei, or cells, or any other structures. And perhaps, in the same specimen, the earlier history of the nucleus itself may be traced; for one sees, together with the nuclei, still smaller corpuscles of all sizes, down to that of the elementary granule, so-called; and these, the larger they grow, the more are they like nuclei; so that, perhaps, the progress of the disease is from these granules, enlarging to the formation of nuclei, and thence pursuing some abnormal course.

"It would be tedious now to trace, from this general sketch of their origin, all the phases through which such cysts may pass. Rokitansky has done it amply. We have here the elementary constituents. But the simple cyst-wall is capable, not only of growing, but of acquiring, by appropriation and development of surrounding blastema, the laminar and nucleated fibrous tissue, which we find in its full estate; acquiring these, we may presume, just as, more normally, the simple membranous wall of a new bloodvessel acquires, as it grows, the nuclei and fibrous tissue that belong to its more perfect state. Such might be the least abnormal course of any cyst: but from this it may deviate; thickening, acquiring continually new layers, calcifying, and in other ways showing the signs of degeneration or disease. The contents, also, of the cyst may assume even yet more various forms: to name only the extremes—they may retain the simple state of liquid; or with liquid there may be a simple, or a specially secreting, epithelial layer; or the contents may acquire the structure of well-organized glands, or cancer, or some other tissue; and between these extremes, according to conditions which we have no power to trace or explain, they may pass in any of the manifold ways of wrong, the ends of which I shall have to describe." (pp. 13, 14.)

We think, however, that Mr. Paget has misinterpreted Rokitansky, in representing him as implying that these cysts take their origin in an element which is altogether new, in opposition to Mr. Simon's doctrine that they are to be regarded as "vesicular transformations of the ultimate structure of the gland," to which Mr. Paget himself adheres, and, as we conceive, upon adequate grounds,—justly remarking, that unless a cyst were really a transformation of a nucleus, or a cell, of the part in which it grows, we could not understand the very general similarity that we find between the contents of certain cysts, and the secretions or structures of the glands in or near which they occur. But so far from opposing this view, Rokitansky seems to us fully to concur in it; for although he speaks of these cysts generally as "new substantive formations with a peculiar elementary basis," we understand him simply to mean that they do not owe their origin to metamorphoses or new developments of pre-formed

structures, but arise *de novo* from tissue-germs. And that he entertains the idea of the identity of these tissue-germs with those of the part in which the cyst is formed, appears to us to be manifest from his description of cystic bronchocele, of which we shall cite the essential part as a pendant to the account given of this affection by Professor Porta, (see British and Foreign Medico-Chirurgical Review, vol. vii., p. 101.) The essential type of this affection, says Rokitansky, consists in a cystic development of the gland-vesicles, and a simultaneous development of new vesicles by the side of those which normally constitute the gland, so that they are not only augmented in size, but also multiplied. He considers that while the lobes are sometimes formed by the enlargement of an acinus, consequent upon the development of new glandular vesicles and the hypertrophy of the surrounding fibrous tissue, each of them ordinarily has for its basis a glandular vesicle enlarged to a cyst, in which an endogenous new formation of glandular substance takes place. The section of a recent lobulated bronchocele shows a number of cysts, in which are found the characteristic glandular vesicles of the substance of the thyroid, in various stages of development. Some cysts contain none of this endogenous production, whilst others are entirely filled by it; but at last the whole cyst is filled, and becomes a new lobe, in which the process of lobular formation is repeated. The cysts may, however, be sterile—that is, may not develop new broods of vesicles in their interior; and they may then be filled with serous fluid, with fat, with colloid,—or a single nucleus may develop itself into a cell, which may enlarge to such a degree as to fill the parent cyst; within this, again, another vesicle may form, which may enlarge in its turn; and thus each cyst may come to have several concentric layers in its walls. This explanation is also applied by Rokitansky to the cysts found in the prostate and kidney; and we cannot but regard him, therefore, as having most fully adopted the view, that these cysts have their origin in the normal elements of the glandular tissues, which have taken on themselves an abnormal mode of development.

The same view may be applied, as Mr. Paget remarks, to many solid tumours; for as the cyst is traced from the mere nucleus, or even from the granule, onwards to its extreme of size or complexity of structure or contents, so, it is very probable, from the numerous correspondences between them, that these solid tumours also have had a similar beginning in some detached element, or tissue-germ, which in their development and growth may coalesce, and may then appropriate, or exclude for absorption, the intervening substance; and this view is strengthened by the correspondence which prevails between the texture of many such tumours and that of the parts with which they are connected, but from which they deviate more and more widely as they grow, both in shape and size.

But even when we have got to this length in our explanation of the origin of certain cysts and tumours, we are still as far as ever from the explanation of the cardinal point in their pathology, *their continual and independent growth;* and we can only point to it as an abnormal manifestation, in the higher animals, of that power of forming new and independent parts, which is normally characteristic of the lower, and which even proceeds in them to the entire separation of a complete and self-dependent being.

The first species of simple cysts described by Mr. Paget, is the *Gaseous*. Of this he is acquainted with but one example, in the specimens placed by John Hunter in his museum; but these, he remarks, are worthy of particular admiration, almost of veneration, for their histories include the honourable names of Hunter, of Jenner, and of Cavendish:

"Mr. Hunter says of them,—'I have a piece of the intestine of a hog, which has a number of air-bladders in it. . . . It was sent to me by my friend, Mr. Jenner, surgeon, at Berkley, who informed me that this appearance is found very frequently upon the intestines of hogs that are killed in the summer months. . . . Mr. Cavendish was so kind as to examine a little of this air; and he found 'it contained a little fixed air, and the remainder not at all inflammable, and almost completely phlogisticated.'' . . . .

"What a relic have we here! Surely, never, on an object so mean to common apprehensions, did such rays of intellectual light converge, as on these to which were addressed the frequent and inquiring observation of Jenner—the keen analysis by Cavendish—and the vast comparison and deep reflection of John Hunter! Surely, never were the elements of an inductive process combined in such perfection! Jenner to observe; Cavendish to analyze; Hunter to compare and to reflect!" (p. 15.)

In striking contrast with the rarity of the foregoing, is the frequency of the *Serous* cysts, which are, of all the order, the most abundant. Although there is scarcely a part in which they may not be found, yet they are so much more commonly met with in or near the secreting glands or membranes, or in the so-called vascular glands, that some have held them to be all examples of perverted epithelial or gland-cells—a supposition which is negatived, however, by the fact of their occasional occurrence in such parts as bones and nerves, among deep-seated muscles, and in the midst of fibrous tumours. Mr. Paget selects for particular description, on account of their surgical importance, the cysts of the neck and those of the mammary gland; in both of which some variety of origin may be traced. Thus a considerable proportion of the serous cysts of the neck appear to spring from the thyroid gland, where they are generated, in the manner described by Rokitansky; although their connexion with that organ may be subsequently obscured. Others appear to be transformations of vascular tumours—i. e., of erectile growths or nævi; a view which is rendered probable by the close connexion which some of these cysts have with large deep-seated veins, by the occasional opening of bloodvessels into their cavities, and by their sometimes distinctly forming portions of vascular nævi, as in the following case:

"A child, three and a half years old, was under Mr. Lawrence's care, two years ago, in St. Bartholomew's Hospital, with a tumour covering the greater part of the left side of the neck, and the lower part of the cheek. Such a swelling had existed from birth, but it had of late enlarged very much. It was composed of a cluster of close-set cysts, containing coagulable fluid; but at its upper part a firmer portion of its mass consisted of a collection of tortuous and dilated blood-vessels, like those of a nævus. The examination made of it, by Mr. Coote, after its removal, was such as to leave little doubt in his mind that it had origin in or with a nævus growth." (p. 16.)

To this point Mr. Paget subsequently returns in his account of erectile tumours, where he refers to some additional cases recorded by Mr. Cæsar Hawkins and Mr. Coote, and cites their explanations of the production of such cysts in nævi or in connexion with veins. It is of considerable

practical importance to arrive, if possible, at an accurate diagnosis of such cases, as the opening of these cysts has frequently occasioned death by hæmorrhage, sometimes venous, sometimes arterial. Like the erectile tumours themselves, these complications are nearly always congenital.

In no organ is the formation of serous cysts (or of cysts in general) more frequent than in the mammary gland; and in none is it surgically more important. That some of these cysts are dilated *ducts*, or portions of ducts grown into the cyst form, is put beyond doubt by their proved continuity with the lactiferous tubes. But Mr. Paget agrees with Mr. Birkett in thinking that the majority of cysts in the mammary gland are, like most renal cysts, dilated glandular vesicles. This seems especially obvious in those cases in which the whole gland is filled with them; and it is an interesting point of resemblance between the mammary and the renal cysts, that although the proper substance of the gland which forms their matrix may be quite healthy, it is more commonly contracted and partially indurated, like the shrivelled and contracted substance of the kidney, in which the renal cysts are so commonly lodged, or perhaps more nearly resembling the shrivelled indurated condition of the lung that may coincide with dilatation of the bronchi or of the air-cells. Although the coincidence of such cystic growths with cancer is not rare, yet there is no reason to consider them as having in themselves a malignant nature. Still, the diagnosis between cystic disease and cancer is not always clear, and many breasts have been removed in this uncertainty, which might have been properly left. The following case is adduced by Mr. Paget as an example of this difficulty of diagnosis.

"A woman, fifty years old, had, in her left breast, just below the surface of the mammary gland, a small, smooth, oval, and moveable tumour. It felt firm, but not hard; but external to it, in a line extending towards the axilla, were two or three small round 'knots,' scarcely so large as peas, and quite hard. In the axilla was an enlarged gland. The breast was soft, flaccid, and pendulous. The tumour was sometimes painful, and a serous and bloody fluid often flowed from the nipple. The patient's youngest child was sixteen years old, and the tumour had been noticed six months, having arisen without evident cause. There was doubt enough about the diagnosis of this case to suggest that the tumour should first be cut into. An incision exposed the cavity of a cyst full of dark, turbid, greenish fluid, and near it many more cysts. Similar cysts pervaded the whole extent of the gland, and the whole breast was therefore removed. Many of the cysts communicated with lactiferous tubes, from which bristles could be passed through the nipple." (p. 18.)

The ground for operation in this case we presume to have been the multiplicity of the cysts, giving rise to the apprehension that the opening of the principal cyst would be followed by the enlargement of some of the others: we can scarcely think that the enlargement and induration of the lymphatic glands could fairly weigh down the scale in favour of removal, if the purely cystic nature of the disease was clear. In some of these cases, the removal or laying open of one of the large cysts has been sufficient to effect a cure; but in other instances the smaller cysts that have been left behind have enlarged, and the recurrence of the disease has necessitated fresh operative interference. Serous cysts of the mammary gland may attain a very large size, and this within a short time: Mr. Paget mentions a case in which nine pounds of limpid serosity were produced within three months, in the breast of a woman thirty years old. It would

appear as if any cyst of the mammary gland may, after some time of existence in the barren state, become proliferous, bearing on its inner surface growths of glandular or other tissue.

Of *Synovial* cysts, including all the abnormal bursæ or ganglions, it will be sufficient to state that of those which originate *de novo*, some appear to be formed by the widening of the spaces in areolar tissue, and by the subsequent levelling or smoothing of the boundaries of these spaces; whilst others appear to be cystic transformations of gland-cells. To the former category may be referred the bursæ which form anew in parts subjected to occasional localized pressure; whilst to the latter seem to belong those which form about the sheaths of tendons at the wrist. Into these sheaths it is well known that the synovial membrane projects in fringes, the cells of which have a glandular character; and the similarity of these to the cells of the villi of the choroid plexus, to which Rokitansky has shown that the cysts which this plexus commonly includes may be with certainty referred, together with the likeness of the contents of the cysts, in the one case as in the other, to the fluids secreted by the fringes in their normal state, are certainly strong arguments in favour of such a view.

The cysts which Mr. Paget designates as *Mucous* seem to belong to the general category of those which have their origin in the elements of glands; their peculiarity being that they occur in glands whose normal product is a mucous secretion. The best examples of this kind are furnished by the Nabothian glandulæ of the cervix uteri, and by the glands of Cowper or Duvernoy near the orifice of the vagina. The former may either protrude alone, or may carry with them polypoid outgrowths of the mucous membrane; and after they have burst and discharged their mucous contents, they may be replaced by others following the same course. The latter are very apt to inflame and suppurate, and to give rise to abscesses in the walls of the vagina; and such abscesses require free incisions, such as may effectually evacuate the contents of the cysts. Cysts having their origin in some of the mucous glands of these parts, sometimes form large swellings in the labia.

The *Sanguineous* cysts are probably in many instances nearly related to the serous; and the presence of blood in them is frequently accidental, arising from their connexion with an erectile tumour, or from the formation of a communication with a vascular trunk. In such cases, however, the blood which they contain is generally in part or wholly coagulated; and there are certainly, in Mr. Paget's opinion, cysts which have sanguineous contents from their very origin, the blood in these being fluid, though coagulating after it is drawn. This distinction is one of much interest, as indicating that these cysts exert some such *vital* influence on the blood they contain, as is elsewhere expended in the elaboration of secreted fluids, or in the production of solid tissue in their interior. Several interesting cases of the occurrence of sanguineous cysts in different parts, are related by Mr. Paget. In the first, in which the cyst lay at the side of the neck, very near the surface, as many as sixteen ounces of fluid blood were drawn off by puncture, after which the cyst closed. This favourable result, however, cannot be generally anticipated; and Mr. Paget appears to consider that the complete removal of the cyst is to be preferred, the cavity of a parotid cyst which he had punctured having filled again with

fluid similar to that which had been drawn off. One peculiar variety of sanguineous cysts appears to be derived from a dilated portion of some vein, which has gradually been separated by the constriction of its neck, and completely cut off from the stream of blood; such an one was removed some years ago by Mr. Lloyd, from the neighbourhood of the saphena vein; and although neither that nor any other vein could be traced into the cyst, yet its venous character was manifested by two valves resembling those of veins, found upon its inner surface.

Cysts containing *Oily* or *fatty* matter, without any more highly-organized substance, are very rare; although fatty matters are frequently found in combination with cyst-contents of a different kind, and are probably to be then regarded as originating in the degeneration of these. We would suggest whether the *proper* oily cysts may not be enlarged fat-cells, their contents, in the two cases cited, being precisely the same as those of the adipose vesicles.

To the term *Colloid* cysts, no very definite meaning can at present be assigned; the term colloid being applied by Frerichs, Rokitansky, and other recent German writers, to "all those morbid materials that are pellucid, jelly-like, flickering, half solid, or more or less resembling the material found in gelatiniform, alveolar, or colloid cancer." In the enumeration which they give, however, it is evident that they include matters which have nothing in common with true colloid but their consistence, such as the contents of ranulæ and of many bursæ; and it is obvious that further observation is required for the more precise separation of the malignant from the non-malignant forms of colloid growths.

The last group of simple cysts includes those of which the contents are *secreted fluids*, like those of the glands from whose dilated ducts or transformed elements they spring. Such are the cysts in the breast that contain milk, and probably many instances of ranula. But some of the most interesting examples of this group are furnished by the *seminal cysts*, including under this name those that are usually called encysted hydroceles, or hydroceles of the spermatic cord. Our readers will recollect that the discovery was made at the same time, and independently of one another, by Mr. Lloyd and Mr. Liston, that the fluid obtained from these cysts usually contains spermatozoa; and the proof was first afforded by the dissections of Mr. Paget, that the cysts had no open communication or other connexion with any part of the secretory apparatus of the testicle, and that their relation to the epididymis, on which they lay, was such as to forbid the supposition of the seminal secretion being transmitted to them from the tubes. It was by him that the idea was first suggested, that they are to be regarded as independent glandular elements; and, though at first deemed improbable, this view has subsequently come to be confirmed by so much evidence of an analogical kind, besides receiving support from the fact that the presence of spermatozoa in these cysts is not an occasional but an almost constant occurrence, that it can scarcely now be set aside. It must, however, be borne in mind, that in some specimens of these "hydroceles of the cord," even when spermatozoa may have been found in them at other times, the fluid may be merely serous. This diversity is common among other glandular cysts, and it seems to have some reference to their period of growth. For, as Mr. Paget remarks—

"When it happens in different periods of the same cyst, it illustrates the general rule that, in the course of time, cysts are apt to degenerate, and to produce less and less highly organized substances or secretions. This degeneration does not take place in any certain time; but generally the larger a cyst grows, the less organized are its products; as if nearly all the formative force were expended in growth, and little remained available for secretion. Generally, also, the longer a cyst has lived, the less organized are its products. However, these rules are only general; and I met with a remarkable exception to them, last year, in a seminal cyst which had existed for many years in a man more than seventy years old. I withdrew from it eighteen ounces of fluid, laden with seminal filaments, and no fresh accumulation took place." (p. 22.)

It was believed by Mr. Lloyd that in some cases he had obtained fluid containing spermatozoa from hydroceles of the tunica vaginalis; and this belief has been confirmed by the post-mortem examination of a case after death, in which the fluid of the hydrocele, repeatedly drawn off, had always contained abundant seminal filaments. The hydrocele was found to present no unusual appearance, except that the inner surface of the tunica vaginalis was rendered uneven by a few small depressions or pouches; whence it may be inferred that near the surface of the testicle there were minute secreting cysts, which by dehiscence discharged their contents into the cavity of the tunica vaginalis.

The essential character of the group of cystic formations to which Mr. Paget applies the designation PROLIFEROUS, consists in the power which they possess, of developing, by endogenous growth, regularly organized and even vascular structures in their interior. We agree with him in considering the term "proliferous" as superior to that of "compound," which is usually applied to these growths; because the latter does not suggest the difference between the cysts with endogenous growths, and those which may appear equally compound, though they are only simple cysts clustered or grouped together—a difference which should be clearly marked in names, as it is generally so in nature, although intermediate conditions here also occasionally present themselves.—What has been said of the *origin* of the barren cysts applies also to that of the proliferous; and the differences between them have reference entirely to the nature of the intra-cystic productions.

The first group includes those which have other cysts growing in or upon their walls; most characteristic examples of which are presented by the complex ovarian cysts, and by the "hydatid mole" or cystic degeneration of the chorion. The history of the former of these has been already so carefully worked out by Dr. Hodgkin, that Mr. Paget passes them by with no more than an allusion to some characteristic examples of their typical forms contained in the Museum, and with a remark upon their multitudinous diversities of form, size, and mode of aggregation. Dr. Hodgkin's doctrines, however, are based upon naked-eye observations; and do not give us any clear insight into the mode of generation of endogenous cysts; this point has been more fully elucidated by Dr. Mettenheimer's recent microscopic study of the constituent cysts of the "hydatid mole," whose true nature, as a cystic degeneration of the chorion, was first pointed out, we believe, by Professor Simpson, many years since, this discovery having been one of the earliest foundations of his subsequent fame.—The general characters of this disease are well known.

A part, or even the whole, of the chorion is covered with pellucid vesicles with limpid contents, borne on long, slender, and often branching pedicles; these cysts are usually either oval or pyriform; they may be either simple, or may bear others projecting from their walls; and these walls may be clear, or may have minute opaque dots. The following is Mr. Paget's account of Dr. Mettenheimer's observations, and of the conclusions which they seem to justify:—

"Dr. Mettenheimer has found that the minute dots besetting these cysts are villous processes, exactly resembling those of the natural chorion, and growing from the walls of the cysts, either outwardly or into their cavities. In these villi he traced the development of cysts. In their natural state, they may be described as filiform or clavate processes, often branching and bearing bud-like projections, and composed of dimly-granular substance, in which are imbedded minute nucleated cells. In this cystic disease, vesicular bodies may be seen scattered among the cells in the villi, which bodies are distinguished from the cells by their pellucidity, their larger size, and double contours; but, from the cells to these, every gradation may be so traced as to leave scarcely a doubt that the vesicles are derived from cells deviating from their normal characters. Thus in some of the cells the contents are seen lighter and less granular; in some they have entirely disappeared, without increase of size; and then, when their contents are thus become uniformly pellucid, and they have acquired the character of vesicles, the cells appear to grow, while their walls become stronger, and they acquire such a size that they are recognised as very small cysts, similar, in all but their size, to those which are visible to the naked eye.

"Now, though this formation of cysts has been traced by Mettenheimer only in the villi which grow on the cysts themselves, and therefore, so to speak, only in the production of the cysts of the second and later generations, yet there can be little doubt that the first cysts in the diseased chorion are formed in its own villi after the same manner. For the villi borne on the cysts are, in all essential characters, like those natural to the chorion; and the cysts of all generations are equally like. The whole process may, therefore, be probably thus described:— Certain of the cells in the proper villi of the chorion, deviating from their cell-form, and increasing disproportionately in size, form cysts, which remain connected by the gradually elongated and hypertrophied tissue of the villi. 'On the outer surface of the new-formed cysts, each of which would, as it were, repeat the chorion and surpass its powers, a new vegetation of villi sprouts out, of the same structure as the proper villi of the chorion. In these begins again a similar development of cysts; and so on *ad infinitum*.' Each cyst, as it enlarges, seems to lead to the wasting of the cells around it; and then, moving away from the villus in which it was formed, it draws out the base of the villus, which strengthens itself, and forms the pedicle on which the cyst remains suspended.

"Such is the account of the minute structure and formation of the cystic disease of the chorion; and perhaps no instance could afford a better confirmation of the production of cysts by the enormous growth of elementary cells, or a better type of the capacity of cysts thus formed to produce structures resembling those in the abnormities of which themselves originated. A similar capacity is among the characters of all the cysts of which I shall next have to speak." (pp. 24, 25.)

The second group of proliferous cysts consists of those which put forth vascular growths from their free surfaces; and in by far the most numerous class of cases, these growths are of a nature exactly comparable to that of a secreting or a vascular gland. Such cysts form part of the group to which the name of "sero-cystic sarcoma" was given by Sir B. Brodie. Of late years the exact nature of the endogenous growth has been clearly ascertained by the assistance of the microscope; and we are now no longer obliged to have recourse to the vague generic term *sarcoma* as a cloak for

our ignorance. As already pointed out, one form of bronchocele consists of proliferous glandular cysts developed in the substance of the thyroid gland; but it is in the mammary gland that this kind of growth is most common and most characteristic. In some instances, the cyst merely contains lobated glandular masses projecting into its cavity, the remainder of which is occupied by serous fluid; but in other cases the solid growth may completely fill the cyst, which then simply forms an investment to it; and its increase may even take place at a greater rate than that of the cyst, so that it bursts its walls, and even protrudes through the integument like a hernia of the brain, growing afresh, too, like such a hernia, when it has been cut away. This last stage, in fact, is often extremely rapid, even when the earlier ones have been very slow; the progress of the growth being frequently quicker than that of most cancerous growths. Such appears—especially from the observations of M. Lebert and Mr. Birkett—to be the origin of the proper mammary glandular tumours, the "chronic mammary tumours" of Sir A. Cooper; and Mr. Paget fully confirms the statements of these investigators, by his own observations on a well-marked case of the kind, in which the tumour had commenced more than thirty years previously, remained like a small knot for twenty-six years, and then slowly increased, until, at the end of five years, a red fungous mass protruded from the breast, bled freely sometimes, and discharged profusely, increasing quickly, and being moreover painful. The whole breast was removed by Mr. Lawrence, and Mr. Paget found that the tumour contained a multitude of proliferous cysts, with glandular endogenous growths in various stages of development.

It is not in all soft intra-cystic growths, however, that glandular structure may be recognised. Mr. Paget has examined three cases in which the contents of the cyst were a gelatinous blastema with no distinct fibres, but having nuclei and cells imbedded in it, which chiefly presented the forms of developing areolar texture like those in granulations or in the lymph of inflammatory exudations. Nearly similar and equally indecisive observations have been made by Mettenheimer and Bruch; and Mr. Paget considers it to be a not improbable surmise, that, as a general rule, the vascular intra-cystic growths have a tendency to assume the glandular structure, but that in such instances as the above they fall short of it, or swerve from the right course. He wisely contents himself, however, with throwing out the hint; deeming it unwise to draw a definite conclusion as to a class of bodies, of whose changes during development, degeneration, and disease, we are almost completely ignorant. It would be well if all pathologists would imitate his caution.

Similar glanduliferous cysts are met with in other glands, as the prostate; and they sometimes occur in other situations, as the lip or the labium, where they may be suspected to have originated in a sebaceous or mucous gland. But Mr. Paget states that their characters may be closely imitated by those of cysts formed in parts altogether disconnected from secreting glands; and he gives three cases of this kind, in two of which the proliferous cyst was situated between the gracilis and adductor longus muscles, whilst in the third it appeared to have grown in the subcutaneous tissue of the neck. The contents of these tumours, however, were not distinctly glandular, but rather resembled those of the softer growths found in the mammary cysts last adverted to; and we would suggest

whether the cysts may not have had their origin in some minute absorbent glandule, or may not have budded-off from a lymphatic, like the sanguineous cysts formerly noticed as budding-off from veins.

A very important practical point connected with these glandular cysts, is their frequent tendency to recur after removal; this tendency being sometimes so inveterate, as to be scarcely surpassed even by that of malignant tumours. In the first of the cases cited by Mr. Paget, which occurred in the practice of Mr. Lawrence, a "sero-cystic sarcoma," or "glandiferous cyst" of large size, was removed from the breast, together with a great part of the gland itself. After fifteen months, another tumour began to grow beneath the scar; and Mr. Lawrence removed this also, with all the surrounding tissues, and (it is believed) with the whole remainder of the mammary gland. About seven months after this second operation (during which interval the patient had fully regained her stout robust appearance) a third tumour appeared, and then a fourth; these also were removed by Mr. Lawrence; but the patient died of erysipelas supervening on the last operation. The removed growths were carefully examined by Mr. Paget, who could discover no trace of cancerous structure in them; their whole character being that of glandular structure, apparently less perfectly developed, however, in the second tumour than in the first. Sir B. Brodie has related a case of single re-appearance of such a tumour; but two cases of much more frequent recurrence are cited by Mr. Paget; one, communicated to him by Mr. Birkett, in which five operations were performed, the last in June 1850, since which time the patient has remained quite well; and another recorded by M. Lesauvages, in which seven operations were performed, but in which the disease still recurred, and the patient died. On these cases Mr. Paget remarks—

"Unfortunately no examination of such a case has yet been made after death; so that it is not possible to say whether the more characteristic features of malignant disease existed, such as the concurrence of similar disease in internal organs. Mere repetition of growth, I need hardly say, does not justify us in calling a tumour malignant; especially in the instance of a tumour in which clusters of cysts are prone to grow together; for the repetition of growth may be due merely to the peculiarity of the cysts growing in succession, and not, as they usually do, together and commensurately." (p. 30.)

But although we are not to consider this strongly-marked tendency to recurrence as in itself an evidence of malignant character of the cystic tumours which present it, yet it must be borne in mind that their mode of growth may be imitated by that of genuine cancerous diseases.

"Cancerous growths may be found in cysts under at least two different circumstances—namely, in cysts that of themselves appear innocent, and in cysts produced within cancers.

"Of the former mode of growth we have examples sometimes in ovarian cysts. Well-marked cancerous growths, usually of the medullary or the alveolar form, may grow from their walls; and herein is the best—perhaps the only unexceptionable—instance of the transformation of an innocent into a malignant tumour.

"The second mode of production of intra-cystic cancers is best shown in some examples of medullary tumours of the testicle. In these we may see a repetition, so far as the outline plan is concerned, of the intra-cystic production of thyroid gland. The great mass of the medullary disease includes smaller masses, completely incapsuled with fibro-cellular tissue, and commonly presenting a lobed and laminated form, at once reminding us of the intra-cystic mammary glandular

growths, and justifying the application to them of the principles of Dr. Hodgkin's theory of the growth of cancers. In these medullary testicles, the intra-cystic medullary growths have usually filled the cysts and coalesced with their walls." (p. 30.)

The last group of proliferous cysts is that in which Skin, Hair, or Teeth, more or less perfectly formed, present themselves in their interior. In some of the *cutaneous* cysts, all the textures have been so fully developed, that the lining of the cyst has completely imitated the true skin. It is very interesting to observe, however, that the *most perfect* of such cysts either occur in the ovaries, or if presenting themselves in other parts, are congenital; and that when they occur anywhere but in the ovaries in later life, the structures they contain are far less complete, their character, however, being shown by the fact, that when laid open to the air, they do not granulate, but assume for their internal surfaces the characters of the adjacent and now continuous skin. That some of the cutaneous and piliferous cysts have their origin in ordinary sebaceous or piliferous follicles, appears probable, not only from their situation, but also from the fact that some of them may be emptied by pressure through a small aperture by which they still communicate with the cutaneous surface. But the greater part of them are closed on all sides alike, so that they must be regarded as cysts new formed; and such cysts sometimes, though more rarely, occur in situations where neither hair-follicles nor sebaceous glandulæ normally present themselves, as in the testicle, lung, kidney, bladder, and brain.—Among the contents of these cysts, extreme varieties may be observed; among which the following are worthy of particular notice:

"1st. We find successive productions of epidermis, formed in layers on the inner walls of the cyst, and thence successively shed, and pushed inwards towards its centre. A section of such cysts (which were particularly described by Sir Everard Home from the Hunterian specimens) presents layers of white soft epidermis, like macerated epidermis of the heel or palm. The external layers are commonly quite regular; but the internal are more disorderly, as if broken up and mingled with less organized productions.

"2ndly. A peculiar appearance is given to contents like these, where, among the layers of epidermal scales, abundant crystals of cholestearine are mingled. They hence derive an appearance like that of the masses to which Müller has given the name of cholesteatoma, or laminated fatty tumour; and, indeed, the few instances of well-marked examples of this disease which I have been able to examine, as well as Müller's own account, make me think that what he named cholesteatoma is only a combination of layers of epidermal scales with crystals of cholestearine. The appearance produced by such a combination is quite peculiar. It forms nodular masses of soft and brittle substance, like wax or spermaceti, the surfaces of which present a bright glistening, like that of mother-of-pearl, while their sections are finely laminated. It is a rare disease; the most frequent seats of well-marked specimens appearing to be in ovarian cysts, and in connexion with the membranes of the brain. The characters are well shown in the contents of a small ovarian cyst in St. Bartholomew's Hospital; and in the tumour within the occipital part of the cranium, in Mr. Hawkins's Collection, to which I have already referred. Striking examples are figured by Cruveilhier, but the want of microscopic examination leaves their constitution uncertain.

"3rdly. In the opposite extreme to these cysts, in which the cuticular product is most perfect, we find an innumerable variety of contents, of buff and ochre yellow, and brownish materials, that seem to consist mainly of degenerate cuticle mingled with sebaceous secretions. The microscope finds in them a confused mass of withered scales, of granular fatty matter, clustered and floating free, of cholestearine

crystals, and of earthy matter in free molecules, or enclosed within the cells or scales. And all these may be floating in liquid, or retained in some soft tenacious mass, or clustered in hard nodular and pointed masses, projecting like stalactites from the old cyst-walls." (p. 32.)

It is not uncommon for cysts of this kind, when subcutaneous, to ulcerate, and for the contents to protrude; and such is liable to occur as a consequence of local inflammation, which seems to have its origin in a general derangement of health. In regard to operative interference with such, therefore, that caution is particularly necessary, which should be observed with respect to all tumours—namely, to abstain from operating when the general state of health is unfavourable; and Mr. Paget candidly narrates, for the warning of his auditors, a case which occurred to himself about three years previously, whose unfortunate issue seemed traceable to the violation of this rule—an operation for the removal of an ulcerated sebaceous cyst from the abdomen of a strong but very intemperate man, having been followed, first by hæmorrhage, and then by abscesses with phlebitis, under which the patient at last sank.

Of the cysts containing *teeth*, those which occur in and about the jaws are probably to be considered as tooth-capsules, from which the teeth have not been extruded, and which therefore remain, becoming filled with fluid, and growing larger. It seems to us probable that, when these occur as superadditions to the regular teeth, they have their origin in that continued gemmation of the normal tooth-sacs, which gives origin to the succession of teeth both of the mammalia and the lower vertebrata, and which, though usually limited in the former, is occasionally prolonged, as in the latter, giving rise to supernumerary teeth, or to the production of a third set. When not thus occurring in the neighbourhood of the teeth, dentigerous cysts are most commonly found in the ovaries; and when perfectly-formed teeth occur in cysts elsewhere, their production seems usually to have been congenital. The perfection of structure in the teeth occurring in ovarian cysts is very remarkable. Professor Owen describes in his Odontography a tooth from an ovarian cyst, in which the enamel, dentine, and cementum, were all completely formed, and exhibited their ordinary relations of position; and we ourselves possess a similar specimen. The shape of these encysted teeth is more or less irregular, but it may generally be referred to one or other of the ordinary types. Here, then, as with respect to the cutaneous and piliferous cysts, we may remark, with Mr. Paget, that "the great formative power which they manifest is consistent with their occurring only in embryo or fœtal life, and in the ovaries, in which, independent of impregnation, one discovers so many signs of great power of development;" and we believe that it would not be difficult to bring together such a continuous series of cases, beginning with dentigerous and piliferous cysts, and ending with complete embryoes, as would show that they must all be considered to have the same origin, and to be expressions of the same kind of power,—the simple cyst being thus a kind of rude attempt at the production of a distinct individual,—and the encysted embryo found even in a male subject being but the result of an unusually high development of a proliferous cyst.

For the reasons already stated, we shall not follow Mr. Paget through his account of the SOLID TUMOURS with nearly the same minuteness that

we have bestowed on his history of the Cystic; and we shall confine ourselves to a selection of those points in which some particular novelty either of fact or of opinion is brought forward.

The characters of *Fatty Tumours* are so well understood and so generally known, that we need not stop to dwell upon them; but in adverting to the capsular investments with which they are covered, Mr. Paget gives an account of their texture which we think worth quoting, as being equally applicable to the investments of most innocent tumours:

"The capsule, then, of such a tumour is usually a layer of fibro-cellular tissue, well-organized, dry, and containing bloodvessels proportioned to the size of the tumour. It appears to be formed of the fibro-cellular tissue of the part in which the tumour grows, increased and often strengthened in adaptation to the bulk and other circumstances of its contents. It grows with the tumour, invests it, and at once connects it with the adjacent tissues, and separates it from them,—as, e. g., similar fibro-cellular tissue does each muscle in a limb. Its adhesion to both the tumour and the parts around it is more intimate than that of its layers to one another; so that when such tumours are cut into, they may be dislodged by splitting the layers of their capsule, and leaving some of it on the tumour, and some in the cavity from which the tumour is extracted. This, at least, can be done easily, unless the tumour has been the seat of inflammation, which may thicken the capsule and make all its parts adherent to one another, and to the tissues on either side of it.

"In the capsule, the bloodvessels that supply the tumour usually first ramify. One principal artery, indeed, commonly passes straightway into the tumour at its deepest part, but the rest branch in the capsule, especially in any thicker parts of it that lie in the spaces between projecting lobes of the tumour. Hence, with the partitions of the tumour that are derived from the capsule, the bloodvessels pass into its substance." (p. 35.)

Under the designation of *Fibro-cellular*, Mr. Paget comprehends a group of tumours "which in their minute structure and general aspect resemble the fibro-cellular, areolar, or connective tissue of the body." Examples of these tumours have been described under various names by Mr. Lawrence, Mr. Cæsar Hawkins, Müller, Vogel, and Rokitansky; but these passing references have not obtained for this kind of tumour a general recognition, and in many works it is altogether overlooked. We shall therefore cite Mr. Paget's account of these tumours in full; first remarking, however, that as *outgrowths* such aggregations are common enough, most of the softer kinds of polypi and condylomata being composed of a tissue resembling the fibro-cellular (or areolar) in a condition of more or less complete development:

"The form in which the fibro-cellular tumours are most frequently seen, is that of oval or round masses of soft, elastic, close, and pliant tissue, smooth and uniform, or, when they grow among yielding parts, deeply and variously lobed. Their exterior surface is connected with the adjacent parts by a capsule of fibro-cellular tissue, which generally splits readily. When handled, they feel peculiarly tense and elastic; their outer surface may shine like a thin sac full of fluid. On their sections we see opaque white bands, intersecting a shining succulent basis-substance, of serous-yellow or greenish-yellow tint. Through this basis the bands course in circles or wavy lines, or form complete partitions; or, in the smaller lobes of the tumour, they run without order, only forming white marks on the yellow-ground colour, but giving no appearance of grain, or of regularly fibrous structure.

"The peculiar yellow colour of the basis-substance of these tumours makes them look, at first, like fat; it is due, however, not to fat, but to a serous or synovial-

like fluid, which is infiltrated through the substance of the tumour. The mass is just like anasarcous cellular tissue; most of all like the subcutaneous cellular tissue of the back, as one sees it dissected in a dropsical body. When such a tumour is cut through or sliced, the clear yellow fluid oozes from it, or may be abundantly pressed out; while the filamentous tissue, contracting, becomes denser and more compact, and more uniformly opaque white, like that of the softer varieties of fibrous tumour. It is to these last-named tumours, indeed, that the fibro-cellular have the nearest relations, and into them that they 'pass' through gradational specimens; but there is just the same difference, as well as just the same relation, between these kinds of tumours, as there is between the natural fibro-cellular and fibrous tissues, and there is a similar propriety in distinguishing them.

"Examined with the microscope, the fibro-cellular tumours display the filamentous tissue or appearance characteristic of that after which they are named. In many cases, or in many parts, parallel, soft, undulating filaments are found collected in fasciculi, which interlace, and from which single filaments can often be traced out; or, where this is not seen, the texture looks filamentous, through markings or wrinkles of its surface. The best developed and most filamentous tissue is in the intersecting white bands; but similar tissue is present everywhere.

"The homology of these tumours, in respect of tissue, is just as perfect as that of the fatty tumours. In chemical analysis they may yield gelatine from the well-formed fibro-cellular tissue; but I believe they yield much more albuminous matter from their imperfectly developed tissue, and from the serous fluid that is soaked in them.

"In general, there is nearly complete uniformity through the whole mass of one of these tumours; oftentimes, however, different portions are more or less œdematous (if I may so call them); and, which is more remarkable, portions of cartilage, sometimes partially ossified, may be found in them." (pp. 37, 38.)

The most frequent seats of these tumours appear to be the scrotum, the labium, or the tissues by the side of the vagina, the deep-seated intermuscular spaces in the thigh, and the scalp; they sometimes attain an enormous size, and become very inconvenient from their bulk and weight. Their special occurrence in the scrotum and labium renders it desirable to point out their main differences from the pendulous tumours of those parts which are known under the name of elephantiasis; and these differences are stated by Mr. Paget as follows:

"1st. These fibro-cellular tumours may be separated or enucleated from the tissues among which they lie; whereas the cutaneous growths have no definite boundary, but are continuous with the proper tissue of the scrotum, or labium, or nympha: in short, the two diseases have the common differences between tumours and outgrowths; the overgrowing is in the one case continuous, in the other discontinuous. 2nd. In the growth of the fibro-cellular tumours, the surrounding parts, including the skin, or the mucous membrane, grow in adaptation to the tumour, but often defectively, or, at the most, only normally; but in the cutaneous outgrowths, all the tissues take part, and the proper tissue and appended organs of the cutis are as much exaggerated as the fibro-cellular substance of the scrotum or other part. And 3rdly. In the tumours, fibro-cellular tissue is the highest form attained, or, at most, a small quantity of elastic tissue is mingled with it; but, in the outgrowths, all the component structures of the skin and subcutaneous tissue are increased.

"The two diseases are thus different. Still, the fact is significant, that the parts most liable to the cutaneous outgrowth are also those in or near which the fibro-cellular tumours most frequently occur; and it may be noted, that among those parts in which fatty tumours are most rare, the fibro-cellular are the most common." (p. 40.)

Occasionally, though rarely, fibro-cellular tumours occur in other parts than those previously named. Mr. Paget mentions one case in which such

a tumour was developed in the sole of the foot; another in which it occurred within the tunica albuginea of the testicle; and a third in which he himself removed one from the orbit. With regard to the history of these growths, he says:

"They have been found, I believe, only in or after the adult period of life, and in persons with apparently good general health. Their causes are wholly unknown: their development appears to be, in most cases, like that of many examples of natural fibro-cellular tissue, through nucleated blastema: for though in some I have found abundant cells lengthening and attenuating themselves into fibres, as in the organizing of lymph or granulations, yet these may have been formed from exuded lymph.

"The growth of these tumours is quick, in comparison with the average rate (so far as we can roughly estimate it) of innocent tumours. They often enlarge very quickly; but this enlargement is probably not growth, but swelling, through increase of the œdematous effusion: (and this difference between growth and swelling may be usefully remembered in the diagnosis of many tumours.) The growth is usually painless; but about the vagina is apt to be too rapid for the superjacent tissues. Its possible extent is very great. I have mentioned one tumour of forty-four pounds weight, and another of twenty-four pounds, which was still growing.

"Of the degenerations and diseases of these tumours nothing has been yet observed, except the sloughing and suppuration that occurred in one of the cases I have mentioned; and, as to their nature, all that has been said implies that they are completely innocent." (p. 41.)

The well-known *painful subcutaneous tumours*, so admirably described, nearly forty years ago, by Mr. William Wood of Edinburgh, may be considered as usually belonging to this class, although their tissue may sometimes be more strictly fibrous, or even fibro-cartilaginous. The special characteristic of these tumours, whatever may be their structural diversity, is the extraordinary intensity and peculiarity of the pain which is experienced in them; and this is the more remarkable, as the careful examinations made by Dupuytren and Mr. Paget himself have failed to detect their nervous connexions, save in one instance in which the tumour was within the sheath of the nerve; whilst, on the other hand, there are, as our readers will recollect, abundant examples of the development of enormous tumours in the course of nerves, with little or no pain. Again, the subcutaneous tumours themselves are often painless for a long time, and then become extremely painful without any apparent change. From these and other considerations, Mr. Paget comes to the conclusion (and, as we think, with good reason) that the peculiar pain experienced by the patient is rather neuralgic or subjective, than dependent upon any peculiarity in the structure or relations of the tumour,—"that it has the tumour, indeed, for an exciting cause, but that it owns, besides, some morbid condition inherent or cumulative in the nerves themselves, so that at times they respond, with a morbid exaggeration, to an habitual or slightly increased stimulus." The pains of many other tumours are probably, in a greater or less measure, of the same nature; and it is very important to bear in mind, that the "darting or dragging" pain of certain tumours in the breast, distinguished by Sir A. Cooper as "irritable," may be simply neuralgic, and is not in the least degree pathognomonic of cancer. It is especially liable to occur in connexion with those tumours which resemble the ordinary substance of the mammary gland; and Mr. Paget gives a very apposite case from his own practice, showing the importance of caution in

the diagnosis. With regard to the expectation of benefit from the removal of "painful subcutaneous tumours," it is satisfactory to be able to state that the operation is usually completely successful, the pain being removed with the tumour, and neither being apt to return, though a reproduction of the tumour *has* occurred in a few rare instances. These tumours seldom attain a larger size than half an inch in diameter; and they have no tendency to ulcerate, or to assume any of the peculiar characters of malignant disease. Mr. Paget states that they are four times as frequent in women as in men, and hence draws an additional argument in favour of the neuralgic character of the pain; we would question, however, whether any such disproportion exists in regard to the existence of the small subcutaneous tumours themselves, for we have often noticed tumours having all their characters, except their painfulness, in men whose attention was not attracted to them; and we surmise that it is only the *painful state* of these tumours, which is so much more common in the female sex.

We next pass on to the proper *Fibrous Tumours*, whose chief characteristic is the likeness of their substance to the natural fibrous or tendinous tissue of the body. The type of these tumours is found in the fibrous tumour of the uterus—a production so common that, according to Bayle (whose estimate is confirmed by Mr. Paget), it is to be found in twenty per cent. of the women who die after thirty-five years of age. But fibrous tumours may occur in almost any part of the body; and they present great variety in consistence, in vascularity, and in the arrangement of their elements. They present, also, considerable diversities in the degree of completeness with which the fibrous structure is developed; and this being a point of some importance, as will presently appear, we shall quote Mr. Paget's account of his microscopic analysis:

"In all, I believe, a large portion of the mass consists of tissue resembling the tendinous or fibrous, more or less perfectly developed, and variously arranged. This is the case in all parts of the tumour—in the more homogeneous basis-substance, as well as in the intersecting bands; the microscopic differences between these parts consisting, I think, only in the less or more regular arrangement of the fibrous structure or fibrous appearance of the tissue. But in different specimens, or even in different parts of the same, the tissue appears less or more perfectly formed; so that, while in some, distinct filaments or undulating fasciculi may be dissected out, in others there is rather a fibrous appearance than a fibrous structure. Commonly, too, one finds nuclei or cytoblasts strewn through the substance of the tumour; the less abundantly, I think, the more perfect is the fibrous character of the tissue. But in all these respects there are not, I think, more or other differences among fibrous tumours than might be found in a series of natural fibrous tissues." (p. 46.)

Other elementary tissues are sometimes found mingled with the proper fibrous structure. Thus muscular fibres are occasionally found, especially in uterine tumours; and well-developed elastic fibres are sometimes intermingled, so as to give to the substance of the tumour something of the character of a fascia. Osseous and cartilaginous formations, again, sometimes occur in them; and generally, it may be stated, that the common characters of fibrous tumours are usually modified towards an imitation of tissues in or near which they are severally placed. Besides these varieties, there are two which must be considered as resulting from degenerations—namely, the formation of cysts, and the deposit of calcareous salts in the fibrous substance. With regard to the mode in which these cysts originate,

Mr. Paget does not seem to have made up his mind; and with regard to the mode in which the calcifying process takes place, he is less clear than we should have wished to find him. He seems to consider that the salts of lime and other bases are deposited *interstitially*, occupying the place of fibrous tissue which has degenerated. All that is known of the process of ossification, however, both normal and abnormal, leads us to the belief that the calcareous and other salts are actually *combined* with the gelatinous substance of the fibre; and we would suggest further inquiries on this point. The substance formed is not true bone, that is, it does not possess the characteristic system of lacunæ or canaliculi: but does it not resemble, on the one hand, the osseous substance in which these hollows and channels are excavated; and on the other, does it not correspond with that of the abnormal "ossifications" that occur in the walls of the arteries, the dura mater, and other normal fibrous textures? We apprehend that in all these cases the calcifying process is essentially similar; and it is no proof to the contrary, that the proportion of the mineral ingredients in these calcified tumours is extraordinarily large (as much as eighty-one and a half per cent. in a specimen transmitted to John Hunter as a calculus), and that some appearance of crystalline arrangement is to be discerned in them. Similar facts are true of calcified textures among some of the lower animals, which are pretty certainly formed by the union of calcareous salts with a matrix of organic fibres; and it may be everywhere remarked, that where *carbonate* of lime is the principal calcifying substance, it is present in much larger proportion to the gelatinous base, than the phosphate ever is.

The *subcutaneous* fibrous tumours are specially noticeable on account of these two peculiarities:—1st, that they very commonly present a low grade of fibrous development, sometimes even "seeming composed of an uniform blastema, with imbedded elongated nuclei, like the material for the formation of new tendons";—and, 2nd, that if in any degree irritated, they soon protrude through the skin, and form vascular masses (sometimes termed fungous growths), which bleed profusely, in a manner not imitated by any other innocent tumour.

As a general rule, fibrous tumours occur singly; but to this rule there are two remarkable exceptions,—those of the uterus, and those of the nerves, which are frequently multiple. Their rate of growth seems to be generally slow; but they not unfrequently attain a prodigious size, sometimes weighing even more than seventy pounds. As we shall presently see, their tendency to return cannot yet be precisely specified; for whilst the larger proportion of true fibrous tumours show no disposition to recurrence after removal, this disposition does manifest itself most obstinately in the case of certain tumours, which cannot be distinguished by any definite structural characters from those properly forming this class.

Under the term *Fibro-Plastic Tumours*, M. Lebert has described a group of which the types appear to be perfectly distinct from those of any other form, although its boundaries and relations are scarcely yet susceptible of perfect definition. The fibro-plastic tumours, which have been heretofore grouped under the general term sarcomatous, bear a slight resemblance both to the fibro-cellular and the fibrous, in their general appearance and usual history; but they are composed of a texture bearing a certain likeness to that which occurs in ordinary granulations, exhibiting

nucleated cells in various stages of metamorphosis towards rudimental fibrous tissue. Thus they bear very much the same relation to the tumours just now spoken of as consisting of a nucleated blastema in process of development into fibres, that the granulation-structure has been shown by Mr. Paget to bear to the new material laid down for the reparation of a subcutaneous wound.* The following are the ordinary histological constituents of these tumours:

"1. Cells of oval, lanceolate, or angular shapes, or elongated and attenuated like fibro-cells, or caudate-cells, having dimly dotted contents with single nuclei and nucleoli.

"2. Free nuclei, such as may have escaped from the cells; and among these, some that appear enlarged and elliptical, or variously angular, or elongated towards the same shapes as the lanceolate and caudate cells.

"3. As the most peculiar form,—large, round, oval, or flask-shaped cells, from $\frac{1}{300}$ to $\frac{1}{1000}$ of an inch in diameter, which contain from two to ten or more oval, clear, and nucleolated nuclei, imbedded in clear substance. These are 'parent cells,' or 'brood cells,' such as one may find sometimes in actively growing granulations, and such as exist among the essential structures of the thyroid gland.

"All these forms of corpuscles lie indiscriminately placed in a dimly granular substance, with abundant granular matter and free nuclei; or else, a material thus composed is traversed by filaments, and bundles of fibro-cellular tissue, and bloodvessels." (p. 53.)

The most characteristic of these elements, the multi-nucleated cells, are comparatively rare in granulations; and herein the texture of these tumours differs from that of ordinary granulations. The fibro-plastic tumours may be found in a great variety of situations, and may present a great variety of forms, their shape being determined in part by the surrounding conditions; their general tendency, however, is towards the spherical. They are usually separable without difficulty from the surrounding tissues, and are frequently limited by a well-defined fibro-cellular investment. The most characteristic examples of these tumours are of firm, fleshy consistence; but even these are brittle, being easily crushed or broken, instead of being tough or elastic, like the fibro-cellular or fibrous tumours. Others, however, are softer in several gradations, to the consistence of size-gelatine. The most characteristic feature which they present to the naked eye, consists in the blotchings of the greyish white or greenish basis-colour of their cut surface, with spots of dark or livid crimson, or of a brownish or a brighter blood-colour, or of a pale pink: the extent of this blotching may vary considerably, however, the ruddiness being sometimes confined to a few points while the general mass of the tumour is pale, and being sometimes diffused more faintly through the whole substance. Like other fibroid tumours they may contain cysts, or scattered osseous particles.

Having thus limited, so far as the present state of his knowledge will permit, the characters of the species, Mr. Paget describes its natural history, from his own observations and those of M. Lebert. On this, however, it is to be remarked, that the cases yet observed are too few to justify many general conclusions; and that M. Lebert's knowledge of the history of his cases after the extirpation of the tumours, appears to have been very slight. From the existing data, however, it appears that these tumours usually occur singly, and most commonly in youth, or before old

* See his 'Lectures on the Processes of Repair and Reproduction after Injuries,' and British and Foreign Medico-Chirurgical Review, vol. v. p. 419.

age; that they generally grow slowly and without pain; and that they occur without any known cause, such as injury or hereditary predisposition. They do not seem prone to any particular degeneration; and do not *usually* show any tendency to return after their removal. But Mr. Paget relates two cases, in which tumours that presented the distinctive histological characters of this species did recur, and cause death; and in one of these, the original tumour being seated near the angle of the lower jaw, four small masses of similar substance were found in the lungs, and a similar material was diffused through one cervical gland. And he concludes the narrative with the following remarks:

"Now, in both these cases, and especially in the last, the whole history of which seems full of anomalies, there were certainly such features of dissimilarity from the usual general characters of the fibro-plastic tumours, that, although the microscopic characters appeared identical, yet they are not enough to prove even the occasional malignancy of the disease: but they are enough to make us cautious—enough to induce us to study this disease very carefully, as one of those that may, in different conditions, or in different persons, pursue very different courses,—appearing in some innocent, in others malignant. The use of such terms as 'semi-malignant,' 'locally malignant,' 'less malignant than cancer,' and the like, in relation to growths of this kind, involves subjects of singular interest in pathology, as well as in practical surgery; but at present it may be well to form no conclusive opinion upon them. I can scarcely doubt that certain tumours, presenting, in all apparent structure, the same characters, may, in different persons, appear 'innocent' or 'malignant:' but respecting the grounds of these differences, I can as yet scarcely offer a suggestion. At present I would rather doubt than adopt any general conclusion on the questions herein involved." (p. 55.)

Now it certainly seems to us quite possible, that if the existence of an "innocent" tumour must usually be attributed to a local perversion of nutrition, whilst that of a "malignant" tumour is an expression of a constitutional taint, a manifestation of a morbid element in the blood, it is quite possible for the two conditions to coexist in various degrees; so that a tumour which has its origin in an otherwise "innocent" excess of tissue-formation in a particular spot, should have imparted to it *some* of the characters of malignancy by a taint in the blood, which might not be sufficiently strong to occasion the production of a proper "malignant" tumour. And this we imagine to be Mr. Paget's own view of the case; at any rate, it accords completely with the following interesting observation, which he appends in a note to the foregoing passage:

"Only, I think I have known cases making it probable that the children of a cancerous parent may be the subjects of tumours which may be like innocent tumours (such as the mammary glandular) in their structure, but may resemble cancers in a peculiar rapidity of growth, a proneness to ulceration and hæmorrhage, and an aptness to return after removal." (p. 55.)

This statement, if confirmed, would be so important in a practical point of view, and would throw so much light upon the scientific questions which the distinction between malignant and non-malignant tumours involves, that we would specially direct the attention of our readers to the subject; in the hope that Mr. Paget's cautiously-worded *thought* may either be made certainty, or that it may be set aside as groundless.

Closely connected in their general appearance and minute structure with the preceding, but still more decidedly tending towards the strictly malignant growths in their obstinate recurrence, are two groups, to the

first of which Mr. Paget gives the name of *Recurring Fibroid Tumours;* a designation which is intended to imply that its characteristic distinction must be considered as lying, not so much in any of the anatomical features of these growths, as in their physiological history. This is illustrated by two cases whose progress was watched by Mr. Paget himself, by a third furnished by Professor Gluge, by a fourth and fifth recorded by Dr. Douglas Maclagan, by a sixth described by Mr. Syme, and by a seventh carefully examined by Professor J. H. Bennett. These seem to prove the existence of a group of tumours resembling the ordinary fibrous tumours in their general aspect, though bearing a closer conformity to the fibro-plastic in their histological nature, save in the absence of the many-nucleated cells; but distinguished from proper "innocent" tumours by their tendency to recurrence after removal, and in the worst extremity, to protrusion and ulceration like a malignant growth (which was particularly noticeable in the successional tumours in Mr. Syme's case); whilst they are also distinguished from "malignant" growths by the absence of those other characters, such as general cachexia and the similar affection of the neighbouring lymphatics or of distant parts, which in them coincide with this local recurrence. As to the interpretation of this singular proneness to recurrence, we shall let Mr. Paget speak for himself; premising that the idea that it can depend upon the incomplete removal of the tumours, which some might suggest, is quite inadmissible.

"Two views may be taken of the facts. 1st. The tumours may, from the first, be formed of a cluster or group, and then the removal of one of them only leaves the remainder to continue their growth; or, 2ndly, the apparent recurrence may be a real one, such as we suppose occurs in the case of cancers; in which we presume that, in a first operation, every morbid structure already formed in a part is removed, and entirely new growths are produced in the same part.

"The former view is supported by whatever of resemblance exists between these and fibrous tumours, whose proneness to multiplicity is remarkable; and by the fact, that sometimes, after the removal of one of these, two, or a more numerous group, have appeared in the same part. Yet the objections to this view appear to me more weighty. If we suppose, in any case in which six or seven tumours have been removed in succession from the same part, in as many years, that all began to grow at or about the same time, the last of these ought, according to the rate of growth of the rest, to have come into view much sooner. If the second tumour were not discernible in the first operation, where, or of what size, was the sixth? —or why did it require six or more years to come to the same bulk as the supposed coeval second tumour acquired in one year? It may be added that some of these tumours appear to have recurred in the substance of a scar left after a former operation—in a tissue, therefore, which did not exist at the time of the previous operations. Nor must we overlook, in connexion with this apparent aptness to recur, the fact that the later-formed of these tumours may assume certain characters of the thoroughly malignant growths which were not observed in the earlier. In one of the cases I have seen, the last tumour was, in general aspect, hardly to be distinguished from brain-like tumour, though, in microscopic characters, essentially like its predecessors. In one of Professor Gluge's cases, the transitions to completely malignant characters appeared yet more sure. Mr. Syme also expresses a similar transition; describing, as the usual course of these cases, that, after one or two recurrences of the tumour, the next new productions present a degeneration of character, excite pain, proceed to fungous ulceration, and thus in the end prove fatal. So that, although there be cases in which this evil career has not been run, yet I think we may regard these tumours as approximating to characters of malignancy, not only in their proneness to recur after removal, but in their

aptness to assume sometimes the more malignant features the more often they recur. Whatever be the truth concerning the supposed transformation of an innocent into a malignant morbid structure, I think it cannot be doubted that, in the cases of some recurring growths, the successively later growths acquire more and more of the characters of thoroughly malignant disease." (pp. 57, 58.)

Still stronger features of malignity are presented by the group to which Mr. Paget has applied the designation of *Malignant Fibrous Tumours.* These seem to resemble, both in general and in microscopical characters, the ordinary fibrous tumours, but differ from them, not only by manifesting a tendency to recur in their original locality but also by frequently appearing in internal parts remote from it. Two well-marked cases of this kind are related by Mr. Paget, in which the purely fibrous nature of the growths was proved by microscopic examination; and he adds—"I have little doubt that others might be added from the cases of tumours of the jaws and other bones, believed from their general appearance to be fibrous, yet pursuing a malignant course." These are clearly distinguishable from the growths termed by Müller and others *Carcinoma fibrosum;* for whilst the former may conform in all particulars of structure and composition to the most perfect types of fibrous tumours, the latter are "infiltrations" of the affected organs, including cancer-cells with their fibrous tissue, and further distinguishable by its peculiar hardness and stiffness.

An extremely elaborate account is given by Mr. Paget of *Cartilaginous Tumours;* a group which, on the whole, admits of being very clearly marked out, by the histological correspondence between the growths it includes and the normal cartilage-tissue. It is interesting to remark, however, that the resemblance of the cartilage-substance of tumours is closer to that of *fœtal* than to that of *adult* normal cartilage; and this circumstance seems to us in some degree to explain the singular fact to which Mr. Paget draws attention at the very outset of his lecture, that although cartilage may thus abnormally grow, as a constituent of tumours, even to a very large size, its normal productive power in man is so slight, that it is not formed in a perfect manner, for the repair either of its own injuries or of those of bone. It is further remarkable, that notwithstanding the general conformity of these growths to a common type, there is a very large amount of minute variation, not only between different tumours, but between different parts of the same tumour, so that the most diverse forms may be seen side by side in the field of the microscope. This diversity, remarks Mr. Paget, "has its parallel, so far as I know, in no other innocent tumour; and the cartilaginous tumours form the single exception to a very generally true rule enunciated by Bruch—namely, that it is a characteristic of malignant tumours, and a distinction between them and the innocent, that they present, even in one part, a multiformity of elementary shapes." We regret that our space will not allow us to extract Mr. Paget's interesting account of his own observations upon this point; and we must limit ourselves to a notice of the observations which have been made by Professor Müller, Mr. Quekett, and himself, upon a certain form of nuclei with irregular outlines, which deserve a particular description, both because they have not yet been found in the normal cartilage of any vertebrated animal, and because their imitating in some

measure the forms of bone-corpuscles might wrongly suggest that they have an important or a constant relation to the ossifying process.

"The phases of the transformation by which they are produced appear to be, that a nucleus of ordinary form, or with one or more oil-particles, extends itself in one or several slender, hollow, and crooked processes, which diverge, and sometimes branch as they diverge, towards the circumference of the cell. Such nuclei may be found within the cells, or within cavities representing cells whose walls are fused with the intercellular substance; but much more commonly it appears as if, while the nuclei changed their forms, the cells and the rest of their contents were completely fused with the intercellular or basis-substance, so that the nuclei alone appear imbedded in the hyaline or pale fibrous substance. Moreover, although, at first, as we may suppose, the nuclei, as they send out their processes, may retain the round or oval form of their central parts or bodies, yet they afterwards lengthen and attenuate themselves, so as to imitate very closely the shapes of large bone-corpuscles; or they elongate and branch, or shrivel up; and in these states, lying in groups, they have the most fantastic appearances. In these various states the nuclei are often only loosely connected with the basis-substance; so that they are easily removed from it, or are found floating on the field of the microscope.

"Now, as I have said, corpuscles like these occur in no normal cartilage yet examined in man or any of the vertebrata. If, then, heterology of structure were indicative of malignancy, the tumours that contain these corpuscles should certainly be malignant; but there is no single fact to make it probable that they are so, and every presumption is in favour of their being all as innocent as the tumour on the great toe in which I found them.

"The only natural cartilage yet known as possessing these corpuscles is, I believe, that of the cuttle-fish; and it is at least interesting, and it may be importantly suggestive, to observe that the morbid structure, deviating from all that is natural in its own species, conforms with that of a much lower creature.

"As to the meaning of these changes of the nucleus,—some like these may be preparatory to ossification, and the metamorphosis of the cartilage-nucleus into a bone-corpuscle; but I believe all I have myself examined were degenerations without reference to ossification. We may believe the nuclei to be changed by a process of degeneration on many grounds; such as (*a*) the fact already mentioned of their likeness to the nuclei of lower cartilages; (*b*) their likeness in shape to ramified pigment-cells and bone-corpuscles which have probably lost all power for their own nutrition; (*c*) the frequent coincidence of more or less fatty degeneration in the nuclei thus changing; (*d*) the usual coincidence of the fusion of the cell-wall and contents with the basis-substance of the cartilage, and the loosening of the nuclei; (*e*) the gradual shrivelling or wasting of the nuclei after the assumption of the stellate form." (pp. 62, 63.)

Mr. Quekett has actually seen the ossifying process ensuing in the substance between these stellate nuclei, and gradually enclosing them, so that they assume the appearance of bone-corpuscles; so that we must accept this as one of the modes in which bone may be formed. But, as Mr. Paget has heretofore remarked, the conversion of normal cartilage into bone must be regarded as in some sort a degeneration; so that its occurrence in abnormal growths need not lead us to look upon this peculiar change in the nuclei in any different light.

In regard to the physiological history of these tumours, we must confine ourselves to a notice of a few important facts. *First*, their occasional very rapid rate of growth; in this respect simulating malignant growths, for which under certain circumstances they may be mistaken, so that an operation may be abstained from as hopeless, which might very

probably have been successful. Of this Mr. Paget gives a very striking example. *Secondly*, their occasional attainment of an enormous size; scarcely falling short of fibrous tumours in this particular. *Thirdly*, their occasional tendency to recurrence after removal, either in the same or in some other situations, with increasing manifestations of malignity, just as in the case of the two preceding groups of Fibrous tumours.

The true cartilaginous tissue, moreover, is not unfrequently found in combination with other textures. As already mentioned, the production of osseous substance in cartilaginous tumours may be considered as a part of their ordinary history; and this more especially when they are developed in connexion with bone. But in the cartilaginous tumours growing in or near the parotid or submaxillary glands, it is not at all uncommon to meet with what appears to be a perverted or imperfect glandular tissue. As we might expect, too, from the frequent association of cartilaginous and fibrous tissue in normal structures, such an association frequently presents itself in tumours; the whole mass having sometimes a regular fibro-cartilaginous structure, whilst in other cases the two tissues remain separate, although forming part of one growth.—But a very important fact in regard to the pathology of tumours of this group, is the frequent blending of cartilaginous and medullary cancerous elements in the same growth; and this happens especially in the testicle, where the two structures are sometimes found quite isolated from each other, but in other instances more or less closely blended. And one case is recorded by Mr. Paget (the preparations being in the Museum of St. Thomas's Hospital), in which a genuine and apparently unmixed cartilaginous tumour having been removed from a man's ribs, another tumour, formed of closely mingled cartilage and medullary substance, appeared in the same spot within three months afterwards, and quickly proved fatal. "I need hardly remark," says Mr. Paget, "on the bearing which this last case may have on the question of the recurrence of cartilaginous tumours, and on that of the changes of character which may ensue in tumours generally, at their successive occasions of recurrence. It gives to all these cases a much higher interest than would attach to them if regarded only as rarities and strange things."

Osseous Tumours may be formed, like normal bone, by the calcification of a matrix either of a cartilaginous or of a fibrous substance. This appears to have been distinctly made out; and it would further appear that every variety of bone-texture, from the solid ivory-like masses, of which some tumours are composed, to the light cancellated substance covered with a thin shell of bone, of which others are formed, may be generated in either mode. It is comparatively rare for osseous tumours to present themselves in any other situations than as connected with bones; and, as already pointed out, there is no really natural line of demarcation between osseous tumours and exostoses, or outgrowths of bone. There is a practical advantage, however, in adopting an artificial distinction between them; that being considered as a tumour, whose basis of attachment to the original bone is defined, and grows, if at all, at a less rate than the outstanding mass; whilst the outgrowths not of the nature of tumours are not only ill defined, but have widely-spread bases of attachment, which are increased by addition in greater proportion than are their

heights or their whole masses. It is of course in regard to the former alone, that there can be any hope of benefit from operative interference.

Having already noticed the tendency to the multiple and symmetrical development of osseous tumours, which occasionally presents itself, the only point to which we shall advert in detail, is the distinction between the proper *osseous* and the *osteoid* tumours, the latter being sometimes hard, and sometimes containing more or less soft texture, so as to be ranked under the vague designation of *osteo-sarcomatous.*

"The distinctions usually to be observed between these hard osteoid and the hard osseous tumours are mainly in these particulars:—(*a*) the osteoid bone is in its mid-substance like chalk, the osseous like ivory, the one dull and powdery, the other bright and wholly void of friability; (*b*) the osteoid is new bone infiltrated, as it were, in some softer tissue, or in the tissues of the original bone, which disappear as it increases; the hard osseous tumour is a distinct outgrowth, attached in a comparatively small part of its extent to the bone on which it grows; (*c*) the outer surface of an osteoid growth is porous and rough, and, if laminated, its laminæ have their edges directed outwards; while the outer surface of a hard osseous tumour is smooth and compact, and, if laminated, the surfaces of its laminæ are directed outwards; (*d*) lastly, the minute characters of bone are far less perfect in the osteoid than in the osseous growth; bone-corpuscles existing indeed, but small, round, irregular, with very small, if any, canaliculi, and embedded in a porous, chalky-looking, basis substance.

"And 2ndly, for distinction between the softer osteoid growths (with which we may class the osseous skeletons of medullary cancers), on the one hand, and the cancellous osseous tumours on the other, we may chiefly observe that (*a*) the osteoid tissue and the bone of cancers are more dry and friable than the cancellous bone of the tumours; and (*b*) the osteoid and the bone of cancerous growths have no medulla, the interspaces between their laminæ being filled with cancerous matter; while medulla is a constant constituent, I believe, of all the cancellous osseous tumours.

"Such are the chief differences generally to be observed between the bone of innocent and that of malignant tumours; differences which it is well to establish, since the fact is sufficiently confusing, that any normal tissue should be formed in subordination to the growth of cancers." (p. 79.)

Of *Glandular Tumours* it is not requisite that we should say much, since there seems a strong probability that they are all of intra-cystic origin, and are, therefore, to be considered as ulterior developments of proliferous cysts, whose contents have grown faster than their walls (*see* p. 12.) The most frequent examples of these glandular tumours are those which occur in or near the mammary gland; the "chronic mammary" tumour of Sir A. Cooper, the "pancreatic" tumour of Mr. Abernethy, the "fibrous tumour of the breast" of M. Cruveilhier. Mr. Paget confesses that he has himself, in the Catalogues of the Museums of the College of Surgeons and of Bartholomew's Hospital, classed these tumours with the fibro-cellular; that tissue being very abundant in the specimens which he had examined, so that he took too little account of the glandular element which they contained. It is chiefly to the investigations of M. Lebert, and to the independent and contemporary observations of Mr. Birkett, that we owe our present more exact knowledge of the real nature of these growths. Both these pathologists apply to them the designation of "Imperfect Hypertrophy of the Mammary Gland;" but, as Mr. Paget justly remarks, all innocent growths are imperfect hypertrophies

in the same sense as these growths are; and if the designation "tumours" is to be withheld from them, it will be very difficult to say what *are* tumours. These tumours vary greatly in their consistence, in the relative proportions of fibro-cellular and of glandular tissue, and in the degree in which the latter approaches the normal structure; and they differ also in their rate of growth, the general (but by no means invariable) rule being, according to Mr. Paget, "that the tougher any tumour is, the slower has its growth been, and the more it has of fibro-cellular mingled with its glandular tissue; whilst the more succulent and vitreous it is, the less perfectly is its glandular tissue developed, and the more rapid is the growth." Equal variations exist as to pain. Commonly these tumours are painless; but sometimes they are the seats and sources of intense suffering, equal to that which is popularly ascribed to cancer, but which cancer in its early stages so very rarely presents. Most of the cases of the "irritable tumour" described by Sir A. Cooper, to which allusion has already been made, seem to have been of this description. The glandular mammary tumour most commonly occurs in young unmarried or in barren women; their beginning often seems connected with disordered menstruation; and it is a most curious feature in their history, that they sometimes disappear on marriage, pregnancy, or lactation, as if the supervention of the "continuous hypertrophy" necessary for the discharge of increased function of the gland, had remedied the "discontinuous hypertrophy" which constituted the tumour. Since the delivery of his lectures, Mr. Paget has seen a specimen—the only one with which he is acquainted—of mammary glandular tumour in the male.

A question arises with regard to the pathology of these tumours, which presents itself with increased force and importance after what has been already stated in regard to the supervention of the features of malignancy on apparently innocent fibrous and cartilaginous growths. Do these glandular tumours ever present a tendency to recurrence? Do they ever themselves undergo cancerous degeneration? Or does their presence predispose the gland to the supervention of malignant disease? To the two last questions, Mr. Paget thinks that a decided negative may be given, so far as the facts at present known justify *any* reply; in regard to the first, he says—"Several may grow in the same breast at the same or successive times; but I have not heard of more than three, either at once or in succession."

Tumours sometimes occur in the lips, having the general characters of gland-substance; whilst in other cases they more resemble those tumours already noticed, as frequently lying over the parotid, and as composed of mingled cartilaginous and glandular tissue. The only case of this kind which Mr. Paget has found upon record, is one noticed by Mr. Lawrence; but he has himself seen two others, of which one seemed purely glandular, whilst the other contained osseous tissue in the midst of the glandular; and he has met with a specimen in the Museum of St. George's Hospital, in which, in one tumour, a cyst and what looks like one of these glandular growths are combined. We are indebted to Rokitansky for the knowledge that what had been previously considered fibrous tumours of the prostate are in reality of glandular structure; to this point, however, we shall advert elsewhere.

The last group of solid tumours is that now generally described as

Erectile, under which head are included the various forms of "vascular nævi," and of "aneurism by anastomosis." The length to which our review has already extended, forbids our following Mr. Paget through his account of these growths; but this we the less regret, as he confesses that he has been able to add little to what was previously known regarding them. We shall only cite the following remarks, in regard to the peculiar features which the anatomical structure of these tumours, and their mode of growth, present for our consideration:

"Chiefly, they present the singular instance of the apparent primary growth of bloodvessels. In all other tumours, as in all anormal products, the formation of bloodvessels appears to be a consequent and subordinate process; as in the natural development of parts, so in what is morbid, organization to a certain point precedes vascularity, and the formation of bloodvessels follows on that of the growths into which they pass. But here the case appears reversed: the calibre of the bloodvessels increases, and the solid tissues between them diminish; all the growth of an erectile tumour is an enlargement of bloodvessels, with diminution of the tissues in which they ramified; or, rather, it is often an enlargement not of bloodvessels but of blood-spaces: for though, in the first stages of the disease, the walls of the vessels may grow, thickening and elongating so that the vessels become tortuous, yet after a time the walls waste rather than grow; apertures seem to form through mutually apposed bloodvessels, and at length, while the blood within the tumour increases, the bloodvessels containing it diminish together with the parts in which they ramified. Hence, at last, in place of branching and anastomosing tubes, there is only a network formed of the remains of their walls. This is therefore an increase of blood-spaces rather than of bloodvessels; so far as solid tissue is concerned, we might call it a wasting rather than a growth; no new materials seem to be added, but step by step the bloodvessels are dilated, and the intervening tissues clear away, leaving room for more and more blood.

"Such a fact is, I think, at present quite inexplicable; and it constitutes a great difference between these and any other diseases named tumours." (p. 87.)

Of the production of cancerous disease in the tissue of erectile tumours, Mr. Paget remarks:

"It seems to be generally regarded as a frequent event, and these are commonly believed to afford the most frequent instances of malignant growths supervening on such as were previously innocent. I will not doubt that such events have happened. Especially, in one case recorded by Mr. Phillips, the transition appears to have been very clearly traced. Yet, I think that in many of the cases which have gained for erectile tumours their ill-repute, a clearer examination would have proved that they were, from the beginning, very vascular medullary cancers, or else medullary cancers in which blood-cysts were abundantly formed." (p. 88.)

In thus concluding our notice of these valuable Lectures, we must add the Postscript, in which, with all the modesty of true merit, Mr. Paget justifies the publication of them, imperfect as he feels them to be. How much is it to be wished, that other observers and generalizers would exercise similar caution, and perform their task with the same scrupulous conscientiousness!

"The frequent confessions of ignorance and of imperfect knowledge contained in the foregoing pages may have suggested to readers, as to myself, that it would have been better not to publish these lectures. Yet, the necessity of such confessions may justify, in some measure, the publication, while much of the imperfection of our pathology of tumours is due to the rarity of the opportunities of studying some among them. Even in the field of a large hospital, one may pass years without an occasion for investigating certain of the points which it is most desirable to determine. In seven years I have been able to collect

complete records of nearly three hundred cases of tumours, and to illustrate most of them with microscopic and other sketches. From such materials the statements I have ventured to make have been derived. But such materials are very insufficient. For example, as I have stated in the last lecture, I have had no good opportunity of examining an erectile tumour; of some others I have seen only one or two instances in the recent state; and some tumours, whose characters as described by good pathologists I cannot doubt, I have never yet seen.

"A good end, therefore, may be served by the publication of the lectures, if I have only shown where our knowledge is most imperfect, and where it may be readily improved if others will engage in the necessary inquiries, or will supply with more ample materials those who are engaged in them. While wishing for such help, I will not omit to thank many who have already given it; and especially my colleagues at St. Bartholomew's, whose cases I have been allowed to study and to publish, with all the advantages of their assistance." (p. 88.)

ART. II.

A Practical Treatise on the Management of Diseases of the Heart and of Aortic Aneurism, with especial reference to the Treatment of those Diseases in India. By NORMAN CHEVERS, M.D., Civil Assistant-Surgeon, Chittagong, Bengal.—*Calcutta,* 1851, pp. 145.

DR. CHEVERS informs us that the present work is a sequel to his Essays on the Diseases of the Vascular System, which have from time to time appeared in the 'Guy's Hospital Reports' and in the 'Medical Gazette.' There is much judicious matter in the work; but we think the author would have done better if he had treated the diseases in detail, instead of clubbing all diseases, inflammatory and non-inflammatory, old and recent, together. The plan of the work is rather straggling, and from its want of method it would not be easy to discover the exact case we might be searching for, had we occasion to wish to know Dr. Chevers' opinion on any given point. We shall, however, analyze the treatise as fully as we can, since we are persuaded there is much in it that will interest our readers. But if our analysis is rather desultory, that is not our fault; Dr. Chevers' arrangement must bear the blame.

Dr. Chevers very justly remarks, that in the treatment of heart-diseases, we have always to take into consideration the reparative and compensatory processes consequent on any given lesion; and our treatment must often tend more to aid these compensative sequences, than to restore the heart to a perfectly healthy condition. A sketch of these adaptations constitutes the first chapter. In the second chapter the leading indications of treatment are thus given:

"1. To diminish, if possible, the valvular or other immediate causes of obstruction.

"2. To endeavour to remove all causes of impediment to the circulation existing in the lungs, abdominal organs, and capillary system generally.

"3. To lessen vascular distention, by reducing the bulk of the circulating fluid without impoverishing the system.

"4. To sustain or restore the power of the heart, and to reduce the capacity of its dilated cavities.

"5. To equalize the circulation, and to maintain free vascular action on the surface, by regulating the temperature, clothing, &c., and to provide due access of pure and well-oxygenized air.

"6. To remove and avert irritation and excitement of the nervous system, and to procure, as far as possible, rest and tranquillity of body and mind." (p. 13.)

These several indications are then discussed in as many chapters.

1. In the attempt to diminish valvular or other obstructions, it is of great importance not to confound old organic changes with recent endocardial deposits; it is only at the commencement of endocardial and arterial thickening, or when a fresh acute supervenes on a chronic attack, that vigorous attempts should be made to remove the deposits by treatment. If the case is clearly in its commencement, the effects of treatment will vary according as the obstruction is from fibrinous deposit in the sub-endocardial fibrous tissue, or from fibrinous coagula on the free surface. In the former case, absorption may occur with tolerable ease; in the latter, complete absorption is seldom to be hoped for. The diagnosis between these two conditions is not laid down by Dr. Chevers with any great precision, nor indeed do we think that precision is possible.

"The sounds will often assist us in deciding upon this point. It is evident that the irregularity of the surface over which the blood has to pass, is infinitely less when the deposit is sub-endocardial, than it is when the free lining membrane is affected; and hence, whenever a discordant, harsh, musical, or in any way singularly-intonated systolic sound is produced in coincidence with other symptoms of endocarditis, and with great hurry or distress in breathing, in a heart which has been previously healthy, the existence of elevated inflammatory deposit upon the surface of the endocardium may be judged with a good degree of safety; while should a diastolic bruit become superadded, the presence of massive clots, either with or without perforation of the valves, becomes highly probable. In ordinary cases of rheumatic endocarditis, the valvular bruit is of course single; and although occasionally rather sharp, it is usually smooth and even in its tone. The constitution of the patient, however, generally affords the surest means of discrimination. When a young person of fairly sound constitution, after violent exertion, or under an attack of rheumatism, suddenly becomes the subject of an ordinary systolic arterial bruit, unaccompanied by the evidences of reflux, it may almost invariably be decided either that the obstructing deposit is sub-endocardial, or that the impediment is formed by those small fringes of vegetations, which, as I have elsewhere endeavoured to show (Guy's Hospital Reports, vol. vii.), are growths from the endocardial surface. Whenever, on the other hand, the subject of rheumatic or of any other form of endocarditis or arteritis is of broken and cachectic constitution, suffers from organic renal or hepatic disease, or is the victim of irregular and intemperate habits, the deposition of fibrinous masses upon the free surfaces is the result to be naturally looked for; where, as usually happens in hospital cases, the previous state of the heart is unknown, these tests of course become of only partial validity, and can in fact merely assist the physician in guessing rather than in diagnosticating." (p. 17.)

We do not see, however, that an absolute diagnosis can be ever attained; and the duration of the disease and the condition of the patient must be the main guide for the activity of the treatment. This treatment, according to Dr. Chevers, should be:—

"The most careful preservation of bodily and mental tranquillity, a judicious maintenance of the bulk of the circulating fluid at the lowest possible standard consistent with proper nutrition and good health; and the long-continued employment of very cautiously conducted and mild courses of mercury, alternating with courses of iodide of iron and iodide of potassium, are our leading indications of treatment with a view to produce the desired absorption." (p. 21.)

The mercury and the iodine are to be given in very small doses. Great care is to be taken to avert every cause likely to occasion the supervention of acute on old endocardial disease.

In the treatment of fresh acute cardiac diseases, Dr. Chevers remarks, that while prompt and decided measures are necessary, heroic treatment is out of the question. The local inflammation is, he thinks, "merely the outbreak of some poisonous agent with which the system is pervaded almost to saturation." He deprecates large bleeding, and recommends local depletion, blistering, calomel, colchicum, and antimony. Mercury he would carry to gentle salivation; but he does not refer to Dr. John Taylor's late observations on the effect of this remedy in pericarditis.

Dr. Chevers incidentally refers to the complication of endocardial disease with rheumatism in India. After alluding to the observations of Webb, Geddes, and Bird, which are, however, incomplete, he comes to the conclusion that although acute articular rheumatism is common in Bengal, "there appears to be every reason to believe that rheumatic heart affections must occur very far more rarely in this country than they do in Europe." (p. 38.) His own materials on this point do not, however, appear to be extensive; and that heart-disease will attend acute rheumatism in Hindoos, appears from two well-marked cases referred to in a note at p. 37. We do not think that there is sufficient evidence to enable us to make any final decision on the point; but from one or two facts which have come to our knowledge, we should not be surprised to find that heart-disease and acute rheumatism are as intimately associated in India as in this country. Certainly the evidence does not warrant Dr. Chevers' conclusion; nor do we hesitate altogether to dissent from his explanation of this alleged difference between the accompaniments of rheumatism in England and in India—namely, "that the association of cardiac disease with acute rheumatism is a pathological *accident*," and "of comparatively recent occurrence."

"If this unhappy complication has always been liable to occur," writes Dr. Chevers, "it is one of the most inexplicable facts in the history of medical literature, that it should not have been generally noticed until it was observed by Dr. Pitcairn, in 1788. In the absence of fuller evidence, it may not be too chimerical to believe that, in India, the accident is still comparatively infrequent." (p. 39.)

This line of argument seems to us to be of no value whatever. The infrequency of post-mortem examinations until within the last fifty years, and the inadequate method of investigation possessed by physicians, prior to the discoveries of Avenbrugger and Laennec, are quite sufficient to account not only for the non-recognition of the connexion of endo- and peri-cardial diseases with acute rheumatism, but for the prevalent ignorance of the existence and course of heart-affections generally. Many rheumatic heart-affections are, at their commencement, unattended with any severe and striking local symptoms, and would in fact be altogether latent were it not for the stethoscope. The wonder is, that the connexion was recognised so soon, not that it was not recognised before. It is certainly not impossible that acute rheumatism may run its course in India without implicating the heart, but we cannot consider that so important a pathological fact is yet established; and to receive it as if it were established, and, for the purpose of explaining it, to degrade rheumatic endocarditis to the rank of an *accident*, is certainly premature.

After discussing this question, Dr. Chevers alludes to another of

considerable interest, and on which we find ourselves entirely of his way of thinking. It has been lately contended by Mr. Simon, that the valvular diseases occurring in rheumatism are simply caused by "fibrinous precipitation from an overcharged solution," and do not arise from inflammation of the endocardium itself. That such deposition may occur in certain cases, we are not disposed to deny; but to refer all the pathological appearances of rheumatic endocarditis to this cause, is not a tenable position. How under this hypothesis are we to account for the *sub*-endocardial deposits so frequent in these cases; for the occasional uniform smooth thickening of the endocardium, and for other changes of softening and laceration which could never arise from deposition merely? There are certain cases of vegetations, in which we can conceive that direct deposit from the blood might account for the appearance, and is indeed their most probable explanation; but we doubt whether the extension of the hypothesis to all cases of rheumatic endocarditis be possible. Mr. Simon's experiment of passing a thread through a vein and artery, appears to us to tell very little in favour of his opinion.

Dr. Chevers makes the following very sensible observations on this point:

"In every case of true arteritis and endocarditis which I have examined, there have been as distinct traces of what we call inflammation behind the epithelial lining (but quite apart from the contractile coat) as upon its surface. Further, it appears that large fibrinous concretions upon the free surfaces are extremely rare attendants on rheumatic endocarditis. In this disease the principal morbid changes are interstitial; clots may adhere when the patient is very cachectic, and they are usually present where the structure of a valve is broken up; but in how many cases, where death results from the consequences of rheumatic endocarditis at periods of two, five, or ten years subsequent to the acute attack, is fatal obstruction found to have been owing to the presence of fibrine *upon* the valves? Clots of comparatively recent date, and crops or fringes of minute elastic vegetations, may adhere to the altered structures, but these will not be regarded as the main cause of death; it is the insterstitial disease, the hardening, shortening, and contraction of the fibrous tissue below the surface which has killed. I have always held (and I believe that the opinion is an old one) that the fibrinous masses which are found covering parts of the diseased surfaces in endocarditis and arteritis, are deposits from the blood attached to portions of the lining membrane which have been altered by inflammatory lesion." (p. 42.)

2. The second indication in the treatment of cardiac affection, laid down by Dr. Chevers, is to remove all causes of impediment to the circulation in the lungs, abdominal organs, or capillary circulation generally.

"The occurrence of death from causes confined to the heart itself is," says Dr. Chevers, "a comparatively rare event. In the generality of fatal cases, life is cut short prematurely, either by the outbreak of acute disease in other organs, or by the super-addition of diseased actions of a more insidious character to those gradual deteriorations, which the fixed error of the circulation is itself imperceptibly working. It is especially the pulmonary and hepatic structures which suffer under these circumstances." (p. 47.)

The measures which are to be employed in removing these pulmonary and hepatic affections, vary somewhat according to the nature of the cardiac affection. When the obstruction arises from original thoracic malformation, or defective thoracic development, little can be done except in the way of palliation. The *removable* physical causes of impediment to

the circulation, external to, but dependent on, the heart, either *per se*, or conjoined with other influences, such as cold, influenza, indigestion, &c., are bronchitis, pneumonia, certain diseases of the pleuræ, congestive disease of the abdominal viscera, ascites, and general anasarca.

"Caution," says Dr. Chevers, "is required in laying down any absolute rule with regard to the measure of treatment to be employed in cases where acute pulmonary disease occurs as a complication of old cardiac lesions. The management must chiefly depend upon the peculiarities of individual cases. Still it will generally be found that, here, the treatment must be conducted with the most cautious reference to the broken state of the patient's constitution. The heroic systems of treatment can be borne only when every vital organ still retains the essentials of integrity. Without drawing a parallel between the two conditions, it may be safe to say that there are few, if any, cases of acute lung disease supervening upon advanced heart affection, in which it will be found safe to use stronger measures than we should employ in quelling acute pneumonia or pleurisy in a phthisical patient." (p. 60.)

In addition to the usual remedies of moderate local bloodletting, counter-irritation, small doses of calomel, antimony, and ipecacuanha, mild evacuants, and diuretics, Dr. Chevers recommends strongly a restriction to be laid on the use of liquids. This is a favourite measure of his, to which we shall have to refer more at large presently.

When hepatic congestion is consequent on, or is aggravated by, heart-disease, blue-pill, taraxacum, the compound decoction of aloes, with temporary abstinence from animal and saccharine food, and from fermented liquors, is recommended. Dr. Chevers objects strongly to hydragogue cathartics in cases of portal congestion, whether attended with dropsy or not:

"I have seen the entire mucous tract of the small intestines perfectly œdematous, in a case of dropsy, where purgatives had been freely given shortly before death; and I believe that this is too often the state which attends those copious 'serous dejections,' the production of which some writers on dropsy allude to with so much satisfaction. Elaterium is always inseparably associated, in my mind, with mortal dropsy. Well-selected laxatives and gentle purgatives are absolutely required to relieve the constipation which attends heart-disease and portal dropsies; but the use of violent cathartics, with a view to the removal of effused or intravascular fluid, should, I feel assured, never be had recourse to except as a rare and almost extreme resource."

We cannot agree with this opinion; nor do we see why, if hydragogue purgatives are hurtful in moderate cases, they can become useful in extreme. That elaterium should be used with the greatest caution, cannot be doubted; but that Dr. Chevers should have been led to associate its use invariably with *mortal* dropsy, causes us some surprise. We should have thought every one had seen extraordinary effects from its use, both in cardiac and in renal dropsy.

3. The third indication is to reduce the bulk of the circulating fluid without impoverishing the system. This is exactly what most practitioners aim at by the administration of elaterium, bitartrate of potash, &c.; but Dr. Chevers would rather attain it by a limitation of the fluid and solid ingesta, by the production of diaphoresis and diuresis, and by the occasional employment of *small* bleedings.

In advocating the first method, Dr. Chevers does not wish to enforce starvation, but simply a systematic spare diet, as far as solids are con-

cerned. But with regard to fluids, he thinks that, as a general rule, men in health are in the habit of taking much more fluid than is necessary for the wants of the system.

"There can be no doubt that, both in hot and cold climates, a large proportion of very temperate individuals convert their bodies into mere filtering machines, by the excessive imbibition of water, tea, and other diluents, greatly to the impairment of the tone of their stomachs, skin, and urinary organs. Judging by my own experience, a man of spare make, weighing between nine and ten stone, will not require more, on an average, than one pint and a half of liquid during the twenty-four hours, in cold or temperate weather, and will scarcely be compelled to exceed an average of two pints in a hot climate. In the case of a sedentary invalid, the function of whose skin is not preternaturally excited, it is not impossible to reduce the supply of fluid considerably below these standards." (p. 73.)

Such abstinence from fluids is no doubt a powerful measure; whether it is of use in such cases as are here referred to, can only be determined by actual observation. The plan of denying water to dropsical patients is a very ancient one, but the extreme difficulty of keeping patients to so severe and painful a regimen has much contributed to its disuse. We are quite unable to speak from personal experience of the value of such a system in cardiac disease, but we shall not hesitate to try it, on Dr. Chevers' recommendation. But in referring to this method of treatment, Dr. Chevers has touched on the very important question of the effect of water on the system generally,—a much more extensive subject than that of the influence of water on cardiac diseases alone. We do not think that his opinion, that evil must result in the most temperate individuals if an excess of water be taken, is at all borne out by facts. The primary effect of cold fluid on a healthy stomach is not debilitating, but the reverse. The fluid does not remain in the stomach; it passes into the circulation; and if liquids thus enter the system in excess, we know that in healthy persons they are removed with extraordinary rapidity. Able physiologists even fancied, some few years ago, that there was some "short cut" between the stomach and the kidneys, so rapidly did these organs act when the stomach was filled with fluid. We cannot conceive that there could be any injurious repletion of the vascular system, in persons whose kidneys, skin, and lungs are all sound. If fluids are not injurious in quality, which is their main fault, we cannot readily believe, that unless pushed to great extent, they can be hurtful in quantity. If a man will keep to harmless liquids, he need not surely limit himself to Dr. Chevers' rather scanty portion, if he feels any desire to take more. Limitation in the quantity of liquid would, we conceive, be worse than excess of quantity. Not only does the system require a vast amount of fluid for its continual changes, but without a certain amount it is probable that there would be an insufficient elimination of effete products. Some of these effete products, as urea, are so very soluble that they will pass out with almost any quantity of water; but uric acid, some salts, and the so-called extractives of the urine, are much less soluble. A certain quantity of water is necessary for their elimination, perhaps also for their formation. At least, when immense quantities of water are taken, as in the so-called "water-cure," the absolute quantity of solids excreted by the urine increases, and water in this case seems to augment the decomposing processes going on in the body. So

convinced are we of the importance of the action of water in the system, that unless experience, which in medicine is our great guide, clearly proves the advantageous effects of abstinence from fluids, we should enforce such abstinence with caution. We should be sorry to see re-introduced into medicine, without sufficient evidence of its utility, that habit of disregarding the natural craving for fluids, and of basing their administration on some fanciful hypothesis, which was only a few years ago very common in this country. These observations, however, have incidentally grown out of Dr. Chevers' remark. His position, that congestions of the lungs and liver, consequent on cardiac affections, are benefited by limiting the supply of fluids to the system, rests on its own ground—viz., the assertion of one who is undoubtedly a very trustworthy authority.

The following remarks on the employment of diuretics in cardiac affections are very judicious. After observing that diuretics usually act well and safely in early cases of obstructive heart-disease, before the kidneys become congested, he goes on to say:

"Unfortunately we are usually called upon to require the aid of the kidneys at a stage of heart-disease and dropsy, where the efficient action of these and other excretory organs can least be demanded. There are few more difficult cases than that of a person who is the subject of advanced cardiac obstruction, attended with bronchitis and serous effusion. Here it will generally be found that the urine is high-coloured and scanty, the bowels torpid, the skin dry, the secretion of bile defective, and transpiration from the pulmonary exhalents impaired by the diseased state of the bronchial mucous membrane; in fact, that every natural outlet for the pent-up fluids is obstinately closed. This is a crisis at which a full bleeding from the arm, a brief course of digitalis, or of squill and juniper, or a dose or two of elaterium, would prove a direct and almost infallible means of dispatching the patient to his grave *secundum artem antiquorum*. Provided anything can be done here,—and it happens, fortunately, that, in many instances, our means of aid are not wholly exhausted, even at this unhappy conjuncture,—the disentanglement of the morbid complications becomes a somewhat knotty task, and no ordinary caution is required in deciding which organ should be first selected with a view to attempting the restoration of its functions. A fatal error will be committed if we at once endeavour to act upon the kidneys; their state of congestion cannot be relieved so long as the liver and the heart are gorged with blood and the skin remains inactive, or while the organs themselves actually suffer from the pressure of large ascitic effusion: under these circumstances all diuretics become local poisons. The organs are for the present physically incapable of being excited to healthy action, but their susceptibility to irritation and inflammation is increased tenfold. The safest and most physiological course, I believe, is, first, to endeavour to restore and to excite the action of the skin. A well-managed plan of active diaphoretic treatment can now scarcely be attended with danger. At the same time, mild expectorants and aperients may be employed, but with a less sanguine hope of present success. A strict limitation of ingesta will tend to relieve the heart, and to reduce the hepatic congestion; and then, as the skin begins to act freely, and dyspnœa and palpitation become somewhat abated, a certain amount of relief to the kidneys may be anticipated, providing the congestion of these organs is not associated with advanced structural disease. This tendency to restoration of function may be aided by local abstraction of blood, the application of heat, counter-irritation, dry-cupping, &c." (p. 81.)

Small bloodlettings are recommended in some cases of obstructive heart disease, when acute catarrh or pneumonia supervenes, and when the already dilated and incapable heart becomes over-distended with blood. Here the organ must be relieved, or the patient dies. Yet these cases are

often most difficult, and bloodletting becomes in some cases literally a kill-or-cure remedy. Dr. Chevers recommends that if it be resorted to, the blood should be drawn from a *small* orifice, and the patient should lie with the head low. He should be most anxiously watched, for, as Dr. Chevers remarks, syncope is fatal. So dangerous sometimes is general bleeding, that in the majority of cases the relief of the over-distended heart should be attempted rather by leeching or cupping over the præcordial region, than by opening a vein.

4. The fourth indication for treatment is to maintain or restore the power of the heart, and to reduce the capacity of the dilated cavities.

It is well known by practitioners, that the most unfavourable condition into which the cavities of the heart can pass, is dilatation. An increase of cavity, unattended with proportionate increase of walls, accompanied, as it almost always is, by structural changes in those walls, is the condition most to be feared in all cases of organic heart-disease; for it is in such a condition that the circulation is most inefficiently carried on, and that stagnation of blood, either in the heart or in some portion of the capillary system, is most common. The converse condition—increase of thickness and power of the walls, as compared with the cavities—is, on the other hand, comparatively speaking, a not unfavourable state of things; for the circulation is by this means still carried on with sufficient integrity to preserve the system from local congestions. Such hypertrophy, consequent as it generally is on obstruction either at the orifices of the heart or in the course of the circulation, is a compensatory change, and is obviously the way in which the altered condition of circulation consequent on the obstruction can be best remedied. A treatment which would aim at preventing such a compensatory increase of power, would evidently be mischievous; and the best practitioners are well aware of this fact, and anxiously avoid any course of excessive lowering and sedative treatment which may tend to convert hypertrophy into dilatation.

But although such hypertrophy is not to be *prevented*, or is even in certain cases to be encouraged, there can be no doubt that in a great number of cases it requires to be *regulated*. It is not always an unmixed good, and, without guidance, may even produce as much evil as that for which it is considered a remedy. For example, let us take a case of rheumatic endocarditis which has injured the aortic valves, has thickened them, and produced obstruction, without permitting regurgitation. The left ventricle enlarges and becomes stronger, as an inevitable consequence, provided the system is tolerably healthy. Such an increase, as permitting the circulation to be carried on in spite of the obstruction with as much vigour as before, is a benefit, and is, with certain provisos, to be encouraged. But suppose that, at the same time, the endocarditis has affected the mitral valve, has produced sub-endocardial exudation and supra-endocardial deposition, and has rendered the valvules soft, yielding, perhaps even fragile? The hypertrophy, which is a compensation for the obstructive aortic valve, is then an additional cause of damage to the mitral. The treatment here is obviously not to allow a sudden and rapid hypertrophy, but to run the risk even of impeded circulation from the aortic obstruction (which may, however, generally be prevented), in order that the changes in the heart may go on as slowly as possible, and may produce the greatest

amount of compensation, without injury to enfeebled textures. In the case simply of aortic obstructive disease, it may be the proper treatment to strengthen the heart at once, by exercise, by tonics, and stimulants, and the like measures; in the case of aortic and mitral, the treatment may be very different, and the tendency to rapid growth of the left ventricle may have to be repressed by rest, by small bloodlettings, even by sedatives. The difference between the two cases is very great, and illustrates forcibly the importance of distinguishing these valvular diseases, the diagnosis of which is considered by some an unnecessary refinement.

In addition to this, we believe that, even in obstructive aortic disease, when the hypertrophy is really, abstractedly, a great benefit, it may sometimes be produced too rapidly, and may require to be regulated. The general state of the patient, as well as the action of the heart, will soon inform the practitioner that the heart must be quieted.

That hypertrophy is in these and in many other cases a morbid condition, and requires treatment, is the opinion held, we believe, by the majority of those who have considered this subject. That hypertrophy is also a compensation and a salutary sequence in certain cases, is also well known. In each case the practitioner must judge for himself of the amount of encouragement or of repression that may be required.

Dr. Chevers, however, does not coincide in these views. He looks on hypertrophy as invariably a compensating change, so completely divested of any evil consequences, that it is never to be regarded as a disease. In answer to the question, "Is hypertrophy a morbid condition?" he writes as follows:*

"As this opinion appears to have led to nearly all the erroneous systems which have been adopted in the treatment of cardiac affections, it will be worth while, before proceeding further, to inquire whether this hypertrophy ever has a separate existence; *whether it is a disease at all; whether it should ever be 'treated' at all;* whether, indeed, it is not, in all its degrees, to be regarded as approaching, as nearly as any mere reparative provision can approach, *to an absolute condition of good;* the removal or diminution of which (apart from the removal of its causes) would *never be productive of benefit*, but, on the contrary, would generally be the means of withdrawing the chief preservative adaptation, which often for years guards the victim of heart-disease from almost instantaneous death." (p. 87.)

Subsequently Dr. Chevers writes:

"I have not seen or heard of any case in which it was demonstrable that excessive muscular development and strengthening of the walls of the heart existed otherwise than as a compensation for impediments of some kind, or for the weakening attendant upon temporary or permanent dilatation of its cavities. Nor have I been able to meet with a heart in which the amount of hypertrophy appeared to be more than compensatory for the coincident dilatation or impediment. Whereas, in nearly every complete case, the symptoms during life, as well as the sum of the morbid appearances, rendered it almost a matter of demonstration that the amount of hypertrophy was not, and had never been, fully sufficient to counteract the impediment or embarrassment to which the organ was subject." (p. 89.)

The practical conclusion from these views is—

"*That an error of the gravest description is involved in every direct attempt to remove or 'cure' cardiac hypertrophy. Our efforts should tend solely to the removal of the cardiac or vascular dilatation*, to which the hypertrophy holds merely the relation of a partially compensating provision." (p. 96.)

* We have *italicized* a few words to mark the extent of the opinions.

We believe that the exclusiveness with which this view is held makes it erroneous; nor do we in the least accord with the view that the compensating hypertrophy is always an exact compensation, and no more, for the obstruction. That it is so in some cases we do not doubt. Every one has seen cases of obstructive aortic disease, in which for years the left ventricle has gradually gone on enlarging and thickening in such exact proportion to the increased necessity for strength, that the patient has suffered no inconvenience in any way, and has never even suspected that his heart was diseased. In such cases the balance has been so truly observed, that no treatment could have changed matters for the better. But in many other cases it is otherwise. The heart does decidedly act too much; and that it does so is proved, not only by the evidence of its own violent action, and by the general suffering of the constitution, but by the relief which tranquillity and the judicious use of sedative measures immediately produce.

Dr. Chevers has some other opinions about hypertrophy, to which we must take exception. The following quotation embodies an opinion which has been formed apparently to support his favourite notion of the uniformly beneficial effects of hypertrophy, but which only shows how a preconceived notion will lead away even an acute and able reasoner. In speaking of the intricate subject of heart-disease and cerebral apoplexy, Dr. Chevers thus writes:

"Arterial cerebral apoplexy not unfrequently occurs *in association* with hypertrophy of the heart. The same causes, especially an excess of circulating fluid, tend to produce both, and the state of the heart may, in some cases, be immediately operative in occasioning the effusion. I believe, however, that this accident occurs far less frequently than is generally supposed. We are called to a patient suffering from apoplexy; he lies motionless and stertorous, and his heart and arteries beat with a rapidity and a vibrating force which it is almost terrible to witness; respiration becomes seriously interfered with, and the patient almost inevitably dies within a few hours. He is found to be the subject of a *central* apoplectic effusion, and his heart is perceived to be unusually large and strong. This is rather a frequent case, and the recollection of the violent action of the heart almost invariably convinces the inexperienced observer that the arteries of the brain were ruptured by the prodigious impulse of the bulky heart. This, however, in all probability, was not the true order of events. The singular disturbance of the heart's action, and the subsequent death from suffocation, were alike the results of the central cerebral lesion, more or less immediately involving the respiratory tract, and causing a fatal interference with the nervous supply of the heart and lungs. This palpitation resulted from the apoplexy, and did not precede or occasion it." (p. 95.)

It does not appear to us that this case is put clearly. We understand the main difficulty to be the recognition of the connexion between a bulky, constantly over-acting heart, and the final apoplectic effusion. Dr. Chevers argues the question as between the violent action which is coincident with the apoplectic effusion, and this effusion. The question is, in fact, evaded, or rather concealed behind another matter much less obscure and difficult. It was necessary for Dr. Chevers, admitting as he does the association of the lesions, to show that the state of the heart operated in producing cerebral hæmorrhage, only in so far as it hindered the free return of blood, and not by sending an augmented current to the brain. As the matter at present stands, the very important point of the influence of an hypertrophied left ventricle on the brain is much too cursorily dismissed.

As hypertrophy cannot, according to Dr. Chevers, be excessive, all sedative treatment is considered inadmissible in cardiac affections, and the great rule to be, to strengthen the heart by all means. Digitalis receives a most unqualified condemnation. We are not at all disposed to agree in these general rules; and however important it may be to strengthen the heart in many cases, the observation of every practitioner must convince him that there are cases where sedatives, and even digitalis, are most undeniably useful. In fact, the numerous variations of cardiac affections cannot be cramped up within a single rule. It may be true that, in the majority of cases, the indications are to strengthen and excite and not to tranquillize the heart; but there are cases in which the latter measures must be employed, as we are certain Dr. Chevers will some day admit.

The fifth indication is "to equalize the circulation and to maintain free vascular action upon the surface by regulation of temperature, clothing, &c., and to provide due access of pure and well oxygenized air." These measures are so obviously useful, that we need not dwell upon them.—The sixth indication is to avoid irritation and excitement of the nervous system, and to procure, as far as possible, rest and tranquillity of body and mind. On this we may make the same remark.

The last chapter of the book discusses the treatment of aneurism of the aorta. Dr. Chevers recommends the reduction of the volume of the circulating fluid, the maintenance of the muscular power of the heart, and of a free circulation through the vascular system generally. He does not advise depletion to reduce the volume of blood, but directs this to be done by cutting off the supplies of food and fluid, and by moderately acting on the skin and kidneys. To maintain the power of the heart, tonics are given. Dr. Chevers also advises the "encouragement of *thin* layers of plastic coagulum within the aneurismal sac." (p. 136.) This is to be done "by tranquillizing the circulation and by keeping the blood in as highly organized a state as possible." (p. 138.) But, unfortunately, we are not told how this is to be done.

We must, however, now take our leave of Dr. Chevers, with the assurance that we have read his book with profit and pleasure, although there are many points on which we take the liberty of differing from him. We would advise him, however, to leave general principles of treatment alone, and to busy himself in collecting observations in that magnificent field which now lies open before him. He possesses observant powers of no ordinary kind; and if he exert them judiciously, he cannot but add greatly to our knowledge of Indian diseases. But in order to do so, we would beg him to lay aside all reasoning about phenomena of which we know only part, and to busy himself with gaining as accurate a knowledge as possible of the phenomena themselves.

Art. III.

Mémoires de l'Académie Nationale de Médecine. 4to, tom. XIV, pp. 850, and XV, pp. 844.—*Paris,* 1849-50.

We have become somewhat in arrears with our notices of the Memoirs or Transactions of the French Academy; but we propose in the present article to present our readers with an account of what there is of most interest in the last two volumes that have been published.

Reports on the Epidemics which have prevailed in France during the years 1841-7. By M. Gaultier de Claubry.

These Reports are in continuation of others which have appeared in former volumes of the Memoirs, from the pens of Double, Piorry, Villeneuve, and Bricheteau, constituting together a very valuable contribution to the medical history of the country. They are the results of the analysis of the various local reports forwarded to the Minister of Agriculture and Commerce by certain medical officers distributed over the different Departments of France, called "Physicians for Epidemics." Upon the prefect of a department receiving intimation from any of the mayors that epidemic disease prevails in his locality, he instructs one of these physicians to repair to the spot, in order to investigate the causes of the outbreak, superintend the application of remedial agencies, and report the results of his observations. In this way a large body of valuable facts is accumulated by competent observers; but as some of the local practitioners feel considerable jealousy at this interference, while in some of the poorest districts there are no practitioners at all, it occasionally happens that the authorities are not advertised of the existence of such epidemic until it has already long prevailed, or has even passed away, so that few correct data can be obtained respecting it. Of the two Reports by M. Gaultier, contained in the present volumes, the first is based upon the minute examination of two hundred local reports for 1841-6, and the second upon the analysis of thirty-one relating to the year 1847.

Typhoid Fever. This is the disease which exhibits by far the largest numbers in both reports. In that for 1841-6, of 10,000 cases, occurring in 142 communes of 28 departments, 1667 (one-sixth) died. Very dissimilar conditions, as regards the salubrity of the localities and the degree of comfort or destitution of the inhabitants, are reported; and some of the epidemics occurred in public establishments, where every hygienic requisite was present. The great majority of the reporting physicians, however, attach great importance, either as immediate causes, or as exerting a modifying influence, to the presence of stagnant water or marshes, the vicinity of dung-heaps, and the overcrowding which occurs owing to the smallness of domiciles in villages otherwise salubrious. In reference to this last point, however, M. Gaultier observes, that the number of attacks is fewest in winter, when these habitations are most crowded; and he adds that, in fact, there is no one condition of heat or cold, dryness or moisture, or habit of life of the rural population, that has not been cited in some of the reports as explanatory of the attack. Of

7348 cases in which the *age* is stated, it was below 15 years in 2282. Some of the physicians report children as young as 4, and one relates a case of undoubted typhoid, occurring in a child but one year old. At the other extreme of the scale, cases are recorded at the age of 60 and 70, and one, verified by an autopsy, at 86. While the *mortality* amounted to one-sixth of the total number of cases, it was but one-ninth for those of less than 15 years of age—a proportion, in both cases, very similar to that observed in the Parisian hospitals. Great variations from this mean were observed in different localities. The greatest diversity of opinion prevails among the reporting physicians upon the question of *contagion.* It is the opinion of many of them, that the having had the disease affords an immunity against future attacks; and this is adduced in explanation of the well-ascertained fact, that the disease ravages a locality with a severity proportionate to the length of time that has elapsed since its last appearance.

In the *treatment* of this disease, many of the reporters have abandoned all attempts at cutting it short, limiting themselves to a rational expectancy, and meeting any important symptom that may arise with appropriate measures. Much diversity of opinion prevails among them as to the propriety of bleeding, but all agree that it can only be resorted to with great reserve. Saline purgatives are strongly recommended by many, as are emetics at an early period. Whenever any paludian complication occurred, quinine was of the greatest service. The same diversified views are contained in the twenty-one reports relating to the typhoid fever in 1847; the eastern departments, as in 1841—6, being those especially affected; but no meteorological, topographical, or anti-hygienic cause explaining their peculiar liability. Several of the reporters adduce striking facts in proof of the transmissibility of the disease by contagion. Of the 1280 cases, occurring in twenty-one localities of nine departments, 181 (about one-seventh) died. Of 1119 patients, whose ages were indicated, 214 were less than 15 years, and among 162 of the fatal cases there were 28 below and 134 above 15.

Miliary Sweat.—In the first report, accounts of epidemics with profuse sweating, from fourteen departments, are noticed. Although some of these were very slight, others were severe and fatal. In the department of the Lot and Garonne 28,307 persons were attacked, of whom 519 died; the mortality varying greatly in the different arrondissements of the department. In other departments the cases were less than 1000 in number; and in the Somme, wherein the disease formerly so prevailed as to acquire the cognomen of "the Picardy Sweat," it is now found only in rare and comparatively isolated cases. The disease observed no particular period of the year in its outbreak, and very great difference of opinion prevails as to the influence of locality, weather, &c. The physician-reporters give little countenance to the popular belief of its contagiousness. It usually attacks persons between 20 and 30, but children less than three years of age, and persons of 70, occasionally suffer. The two characteristics of the disease are, first, *profuse sweating* (so that changes of from eighty to two hundred shirts are spoken of as being required in four or five days), having an odour *sui generis*, but often compared to that of rotten straw; and secondly, an eruption of simple *sudamina* or of miliary vesicles containing a turbid fluid, frequently accompanied with

more or less vascularity of the skin, and terminating in desquamation. Sometimes there is intense fever or gastric derangement; but the tongue, sometimes loaded and at others clean, is always broad and moist, never red and dry; while amidst this profuse sweating there is complete absence of thirst. The eruption usually appears after the fever and sweating have lasted twenty-four to thirty-two hours, the disease then beginning to abate, and the vesicles drying up in two or three days, leaving the patient well, save for excessive debility. The disease exhibits itself in the *benign* form, in which the patients do well under the most varied, or in the absence of all, treatment; and in the *severe* or *remittent* form, in which it becomes complicated with pernicious fever, and frequently proves fatal, especially if quinine be withheld. Some of the cases present cerebral symptoms, which are attended with a fatal issue when treated as inflammatory. Owing to the invincible repugnance of the rustics to post-mortem examinations, little or nothing is known of its pathological anatomy; and in respect to its *nature*, all the reporters reject the idea of its localization in any organic apparatus, most seeming to consider it as an affection of the blood, engendered by some atmospheric poison, itself, probably, a product of the insalubrious localities where the disease usually prevails. The treatment seems usually to have been expectant, encouraging the development of the eruption, and meeting severe congestion or inflammation of the respiratory organs with very cautious bleeding, or with revulsives.—In the second report, only one small epidemic (affecting 90 persons, of whom 28 died) is mentioned.

Diphtheritis.—A small portion of the arrondissement of Laon, in the Aisne, was visited several times by this affection between 1837 and 1841; and at the end of the latter year an epidemic appeared, which continued for six months, during which 128 out of 229 inhabitants were attacked, of which number 9 died. At an earlier period, when the disease was actively treated as a purely inflammatory affection, the mortality had been greater. During another epidemic at Brenville, in La Manche, 73 of 523 inhabitants were attacked, 59 of the number being children. Of the 23 fatal cases, 20 occurred in children, those of wealthy parents being equally attacked with the children of the poor, although the disease did not take on a contagious character.

Dysentery. — Twenty-eight reports of epidemics of dysentery were received. Different causes of its prevalence are assigned; but among these the excessive heat of the summers of 1842 and 1846, with the cold and wet autumn of the former year, are especially dwelt upon. The vicinity of miasmata, and the bad hygienic condition of the persons actually attacked, are also several times noted. The proportion of cases to the number of inhabitants varied much, from $\frac{2}{3}$, $\frac{1}{4}$, $\frac{1}{6}$, to $\frac{1}{60}$; but taking the mean of all the epidemics, it amounted to $\frac{1}{10}$. This is only a rough statement, however; for, in adjoining communes, apparently subjected to identical hygienic conditions, the greatest diversities prevailed. The same observation is applicable also to the proportionate mortality, which on a mean was $\frac{1}{8}$.

Passing over the accounts of epidemics of intermittent fever, cholerine, measles, and scarlatina, as possessing little interest, we may note that two slight ones of *cerebro-spinal meningitis* are recorded. In one of these, 16

persons (9 dying) were attacked, 12 being children, of which number 7 died. In the other, 26 persons were attacked, and 6 died, 8 being children, 3 of whom died. In the village in which the first of these occurred, most of the inhabitants who were not attacked, yet suffered from severe spinal pains until a diarrhœa supervened, when these ceased. M. de Claubry regards this disease as typhus fever, with predominant affection of the spino-cerebral apparatus. Its visitations have been hitherto nearly confined to military garrisons; and its history presents many interesting features, which we hope to be able to advert to at length on a future opportunity, while giving an account of the recent publications upon the subject.

Report of the Committee on Mineral Waters for 1847-8. By Dr. Patissier.

France is rich in mineral waters, and attached to the principal sources of these are 104 "physician-inspectors of mineral waters," who, besides superintending the administration of the waters, are expected to forward an annual report to the Minister of Commerce upon the various diseases which have been treated at them each thermal season, usually lasting from the 5th of May to the 5th of October. This duty has, however, been very imperfectly performed, as only twenty-five such reports were received during 1847, and only twenty during 1848; some of these even proving of a very barren character. The business of the present committee was to consider these reports, and how far the administration of mineral waters might be farther extended to the poorer classes.

On perusing the various reports, the committee was struck with the fact, that most chronic diseases were found almost equally curable at the different establishments, whatever might be the composition of the water. These waters, in fact, act as medicinal stimuli, of various degrees of strength; and the skill of the inspecting physician chiefly consists in adjusting the amount of such stimuli to the temperament of the patient, and to the various forms of morbid manifestation. Still, there are various exceptions to this general rule, certain waters having long enjoyed a well-deserved reputation of possessing curative properties in certain affections—as, e. g., those of Vichy, in chronic disease of the digestive organs and calculous affections; those of Bourbonne and Balaruc, in paralytic affections; those of Barèges and Luchon, in chronic cutaneous diseases, &c. But, just as in the case of any other active medicament, the mineral waters are only serviceable in proportion as they are administered with discernment both as regards the stage of the affection and the varying strengths of the water as found at different springs. Not only, too, do the number and the temperature of the baths (these mineral waters are used more in the form of baths, and less in that of drinks, than with us) require variation according to the age, sex, and temperament of the patient, but also according to the condition of the atmosphere. In order to sufficiently stimulate the skin, whip up its flagging action, and effect through its agency a salutary reaction, the bath requires to be of a higher temperature, and more frequently repeated during a rainy period, than when the atmosphere is hot, dry, and charged with electricity. This consideration of the meteorological condition of the atmosphere is of great importance during the treatment of the

diseases of the nervous system by the agency of mineral waters. So, too, the difference of sensation must be taken into account; for persons of the labouring classes, owing to the inferior excitability of their skin and nervous system, require hotter baths than do the inhabitants of large towns, especially those of the higher classes, in whom the psychical predominates over the material element of their existence. All these matters can only be adjusted by competent medical authority on the spot; and much harm results from the abuse of mineral waters by persons who take them without such advice.

M. Patissier believes unjustifiable disappointment to have arisen, from the expectation so often entertained that mineral waters are *at once* to effect a cure, and from the practice of continuing their administration too long under this idea. Their doing so is always exceptional; the most that can be expected from them being that they should put the patient on the road to recovery, and the reality of their operation being only manifested some months after their employment. Their *modus agendi* is a contested point; for while some of the reporters regard them as partial or general stimuli, others, imbued with the doctrines of the Italian school, consider them as hyposthenic in their operation. M. Patissier is strongly of opinion that a true stimulation of the organism takes place, the reality of which is exhibited in the augmentation of digestive power, and the increased activity of the circulation and of the functions in general, which are induced,—such excitation being usually proportioned to the quantity of saline or gaseous contents, and to the temperature of the water.

"On examining the reports of the inspectors, one cannot but feel surprised at the frequency with which certain forms of disease are met with; as, for example, uterine lesions, chronic myelitis, and gastro-intestinal neuroses. It would seem as if a revolution had occurred in the most common diseases; and this is a phenomenon worthy of the attention of the medical observer. There are also met with in the thermal establishments a great number of morbid lesions, which it is difficult to range in any nosological chart. *Multa sunt in praxi, quæ nec dici nec scribi possunt.* They are pathological conditions in which the entire economy suffers without any appreciable lesion. There is a multitude of languid, valetudinarian beings shut up in large towns, who are neither well nor ill: some suffering from a delicate state of the nervous system, and others being the subjects of *migraine* or melancholia. Among the affections of indeterminate seat we may place the varied accidents accompanying the critical age, prolonged convalescence, the debility dependent upon a sedentary life, protracted watchings, excitement of mind and heart, depressing moral affections, abuse of pleasures of various kinds, or masturbation, and finally, many of the transient pains, both chronic and acute, which affect all the organs, are aggravated by the use of medicines, and drive the physician to despair. It is especially in this description of deteriorated health, that the concurrence of hygienic influences so powerfully aids the operation of the waters. What numbers of such patients recover their appetite, strength, flesh, and cheerfulness near the beneficent sources which Providence has so largely distributed over the soil of our beautiful country!" (tom. xv. 53.)

For the purpose of examining the reports in detail, M. Patissier distributes them into five classes, according to the predominant character of the waters to which they relate. These are—

1st. The *Sulphureous waters*, found especially among the granitic rocks of the Pyrenees. Employed as baths, and taken internally, they act as stimuli upon the skin and mucous membranes, inducing, according to how

they are digested, increase or loss of appetite, constipation or diarrhœa. They augment the rapidity of the pulse, and induce a feeling of internal heat, sleeplessness, and a restlessness like that produced by coffee, until at last there results an abundant sweating, exanthema, or a free flow of urine, operating critically in most chronic diseases. Their activity requires that they should be commenced in small doses, and only administered in cases in which it is desired to arouse the vital action, and in lymphatic preferably to sanguineous temperaments.

2nd. *Acidulated Waters.*—These are characterized by the predominance of carbonic acid gas, and operate as cooling sedatives, acting sometimes through the bowels, but chiefly by the kidneys, the secretion of which they much augment. They affect the head more than other mineral waters, inducing a temporary intoxication and a tendency to sleep. They favour digestion, and are indicated in all those diseases in which traces of irritation yet remain.

3rd. *Alkaline waters* are eminently alterative, "that is, possessed of the power of changing the constitution of the liquids and solids of the economy." They diminish the plasticity of the blood, render the humours more fluid, and impart an alkaline character to the acid secretions, especially the urine and the sweat. They are well fitted to relieve an embarrassed condition of the vena portæ, to remove "obstructions," and to dissipate nephritic colic, by expelling the calculi. Among these, the waters of *Vichy* enjoy a wide-spread reputation; their claim to which we lately adverted to (vol. vii. p. 415).

4th. The *Ferruginous waters* are those which derive their fundamental properties from iron; many other waters containing this metal in too small a proportion for it to constitute their essential character. The iron in ferruginous waters is found in the state of sulphate, or carbonate, or most commonly as a crenate. They act as alteratives, increasing the colour and plasticity of the blood, and modifying its composition. They are indicated in protracted convalescence, in the diseases of debility, and in chlorosis or anæmia succeeding hæmorrhage.

5th. Waters are termed *Saline*, which hold in solution a considerable quantity of neutral saline substances, together, generally, with a very small proportion of gas. Some have soda, others lime or magnesia, for a base; while others contain iodine or bromine. Their mode of action and therapeutical properties vary with their composition.

Under each of these classes M. Patissier describes the various establishments, the diseases treated at them, and their pecuniary emoluments.

In the concluding portion of his report, he adverts to the question as to how far mineral waters can be made to enter into the system of public charitable administration. Whatever it may be here, the mineral waters are resorted to in France by many others than the wealthy classes; for more than three-fourths of the patients in various of the establishments belong to the peasant class, the majority of these suffering from rheumatism. In most of the establishments there are indeed gratuitous baths accorded to those who produce certificates of poverty; while in several there are special hospitals for their reception, and, at a great many, baths are provided for them by legacies and other charitable means. The difficulty, indeed, is not so much to provide access to the baths, as to supply the

means to the poor for living while availing themselves of these. The reporter does not recommend the erection of additional hospitals, which he believes would be attended with too much expense, and be liable to abuse; but proposes that the general councils of the department should be compelled by law to vote annually a special fund for the support at the bath-places (calculated at one franc per diem) of those indigent persons whose condition, both social and pathological, has been properly investigated. He prefers entrusting this duty to the "general councils," (analogous to our sessions of county magistrates.) rather than to the "municipal councils," (analogous to our boards of guardians,) as these last are "often composed of parsimonious citizens, who would never vote additional pence, a share of which they would have to pay without enjoying any personal profit therefrom." This is a preference we can well appreciate on this side of the Channel! He believes, also, that the prices of the baths require to undergo some adjustment; for as the private baths are at least one franc each, many persons of limited means are prevented from remaining long enough for these to be of service. He recommends that much more frequent resort should be had to baths in common, the cost of which would be comparatively insignificant, and their employment often yet more useful. The separation of the sexes, and the custom of bathers of both sexes in France employing a costume, secure a decency too often grossly violated in this country.

On Luxation of the Bones of the Pelvis. By Dr. Murville.

This subject seems to have excited but little attention until the end of last century, when Louis published his essay on the separation of the pelvic bones, in the 'Mém. de l'Acad. de Chir.;' one of the two cases cited by him, however, evidently being no dislocation at all. A case is reported in the 'Journ. de Méd., 1765,' in which a wide separation of the symphysis pubis without fracture took place as the result of a fall from a height, the patient dying immediately, probably from lesion of the spinal marrow. No details are given. Sir A. Cooper reports two cases of luxation with fracture. That this accident is not necessarily fatal is shown by two cases, which were recorded at the end of last century, and which are now republished by Dr. Murville, as well as by two new ones which have occurred to himself. The *first* case occurred in a strong man who fell forty feet. The left side of the pubis rose at least two fingers' breadth above the right, and all attempts at replacement were fruitless, first, on account of the acute inflammation which was present, and afterwards, from the severe pain they caused. When the patient was enabled to try to walk, the replacement was effected, to a certain degree, of itself. What was at first mistaken for a dislocation of the femur in the *second* case, (occurring in a soldier who fell twenty feet), reported by Thomassin, really proved to be a separation of the sacrum from the left ilium. After the inflammatory symptoms had subsided, a firm bandage imparted such support, that he could take a few steps in six weeks, and by its aid was eventually able to resume his duties, though the bones, at the last report, had not completely recovered their solidity.

The *third* case occurred in a lieutenant, aged thirty-eight, of good constitution, and above the common height, who fell from a third floor, the

tuberosities of the ischia coming in contact with the pavement, and an extensive luxation of the sacrum downwards taking place. Intense pain was produced by pressure on the sacro-iliac symphyses, and by the least motion of the legs. The cristæ ilii were so much raised as to mount up above the level of the last false ribs, while the portions of the coccyx were separated and thrust downwards. Complete paraplegia and great exhaustion seemed to announce approaching death. Life, however, was saved; but, owing to the dreadful pain which the slightest motion caused, no attempts at reduction could be endured. The paralysis was only in part relieved, so that when seen ten years after, the patient, though in good health, could only take a few uncertain steps. An examination left no doubt as to the true nature of the case. The pelvis in all its iliac contour was perfect, while the coccyx was so depressed as to touch the anal orifice. The bust was shortened proportionately to the elevation of the ossa innominata, and the folds of the buttocks were effaced, without any change of the relations of the femora with the pelvis being discernible.

Colonel C——, aged fifty-six, tall and robust, the subject of the *fourth* case, was riding an unmanageable horse. While compressing the saddle with the adductor muscles with extraordinary force, a movement of the horse threw him into the air, and he came again on the saddle, which thus acted as a wedge for the separation of the thighs. As the violent adductive effort rendered these well nigh immovable, the separating force was spent on the perineum, which was felt to give way on the left side. A second shock led to a continuation of the rupture, and all the means of junction of the symphysis pubis, including the inner pillar of the inguinal ring, were destroyed. A large hernia at the perineum, and one at the left inguinal ring, at once resulted. A separation of five or six centimetres occurred between the pubes, but the bones did not alter their level. The perineum, except its cutaneous covering, was so torn, that the hand could be passed into the pelvis just as after delivery. The herniæ were reduced, the thighs flexed and approximated, a strong bandage passed around the pubes, the separation of which had become approximated, and a severe antiphlogistic treatment, consisting in repeated bleeding and low diet, resorted to. The chief pain was felt at the sacro-iliac symphysis, and a considerable ecchymosis was observed over the sacrum. The accident occurred in October 1845, and by December he was able to walk a few paces with aid, cross one leg over the other, and the like. Stimulating means were employed to restore the impaired sensation of the lower extremities, electro-puncture being found of marked service. In January he was able to leave the hospital. After his recovery, an examination showed complete consolidation of the pubes, while so firm had become the union at the floor of the perineum and the inguinal ring, that a radical cure of the herniæ was produced. Eventually the movement of the limbs became quite normal. He led a most active life until May 1847, when he died of cancer of the stomach, riding about inspecting the troops until the day of his death. No autopsy was permitted.

The mode of the production of the accident in this case may be regarded as unique. The lesion that resulted may be in some measure likened to the *division of the symphysis pubis,* formerly recommended to facilitate delivery; and perhaps the serious results which followed that operation may have been

due to the exposure of the divided parts to the air, which did not take place in this case, as well as to the great susceptibility of puerperal women to inflammatory accidents. Various conclusions have been come to, as to the effect which this separation exerts upon the *sacro-iliac symphysis*, accordingly as experiments have been conducted upon subjects in different states of health or disease, or in different stages of pregnancy. Dr. Murville has instituted ten experiments to ascertain what is the effect of a separation of the pubes to five or six centimetres in an adult man—so as to obtain some idea of the changes which occurred in the above case. In five of these there was strong tension but no rupture of the anterior sacro-iliac ligaments, and in the five others these were ruptured on one side, towards the base of the sacrum, where they are very thin. Beyond the tearing of the cellular tissue, and of the small vessels between the symphysis pubis and the bladder, the pelvic organs underwent no appreciable lesion.

On Insanity as induced by the Silent System of Imprisonment. By M. Joret.

M. Joret, who is physician to the female House of Correction at Vannes, seems to have studied the unfortunate beings placed under his charge with the eye of a philosopher; and we regret that we have not space to transcribe some of his preliminary remarks upon the characteristics of a prison population. He describes it as a separate caste of society, which must always exhibit a larger proportion of insane than is met with amongst persons at large; and declares that a careful study of their peculiarities, made while living amidst them, must lead to the conviction that the "moralization" of the great majority is well nigh impossible, whatever system may be adopted. His observation confirms the opinion of those who believe that real good is rather to be effected by snatching the children of these wretched beings from the demoralizing influences that surround them. The great majority of the women who are admitted at Vannes can neither read nor write; but a large portion of them manifest great, and for society a dangerous, intelligence. Some of the worst thieves admitted are those who when in prison show themselves most tractable and obedient to the regulations, although these exert not the slightest reformatory effect upon them. The class of prisoners who seem most amenable to attempts at their reformation, are those committed for infanticide, being usually country girls of very limited capacity, and most absolute ignorance.

Treating of the influences exerted by the silent system, the author observes, that it is especially ill-suited for so conversational a people as the French; and that the majority of the female prisoners undergo punishment upon punishment, rather than not indulge in their propensity to speak. It is not these, however, who become insane:

"But amidst these, other prisoners are found, and be it observed, they are the quietest, and those who fulfil their tasks with most punctuality, lead more meditative lives, and are sensitive to punishment. They live solitarily amidst others, and have a conscience which yet speaks to them. They are sincerely repentant, and it is because they desire to make amends for their offences or their crimes by exemplary conduct, that they seek carefully to avoid incurring any punishment. They are constantly engaged in overcoming themselves; but in most cases their success is attended with such a constant state of mental tension, as to lead to disturbed menstruation, cephalalgia, giddiness, singing in the ears, and afterwards to perversion of the intellect." (tom. xiv. 334.)

Condemnable, indeed, would any system of punishment be, if its worst effects were wreaked upon the least deserving objects of it. A tendency to insanity may be also called into activity (for we doubt not that criminals should be regarded as possessing a cerebral organization rendering them prone to become insane) by the mere chagrin which any form of imprisonment would produce, as also by the deficient amount of bodily exercise, especially in such of the prisoners as had prior to their incarceration been accustomed to much of this. M. Joret, by means of synoptical tables, endeavours to set forth the influence of each of these circumstances in the cases that have occurred at Vannes. Between the years 1842-6 there were admitted 872 women, and of this number 68 became insane. But as 38 of these had already exhibited signs of insanity prior to their arrival, their cases are eliminated in considering the effect of the silent system, leaving thirty for comment. Each of these cases is detailed; but we have only space to state the general results. Of the entire 68, the greater number were between 30 and 40 years of age, fewer between 20 and 30, and fewer still between 40 and 60—a result conformable to that drawn by Georget from 4409 cases. The numbers of those who had been found guilty of robbery (43 of 68) far surpassed that of all other crimes put together; as might be expected, indeed, from the larger proportion of committals for crime against property than against persons. As to the *form* of the disease, it showed itself as monomania in 41, as acute mania in 14, as imbecility in 12—all these last manifesting it on admission. Of the 30 cases which commenced within the prison, 11 became developed between the third and sixth month of imprisonment, 6 between the eighth and tenth month, 6 between the twelfth and fifteenth, and 7 between eighteen months and three years. These thirty cases are arranged by M. Joret in three categories. In the first, consisting of fourteen cases, the primary cause of the insanity seemed to be the absolute silence insisted upon. In the second (ten cases), the women were the victims of severe chagrin; and in these the silent system seemed to have acted secondarily. The six cases of the third category are attributed to defective exercise, one or both of the other two causes co-operating. Admitting the validity of all these causes, we are quite unable, after the perusal of the author's cases, to discover the reasons which have guided him in apportioning out their respective operation in each instance. Notwithstanding the advantages of the earliest treatment, and removal to hospitals for the insane, only eight recoveries occurred among these thirty cases! M. Joret, comparing the proportions of insane among persons at liberty, or prisoners in general, and in those submitted to the silent system, states the first as 1, the second as 12, and the last as 37 per 1000: but the figures from which he deduces these proportions are, we believe, too inexact, and not sufficiently numerous, to be relied upon. We regard his paper, however, as an important contribution towards the mass of facts that will require examination, for the solution of one of the most important social problems of the time.

On the Action of the Ergot of Rye, and the Employment of its Aqueous Extract, in Internal Hæmorrhage. By Dr. Arnal.

Dr. Arnal, as the result of extensive clinical and experimental observation, states that the *aqueous extract of the secale cornutum* possesses

great power as an hæmostatic in internal hæmorrhages. From his experience in employing it, and from numerous experiments he has made upon poultry, by giving every variety of preparation and dose of the ergot, he comes to the following conclusions:

1. The ergot of rye contains a poisonous principle, productive of death, but by no means so energetic as usually represented.—2. Given in the entire grain it acts much less energetically than when powdered.—3. Recent ergot does not act more efficiently than older; but, on the contrary, this last is sometimes the most active of the two. In order to produce the *summum* of effect, it is necessary for it to undergo, in the vessels in which it is kept, a peculiar change, which softens it, and imparts to it an odour *sui generis*. Thus it should not be ordered to be powdered just before using.—4. Much greater effect is produced by a certain quantity, in fractional doses, than when given only at twice, probably because less escapes the influence of the digestive organs; one of the effects of divided doses is to produce a loss of feathers; but in all his numerous experiments, both with large and small doses, Dr. Arnal has never met with anything analogous to the dry gangrene, said to be produced by ergotism in man; but which, seeing that ergot exerts a fluidifying effect upon the blood, he is disposed to attribute to other causes.—5. The ethereal oil of ergot has not proved fatal in his experiments as it did in those of M. Bonjean, and he attributes the issue of these latter to the fluid having entered the air-passages, when it proves rapidly fatal.—6. The watery extract does not contain poisonous matter, or it does so in such small proportions as to prove injurious only after prolonged use. The toxical principle thus insoluble in ether or water, is found in the residue, which kills animals just as the ergot does.—7. The ergot, however given, is very slow of digestion; and when given in excess, it produces lesions of the digestive organs. Some of these are found on post-mortem examination to resemble precisely those observed in typhoid fever, and the author exhibits a parallel of the symptoms of typhoid and poisoning by ergot.—8. The ergot modifies the composition of the blood, rendering it more diffluent; and if exhibited long enough, in divided doses, it will induce all the symptoms of scorbutus. Nutrition especially suffers from its deleterious action, as is seen by the rapid emaciation that takes place in the animals to which it is given. The aqueous extract exerts a much less modifying power upon the composition of the blood, than do the other preparations.—9. The ergot, in experiments made upon man, reduces the pulse by several beats for some hours; but even by repeated doses, Dr. Arnal has never known these reduced lower than forty-eight, even in the aged.—10. The beneficial effect which ergot exerts upon uterine hæmorrhage, has led many to believe that its action is elective, as regards the uterus; but in thirty cases of other internal hæmorrhages, in which the *aqueous extract* has been administered by the author, a cure has been effected, or, when the presence of organic disease prevented this, amelioration has been procured. It is, however, not so applicable in all forms of hæmorrhage as in uterine. It is rare for active, idiopathic hæmorrhage to resist its action for more than twenty-four or forty-eight hours; but when this has become passive, the remedy may even prove mischievous if it be continued too long, or the dose

be too large. It is also inefficacious in subjects originally feeble, or exhausted by protracted disease. Even in subjects of good constitution, when given too long in large doses, it may produce bleeding of the gums, and an injurious depression of the circulation. In hæmorrhage symptomatic of organic lesion, the ergot acts as a hæmostatic, but cannot prevent the return of the bleeding. Yet in the case of hæmoptysis, dependent upon tubercle, it may act beneficially, not only by suspending or moderating the *molimen hæmorrhagicum*, but also by moderating the inflammatory action of the portion of lung surrounding the tubercular deposit. In the same way, it has proved of constant service in acute bronchitis; and in pneumonia it has rapidly suppressed bloody expectoration, and moderated other symptoms. So well does the author think of it in this point of view, that when the patient's strength requires husbanding, and the pneumonia is not too extensive, he recommends commencing the treatment with the ergot, which, by its deoxidizing agency on the blood and retarding power over the heart's action, is an antiphlogistic, *par excellence;* the debilitating effects which attend other means being either not produced by it, or, if they should present themselves, ceasing on the discontinuance of the remedy. M. Arnal believes that the experiments of arresting traumatic hæmorrhage by the local application of the extract, so favourably reported on by M. Bonjean, require repetition and extension to larger vessels.—11. Ergot in its native state is more active in its operation, but its watery extract is less dangerous.—12. M. Arnal takes the present opportunity of confirming the favourable accounts he formerly gave of the utility of the extract in *chronic engorgements of the uterus.* Some of these cases, however, require a very prolonged perseverance in the use of the remedy.

Eighteen cases of hæmatemesis, epistaxis, hæmoptysis, &c. &c., are related in illustration. The following is the formula prescribed: Lettuce water, ℥iv; gum-syrup, ℥j½; aqueous extract of ergot, 15 grains. A tablespoonful every hour and a half.

On Esthiomenos or Lupus of the Vulvo-Anal Region. By M. Huguier.

M. Huguier considers it not a little remarkable that this disease, as affecting this particular locality, has been so little noticed by writers upon diseases of the skin or upon scrofula; especially as the structural analogies between this part and the face, where the disease is common, ought to have aroused attention to it. He draws a parallel at great length, and with some stretch of the fancy, between the structure, configuration, relations, and sensibility of these two regions, which "form the two poles of the organic axis." He then goes on:

"I should fear having enlarged at too great length upon this resemblance, did there not spring from it this great principle, unperceived to this day by practitioners—*the similitude of the diseases of the ano-vulvar region to those of the face.* Thus, we frequently meet with these: pityriasis, hepatic ephelides, erythema, erysipelas, eczema, herpes, acne, acne rosacea, ecthyma, follicular exdermoptosis, syphilitic condylomata, nævi, furuncles, phlegmons, œdema, angioleucitis, mucous cysts, sanguineous tumours, cancer, and neuralgias, as well as enlargement of the lymphatic glands in the vicinity, upon which the irritation of these different affections is reflected. So complete is this sympathy and analogy, as regards condylomata, that when these exist on the face, we may boldly declare, in nineteen

out of twenty cases, that they also exist at the ano-vulvar region. On the other hand, a proof of this anatomical, physiological, and pathological resemblance is further found in the fact, which no dermatologist will contradict, that when cutaneous affections are rare on the face, so are they at the vulvo-perineal region, the soil of either locality being unfitted for their development. It is not easy to see, then, why esthiomenos, which is an affection of the skin and subcutaneous tissues, should escape an analogy which groups together such a tribe of affections in these regions. But it will be shown, not only that no such exception does exist, but, moreover, that all the varieties and complications of this horrible malady exhibit themselves here just as on the face. The activity of function of these two regions, and the frequent and energetic excitements they are subjected to, are probably also causes why the same affections are there met with." (tom. xiv. p. 506.)

It is usually observed between the ages of 20 and 50, only one case having come under the author's notice at so early an age as 15. Most of the patients are of lymphatic temperament, or present traces of scrofula, and the disease has hitherto been observed only in the lowest order of females. It seems, in fact, essentially to depend upon the change of condition effected in the fluids by scrofula, or upon a degenerated syphilitic virus. Not only is the disease not a rare one in these organs, but the three principal species described by Biett to be found on the face are found existing here also, either separately or in combination. Of each of these species and their varieties, Dr. Huguier presents a full and detailed account, illustrating several of them by lithography from original cases. He thus treats of, first, the *superficial* form, which is divisible into two varieties—the *erythematous* and the *tubercular esthiomenos*. Secondly, the *perforating* form produces, by slow degrees, the most frightful ravages in this region. Thirdly, in the *hypertrophic* form, while one portion of the part affected is being destroyed, or eaten away, another portion is undergoing abnormal development. This, too, the author divides into two varieties. The *vegetating hypertrophic form*, in which, after a variable duration of one or other of the varieties of esthiomenos, numbers of small vegetations spring forth from one or several parts of the dark, indurated integument, or from the ulcerated surface, their intimate texture being composed of a very dense and very vascular tissue. The other variety of hypertrophic esthiomenos is termed the *œdematous* or *elephantiac*, the change here principally consisting in chronic induration and infiltration of the tissues, due (according to M. Alard's researches on elephantiasis) to chronic inflammation of the lymphatics, as also to venous obstruction. The subjects of esthiomenos are very liable to erysipelas, and it is only after repeated attacks of it that this chronic induration or variety of hypertrophy is manifested. In this way may enormous masses be generated, completely disguising the natural appearances of the parts, and narrowing the various outlets. This form of the disease is frequently mistaken for syphilis, for cancerous degeneration, or for elephantiasis, according as the ulceration, the hypertrophic tumefaction, or the marmorized induration, are more developed or differently combined. Cases are met with in which the three forms of the disease are combined in the same individual, the sexual organs then presenting a most terrible and repulsive appearance.

It is rare for this disease, however old or severe, to give rise to serious general disturbance of the system, most of the patients preserving their

flesh, and some even their complexion. Even the local disturbance is generally but trifling, no excessive pain or itching being present; while menstruation and evacuation of the fæces and urine continue, and sexual intercourse is indulged in. In other cases, in which the advancing disease induces stricture of the bowel, the patients die of marasmus. The discharges proceeding from these ulcers do not communicate disease. M. Huguier enters into great detail as to the signs which distinguish this disease from some forms of *syphilis;* but admits that, with every care, cases do occasionally occur in which the surgeon finds it impossible to make a certain diagnosis. So, too, the perforating and hypertrophic forms are easily and constantly mistaken for cancerous degeneration. Among the distinguishing marks is the more general destruction of the external genital organs by cancer; while in esthiomenos the ravages are more limited, and infinitely slower in progress. The author has never met with more than two cases in which the disease had not continued for several years. Esthiomenos also does not so easily penetrate the partitions which separate the various organs; so that M. Huguier knows of no example of an urinary fistula being produced by it, though some of his patients had suffered from it for six or seven years. The severe pain of cancer is also absent in esthiomenos. Vulvar esthiomenos is even yet a more serious affection than is facial; the *morale* of the patient suffers more from its presence; and although its progress is so slow, it is more rapid than the facial, while its cure is much more difficult, owing to the more constant irritation from the various discharges to which the parts are liable. Secondary stricture of the rectum may also become a fatal complication. The general *treatment* of the disease is the same as that of the facial; but the local management requires greater attention to cleanliness and repose. In the superficial form, the nitrate of silver is sometimes useful; and in the condition of hypertrophy and chronic infiltration, various stimulating ointments are of use. After local applications have been tried in vain, extirpation of the part is often of far easier accomplishment here, than when the disease is seated on the face.

Nine cases are related in considerable detail.

On Three Cases of Embryotomy and Cæsarian Section. By M. Guisard.

We noticed this essay when the late M. Capuron reported upon it to the Academy (British and Foreign Medico-Chirurgical Review, vol. iv., p. 562).

Researches on the Contagion of Typhoid Fever, and especially in relation to the circumstances under which it takes place. By Dr. Piedvache, of Dinan.

The Academy first prize of 1500 francs was adjudged to M. Piedvache for this essay, by a committee composed of MM. Louis, Chomel, Bricheteau, and Melier, with M. Gaultier de Claubry as reporter. Seventeen essays were sent in, thirteen affirming and four denying contagion, one of these last still adhering to the belief of typhoid fever being a gastro-enteritis. The successful essay seems to have well deserved the distinction conferred upon it, being conceived and executed in a true spirit of philosophic observation. The author had been accumulating facts upon the contested question of the

contagion of typhoid fever since 1839; and upon the announcement of the subject for the Academy prize, he set to work to analyze them, together with those which had been already published by preceding authors. As the result of his examination, he is enabled to declare himself a moderate contagionist—that is, while he exhibits the groundlessness of the statements that the disease is never contagious, he does the same with those which make it so always; and while proving that it frequently is so, he indicates the circumstances under which it becomes so, and by the avoidance of which it ceases to be so. In fact, it seems to us, the views held by so many able observers in this country, in respect to the conditions under which typhus and cholera may be propagated, receive from M. Piedvache's facts a peremptory application as regards typhoid fever.

In the nine years, 1839-48, he had the opportunity of observing two epidemic visitations of the disease, besides a few sporadic cases occurring in intervening years; and he has collected during this period 452 cases, the history of which, as regards the propagation of the disease, he was enabled to procure with exactitude. Although, owing to the intense prejudices prevailing in the rural districts, he was enabled to obtain post-mortem inspections only in two of the cases that terminated fatally, he states that the symptomatology of the disease was identical with that with which he was familiar in the Paris hospitals.

Professing to throw no additional light upon the cause of the disease, M. Piedvache premises what he has to say upon its propagation by a notice of some of the circumstances which may have predisposed or contributed to its development. As to the *age* of the patients—of the 452 cases, 45 were less than 10 years old, 123 between 10 and 20, 144 between 20 and 30, 79 between 30 and 40, 47 between 40 and 50, 9 between 50 and 60, and 5 between 60 and 70; these latter numbers being proportionally high compared with those of any published statistics of the disease. *Sex*—There were 201 males and 251 females. The *regimen* of those who were attacked differed in nowise from that of those who escaped. No famine prevailed, nor were any especially injurious articles of diet employed. Drunkards did not seem more liable than the sober. The operation of *season* was also quite secondary, the disease occurring amidst the snows of winter and the heats of summer; while augmentation in the number of cases was not observed to coincide with great variations of temperature. Patients were found in all descriptions of *habitations*, these differing much in regard to site, construction, and cleanliness. Those which were crowded and ill-ventilated furnished most cases; but this is the condition of the houses of the bulk of the inhabitants. The *encombrement* or crowding of the habitations is not, indeed, the *cause* of the disease, as this always prevails, while epidemics of typhoid are only occasional. One thing has struck all observers of the disease in the rural districts—viz., that almost always several cases occur in the same house or family. Much influence has been attributed by many to the presence of stagnant waters, and of putrefying animal or vegetable substances; but the existence of such conditions is almost a general rule with the rural habitations, while the liability to disease is by no means contingent upon the inhabiting such dwellings. Such circumstances, however, influence much the *number* of cases occurring in the various houses attacked.

For the purpose of elucidating the operation of *contagion*, the author distributes the cases he has himself observed, or collected from the writings of others, into four classes or categories:

1st. *Typhoid fever, after attacking an individual, attacks other members of the same family in succession.*—From among M. Piedvache's 452 cases, in 49 there were two patients in the same house, in 30 three, in 14 four, in 13 five, in 2 six, in 1 seven, in 2 eight, and in 1 ten,—leaving but 92 ($\frac{1}{5}$), in which only one case was observed in the same house. The houses attacked in nowise differed from those that escaped; and that the attacks of so many individuals in the same abodes did not result from mere local causes, is seen from the fact that such attacks were not simultaneous, three or four weeks always intervening between the first patient and his successors. The houses in which but one case occurred, were better than the others in regard to *ventilation*, as will be afterwards noticed.

2nd. *An individual attacked with the fever, and transported to a family inhabiting a locality where it did not prevail, communicates it to that family.*—This is the fundamental proof relied upon by all those who have advocated the contagious nature of the disease, and especially by M. Gendrin ('Journ. des Connaiss. Médic.,' vols. i. and ii.), one of the ablest of these. This class of cases resembles the other in attacking several members of the same family; but the possibility of referring the propagation to any cause proper to the house which the patients inhabited, is quite cut away. The development of the disease too constantly followed the arrival of the patient to be regarded in the light of a coincidence; the family enjoying good health prior to such arrival, and giving no signs of the disease until three or four weeks after it.

3rd. *An individual attacked by typhoid transmits it to the persons who are in immediate attendance upon him, while the rest of the family does not suffer.*—Several examples of this are cited, in which the disease occurred in localities where it was not prevalent, and among the more comfortably circumstanced portions of society. In the other category, in which the disease was transmitted to *several* members of the same family, the lower orders were alone those so affected.

4th. *The attendant to whom the fever has been communicated transmits it to others.*—Among the 452 cases of typhoid fever which form the groundwork of this essay, 411 either transmitted the disease, or were themselves the product of transmission—that is, $\frac{9}{10}$ of the entire number; while those cases in which no such transmission occurred, only confirm the author's view as to the mode in which such transmission was effected in the others.

Opposed to these statements, we have the *negative* ones advanced by some of the ablest physicians attached to the Parisian hospitals; but these cannot destroy the value of *positive* facts, and only show that there are circumstances under which the phenomenon of contagion is not exhibited. Even among M. Piedvache's own cases, while of the entire 452 transmission was proved in 411, it was so in 393 of 419 country cases, and only in 15 out of 33 which occurred in the town of Dinan, and some of these 15 lived in the environs; so that in strictness they should be considered as rural cases also. Seeing the great difference as to the manifestation of contagion which occurs in town and country, the comparative condition of the patients

in each should be inquired into. As peculiarities attaching to the rustic patients, M. Piedvache mentions the number of their visitors, and the great amount of personal attendance they receive from their neighbours; the bad construction and over-crowded state of their sleeping-rooms preventing effectual ventilation; and the utter neglect of all cleanliness,—though this last is of very secondary influence as compared with defective ventilation. It is obvious that in hospitals, and in houses of the gentry, where transmission of the disease is so rare, these conditions do not prevail. To the accidental obtaining a better ventilation, by sleeping in another room, the members of families among the poor have entirely owed their exemption when this has occurred; and to the more defective ventilation that is then procured, the author attributes the fact of more cases occurring in winter than in summer. He has never seen an example of contagion from a mere visit to a patient (the sojourn of a night in the patient's chamber seeming essential); and priests and medical men, who are brought into very close *temporary* contact with the patient, very rarely acquire the disease.

An epidemic of typhoid does not spread from a centre, over a more or less considerable extent of country, like those of variola, scarlatina, &c. Only portions of a commune, a hamlet, or even a single house, may suffer; and once arrived in a locality, the length of its sojourn there is always remarkable. In the majority of the author's cases, the disease had reached the fourth week before becoming communicated. It was so often later still, but very rarely so soon as the third week.

As several persons placed in favourable circumstances for acquiring the disease escape doing so, some of the causes of their immunity may be adverted to. The first of these is the protection derived from a *prior attack*. M. Piedvache has never known an instance of its occurring a second time, and a patient surrounded by persons who have already had the disease does not propagate it. *Aged persons*, again, contract the disease with difficulty, though when exposed to circumstances favourable to contagion, they do not always escape. Among 138 cases, Louis found none above 39. Chomel supposed no case had been met with above 52; but since then Rayer has cited one of 56, and Prus one of 78. During the two epidemics witnessed by the author, almost all the aged persons living in houses where the disease prevailed escaped it. Still, 9 between 50 and 60, and 5 between 60 and 70, were attacked; and MM. Gendrin and Jacquet report similar examples. It was once believed that *young children* were very rarely attacked; but the researches of Rilleit and Barthez and Taupin show how erroneously. The attacks of children, in fact, often pass unperceived, and this may form some explanation of unlooked-for immunities in after-life. Lastly, there are individuals whose constitution or idiosyncrasy resists the effect of contagion, under whatever circumstances they may be placed.

The absolute denial or absolute admission of contagion, under all circumstances, has arisen from the observers having concluded that to be a general fact or law, which only prevailed under the particular circumstances in which they were placed. Notwithstanding the rarity of the propagation of the disease by contagion in hospitals, owing to the superior ventilation there present, yet Louis, Lombard, and Chomel, have recorded its occasional occurrence in the hospitals of Paris and Geneva. Yet is its

spread by contagion even in the country infinitely less frequent than is that of variola, &c.; and M. Gendrin does harm to the cause he advocates by maintaining the contrary. So, too, have Gendrin and his followers been guilty of exaggeration in maintaining that contagion is the sole cause of the epidemic spread of the disease. In the epidemics witnessed by M. Piedvache, there have always been a certain number of cases to which it was impossible to trace the transmission, and which yet possessed the power of imparting the disease to others.

"This is what observation has taught me; and it will not do to be more of a contagionist than the facts allow. At certain epochs, in certain years, without anything in the present state of our knowledge revealing wherefore, a larger number of typhoid fevers appears; but I have never seen two species of it, the one epidemic, the other sporadic. According to circumstances, and especially according to the condition of the habitations, the cases at one time remain isolated, and at another are propagated. I have thus observed, not great epidemics of the disease, but a certain number of small epidemics, some appearing simultaneously, others in succession. Without contagion, no epidemic properly speaking would have occurred. The cases of typhoid would have been more numerous in one year and fewer in another; and that is all. But I am far from affirming that this must always be so. I even will say that it is very probable, and even certain, that typhoid fevers, under some circumstances, may become manifested in sufficient numbers to constitute an epidemic, independently of contagion. The appearance of the different cases is successive, and follows the order of the intimate relations prevailing among the individuals attacked, when they are the product of contagion. Such appearance is simultaneous, and independent of those relations, when it depends upon what it is usual to term the epidemic influence." (tom. xv. p. 368.)

Admitting that contagion is the great cause of increasing the number of cases, the combating its production will be the best means of restricting the ravages of typhoid. M. Gendrin, and other absolute contagionists, recommend isolation of patients and convalescents; but Dr. Piedvache, knowing the people he has to deal with, amidst an ignorant rural population, believes that such a measure would spread a panic among them, and deprive the sick of the necessary attendance. The disease should be viewed as one of *limited contagious properties*, which are only brought into operation by the neglect of such precautions as a sound hygiene dictates. The air of the room should be renewed as often as possible, and the healthy members of the family should not sleep in the patient's room, or remain in it longer than necessary. Those who are in immediate attendance on the sick should be relieved often enough to admit of their taking some rest, and respiring a purer air. When possible, they should be selected from among persons who have already had the disease, or from those somewhat advanced in years. Patients, too, should not be accumulated in the same room; and in hospitals they should be as much dispersed as possible. The author believes the erection of special fever hospitals would be a very mischievous procedure. If these were intended to supply the place of the wretched domiciles he describes patients as being crowded into, we cannot agree with him in the objection; but that the reception and dispersion of such persons in a general hospital, rather than their accumulation in a fever hospital, would be the proper practice for the prevention of propagation by contagion, the experience of our own hospitals, as respects typhus, amply proves.

On Chorea; and the Relations which Rheumatism and Diseases of the Heart have with Nervous and Convulsive Affections. By Dr. Sée.

The Academy Committee, composed of MM. Fouquier, Collineau, Jolly, Baillarger, and Bouvier, decreed the prize of one thousand francs for this essay; and although we cannot quite agree with Dr. Sée in the absoluteness of his conclusions as to the humoral and rheumatic character of the disease, we must admit that he brings forward much evidence in favour of his views, and has treated the entire subject with ability and industry. His statements are chiefly founded upon the observation of two hundred and thirty-eight cases at the Hôpital des Enfans and the other hospitals of Paris; but he has compared these with the cases recorded and the statements made by other writers, especially those of our own country, whose contributions are quoted with a frequency quite unusual to French authors. Indeed, in giving a fair account of chorea, it could hardly be otherwise, as from the time of Sydenham, who may be said to have been the first to give any very accurate account of the disease, its illustration seems to have been a favourite topic with English writers. Dr. Sée states that the treatise of Bouteille, which appeared as late as 1810, was the first which treated of chorea *ex professo* in France. Of course it is not our intention to follow the writer closely through his description of a disease so well known and so often described as this, but rather to select from the essay certain points which present some novelty, or some account of the author's personal experience.

1. *Pathological Anatomy of the Disease.*—Most of the material for the illustration of this branch of the subject is derived from an examination of the cases published in the various medical periodicals, especially the English. The eighty-four necropsies of which the author has obtained accounts, he distributes, according to the results they furnish, into three categories. The *first* of these, consisting of thirty-four of the eighty-four cases, which is the most numerous as well as the most homogeneous class of cases, comprises the pseudo-membranous or purulent inflammations of the serous membranes, and especially of the pericardium and arachnoid. The author's attention to this class of morbid appearances, and consequent conviction of the frequency with which a rheumatic character should be assigned to the disease, was first attracted by the perusal of a case recorded by Copland, in the fifteenth volume of the 'London Medical Repository.' Three years' subsequent clinical experience rendered this correlation of chorea with cardiac and synovial rheumatism an undoubted fact, both in his eyes, and in those of the physicians of the Hôpital des Enfans. The analysis of already published cases soon exhibited how frequently rheumatic chorea had been recorded under other appellations. Of the thirty-four cases thus recorded, in seventeen rheumatism is mentioned in formal terms as having existed, so as to leave no doubt as to the nature of the various serous inflammations met with after death. In five other cases no indications of the existence of rheumatism are mentioned, but the multiple serous phlegmasiæ exhibit all the signs of the rheumatic diathesis, except the articular inflammation, which may have been omitted to be recorded. In twelve of the cases there was only one serous membrane inflamed (the arachnoid, peritoneum, or pericardium); but the

frequency of the rheumatic origin of such inflammations is well known. In the *second* category are found twenty-two cases, in which, in place of the before-mentioned changes, more or less considerable alteration of the nervous substance, or meningeal hyper-secretion, was observed. Most of the latter cases are explicable by the occurrence of complications; e. g., hepatized lung, exanthemata, &c. In the *third* category, of sixteen cases, the necropsies only offered negative results—chorea, not the result of a diathesis, seldom presenting any precise anatomical characters. The results are thus summed up:

"1. We may then say, that in the majority of cases, and especially in such as are the best verified, chorea is the result of the rheumatic diathesis, and manifests itself by plastic inflammation of the membranes of the heart, of the meninges, the pleura, and the peritoneum, with or without articular rheumatism.—2. That when, in other ill-defined circumstances, the choreic phenomena are accompanied by an arachnoidal effusion, or a disorganization of the nervous substance, it is rare to find a direct relation of cause and effect between the anatomical conditions and the functional disturbances. Unless both are dependent upon a common cause, such as the tubercular diathesis; they offer in general but doubtful and uncertain relations, the result of fortuitous coincidence.—3. Finally, there are cases of chorea which seem to be independent of any appreciable modification of the nervous system, and of all general change in the condition of the economy—cases which may be regarded as essential nervous affections, as neuroses." (tom. xv. p. 390.)

Admitting that the thirty-four cases of the first category can be adduced in favour of the rheumatic character of the disease, although respecting many of these the conclusion seems conjectural, there remain the thirty-eight cases of the two other categories combined, which, the author admits, exhibit no signs of the rheumatic diathesis. This seems to destroy the preponderance of rheumatism as a cause, besides which there remain twelve of the eighty-four necropsies unaccounted for, which, had they exhibited signs of rheumatism, certainly would have been adduced.

2. *Symptoms.*—We select a few of the author's remarks respecting some of these. The *arm* is generally the part first affected in chorea, while claudication, supposed by Sydenham and Bouteille to be the primary phenomenon, was only observed to precede in 5 out of 158 of the author's cases. Next in frequency to the affection of the muscles of the limbs, comes that of the muscles concerned in the formation of words (40 in 158), in the motions of the lips (29), motions of the head and trunk (19), of the eyes and eyelids (14), the tongue (13), deglutition and mastication, the voice, and sphincter ani (of each 3). In general, several of these parts are attacked at the same time, and on both sides; and when the frequency of hemi-chorea has been stated, this has only related to the commencement of the disease. At the initial period, almost always but one side, or even but one limb, is affected, the disease generalizing itself afterwards. Of 223 choreas there were 143 *general*, affecting the four extremities, or the two upper ones and the face; but in all but 69 of these, the disease affected one side more than the other. It is this circumstance that has led to the belief of the frequency of *hemi-chorea*, while it really was found in but 62 of 223 cases, and *partial chorea* but 16 times—the two together constituting but a third of the entire number of cases. The predominance of the *left side*, noticed by so many authors, has been also found in the author's cases: so that of 154 cases,

in 97 the chorea was found either localized or more marked in the left. In the most simple as in the most serious cases, the functions of the arm, when not entirely lost, are very irregular and imperfect. If the patient presses one's finger, he does so with the same power as in the natural condition; but he cannot continue to do this for some seconds, without our perceiving a series of unequal efforts, the muscles constantly abandoning the finger. It seems impossible for these patients to maintain the muscles in a state of permanent equilibrium; and in order to compensate for defective stability, the number, rapidity, or force of the contractions are increased, producing a series of unharmonized actions. What is very remarkable in this disease is, that the intensity and persistence of the movements neither fatigue nor engage the attention of the patients. When they do complain of fatigue or suffering, it is not after the exacerbations, or in the exact course of the muscles engaged, that these feelings are exhibited, but rather in the vicinity of the articulations, accompanied or not by other signs of rheumatic affection there. *Intellectual disturbance* in chorea is of far less frequent occurrence than extreme irritability and impressionability, and chiefly manifests itself in loss of memory. This, joined to the stupid look of the countenance, and the difficulty in speaking, has given rise to the erroneous opinion that imbecility is often conjoined with the disease, though some such cases do exist. Many patients complain of palpitations of the *heart;* and an exaggerated impulse, even with a true metallic resonance, accompanied or not with irregularity of rhythm —the so-called *chorea of the heart*—is present. This condition may be dependent upon nervous excitement, a chloro-anæmic condition, or, more rarely, upon inflammatory or organic disease of the heart.

3. *Duration and Terminations.*—Whatever may be the duration of the disease, its progress is continuous; as what has been termed *intermittent chorea* relates either to choreo-mania, or to chorea complicated with intermittent fever, which yet rather exasperates the chorea than renders it intermittent. Of 158 cases, only four passed into the *chronic* state; and, in a rigorous examination of 117 cases, the disease was found to occupy, from its beginning to its end, a mean period of sixty-nine days. In 158 cases 37 *relapses* occurred, of which number 17 became arrested after the second attack, 13 reached a third, 6 a fourth, and one had seven distinct recurrences—the attacks in these cases being separated by intervals of from a few months to two or three years; but usually occurring annually, and especially in autumn. All these cases were attended with a constant decrease in the duration of the successive attacks. The first attack was prolonged to a mean period of 139 days (or twice as long as a chorea not to be followed by a relapse); the second usually continued but 80 days, and the third but 55—a circumstance to be borne in mind in appreciating the value of different modes of treatment. Of the 158 cases, 9 were *fatal;* but the particulars of the necropsies are not given.

4. *Etiology.*—(1.) *Relations of causality and coincidence of chorea with other diseases.*

(a.) *Relations to Fever.*—Difference of opinion prevails as to the effect exerted by febrile diseases in chorea. Dr. Sée examined 70 examples of these (25 of rheumatic fever, 17 exanthemata, 12 of ephemeral, essential, or catarrhal fever, and 16 phlegmasiæ), all having nothing in

common but the presence of fever; and found that they all exerted similar influence on the chorea. All commenced by inducing an exasperation of the choreic movements, which continued until the febrile action had attained its maximum, when the movements again diminished, or even soon disappeared temporarily, or definitively after treatment. If the fever was attended by recrudescence, however, the choreic movements only became tranquillized when the disease had definitively terminated—so that when the movements had not become modified by the invading fever, a recurrence of this could always be prognosticated. If, in place of abating, the fever augmented, and especially if it was to prove fatal, the chorea, so far from improving, became more and more aggravated, until the exhaustion of the strength or death occurred.

(b.) *Relation to Rheumatism.* — Of 128 cases of chorea met with among 11,500 patients admitted into the Hôpital des Enfans during four years, 61 manifested articular pain or inflammation—a proportion the more remarkable, as there were only 48 cases of simple rheumatism, unconnected with chorea, met with in the same number. Thus, we may say of two children suffering from rheumatism, at least one shall be an example of chorea; while of two cases of chorea, one shall depend upon the rheumatic diathesis. The following is the summing-up of the author:

"1. Rheumatism frequently affects the nervous system, taking-on the characters of the neuroses, or simulating the isolated or combined phenomena of the diseases of the brain, spinal marrow, or their membranes.—2. The forms which it usually assumes are those of chorea, spinal or cerebral meningitis, contractions, tetanus, or paralysis. Sometimes it also manifests itself under the appearance of an apoplectic or convulsive attack.—3. The worst form of rheumatism, and that complicated with internal phlegmasiæ, seems more especially to imitate meningitis and tetanus. When it is less intense, apyretic, and sub-acute, it more frequently gives rise to contractions, paralysis, or chorea, this last being found, moreover, under all the conditions of the rheumatic diathesis, and being at least once in two cases the result of this morbid cause.—4. In regard to localizations, nervous rheumatism is characterized either by rheumatismal arthritis, or articular pains, occurring alone, or in combination with inflammation of the meninges, the pericardium, the endocardium, or even the pleura and peritoneum.—5. But whatever may be the number and form of these phlegmasiæ, the rheumatism may proceed from the synovial membranes towards the internal parts (which it does in five times out of seven). Sometimes, on the other hand, it pursues the inverse direction, from within outwards (one time in seven); while in others, finally, it remains limited to the internal parts, attacking only the serous visceral membranes, as the pericardium or arachnoid. These cases are quite exceptional, and are usually accompanied by only slight and temporary external manifestation." (tom. xv. p. 431.)

(c.) *Relation to Cachectic Conditions.*—The conditions of the system in which albuminous urine prevails, and which are frequently connected with the rheumatic diathesis, have been rarely found, as far as the author's investigations have gone, present in chorea. The condition of the fluids prevailing in chlorosis and oligæmia from loss of blood, though shown by some published cases to be capable of giving rise to chorea, are more often attended with neuralgic or hysteriform symptoms. But if chorea is rarely met with under these conditions, it is not unusual to find it becoming accompanied in its course with an anæmic or hydræmic condition, to which, indeed, various of its incidental symptoms are referrible. Such vitiation of the fluids is especially observed in old choreas and in those of a rheumatic origin.

(d.) *Relation to the Conditions of the Nervous System.*—The influence of other conditions of the nervous system, which, from the great number of so-called epileptic and hysterical choreas on record, might be thought to be very considerable, the author believes, on strict analysis of such cases, would be reduced to very little. Mere transitory symptoms accompanying the onset of the disease have been mistaken for such conditions.

(e.) *Diseased Conditions of the Brain or Spinal Marrow.*—Either these are local in their operation, or are mere parts of a general condition of the economy, as rheumatism or tubercle. In the former case they do not give rise to true chorea, but to choreiform symptoms, just as tumours, wounds, and contusions may do.

(f.) *Intestinal Disorder and Constipation* have, since the time of Hamilton, been frequently supposed to be the cause of chorea. Dr. Sée, however, believes that the constipation is only that met with in all nervous diseases, and that the purgative treatment, the efficiency of which has been exaggerated, only operates empirically by relieving the chloro-anæmia.

(g.) *Influence of Worms.*—Notwithstanding the importance which has been attached to the agency of worms in the production of chorea, the author is of opinion that the statements made are entirely hypothetical, and he is not aware of any true cure of the disease ever having resulted from the expulsion of these parasites. They are mere coincidences, or the effect of an enfeebled state of the general organism.

(2.) *Influence of Menstruation and Pregnancy.*—The delayed appearance of menstruation, or its irregularities and suppression, to which the production of chorea has been attributed, are themselves in general but the result of the chloro-anæmia, or excitement of the nervous system dependent on the chorea; and when the condition of the blood, or of the nervous system, undergoes advantageous modification, and the chorea disappears, the condition of the menstruation may become regularized, though sometimes the spasm and the menstrual disturbance do not observe any such concordant amelioration. The author has collected the histories of fourteen cases of *pregnancy*, almost all primiparæ, between nineteen and twenty-four years of age. In some, the chorea occurred during the first two months, but in seven not until between the third and fifth month, and in three between the fifth and ninth month. Gestation only acts by favouring the production of the habitual causes upon which chorea depends. In fact, of the fourteen cases there were only three in whom either former attacks of chorea, or some of the conditions of the system predisposing to it, did not exist; and the history of these three cases was very imperfect.

(3.) *Physiological Causes.*—Whatever may be the true nature of chorea, it is only developed under the concurring influence of certain conditions in respect to age, sex, constitution, &c. Of the 531 cases treated during the last twenty-two years, at the Hôpital des Enfans, 393 were *girls* and 138 boys, exhibiting the greatest predominance of *females* observed by various authors. The *ages* of the 531 were as follow: 28 were less than six years, 218 from six to ten, and 235 from ten to fifteen—the disease thus seeming, as so termed by Sydenham, Cullen, Stoll, Pinel, and Baumes, one of puberty, or, in the words of Bouteille, "a state contrary to nature, a puberty difficult of establishment." The figures, indeed, being copied from the hospital register, are not an exact expression of the truth,

as the patients do not always apply at an early period of the disease. In 191 cases of chorea, the period of the origin of the disease was rigorously inquired into, and of this number there were found to be 11 under six, 94 between six and eleven, 57 between eleven and fifteen, 17 between fifteen and twenty-one, and 12 between twenty-one and seventy. The maximum lay thus between the sixth and eleventh year, and especially at the tenth, while *puberty* does not occur in France until between thirteen and sixteen. The movements which have been described as those of the chorea of young infants, are regarded by the author rather as the oscillatory and convulsive motions produced in hydrocephalus, than as true chorea.

"The common, or essential chorea, is scarcely ever met with until after the earlier periods of life, and especially is it so at the time of the second dentition. Next comes the period comprised between the eleventh and fifteenth year, and then that between the fifteenth and twenty-first. After this period there is a pretty rapid diminution of cases, until the commencement of old age, when it may be again met with. Chorea is thus compatible with every epoch of life; but the two which most favour its development correspond to the period of the second dentition (six to ten years), and to that of the approach of puberty—that is, to two stormy epochs of life, which both expose the child to nervous affections, and perhaps by at the same time rendering it more sensible to external impressions, render it more apt to contract rheumatic diseases. It is this same commingling of the nervous and rheumatic element, which likewise prevails in the history of hereditary chorea." (tom. xv. p. 450.)

The *hereditariness* of chorea has been admitted by Andral, Georget, and Elliotson; but no one has examined, in reference to this point, the relations of chorea to the various morbid affections from which the parents may have suffered. It is remarkable that the only diseases which figure as antecedents of the chorea are of a hereditary nature—namely, nervous affections, rheumatism, and the tubercular diathesis. Of 98 cases carefully examined in this point of view, 58 furnished results, though not all equally conclusive ones, which could be distributed among these three classes of diseases. In 18 cases, collected or observed by the author, chorea itself was transmitted, and sometimes to several children.

With respect to *constitution* and *temperament*, of 107 patients 68 were regarded as feeble, 20 as robust, and 22 as between these conditions. Of 79 temperaments, recorded with exactitude, 40 were lymphatic, 21 lymphatico-nervous, 4 lymphatico-sanguineous, and 14 sanguineous. Of the 79 children, 51 were remarkably thin, 7 stout, and 21 intermediate. In more than half, the countenance was quite pale, and in several remarkably so.

"Thus, then, the majority of children suffering from chorea present the appearance of bad health, of the lymphatic or lymphatico-nervous temperament, and of a weak constitution, or one enfeebled by disease. The predilection which nervous affections have for weak and irritable subjects, and that of rheumatism for persons of the lymphatic temperament, or delicate and perspirable skin, is well known. So, too, these are the physical conditions most conformable to the production of chorea, which offers at once the double character of a rheumatic and nervous affection. All these physiological data mutually corroborate each other, and confirm the deductions which flow from the study of the morbid causes." (p. 455.)

(4.) *Moral Impressions.*—Moral impressions may, by causing debility, induce the disease, but their influence has been exaggerated. Imitation, however operative in choreomania, is scarcely ever the cause of true chorea;

and although these patients are so constantly exposed to the view of other children, one case of such is alone on record (by Rilliet and Barthez). *Fear* and painful spectacles more frequently induce the disease, though, as Guersant observes, the disposition to fear, which is one of the first symptoms of the disease, has been mistaken for fear itself. In several of the cases recorded, the result is too far removed from the supposed occurrence. to justify belief in the operation of this. In 128 of his own cases, the author found this cause attributed in 25; but in 14 of these it was combined with rheumatism, and in 10 with the influence of damp. "We may say, then, that its influence is rare and doubtful; but that, coming in aid of other predispositions, it may hasten, or even more or less immediately determine, the appearance of the morbid accidents."

(5.) *Influence of External Causes.* — The various special conditions requisite for the production of chorea contribute to render it a rather rare disease, even in countries where it is most commonly observed. Thus, among 84,968 children admitted into the Hôpital des Enfans, there were only 531 cases of chorea, or 1 in 161, while Reeves and Rufz reduce the proportion to 1 in 367. So rare is it in hot climates, as to be hardly known there. Temperate and cold climates, and especially when they are also humid ones, as Germany, Lithuania, France, and, above all, England, (the coldest and wettest country in all Europe, says our author,) are those which favour most the production of rheumatism and chorea, which is so intimately united with it. The effect of *season* confirms this view, and it is in the winter months, or the variable ones of September, October, and March, that we find most cases. The six winter and autumn months, united, absorb 307 of the 531 cases, or nearly three-fifths of the whole. On interrogating the patients, too, we have ample evidence of the influence of cold and humidity. We may give the author's general recapitulation of the causes of the disease:

"1. Chorea is observed principally in cold and temperate countries, in humid localities, in the season of autumn and winter, and sometimes as a result of the direct impression of cold, whether acting alone or in combination with fear.—2. The individuals who are most exposed to it are children from six to eleven, or from eleven to fifteen, and especially emaciated, nervous, or lymphatic girls, with a delicate and perspirable skin, whose parents have suffered from neuroses, rheumatism, or diseases of the heart.—3. In the majority of cases, chorea constitutes a secondary affection—a symptom of a local or general malady. It is only seldomer (one case in four) we can regard it as an essential neurosis.—4. Among the diseases which most frequently produce it, rheumatism stands on the first line (two cases in four). The other causes are distributed among the anæmic condition, which, though generally consecutive, has a certain number of symptoms dependent upon it; the tubercular diathesis, whose mode of operation is as yet ill-determined; and, finally, local alterations in the nervous centres, the influence of which on chorea is not completely demonstrated even in an anatomical point of view." (tome xv. p. 460.)

Treatment.—Dr. Sée enters into a critical examination of the various modes of treatment, and makes some very interesting observations upon the employment of gymnastic exercises and sulphureous baths. *Gymnastic exercises*, suggested long since by Darwin and Good, have been recently employed at the Hôpital des Enfans with the most marked success; and as the subject is of great interest just now, when we are commencing the establishment of children's hospitals in this country, we may state the

general results of their introduction into that of Paris to a much later date than M. Sée's essay refers to. They were first employed there in 1847, under the guidance of M. Laisné, gymnastic professor at the Polytechnic School, their effects being first tried on scrofulous children. Commencing with simple movement of the legs and arms, accompanied by appropriate songs, the children's progress was so rapid, that they were soon able to employ the orthopædic ladder, the parallel bars, and other machinery, in succession. By the twentieth lesson they were exercised in wrestling, and afterwards in running, special exercises being devised for the lame. From the first lesson the children became fired with emulation, and movements which seemed impossible were soon executed with ease and pleasure. A marked amelioration was speedily observed, their countenances becoming animated, their flesh firm, their voices stronger, their appetites keener and more regular; glandular swellings, which had long resisted all treatment, were resolved, and fistulous sores that had been open for years closed up. The lessons, one hour each, were given three times a week; and in the intervals the children amused themselves by repeating such of them as did not require machinery. The entire appearance of the wards was changed. In place of the children lying or sitting about listlessly, they were now seen practising their marches to their songs, running, wrestling, and trying to surpass each other,—the girls nowise yielding to the boys. The beneficial agency of such activity imparted to these naturally indolent and apathetic subjects may easily be conceived. These favourable results led to an enlargement of the sphere of the experiment; and the treatment was extended to nervous affections, partial paralysis, rickets, and especially *chorea*. Since 1847, there have been ninety-five children suffering from chorea, sometimes so obstinate as to have resisted the most varied treatment, cured either by this means alone, or by its conjunction with other means; and during the four years, no accident whatever has resulted from the employment of the exercises.* Dr. Sée states, that in applying them to chorea, care is taken to graduate them according to the severity of the case; and that they are repeated daily, but not for more than from fifteen to twenty-five minutes, so as not to induce fatigue and palpitation. Improvement is sometimes seen after the first lesson, and at latest after the fifth or sixth; so that at the end of a week, we can judge whether the means is likely to prove efficacious; and if manifest improvement has not then taken place, it is doubtful whether the cure will be thus effected, or if it is, it will be so only after a long time. The worst as well as the slightest cases have reaped equal benefit, the cure in the favourable ones only requiring a mean of twenty-nine days, and old or relapsed chorea being more amenable than recent. Dr. Sée has found that when other remedies are conjoined with the gymnastics, the proportion of cures is less, and the period of their attainment later; and he recommends no other adjunct to be employed than good diet.

Sulphureous Baths, as devised by M. Baudelocque, is another valuable means, fifty-eight rapid and definitive cures having been obtained in sixty-five cases. Thirty drachms of sulphuret of potash are added to each bath, which is employed for at least one hour daily, at a temperature of 91°. Generally, amelioration occurs after the second or third bath, but some-

* Gazette des Hopitaux, 1851, No. 87.

times not until after twelve or fifteen days, a mean of twenty-two days having served for the cure of fifty out of fifty-seven cases. Where the cure is retarded, it ordinarily depends upon the patient's powers being lowered by other remedies or insufficient diet, upon irritation of the skin induced by the bath, or upon acute irritation of the internal serous membranes—circumstances contra-indicating the baths while they continue. The conjunction of other remedies retards rather than aids the cure. Deducting the cases in which the bath was improperly used under the above circumstances, there remain but nine true failures in eighty-one cases, these being almost all recent or rheumatic choreas.

We have not space to follow Dr. Sée in his criticism of the numberless other remedies that have been advocated by various practitioners; and can only just glance at his general appreciation. He observes, that after the lapse of a certain period chorea tends to a natural cure, though this is infinitely more tedious than when curative means are employed.

"A mode of treatment does not become entitled to superiority because it cures old choreas, which have already undergone other modes of treatment; for there is infinitely more difficulty in rectifying the perversion of the functions at the commencement, and eradicating the disease at once. Such a power only appertains to well-proved remedies; and when these at the same time enjoy the faculty of curing more readily and more constantly than others, their great utility is obvious. It is by aid of this double criterion that we are enabled to class a certain number methodically, according to their practical relations. In the first line we place sulphureous baths and gymnastic exercises; and in the second ferruginous tonics and purgatives, the latter seeming to act most rapidly in the cases in which they suffice for a cure. Next to these means of treatment, those which offer most advantages are, on the one hand, cold baths, and on the other, iodide of potassium, from the latter of which, judging by the favourable trials that have been made of it, we may hope for fortunate results. Here terminates the series of means, the utility of which is confirmed by sufficient experience. Perhaps electricity, strychnine, bleeding, narcotics, and even nitrate of silver and arsenic, may have rendered service under certain special circumstances; but employed as a general medication, their good effects are very doubtful even when they do not prove mischievous.

"As to mixed forms of medication, they offer no kind of advantage. Sulphureous baths and gymnastic exercises have been associated with antispasmodics, ferruginates, opiates, or antispasmodics, without any advantage either as relates to the duration of treatment, or certainty of result being obtained. The combination of remedies which are separately useful, seems rather to oppose than to favour the effects of either of them taken separately. Nevertheless, if polypharmacy can rarely be relied upon, this is not the case with regard to the variety of remedies. The most efficacious forms of treatment rapidly lose their power, and require to have their action replaced by other means, which have retained their primary properties." (tom. xv. p. 512.)

The author explains at some length that these various remedies should not be employed empirically, nor even absolutely in relation to the physiological peculiarities of the individual, but selected and modified in their application with a due regard to the primary cause of the chorea, whether this be the rheumatic diathesis, or vitiation of the humours, or an affection of the nervous system—the first of these, we need not now add, being, in his opinion, that which is usually operative.

On the Diseases of the Secretory Apparatus of the External Genital Organs of Women. By M. Huguier.

This memoir occupies three hundred pages, and is chiefly devoted to giving an account of the anatomy and diseases of what M. Huguier designates the *vulvo-vaginal gland*, the glandular body described by Bartholinus and other anatomists of the seventeenth century, but much overlooked by those of later times. Some account also is given of the diseases of the sebaceous and piliferous follicles of the vulva. M. Huguier, being surgeon of the Lourcine, has had abundant opportunities of studying non-venereal diseases of the genital organs of the poorer classes of Parisian females, and has turned them to good account, both in the present and various other publications. Of the contents of so long a paper, which is chiefly made up of minute description and the narration of cases, we can give but the merest sketch; but we may with great confidence refer our readers to the original, as containing perhaps the only account of a class of diseases, some of the forms of which have been confounded with syphilis, and have consequently been maltreated.

The *vulvo-vaginal gland*, or secretory apparatus, is placed on each side of the lateral and posterior part of the vagina, on the limits between it and the vulva, at about one centimetre above the superior surface of the hymen or carunculæ, in the triangular space formed by the approximation of the rectum and vagina. It is from one to one and a half centimetre from the internal surface of the ascending ramus of the ischium, and two, three, or more centimetres from the free edge of the labium. It may be compared to the apricot kernel surrounded by its episperm, and resembles more the lachrymal gland than any other organ. Its shape varies, as does its size, it being usually most developed between sixteen and thirty-eight years of age. It is firm and elastic to the touch, resisting this as do the salivary glands. It possesses an excretory duct, of variable length, but usually seven or eight lines long, which opens into the vulva at the angle formed by the union of the larger circumference of the hymen with the vulvar aperture, or, in the absence of the hymen, at the angle of union with the base of the posterior and lateral carunculæ. The aperture being covered with the hymen or carunculæ, it becomes necessary to push these parts inwards to discover it, drawing the mucous membrane of the vulva downwards and outwards. In almost all women it is surrounded by a bright red vascular circle, which seems to distinguish it from surrounding parts. On incising the gland it is found to contain a thick, unctuous fluid. In proportion to its size it is richly supplied with vessels and nerves, and its secretory action is in intimate relation with the condition of the ovary and clitoris. The glands on each side are by no means always symmetrical either in shape, size, or connexions. It is very rare for these glands to be congenitally absent, although in girlhood one of them may be exceedingly small. It may, however, be destroyed by abscesses formed in its substance, or by neighbouring syphilitic or other ulceration; and it sometimes suffers considerable injury during accouchement. Sometimes it becomes much hypertrophied, or a glandular lobule may be developed from it. The older anatomists were, in the author's opinion, quite correct in regarding these glands as the analogues of the

urethro-bulbar, or Cowper's glands in the male. They are only found in the females of those males which possess the urethro-bulbar glands.

In regard to the diseases of this organ, we shall present an abridged summary of the author's conclusions:

"1. The glandular apparatus is sometimes the seat of a simple or a purulent hypersecretion, which may be produced by involuntary pollutions, or the habit of masturbation; may be confounded with inflammation of the muciparous follicles, leucorrhœa, vulvar blennorrhagia, or abscess &c. of the vulva; and may, when prolonged, lead to various affections of the genital organs, and functional disturbances, whether of the organs themselves, or of those which sympathize with them.—2. The gland is rather frequently attacked with chronic *engorgement*, accompanied or not with hypersecretion, and is then often mistaken for an enlarged absorbent gland, or for an old cyst, or abscess.—3. This engorgement often leads to acute inflammation, or abscess.—4. The organ may undergo fibrous degeneration, so that no resource but extirpation remains.—5. The excretory orifice may inflame and give rise to a small abscess; and although this promptly bursts, it is easily reproduced at the menstrual periods, or after excessive coition; and leaves after it either a purulent hypersecretion, or an ovular cavity, that may give rise to many errors of diagnosis. —6. The gland itself is often the seat of acute suppurative inflammation, the affection rarely existing on both sides to the same degree, and at the same period. In seventeen out of twenty cases, first sexual intercourse, or the abuse of coition, was the cause: and we may state, as a general rule, that when abscesses of the vulva are thus produced, this glandular apparatus is their seat. Next to this, blennorrhagia is their most frequent cause. In the inflammations of this gland, the morbid phenomena rarely extend far to surrounding parts; and the abscess never opens on the external surface or free edge of the labium, nor into the rectum, so that a recto-vulvar fistula never ensues.—7. These abscesses may be confounded with pre-rectal abscess, but the pus proceeding from the latter is more abundant, blackish, and fetid, and may make its way through the rectum, leaving behind it also an indurated cylindrical cord, which stretches from the vulva to the rectum.—8. The abscess should not be opened upon the external surface of the labium, but within, at the bottom of the nympho-labial fold, so as to avoid the formation of a *cul-de-sac*, in which the fluids of the parts might collect.—9. In relapsing abscess of the duct, the cutting instrument should permanently destroy the continuity of the canal, while in the case of the gland this should be extirpated.—10. The orifice of the excretory duct is sometimes narrowed or obliterated, and this may give rise to an abscess or mucous cyst. At others it is enlarged or quite destroyed, and replaced by an opening leading to a *cul-de-sac*, in which the genito-urinary fluids may collect.—11. This apparatus is very frequently the seat of mucous cysts, which have been confounded with other cysts of the vulva, and the frequency, nature, seat, and development of which have been misunderstood. The properties of the fluid they contain, and the organization of their walls, completely demonstrate their mucous nature. Sometimes they occupy the excretory duct alone, at others the granules, and in other cases both portions of the apparatus. They adhere closely to the neighbouring parts, and are enucleated with difficulty. Simple incision of the cysts, followed by irritation, cauterization, or partial excision of the walls, suffice for a cure. Relapse does not depend upon non-extirpation, but upon the subsequent development of new cysts, or the enlargement of very small ones. The incision should be practised in the same direction as in opening an abscess, except the cyst be very large, when it ought to be made on the free edge of, and parallel to, the labium.—12. The apparatus may be the seat of blennorrhagia, syphilitic abscess, chancres, and vegetations." (tom. xv. p. 844.)

The other affections of the vulva, described by M. Huguier, are denominated by him vulvar folliculitis, vulvar acne, steatomatous or sebaceous cysts of the vulva, and hypertrophy or exdermoptosis of the vulvar fol-

licles. In the consideration of these various affections, most of which are unnoticed by preceding writers, he finds additional proof of the analogy, both of structure and disease, which prevails in the facial and vulvar regions, to which we have, in noticing his former paper, adverted. The minuteness of the details he furnishes concerning these various diseases quite preclude our attempting to give any abstract of his descriptions; and we must content ourselves by referring those interested in the subject to the essay itself, merely observing, that it results from M. Huguier's observations, that several of these affections occurring in hard-working women, careless of their persons as regards cleanliness, are constantly mistaken and treated for syphilis. For this reason the minuteness of his descriptions, as furnishing the best means of diagnosis, are of value. The memoir contains detailed reports of forty-three cases, and is illustrated by several lithographs.

Besides the papers we have noticed in the above analysis, the fourteenth volume of the Memoirs contains the following:—an Éloge on Broussais; papers by M. Bally on the Cholera, as observed on the shores of the Mediterannean; and by M. Royer-Collard on Life and the Soul; and a very long essay upon Disease of the Bones in Scrofulous Subjects, by M. Lebert. This last, from its importance, would have engaged our attention, but as it only forms one chapter of a voluminous prize-essay, since published by its author, we shall have another opportunity of considering it.

The fifteenth volume also contains an Eloge on Baron Dubois, and a prize-essay, by M. Segond, on the Comparative Action of Animal and Vegetable Regimen on the Physical and Moral Constitution of Man, in which we can discover little merit and no novelty.

Art. IV.

The Anatomy and Diseases of the Prostate Gland. By John Adams, Surgeon to the London Hospital, &c.—*London,* 1851. 8vo, pp. 160.

It is a consideration not unworthy of remark, that as inflammation of the Larynx is perilous from the accident of its position, in a degree quite disproportionate to its intensity; so is human life rendered miserable by diseases of the Urinary organs, that are quite out of proportion in their nature to the effects which they produce on the constitution at large. It may truly be affirmed, that the prostate—a gland, the use of which is hardly as yet definitely determined—which in many animals serves at least but a temporary purpose, growing larger or declining as the sexual appetite is excited or palls—and which is evidently not even essential to the perfection of this process, as is proved by the persistence of the procreative faculty when the gland is nearly destroyed by disease,—becomes, in advanced years, the source of not less danger to life, and of infinitely more misery, than any other structure in the whole body. That the lives of so many of the best and wisest of our species should have been shortened and embittered by disease of an organ of such apparently trifling importance, and that our art should have made so little progress in preventing and relieving the sufferings thus occasioned, are reflections not a little humiliating, and not a little salutary to a sanguine mind. Nor

can it occasion surprise, that where so little is known, so much should be written. The almost universality of prostatic disease in advanced life, and the known difficulty of its cure, hold out inducements to an unscrupulous practitioner that are not to be resisted; but the very same considerations act as restraints upon the professions of the genuine lover of science.

It therefore becomes the duty of those who undertake to watch over the surgical literature of the country, most jealously to examine all works upon the more popular urinary diseases, that the mere practice-hunter may be detected and exposed, and that the public may be warned against arts that are as dishonest as they are injurious to society.

Mr. Adams's established reputation as an honourable and pains-taking man, affords of itself a guarantee that his motive in publishing the present work is a genuine and laudable one; and it gives us great pleasure to express our belief, that within the compass of one hundred and sixty very readable pages, he has brought together nearly all that is certainly known of the nature and treatment of prostatic diseases, and has made several additions to our previous information.

We confess to a great predilection for these Monographs upon obscure and important subjects; for not only do they share in the general benefits attending the division of labour, but the size and inviting appearance of the volumes themselves give them a better chance of being profitably read by the toil-worn practitioner, who has but little time or inclination to hunt out what he wants from the pages of an overgrown treatise. In these go-a-head days, when all men seem to be living so fast, we have no time for a course of black-letter lore, or for sifting mere speculative opinions; that which we want is, to become acquainted with the actual state of our knowledge; what are the lights which modern investigations have thrown upon disputed questions; and above all, what are the practical advantages of peculiar methods of treatment. Such works as Syme and Curling upon diseases of the Rectum; and in Pathology, such a little book, pregnant with instruction, as Mr. Simon lately published; are examples of desiderata in other branches of science. It is a mistake to call them superficial, because they do not comprise everything; or to suppose that the profundity of a treatise is proportionate to its length, or its display of learning; the ratio, indeed, is often the other way. Never was there a time when we could with more safety pronounce a great book to be almost surely a great evil. We crave for *condensed* knowledge; for books that will *suggest* to us, rather than *think* for us; and of this character are the three we have named above.

Our readers must not quarrel with us, if it is our habit to detain them some short time ere we plunge into the midst of our critical labours. Every book, whether good or bad, makes some impression apart from its subject-matter. Perhaps some isolated passage, or chance expression,—perhaps its tendency or tone, or even its mere outline and form,—awaken in the mind thoughts that it is well to express, because they set other minds a-thinking; and at all events, give some clue to what is passing in the reviewer's mind, and put the reader on his guard against what may be prejudices, that warp and invalidate his judgment. Let us, then, confess that we sat down to peruse Mr. Adams's treatise with a

prestige in its favour,—1st, because of the author's reputation; 2ndly, because his book is a little book; and, 3rdly, because it is a monograph upon an important and obscure subject: and we have risen from our labour with the conviction that it is a good book, because it is clear, well-reasoned, and practical, and brings the light of modern science to bear upon points of difficulty. Whether our judgment is well founded, will become apparent as we proceed.

The first twenty-five pages are devoted to the Anatomy and Physiology of the prostate gland. This part of the work, with some other passages, has already appeared in the 'Cyclopædia of Anatomy and Physiology.' Of the third lobe, which is so interesting to us as surgeons, Mr. Adams states that he believes it to be occasionally altogether wanting, and, when present, to vary in size between a simple elevation of glandular structure, and a mass so large as to appear a distinct growth. He has also no doubt that the urethra is completely surrounded on all sides by the prostate; for he has examined with the microscope that debatable ground which is situated over the upper surface of the urethra, and finds it not membraneous, as some think, but identical in structure with the rest of the organ, and even sometimes containing calcareous concretions. The intimate structure of the prostate gland is not complicated. Mr. Adams thus describes it:

"Briefly, it may be said to be composed of minute terminal follicles, opening into canals or tubes, which unite together to form ducts, which open in an oblique manner on the prostatic portion of the urethra. The orifices of the prostatic ducts are situated principally close to and around the most elevated portion of the veru montanum, in the form of a crescent, the larger ducts on the side, and the smaller on the posterior aspect of this body. If a longitudinal, vertical section is made, many of the ducts of the prostate are seen passing upwards, towards the under part of the veru montanum, in a straight direction: the interior of some of them being slit open in the section, whilst others pass obliquely beneath the mucous membrane for some distance prior to their termination. They vary in number from ten to fourteen, but as many as thirty have been seen. Their diameter ranges from one-sixth to one-fourth of a line. It sometimes happens that two or more ducts unite, and open by one common orifice, large enough to admit the end of a small probe." (pp. 10, 11.)

Each of the ducts must be separately injected, for they have usually no communication with each other, but terminate separately in minute cells or follicles. An intermediate tissue fills up the spaces between the follicles, this tissue, according to Quekett, being fibro-cellular, and according to Dr. C. H. Jones, white fibrous tissue, with numerous bands, resembling those of organic muscle. Kölliker also has described a large quantity of muscular fibres as entering into the structure of the prostate. Mr. Adams does not favour us with as minute an account of the anatomy of the diseased prostate as might have been expected from his evident familiarity with the microscope, and states nothing with sufficient certainty to call for reproduction in these pages. This is the point on which we most desiderate information, and we shall return to it again, to give an account of Professor Rokitansky's recent researches.

The *secretion* of the prostate gland can only be examined microscopically, not being obtainable in sufficient quantity to be submitted to chemical analysis. After death it is well known to be of a milky hue,

but Mr. Adams thinks it is probably clear and transparent when first secreted. Setting aside all fanciful notions of its use in diluting the semen, or in defending the ejaculatory ducts from the urine, the author throws out the following ingenious suggestion:

"I think the fact of the prostatic secretion being naturally, as I believe, acid, is a circumstance of some interest. The secretion of the testes is well known to be alkaline. Is it not probable that the reaction of the prostatic on the seminal fluid may be of use in the maintenance of the fluidity of the latter? The idea is somewhat confirmed by the fact, that in women the acid secretion of the vagina p events the coagulation of the menstrual blood, and thus favours its discharge. This has been proved by Mr. Whitehead, who found, that if the menstrual fluid was received directly from the os uteri into a speculum, it coagulated like ordinary blood." (p. 25.)

The author also writes at some length respecting the "*sinus pocularis*" and the "development of the prostate and vesicula prostatica;" but our limits forbid us to dwell now on these points, and we pass at once to the Diseases of the prostate.

Here *inflammation* holds a chief place. It is a common consequence of acute gonorrhœa; but we are unable altogether to agree with Mr. Adams's opinion, that inflammation of the prostate is often due in these cases to attempts at rapidly curing a clap.

"The internal use of copaiba, cubebs, and the application of astringent injections, in proportion as they arrest the progress of the disease in front, tend to give rise to it at the back part of the canal, or to increase it if already existing in this region of the urethra." (p. 29.)

There are few practical points upon which we are more convinced, than that gonorrhœa is a disease confined, in the first instance, exclusively to the anterior part of the urethra, extending probably at furthest not more than two inches or two inches and a half from the meatus, and capable of being safely cured at its onset by the injection of nitrate of silver, in the proportion long ago recommended by Ricord, of one grain to eight ounces of water; and it has not fallen within our experience to find this method of cure to be followed by prostatic inflammation.—On the following point, however, we think Mr. Adams very probably correct:—

"I have little doubt," says Mr. Adams, "that the discharge of what is very commonly termed a gleet frequently depends on an increased and altered secretion of the follicles of the prostate, and hence the great difficulty so often experienced in the cure of gleet by the use of astringent injections; for it is difficult, nay, even often impossible, to reach by these means the true seat of the disease; whereas the exhibition of the chia turpentine in five-grain doses often puts a stop to the discharge instantly; acting, as I presume it does, specifically on the prostatic portion of the urethra, and on the ducts and follicles of the prostate itself." (p. 35.)

Mr. Adams's treatment of an inflamed prostate gland is such as is commonly recommended in text-books—namely, leeches, and the moderate use of mercury. He details the particulars of a case in which the plan was followed by himself, not certainly with brilliant success, for the gland suppurated and discharged the contents of the abscess by opening in othe urethra, and left behind it symptoms, which now, after the lapse of nine years, have not ceased to tease the patient. To us the result is not surprising. Inflammation of any glandular structure, treated with repeated leeching and the internal administration of mercury, even as Mr. Adams

recommends, short of mercurialization, and merely as a febrifuge, is almost sure to result in suppuration. The *local* application of mercury, however, has a remarkable effect in curing glandular inflammation, strikingly contrasted with its effects when administered internally. There is no better proof of this, than the results of thus treating an inflamed breast. Leeches &c. will almost certainly favour suppuration; but if the breast be covered with the strong mercurial ointment, thickly spread on strips of lint, and the part be enveloped in cotton wool, most probably, if the treatment be adopted early, the inflammation will be entirely resolved; or else, if the disease do go on to suppuration, that suppuration will be much circumscribed. The tincture of iodine acts in the same way, in resolving glandular inflammations, as is well known in the treatment of buboes; and though not so certain in the cure of the prostate gland, on account of its position, and distance from the surface, yet one or other of these two means may be used, with a better chance of success than the leechings recommended by Mr. Adams. Tartar emetic, in depressing doses, also acts here, as it does in inflamed testicle, with remarkably good effect.

Mr. Adams's remarks upon chronic prostatitis from masturbation are graphic and sensible, offering, we believe, the most correct account of the disease that has ever appeared in print; and as we are unable to reproduce it entire in our pages, we shall avail ourselves of the accompanying case from Mr. Adams's work, which serves to illustrate his observations:

"Whilst writing the above, a gentleman, apparently in perfect health, called on me in a state of great distress of mind, having been induced to consult an advertising quack for supposed spermatorrhœa. I found that the only disorder he laboured under was an occasional nocturnal emission, which took place about once in a fortnight or three weeks; this, together with the escape of half a teaspoonful of glairy fluid during the evacuation of the rectum, when the bowels were costive, although his general health was not affected, rendered him morbidly sensitive to his supposed infirmity, and induced him to seek advice. The empiric made a great parade of examining his urine, and having received some of it into a glass, he introduced an hydrometer, and showed him what he said was the seminal fluid collecting around the bulb. When I examined this gentleman with the catheter, I found that the prostatic portion of the urethra was rather more sensitive than natural, and that the orifice of the urethra at the glans was a little reddened, and the lips of the urethra agglutinated by mucus. I examined the rectum with my finger, but could detect nothing abnormal about the prostate or neck of the bladder. I directed him to use a cold water injection night and morning, to take ext. conii, grs. v., omni nocte, and a little liq. potassæ in infusion of gentian. His urine was acid, but not especially so." (pp. 56, 57.)

In reference to these cases the author also observes:

"I cannot believe that any escape of seminal fluid ever happens without the characteristic signs of ejaculation, however modified; and in these cases such signs are frequently altogether wanting." (p. 53.)

The so-called cases of spermatorrhœa are very common. The real disease, which might be called paralysis of the genital organs, is, in our experience, equally rare. The first-named is readily enough cured by the application of the argentum-nitratum, as recommended by Lallemand; the cause of the last lies deeper, and is little influenced by topical remedies.

The experience of a large hospital has brought us numbers of these cases; and we deliberately repeat our conviction, that spermatorrhœa *does*

not really exist in a tithe of the examples in which it is predicated. The constant escape of the seminal fluid with the urine, as often described, is, we believe, a fallacy; and as for the evidence which is sometimes adduced in support of such an opinion, that of a greasy pellicle floating on the top of the urine, everybody knows that this appearance is commonly occasioned by the presence of minute crystals of the phosphate, and that patients who think themselves impotent, or about to become impotent, are persons extremely liable to mental depression, and phosphatic urine with its rainbow colour and rapid decomposition.—Mr. Adams thus accounts for the serious effects which the habit of masturbation induces in the system:

"The first effect resulting from the frequent seminal emissions thus voluntarily induced by the patient without the stimulus of natural copulation, is an irritable state of all those parts sympathetically called into action, as the extremities of the *vasa ejaculatoria* and adjacent portion of the urethra, and the prostate gland: the urethral membrane at this part is endowed with exquisite sensibility; I have no hesitation in affirming it to be the most sensitive part of the canal. This sensibility obviously depends on the large supply of nervous filaments distributed about the neck of the bladder; for although it is not possible to trace with the scalpel the ultimate termination of such nervous filaments, their destination must be the point referred to. Hence arises the pain, or at any rate the unpleasant sensation, experienced on the introduction of the catheter even in the healthy condition, and the acute and stinging sensation when inflammation exists in the canal. Through this nervous distribution arises the sympathy of this part with the bladder, vesiculæ seminales, and testicles, on the one part, and with the glans penis on the other; a sympathy illustrated by the desire the patient experiences to evacuate the bladder the moment the bougie or catheter reaches this sensitive part: a sympathy illustrated also by the seminal discharges arising from the irritation of this part by the introduction of foreign bodies, when the natural irritability of the sentient papillæ of the glans penis has been exhausted by the long-continued practice of masturbation, of which the unfortunate case mentioned by Richerand affords so graphic an example. This nervous supply, derived both from the cerebro-spinal and ganglionic systems, affords a ready clue to the explanation of those general phenomena attending these cases; thus, to speak of natural sympathy, the fainting and sickness, so often induced by the first introduction of the bougie, result from the irritation propagated to the heart and stomach by the ganglionic nerves: hence, also, to speak of unnatural sympathies, depend the pain in the loins in onanism, and the deep pain in the dorsal region. The sympathy between the testicles and kidneys is still further anatomically explained by the nerves to both being derived from the same source: the irritation of the kidneys, as exemplified by the copious secretion of limpid urine, results from the same cause; so further, the general nervous exhaustion, as shown by the weakness in the gait, by the *tinnitus aurium*, by the *muscæ volitantes*, and by the general indisposition to mental exertion under the same condition; it is the derivation of the nerves of the testis from the renal plexus, and the connexion of the latter with the cerebro-spinal axis in the lumbar region, that explain the draught, so to speak, upon the central nervous system, which is constantly taking place under the repeated and unnatural excitement of the organs of generation thus constantly indulged in." (pp. 50—52.)

His treatment of these cases, besides the local application of caustic, consists in the exhibition of conium and soda in the infusion of gentian, cold bathing, fresh air, and almost entire abstinence from alcoholic fluids. In reference to wine, he very sensibly observes:

"I believe the internal use of alcoholic drinks to be most injurious, and that it is infinitely better to deter the patient from the use of wine, or even porter, altogether, than to give them to the extent of stimulating the heart's action." (p. 58.)

In our own practice we have found much benefit, after quieting the patient's mind, and getting his secretions into good order, from a mixture containing small doses of the tincture of cantharides and the sesquichloride of iron in a bitter infusion. With this it is often necessary to give an opiate at night, and to prohibit the patient entirely from taking any sort of *beer*, which we regard as far more injurious than wine or spirit. On such a plan as this it is often unnecessary to resort to the nitrate of silver, which, we suspect, sometimes leaves behind it unpleasant results. Our author's observations conclude thus:

"This is a subject of much delicacy, and by many will be regarded, probably, as beyond the scope of ordinary medicine; I cannot coincide in that opinion, and I am fully convinced that a careful attention to this subject will well repay the trouble devoted to it, and that good effects will result to society from its consideration; the disease being thus placed in the category of those conditions which are amenable to medical treatment. (p. 60.)

We hope and believe that the day for such false delicacy has passed, and that all now recognise the proper object of the healing art—the endeavour to alleviate every ill that human flesh is heir to. To the wise and honest practitioner, no disease is so trivial, or disgusting, or painful, as to be unworthy of attention. Everything suffered by a fellow-creature interests him; from flatulence to fever, from corns to compound fracture, there is nothing that he despises.

Hypertrophy is the great change to which the prostate gland is obnoxious, and is so common after the age of fifty, that Mr. Adams says it may be almost regarded as one of the necessary contingencies of old age, supervening in a manner wholly independent of inflammation. The condition of the bladder varies remarkably in this disease; occasionally, it is so contracted and thickened as to hold but a very small quantity of water, and in other cases is so much expanded as to hold many pints.

In the first of these conditions, Mr. Adams believes that there has been inflammation of the prostate, extending to the bladder, and *sudden* increase in the size of the gland: and in the second, that the increase has been gradual, and accompanied by compensative dilatation of the bladder. This dilatation, proceeding in every direction, presses sometimes on the rectum, and produces obstinate constipation, giving rise to that pouch which is so convenient a receptacle for small putrefying quantities of urine, or for calculi. There often happens, also, a hernia of the mucous coat of the bladder through its thickened muscular fibres, which materially aggravates the patient's danger; for these cysts or supplementary bladders answer admirably to contain the urine, but, being destitute of muscular fibres, are unable to expel their contents, which, rapidly decomposing, inflame the bladder, and not unfrequently set up the peritoneal inflammation, which finally kills the unfortunate patient. The author lays particular stress upon what is called the *fluttering blow of the bladder*, as a pathognomonic sign of this condition; and as this is an important practical point, we shall present our readers with his exact words:

"As this is a subject in a great measure passed over by writers on diseases of the urinary organs, I shall take the liberty of dwelling somewhat upon it. So far as I have observed, these cases are usually attended with pain about the region of the pubis, and in the perineum, and along the urethra, especially after the evacuation of the bladder; there is generally more or less irritability of this viscus evinced by a frequent desire to pass water; hence the surgeon's attention is directed to the state of the bladder; the catheter is introduced—it may be without difficulty, or at any rate with no more difficulty than commonly attends its introduction when the prostate gland is hypertrophied; and on the escape of some urine, the flow of water suddenly stops, and *a fluttering blow* is struck against the point of the instrument, as if a solid body came in contact with it: the surgeon, believing that he has drawn off the whole of the urine, is about to withdraw the catheter, when a small quantity more escapes, and perhaps the blow is repeated. The impression first conveyed to the mind of one unaccustomed to it is, that there is a stone in the bladder; but the experienced hand at once detects the nature of the case, or at any rate the idea of the existence of stone is at once removed from his mind. Mr. Guthrie suggests that these are the cases which have been so often mistaken for stone in the bladder, and in which the operation for lithotomy has been attended with a cure of the symptoms from the division of the neck of the bladder. Although such may have been the case occasionally, I am under the impression, that in those cases in which lithotomy has been unnecessarily performed, there is an hypertrophied condition of the *detrusor urinæ*, and that the deceptive sensation which favours the impression that calculus exists, arises from the grating of the point of the sound against the hard *rugæ* of the bladder, formed by the columns of this muscle increased in size and density. It is not long since that I was in attendance on a valued medical friend, who was labouring under all the symptoms of hypertrophied prostate, with its concomitant horrors, the disease approaching rapidly a fatal termination, when his medical attendant in the country assured me that he had detected the cause of his disease in the existence of a stone in the bladder. On introducing the catheter, I was at once convinced that the opinion was erroneous: I felt the fluttering blow upon the catheter, and ventured to diagnosticate a considerable pouch in the bladder: my opinion was verified on the examination of the body after death, which took place soon after." (pp. 76, 77.)

The morbid anatomy of the hypertrophied prostate, as we before said, is not much elucidated by Mr. Adams; and he does not seem acquainted with Rokitansky's observations upon the cystic origin of much of the abnormal gland-structure to which he refers. Of these and of his own inquiries we take the following account from Mr. Paget's lectures:

"We owe to Rokitansky the knowledge that the tumours in the prostate gland, which were commonly, and till lately even by himself, regarded as fibrous tumours, are composed of tissues like those of the prostate gland. In enlarged prostates they are not unfrequently found. In cutting through the gland, one may see, amidst its generally lobed structure, portions which are invested and isolated by fibro-cellular tissue, and may be enucleated. Such portions have, I believe, been sometimes regarded as tumours, or as portions of prostate gland, in operations of lithotomy. They lie embedded in the enlarged prostate, as sometimes mammary glandular tumours lie isolated in a generally enlarged breast. They look like the less fasciculate of the fibrous tumours of the uterus; but to microscopic examination they present such an imitation of the proper structure of the prostate itself, that we cannot distinguish the gland-cells on the smooth muscular fibres of the tumour from those of the adjacent portions of the gland. Only their several modes of arrangement may be distinctive."

Sometimes these new glandular growths are completely isolated from the prostate. Thus, Mr. Paget mentions a specimen sent to him by Mr. Wyman, in which a mass that measured 2½ inches by 1½, was found "lying loose

in the bladder, only connected to it by a pedicle, moving on this like a hinge, and when pressed forwards, obstructing the orifice of the urethra;" yet neither in its general aspect nor in its microscopical characters could this be distinguished from prostatic substance.

The observations of Mr. Adams that chiefly bear on this point, are as follow:

"Putting out of our consideration all those accidental conditions which now and then accompany the enlargement, as fibrous tumours, the deposit of scrofulous and scirrhous tubercle, we may regard the disease as an universal increase in the natural elements of the gland: thus, when examined by the microscope, its blood-vessels are found numerous and large, its ducts and follicles are immensely increased in diameter, they are loaded with concretions, and there is a remarkable increase in the deposit of the white fibrous and muscular elements which fill up the spaces between the follicles. Occasionally we find large tumours developed in the lobes analogous to the fibrous tumours which occupy the female breast, and which are constituted of a genuine hypertrophy of the glandular tissue. Sometimes the enlargement depends on the growth of distinct oval and circumscribed tumours growing within the gland. And not uncommonly true hypertrophy of the prostate is accompanied with the growth of a large projecting tumour growing into the bladder, and having no immediate connexion with the gland itself, but attached to the neck of the bladder, and which is often mistaken for an enlargement of the prostate. The mistake is not of the slightest moment, as the character of these tumours cannot be verified except by microscopical examination after death. They are composed of fibrous and cellular tissue." (pp. 88, 89.)

He also lays much stress on venous congestion induced by venereal excitement, as a common cause of the sudden retention which attacks old men with enlarged prostates.

The mode of treatment to be followed in cases of hypertrophied prostate gland is clearly enunciated by the author; though, of course, his remarks are not strikingly novel. He prefers that the patient should be in the recumbent posture when introducing the prostatic catheter, as, in case the chief disease is of the third lobe, it is liable to fall down and entangle the point of the instrument, should the operation be attempted in the erect position. We are somewhat surprised, however, to find that Mr. Adams makes no mention of the flattened catheter, invented, we believe, by Mr. Wormald, for use in extensive enlargement of the lateral lobes; but as we find that he has himself contrived an instrument for the same class of cases, it is no wonder that he should entertain a natural affection for his own offspring. Mr. Adams's invention consists of a silver instrument, with an opening at the end, sufficient to permit a long elastic catheter to be passed through it, when the silver catheter has reached the prostatic part of the urethra.

Both as an example of the admirable vein of good sense which runs through Mr. Adams's work, and also because his position in the profession, and his connexion with one of the largest and most flourishing of the metropolitan hospitals, entitle his remarks to peculiar attention, we shall bring under our reader's notice some observations which he makes on the subject of the neglect of *Practical Instruction* in our scientific medical schools. They are called forth *apropos* of introducing the catheter; and are so true and forcible as to require, we should think, but little recommendation beyond their intrinsic merit. Again and again would we urge the parents and guardians of medical students seriously to consider whether it is possible for all that is *professed* to be actually taught in the three years commonly

devoted to metropolitan education. How much is it to be wished, that they would select hospitals where the medical officers really take pains to impart practical bed-side instruction to their pupils, rather than such as offer their chief inducement in the shape of a long array of worthless prizes upon all conceivable subjects. It was well said by one of our chief living physicians,—himself one of the greatest, if not the greatest, of clinical teachers,—that it is appalling to think of the quantity of knowledge running to waste in our hospitals. Mr. Adams observes:

"It is of the highest practical importance that the surgeon should have clear and definite ideas of the cause of difficulty in the treatment of these serious but very common cases: there can be no doubt that many valuable lives are sacrificed through ignorance of this particular: cases of retention, from enlarged prostate, are of daily occurrence, and every surgeon of common experience has had cause to lament the serious mischief resulting from a want of attention to this subject; nay, it occasionally happens, that, on being called in consultation to cases of this description, the surgeon finds his brother practitioner ignorant of the true cause of the retention, totally unpractised in the use of the prostatic catheter, and in all probability without such an instrument in his *armamentarium*:—this ought not to be, and there is no excuse for such negligence: every surgeon on commencing practice ought to be aware that such cases *must* of necessity present themselves to his consideration, he is therefore in moral duty bound to provide himself with the necessary information on these topics, and he is equally bound, for his own sake as well as for his patient's, to have such appliances ready as he must at some period or other of his practice be called upon to use.

"And, whilst I am on this subject, I cannot help making a brief digression on what I conceive to be a paramount duty of those who undertake the instruction of students at our large hospitals. I conceive that they have not done their duty to their pupils unless they have not only directed their attention to this important point, but have instructed them in the actual manipulation requisite to relieve their patients in this most perilous strait: a student, however intelligent and zealous in the acquisition of knowledge, cannot attain dexterity by *intuition* or by reading, he must be taught under the eye of his instructor how to handle the catheter, where to expect the difficulty in its introduction, and how to overcome that difficulty; by this he gets a confidence which is essential to his success, and which in no other way can he by possibility acquire." (pp. 101, 102.)

When hæmorrhage occurs from the prostate, and there is an accumulation of clotted blood in the bladder, the author says he has formed an excellent extemporary suction-pump, by introducing into a catheter passed into the bladder, a long piece of wire, to which a bit of lint, large enough to fill the catheter, has been attached, which is to be employed as a piston. It is more serviceable for removing clots of blood than the common lithotrity syringe.

Scrofulous disease and abscess of the prostate gland are diseases that have occurred to Mr. Adams, in common with other surgeons, and he has also met with cancer in this situation.

Prostatic concretions are so common, that, according to the author, they may be found in almost all cases, on making a microscopic examination of a thin section of the gland—and are consequent, most probably, on a deranged action of the mucous membrane, as formerly maintained by Dr. Prout. When of small size and imbedded in the substance of the gland, they give rise to little inconvenience; but if once loosened, may of course pass backwards into the bladder, or forwards into the urethra, whence they may often be extracted by forceps.

Examples of neuralgia of the prostate have been met with by Mr. Adams, and found as difficult to cure as neuralgia in other situations.

The book concludes by a few observations on the dilatability of the prostate, and on Dr. Willis's proposal of dilating the neck of the bladder by fluid pressure—a proceeding which does not find much favour with Mr. Adams.

ART V.

1. *Compendium de Médecine Pratique, ou Exposé Analytique et Raisonné des travaux contenus dans les principaux Traités de Pathologie Interne.* Par M. LOUIS DE LA BERGE, Docteur en Médecine, Agrégé à la Faculté de Médecine de Paris, Chef de Clinique Médicale à la même Faculté; M. ED. MONNERET, Agrégé à la Faculté de Médecine de Paris, Médecin du Bureau Central des Hôpitaux; et M. LOUIS FLEURY, Agrégé à la Faculté de Médecine de Paris, Membre Correspondant de l'Académie Royale de Médecine de Belgique. Ouvrage Autorisé par le Conseil Royal de l'Instruction Publique et par le Conseil de Santé des Armées de Terre.—*Paris,* 1836-46. 8 vols. 8vo, pp. 698, 638, 642, 636, 639, 634, 615, 496.
2. *Guide du Médecin Practicien, ou Résumé Général de Pathologie Interne et de Thérapeutique Appliquées.* Par F. L. I. VALLEIX, Médecin des Hôpitaux de Paris, Membre Titulaire de la Société Médicale d'Observation et de la Société Anatomique, Auteur de la 'Clinique des Maladies des Enfans Nouveau-nés,' du 'Traité des Névralgies,' &c.—*Paris,* 1842-47. 10 vols. 8vo, pp. 576, 600, 627, 559, 632, 608, 586, 599, 847, 1006.
3. *Handbuch der medicinischen Klinik.* Verfasst von Dr. CARL CANSTATT, koniglich-bayerischem Gerichtsarzte und Mitgliede mehrerer gelehrten Gesellschaften. Zweite vermehrte Auflage.—*Erlangen,* 1843-47. 4 vols. 8vo, pp. 382, 1102, 919, 1109. Also published with the second title of *Die specielle Pathologie und Therapie vom klinischen Standpunkte aus bearbeitet von* Dr. CARL CANSTATT, &c.
4. *Handbuch der Pathologie und Therapie.* Von Dr. C. A. WUNDERLICH, Professor der Medicin, Vorstand der medicinischen Klinik zu Tübingen. Dritter Band.—*Stuttgart.* 8vo, pp. 1238.

WE proceed, in accordance with the promise made in our last Article (see vol. XIV. p. 358), to the consideration of the diseases of the Respiratory Organs, and we shall still retain Wunderlich as our principal guide.

The following are some of the most important of the general causes of diseases of the air-passages and pleura:

A. An hereditary constitutional tendency, as, for instance, to tuberculosis.

B. Causes dependent on anomalies in the development of the air-passages, and on the age of the patient.

Passing over the subject of Atelectasis, we may observe, that during the first few weeks diseases of the respiratory organs are comparatively rare; but, as Wunderlich suggests, this may be chiefly owing to very young children being little exposed to noxious influences: when, however, these diseases do occur, they are very fatal. During early childhood there

is a tendency to inflammatory affections, as coryza, bronchial catarrh, croup, and broncho-pneumonia, and to disturbances of the nervous system, as spasm of the glottis and hooping cough. After the fifth year, this tendency to acute diseases of the air-passages gradually disappears, and attacks, when they do occur, approximate in character to those presenting themselves in adult life. During the second year, or even earlier, there is a tendency to chronic tuberculous depositions in the bronchial glands and the pleura, where, at an early age, they are of more frequent occurrence than on the lungs. During and immediately after the development of puberty, there is a tendency to pulmonary congestion, hæmorrhage, and pneumonia, and an increased tendency to tuberculosis, especially when the lungs and thorax are only imperfectly developed. This disposition is retained during adolescence and the first stage of adult life. In declining life, the disposition to inflammations of the air-passages remains, but the tendency to tuberculosis diminishes, and in its place we have chronic catarrhs, asthmatic attacks, and emphysema; while in old age the motor fibres of the air-tubes have a tendency to paralysis, giving rise to suffocative catarrh, a frequent termination of many diseases of advanced life; and hypostatic and catarrhal pneumonias are also common.

C. External influences are next considered. Under this head Wunderlich duly notices external mechanical injuries, the inhalation of finely comminuted particles of steel, stone, wool, &c., and of acrid vapours and gases; the effects of different temperatures and degrees of moisture; and, finally, certain epidemic, miasmatic influences (possibly connected with the occurrence of an excess of ozone in the atmosphere), giving rise to general coryza, influenza, &c.

D. The causes connected with incidental conditions of the air-passages call for no remark.

E. The influence of other organs is very important in relation to the general etiology of this class of affections.

(α) The simplest way in which the air-passages can be secondarily affected, is by the topical extension of diseases from adjacent parts: thus, diseased conditions of the mouth, throat, and pharynx may extend to the larynx; disease of the œsophagus (especially cancer) may extend to the larynx and trachea; of neighbouring arteries (in cases of aneurism), to the bronchi and trachea, &c.

(β) When neighbouring parts undergo any great alteration of volume, or change of position, they may interfere mechanically with the air-passages. Thus, injurious pressure on various parts of the respiratory apparatus may be caused by abscesses in the vicinity of the trachea, enlarged thyroid and thymus glands, tumours in the neck, aneurism, morbid growths in the mediastinum, pleuritic and pericarditic exudations, distention of the abdomen with gas, effusions into the peritoneum, and pregnancy.

Under this head we may also place elongation of the uvula, which often acts like a foreign body, and sets up an irritative action that often extends over a considerable tract of the pulmonary mucous membrane.

(γ) A firm adhesion of a part of the lung to the adjacent parts often impedes the proper respiratory movements and gives rise to pulmonary diseases, especially to atrophy, local congestions, and tuberculous depositions.

(δ) Impediments to the free and uniform circulation of the blood through the lungs is a common source of this class of diseases. The cause of these impediments may lie in the heart itself, in its imperfect or tumultuary motions, in obstruction or insufficiency of the mitral valve, or in hypertrophy or dilatation of its different cavities. The cases are rare in which too much blood is sent by the diseased heart to the lungs. (This only occurs in hypertrophy of the right ventricle.) It is much more common to find that the blood entering the pulmonary veins is obstructed in its passage through the heart. The free circulation may also be impeded by the pressure of tumours on the pulmonary vessels, or by contraction or occlusion of some of the vessels themselves. Moreover, any great dilatation or contraction of the venous trunks opening into the right auricle must more or less influence the pulmonary circulation. All these disturbances create a tendency to either acute or chronic congestion, from whence arise catarrhs, serous and plastic infiltrations, and frequently even extravasations into the tissue of the lungs.

(ε) Changes in the quantity or quality of the blood may give rise to various pulmonary affections. Thus, general plethora may cause pulmonary congestion, and, secondarily, catarrhal or asthmatic affections; or, if it is associated with an impeded flow of any ordinary discharge (as, for instance, the menstrual discharge, or hæmorrhoids), it may cause hæmoptysis; general anæmia, on the other hand, has been supposed by some writers to induce a tendency to gangrene of the lungs. In reference to changes in quality of the blood, an augmentation of the fibrin disposes to fibrinous exudations, not only in the lungs, where they are most frequent, but also in the bronchi, trachea, and larynx; a diminution of the fibrin, on the other hand, may cause stases (especially in the lowest parts of the lungs), and give rise to catarrhs, and even to mortification; a diminution of the cruor predisposes to asthmatic and other nervous attacks, to obstinate catarrhs, and to serous infiltration of the lungs; pyæmia is very often followed by disseminated, circumscribed infiltrations of a purulent or plastic nature.

(ζ) Many constitutional diseases, both acute and chronic, give rise to, or are attended by, affections of the air-passages. As examples of acute general diseases affecting the lungs, we may mention typhus, the plague, and the acute exanthemata; and of chronic diseases, scrophulosis (where there is almost always a tendency to catarrhal affections), arthritis (causing asthma and chronic catarrh), cancer (when cancerous deposits often occur in the lungs), and syphilis (giving rise to ulceration of the larynx).

(η) The influence of the nervous centres on pulmonary disorders is often very distinct. Wunderlich refers to the fact, that psychical conditions of a depressing nature frequently induce spasmodic or paralytic attacks of the respiratory organs, and that spinal irritation may occasion spasm of the glottis. While severe lesions of the medulla oblongata cause sudden death, by suspending the respiration, lesions on the level with the third cervical vertebra gradually impede the actions of the diaphragm, render the respiration more or less laboured, and finally, if the lesion be severe enough, induce fatal asphyxia; affections in the dorsal region exert no influence on the movements of the diaphragm, but interfere with the due respiratory actions in so far as the intercostal nerves are concerned.

(θ) The sympathetic relations (to use a bad but an intelligible term) claim a passing notice in our remarks on the general etiology of these diseases. The most important are—(1.) Between the skin and the air-passages, especially the lungs; thus cutaneous eruptions often alternate with pulmonary affections; the too speedy cure of chronic exanthemata, ulcers, and setons, may cause the rapid supervention of pulmonary œdema or pneumonia, or may even give rise (according to Wunderlich) to tuberculosis; we presume he only means that latent tuberculosis may be thus excited to more obvious and rapid development. (2.) Between the intestinal tract and the air-passages; as, for instance, the sympathetic cough in affections of the stomach (which is easily explained by a consideration of the distribution of the pneumogastric nerve), and the itching of the nose in cases of worms; and (3.) between the organs of generation and the respiratory organs, particularly the larynx; thus, in women especially, an irritable condition of the generative organs may cause spasm of the glottis, cough, and loss of voice; and according to Duchatelet, venereal excesses give rise to chronic hoarseness; in the other sex the close connexion between the development of the larynx and of the testicles is too well known to require observation.

The *phænomenology, diagnosis*, and *general therapeutics*, are fully discussed in three somewhat lengthy chapters, which contain an excellent abstract of our present knowledge on these subjects, but offer no peculiar novelty. We proceed, therefore, to *the special consideration of the affections of the respiratory organs and the pleura.* The same arrangement is adopted as in "the diseases of the digestive system;" he first considers the NERVOUS AFFECTIONS and then the ANATOMICAL DISTURBANCES.

Spasm of the glottis is considered under three forms:

1. The acute form of spasm of the glottis in children (asthma Millari, asthma acutum periodicum, pseudo-croup, laryngitis stridulosa, laryngitis stridulus, &c.)
2. The chronic form in children (asthma Koppii, asthma thymicum).
3. Spasm of the glottis in adults.

Pseudo-croup perhaps conveys the best idea of the character of the first of these three varieties. Valleix has collected records of twenty-six well-marked cases of this affection; none of the patients exceeded their eighth year, and one was an infant only three days old; from eighteen months to two years is apparently the most common age. It is a more frequent affection in the rich and comfortable classes, than among the extremely poor; hence it is much more commonly encountered in private than in dispensary or hospital practice.

This affection is remarkable for the rapidity with which it developes itself; and this is a point on which we would lay much stress in diagnosing between it and croup. The patient goes to bed in the ordinary health, and after some hours' quiet sleep—usually about or after midnight—suddenly wakes up in a state of suffocation. The difficulty of breathing is intense, and owing to the constriction of the glottis, each inspiration is accompanied by a peculiar shrill tone. The face is at first red and afterwards bluish, the superficial veins are distended, and the pulse is small and frequent. After some minutes there is a remission of the spasm; the child

then sometimes vomits, and is usually heavy and anxious to sleep. On its waking the next morning, the only traces of the attack are a slight heaviness and drowsiness; and a careful physical examination affords no indication of disease.

A second attack soon follows, generally within twenty-four hours. If the disease occurs in a comparatively mild form, the second and all succeeding attacks are milder than the first, and the child is often well in three or four days; when, however, it is going to terminate fatally, the second attack is usually more severe than the first, and leaves the patient in a state of extreme depression. Fresh attacks of increasing intensity and duration follow, and death either ensues from suffocation during an attack, or from the general exhaustion of the nervous centres. In the latter case the attacks become less severe, or are altogether suspended for some hours or even days before death; there is less general depression, the cough is not so frequent, and the respiration, though weak, is not impeded; but the child generally becomes soporose, the pulse is thready, and can no longer be counted, the face blanched, and dissolution occurs without any indication of suffocation.

It is impossible with our present *data* to arrive at any trustworthy results in reference to the fatality of this disease.

In regard to diagnosis, we must be especially careful not to confound it with true croup. In one case the prognosis is comparatively unfavourable, and the treatment from the commencement should be very active; while in the other, notwithstanding the violence of the attack, our prognosis may be generally favourable, and only the simplest forms of treatment are demanded.

The present disease may be distinguished from croup (1) by the greater severity of the general symptoms at the commencement of the latter disease. There is greater pain in the larynx, and more general disturbance; but, on the other hand, less tendency to suffocation at the very beginning of the attack.—(2.) In the intervals in croup, there are very much more marked symptoms than in the intervals of this disease.—(3.) The voice is comparatively little altered in this disease.—(4.) There is always (according to some authors) an engorged condition of the lymphatic glands of the neck in croup, which is not present in this affection; we do not, however, place much reliance on this distinction.—(5.) In pseudo-croup there is no expectoration of false membrane, nor is any diphtheritic false membrane to be seen on the tonsils.

Many diagnostic rules have (we think somewhat unnecessarily) been laid down to prevent our confounding this disease with acute laryngitis, the suffocation of epilepsy, capillary bronchitis, or with the symptoms induced by the presence of a foreign body in the larynx.

It is difficult to form an opinion, from the statements of different writers, regarding the relative values of the different medicines that have been recommended in this disease. Emollients, purgatives of every degree of power, emetics, bloodletting, counter-irritants, antispasmodics, tonics, and narcotics, have respectively found their advocates. We believe that an injection of assafœtida, as recommended by Millar, and the application of steam or hot water to the neck during the paroxysm, and attention to the digestive organs in the interval, constitute the most important treatment.

Passing over *asthma Koppii*, or *thymic asthma*, which may be regarded

as a chronic form of spasm of the glottis in children, we proceed briefly to notice *constriction of the glottis in adults.* The *rima glottidis* may be contracted either by spasm or paralysis, and in either case there is usually more or less hindrance both to free respiration and to speech. Most writers only incidentally notice this affection as one of the symptoms of other diseases, and especially of hysteria; it deserves, however, a more general and an independent consideration.

As a general rule, spasm of the glottis is most commonly observed in the female sex, and in young rather than in old persons. It must be deemed a more serious affection in the male than in the female sex, for when it occurs in men, it most probably depends on an anatomical disturbance of the nerves of the larynx, whereas in women no anatomical changes can usually be detected.

The following are the most common causes of this constriction of the glottis:

1. The cause may be in the larynx itself, depending on the presence of a foreign body, or of an affection of the larynx predisposing to spasm. Prolonged exercise of the larynx, as in singing, or reading, or speaking aloud, occasionally gives rise to this affection, or at all events increases the tendency towards it.

2. It may be caused by the pressure of aneurisms or other tumours on the recurrent laryngeal nerve.

3. Or by reflex action from the pharynx or œsophagus.

4. It is often connected with irritation of the uterine organs, especially with painful or disordered menstruation.

5. It often accompanies or alternates with convulsions of other muscles; thus it is not unfrequently met with in epilepsy, tetanus, hydrophobia, and hysteric paroxysms.

6. Psychical influences (anger, chagrin, &c.) undoubtedly tend to produce it.

7. Plethora, anæmia, and lead-poisoning, have been likewise mentioned among the causes of this affection.

This form of disease very rarely terminates fatally; as, however, spasm of the glottis is sometimes the precursor of a more general disorder of the cerebro-spinal system, these cases should be carefully watched.

Nervous aphonia may be regarded as the mildest form of this disease. There is no positive constriction of the glottis in these cases, but merely a want of control over the muscles of the larynx, without paralysis. It is most commonly met with about the period of puberty, and may last for weeks, months, or even years, often leaving the patient as rapidly and as unaccountably as it attacked him. In olden times this nervous aphonia (like many other nervous affections) seems to have been occasionally epidemic.

Wunderlich briefly notices the singular case of a married woman with several children, who for twenty years has continually lost her voice, but immediately recovers it as soon as she has lost a few drops of blood, either by venesection or the application of leeches, and then retains it for some days, or even for a few weeks. The bloodletting must act psychically, rather than materially. Her arms are covered with scars. A very singular case has also been recorded by Ollivier.*

* Archiv. Gen., t. xx. p. 247.

Asthma next demands our attention. Wunderlich's statistics confirm the view which we have long entertained, that genuine asthma is a very rare disease; for out of 10,000 clinical patients who passed through his hands in six years, he only observed three cases at all presenting the character of asthma.

The pathological anatomy of asthma is still very unsatisfactory and imperfect. Amongst the disturbances that may give rise to this affection, Wunderlich mentions—

(1.) Those occurring in the respiratory organs. Thus, congestion, extensive tubercular or cancerous deposits, emphysema, œdema, chronic bronchial catarrh, affections of the glottis, pleural adhesions, &c., have been mistaken by some writers for asthma itself, and have been regarded by others as causes of that affection. These are often complications of asthma, but we agree with Wunderlich in doubting whether they ever actually give rise to it.

(2.) Asthmatic attacks sometimes appear to depend on changes in the brain or spinal cord and their membranes, and on tumours pressing on the pneumogastric nerve. Alteration of structure in the brain near the origin of that nerve, chronic myelitis, ossification of the pulmonary plexus, and a tuberculous condition of the bronchial glands pressing on the pneumogastric nerve, have been described by different writers as causes of asthma. Wunderlich narrates an obstinate case of asthma recurring every week or two for several years. The patient was a man who had a small tumour under the anterior origin of the sterno-cleido-mastoid muscle, which gradually extended beneath the clavicle, and probably pressed upon the pneumogastric nerve. There was also slight emphysema, but not enough to account for the severe asthmatic attacks, which usually lasted for from three to eight days, and occurred at very short periods.

(3.) Affections of the heart and pericardium, and aneurism of the large thoracic vessels, have often been described as causes of asthma. Rostan even went so far as to maintain that asthma is always a symptom of an organic disease of the heart or of the large vessels. We should think that these diseases would primarily induce mechanical dyspnœa, rather than spasmodic asthma; they may, however, in a secondary manner, irritate the pneumogastric nerve, and thus actually give rise to the disease in question.

(4.) It has been generally assumed that deranged conditions of the stomach and intestinal canal, the liver, and the genito-urinary organs, may directly induce asthma. Whether this be the case or not, it is at all events certain that errors of diet, sexual excesses, and derangements of the digestive system, increase and aggravate the attacks of persons liable to asthma.

(5.) Certain conditions of the blood may cause asthmatic attacks. A superabundance of blood, or blood too rich in solid constituents, may give rise to symptoms that may be mistaken for asthma, although they are in reality symptoms pertaining to pulmonary congestion; and, on the other hand, a deficient supply of blood, or blood that is very poor in solid constituents, may give rise to symptoms which are, however, rather those of spasm of the glottis and of the diaphragm, than those of true asthma. The paroxysms of dyspnœa occurring in putrid and purulent poisoning of the blood, do not constitute true asthma. Wunderlich seems to doubt

whether the attacks of dyspnœa, which are so common when the kidneys are inert and the urinary constituents are retained in the blood, are really deserving of being considered asthmatic; indeed, he plainly expresses it as his opinion that in all probability the *asthma urinosum* of the older physicians and of Schonlein, is nothing more than pulmonary emphysema. As, however, he grants that lead, when introduced into the system, often produces asthma, and that other poisons not unfrequently do so, we see no reason why he should object to allow that the dyspnœa produced by the accumulation of urea in the blood is of an asthmatic nature.

Whatever may be the anatomical changes which are found in examining the bodies of persons who have suffered from asthma, the disease seems essentially to consist in a temporary and paroxysmal spasmodic constriction of the smallest bronchial canals (for the cartilaginous structure of the larger tubes will not allow of their being thus affected).

Asthma is seldom the immediate cause of death; but frequent attacks give rise to hypertrophy of the bronchial tubes, emphysema of the lungs, or certain forms of heart-disease, which modify and aggravate the original affection, and hasten a fatal termination.

We find very few points of novelty in any of our authors in relation to treatment. Wunderlich recommends that during the paroxysm, if it comes on suddenly and with great intensity, the bowels should be emptied by a powerful injection; and he seems to think that there are many cases in which venesection would be serviceable. As, however, he adds, the arguments for and against bleeding are often nearly balanced, it is sometimes expedient to open a vein, and either to close it almost directly, or to allow the blood to flow, according to the immediate effect produced on the patient. The general testimony is, however, strongly against venesection. If the patient can drink, he should take a little warm tea containing opium, stramonium, or lobelia inflata. Strong black coffee and iced drinks have also been recommended. In those cases where the attack is more gradual, an emetic often gives great relief, and sometimes at once cuts short the paroxysm. Ipecacuanha in small doses is sometimes useful; but what Wunderlich has found most useful is a combination of tartar emetic with large doses of opium (from half a grain to one grain every hour). Stramonium smoked with an equal quantity of tobacco relieves the pains which are usually felt in the chest, but is of no other use. (This, at least, is Wunderlich's experience, who adds, that the much-praised camphor cigarettes are even less efficacious.) Canstatt, as we have already had occasion to mention, enters more fully into the consideration of the individual therapeutic agents, than Wunderlich or any of our authors. We give his observations on a few of the medicines that have at different times had a high reputation in asthma:

"The Datura Stramonium has obtained a character as a specific against asthma (Krimer, Cruveilhier, Reimer, Ziegler, Ward, Marcet, Meyer, Helm, Cunningham, Hegewisch, Ferrus, Elliotson). It is given in powder, in gradually increased doses, from two to twelve grains; in extract, from a quarter of a grain to four grains; or in tincture, from five to twenty drops. We frequently recommend the stalks and leaves to be smoked, the patient being directed to swallow his saliva. Hegewisch orders that the number of pipes should be increased, till all the urgent thoracic symptoms have disappeared; if a relapse comes on, the patient must again begin with one or two pipes, and proceed as before. Unfortunately, asthmatic

patients are often unable to inhale the smoke. The Datura fastuosa has been employed by Adams in the form of a tincture, of which thirty or forty drops were to be taken every two hours; and by Skipton in the form of decoction of its bark.

"The Lobelia inflata (Indian tobacco) was first employed by American physicians, and has recently obtained a high reputation as a remedy for asthma (Eberle, Barton, Chapman, Whitlaw, Andrews, Bidault, De Villiers, Reece, Sigmond, Elliotson, Newmann, Forbes, Morelli, Noack). We usually give twenty or thirty drops of the tincture of the leaves at the commencement of the paroxysm, or shortly before an attack is expected; more rarely we give the powder in doses of fifteen or twenty grains. It appears to exert a specific influence on the respiratory nervous system.

"Common tobacco acts in a similar manner; it may be smoked or given in the form of tincture. . . . . The Bignonia catalpa (indigenous in Carolina) was described as a specific by Kämpfer and Taubery; and has been more recently extolled by Brera, Brudent, Antomarchi, and Antonisca." (Canstatt's 'Handbuch der medicinischen Klinik,' vol. iii. part 2, p. 450.)

Pertussis is the last of the purely nervous diseases to which we shall advert, and our remarks upon it will be very brief. The following table, taken from Wunderlich, shows the different ways in which this disease may prove fatal. Death in cases of pertussis may take place,

A. During the paroxysm of the cough,
   1. By suffocation.
   2. In very rare cases, by extravasation of blood into the brain.

B. By complications—namely,
   1. By convulsions.
   2. By broncho-pneumonia.
   3. By meningitis, and acute hydrocephalus.
   4. By softening of the stomach.
   5. By general dropsy.

C. By secondary diseases:
   1. By tuberculosis.
   2. By pulmonary emphysema, and dilatation of the bronchial tubes.
   3. By marasmus.

The only diseases with which it can be confounded are:

(1.) Bronchitis and bronchial catarrh, when accompanied with spasmodic cough; and (2.) tuberculosis of the lungs, and especially of the bronchial glands, particularly when accompanied with a spasmodic cough, strongly resembling that of pertussis, and when the physical signs do not clearly indicate the presence of tubercles.

We extract the following tables from Valleix:

"I. *Distinctive signs between pertussis and acute bronchitis with spasmodic cough.*

PERTUSSIS	ACUTE BRONCHITIS WITH SPASMODIC COUGH
Commences with catarrh.	Often commences suddenly with cough.
Fits of coughing terminate with whistling inspiration, tenacious expectoration, and vomiting.	Fits of coughing less severe; no whistling inspiration, tenacious expectoration, or vomiting.
No fever in uncomplicated cases.	Much fever at the commencement.
Respiration natural; the normal respiratory murmur being heard in the intervals between the fits of coughing.	Different *râles* in the chest.
Only occurs once in the same subject.	May attack persons frequently.
Symptoms intermittent.	Symptoms continuous.

"II. *Distinctive signs between pertussis and tuberculosis of the bronchial glands.*

PERTUSSIS.	TUBERCULOSIS OF THE BRONCHIAL GLANDS.
A contagious and often an epidemic disease.	Neither contagious nor epidemic.
The fits of coughing prolonged, terminating with a whistling inspiration, tenacious expectoration, and vomiting.	The fits of coughing very short, and not terminating with whistling inspiration, tenacious expectoration, or vomiting.
No general symptoms in uncomplicated cases.	Symptoms of hectic fever.
Voice natural.	Voice often altered."

The treatment both in France and Germany is much the same as in this country. Emetics rank the highest, and then narcotics and antispasmodics. Valleix lays down the following scheme for the treatment of a young child in a case of uncomplicated hooping-cough. (1.) A simple *tisane*. (2.) An emetic mixture of tartarized antimony, syrup of ipecacuanha, and infusion of violets, to be taken by spoonsful till the child has vomited two or three times. (3.) Belladonna ointment, consisting of about one drachm of extract of belladonna to four drachms of cerate, to be rubbed night and morning over the epigastric region. (4.) Diachylon plaster, to be applied to the walls of the thorax if they are painful.

The *anatomical disturbances* of the respiratory organs next claim our attention. They are arranged by Wunderlich in the following manner:

I. *Those of the nasal cavity.*
II. *Those of the larynx and trachea.*
III. *Those of the bronchial tubes.*
IV. *Those of the lungs.*
V. *Those of the arteries, veins, and lymphatics of the lungs.*
VI. *Those of the pleura.*
VII. *Those of the mediastina.*

Croup or *croupous laryngo-tracheitis*, is the first disease we shall notice. No definite allusion to this disorder is found until towards the close of the sixteenth century, when Baillou records a *post-mortem* examination (in 1576), which is obviously a case of croup. In the beginning of the eighteenth century several epidemics of this disease seem to have occurred, and the symptoms were fairly described; but it was not till Home of Edinburgh, published his 'Inquiry into the Nature, Cause, and Cure of Croup,' in 1765, that the independent nature and true pathological character of this affection were fully established; indeed, as far as we recollect, it was not till this period that the term *croup* found its way into scientific medical literature.

As some writers have denied that croup is ever epidemic, we may observe that the evidence collected by Valleix and the other writers whose works we are now considering, determines this point most satisfactorily in the affirmative. Perhaps one of the most decisive epidemics on record is that recorded by Ferrand (*De l'angine membr.* Thès. Paris, 1827); in a neighbourhood of very small extent, no less than *sixty* cases of croup, *all of which were fatal*, occurred in six months. It is well known that Bretonneau and Guersant (to say nothing of other observers) strongly maintain the *contagious* nature of croup. Its epidemic tendency may perhaps serve to explain how it is that instances of *apparent* contagion have been

noticed. The diseases with which, under certain circumstances, croup may be confounded, may be divided into those seated in the pharynx, those in the larynx, in the trachea, the bronchial tubes, and possibly those in the thymus gland.

1. *Diseases situated in the pharynx.*—It is very seldom that simple inflammation of the pharynx can interfere so much with the respiration as to be mistaken for croup; yet when the tonsils are very much swollen, we occasionally observe a considerable impediment to the breathing, and a well-marked alteration of the voice. In this case, however, the sensation of suffocation is continuous, whereas in croup it is more or less paroxysmal. The voice is different; in this affection it is nasal, while in croup it is often scarcely perceptible; moreover, there is little or no cough, while in croup there is always a rough, dry cough.

2. *Diseases situated in the larynx.*—We sometimes meet with cases in which it is extremely difficult to distinguish between croup and intense acute laryngitis. The following are the most important points in reference to the diagnosis between these diseases. In croup there is viscid expectoration, frequently containing shreds of false membrane, while in simple laryngitis the expectoration is viscid, thready, and frothy. In examining the throat in croup, false membranes are often seen in the tonsils, while in acute laryngitis, the very most that is perceptible is a slight redness or a swelling of the epiglottis. We have already noticed the leading points of distinction between croup and laryngismus stridulus, or pseudo-croup (see p. 94). The means of distinguishing between croup and the œdema of the glottis (*laryngite œdémateuse*) are not always very satisfactory. The leading points of difference are—(1.) that œdema of the glottis is most commonly a consequence of an old ulcer of the larynx, or else it occurs during convalescence from some acute disease, while croup occurs either during perfect health, or at the close of one of the exanthemata; (2.) that in œdema of the glottis there is little or no fever, and there is no enlargement of the ganglia of the neck, both of which are commonly observed in croup; and (3.) that the voice is differently affected in the two diseases.

3. *Diseases situated in the trachea and bronchial tubes.*—There is little danger of confounding croup with either simple or pseudo-membranous tracheitis. Capillary bronchitis, however, in some respects, presents symptoms not very distinct from those of croup. The distinctive signs and symptoms are given in the following table:

CROUP.	CAPILLARY BRONCHITIS.
Dyspnœa in more or less marked paroxysms.	Continuous intense dyspnœa.
Whistling inspiration; respiration laboured.	Inspiration somewhat stertorous; respiration very rapid, short, and panting.
Voice almost gone.	Voice not affected.
Expectoration of false membrane in the form of a large tube, or, more frequently, in shreds.	Expectoration of ramifying false membranes. (This does not very often occur, but when it is present it is decisive.)
On auscultation we hear whistling or hissing inspiration, and a weak respiratory murmur.	On auscultation we have mucous and sonorous *râles*, often extending over a large part of the chest.

4. Lastly, *Thymic asthma* and croup essentially differ in the circumstance, that in the former affection there is not that suppression of the voice which we observe in the latter.

We now proceed to give a very brief abstract of Valleix's observations on the treatment of this disease.

Bloodletting has almost invariably been advocated in this disease; but upon weighing the evidence for and against it, we cannot help feeling assured that it has, to say the least, been too highly extolled. It is, we believe, generally acknowledged that the *type* of disease, especially of the so-called inflammatory disease, has undergone a great change during the last quarter of a century. Diseases no longer present the vigour which they seem to have enjoyed in days of yore; and the heroic treatment which even our own immediate predecessors were in the daily habit of employing, would now be fully as likely to prove detrimental to the patient as to the disease. We entirely concur in the opinion, distinctly expressed by Bretonneau and Valleix, that bleeding, either general or local, is devoid of that powerful curative effect which the majority of writers have assigned it, and that it does not strikingly arrest the progress of the case. When the disease is epidemic, bloodletting is often worse than useless. A moderate abstraction of blood, combined with other means, is unquestionably of service in many cases; but it must be candidly confessed, that at present we have not the *statistical data* from which we can judge of the value of bloodletting, or any other isolated remedy or mode of treatment.

Valleix has collected and analyzed fifty-three cases, in thirty-one of which emetics (tartar emetic and ipecacuanha) were the principal remedies. Of these thirty-one, fifteen, or very nearly one-half, recovered; while of the remaining twenty-two, to whom emetics were either not at all or only sparingly given, only one recovered—a difference far too great to be the result of a mere coincidence. Of the thirty-one children treated with powerful emetics, twenty-six ejected false membranes in the effort of vomiting; and of these twenty-six, fifteen, or nearly three-fifths, recovered. In the five remaining cases, which all proved fatal, not a trace of false membrane was ejected. Of the remaining twenty-two, to whom powerful emetics were not administered, two ejected false membranes, and it was one of these that recovered. Tartar emetic, either alone or in combination with ipecacuanha, is chiefly given by French and English physicians; the Germans seem, however, to prefer sulphate of copper to any other remedy of this class. Frelitz (in a memoir in Hufeland's 'Journal' for 1834) observes, that "when the false membrane is formed, emetic doses of sulphate of copper have a miraculous effect, not only in removing the coagulated lymph which has transuded and accumulated in the bronchial tubes, but also by acting in a special manner on the sensibility of the nerves of the lungs and the larynx, by setting up a sort of counter-irritation in the stomach and intestines, and establishing perspiration; and by these means arresting the process of transudation."

Amongst the medicines to which a certain specific virtue has been ascribed, Valleix mentions—(1.) *Sulphuret of potash;* but as, in the first place, there is little or no sound evidence of its efficacy, and as, secondly, it is a dangerous remedy, we may pass it over without further remark. (2.) *Polygala Senega* has enjoyed a high reputation in croup; but most of the

physicians who have advocated its use, at the same time employ calomel, diaphoretics, and tartar emetic—one or all; and commence with the abstraction of blood. Hence, we have no means of ascertaining what the efficacy of polygala, given alone, would be. We know, however, that this plant has well-marked emetic and purgative properties; and it is to these, and chiefly to the former, that it doubtless owes its value in such diseases as croup. (3.) *Mercurials.* Calomel seems as freely given in Germany, if not in France, as in England. A case is alluded to, in which Autenrieth gave a scruple and a half in twenty-four hours to a child aged five years and a half. Valleix believes that there is no evidence that calomel can cure croup.

Local counter-irritants applied to the skin.—Blisters to the neck and chest, and sinapisms to the legs and feet, have been warmly recommended by several writers: we believe that cloths, soaked in very hot water, form the best counter-irritant for the throat and neck. The insufflation of various powders, especially of alum, calomel (one part to twenty of powdered sugar), and of red precipitate (one part to twelve of powdered sugar-candy, according to Trousseau), has been found serviceable in some cases. Dr. Horace Green's local application of nitrate of silver has been so recently noticed in this journal, that we need not advert to it.

Of all these medicines, then, which are we to select? The answer is by no means easy. Putting out of the question Dr. Horace Green's treatment, of which we have not yet had sufficient experience to speak as to its safety, we believe that our principal reliance must be placed on the class of emetics, especially tartarized antimony, and on counter-irritation, by hot cloths or sponges to the neck and throat. Whether bloodletting should be adopted is a question that must be determined by the consideration of two points—viz., 1st, the type of the disease; and 2ndly, the general condition and strength of the patient.

Valleix devotes a considerable space to the subject of tracheotomy in croup. We have condensed from his pages the following brief rules, by which the practitioner may decide whether or not tracheotomy is advisable:

1. Tracheotomy should be practised in all cases in which decided symptoms of asphyxia have set in, when the voice is gone, and when the efforts of the cough and of vomiting appear insufficient to expel the false membranes. In such a case we should not hesitate to operate, unless there is present one of the contra-indications presently to be noticed.

2. It is also expedient that it should be had recourse to in less advanced cases—namely, where there is great difficulty of respiration, and where the most powerful remedies will not cause the expulsion of the membrane, before the forces are too far depressed, or a false membrane is formed in the bronchi. The number of fatal cases of tracheotomy in croup would doubtless have been much smaller, if the operation had been not too long postponed.

The following are the contra-indications:

1. As a general rule, although at least one exceptional case is on record, when the false membrane extends to the bronchi.

2. When pneumonia, pulmonary tubercles, or any far-advanced organic disease, is present.

The next disease to which we shall advert is *œdema glottidis*, an affection essentially of adult life, and more common in the male than the female sex. It seldom attacks perfectly healthy persons, its most common victims being convalescents from serious diseases—as, for instance, typhus, the exanthemata, pneumonia, &c. Nor is it uncommon in connexion with other diseases of the larynx, especially with ulceration, or abscesses in that portion of the neck.

Sub-mucous infiltration of the glottis is generally induced by more or less intense hyperæmia; when, however, it occurs during convalescence from typhous or exanthematous fevers, there is mere serous effusion into the cellular tissue, without any hyperæmia. It sometimes also occurs in general dropsy; it is then more chronic than in the preceding cases.

Of forty cases collected by Valleix,* two occurred in children less than ten years of age; the ages of the others ranged from eighteen to seventy-one; twenty-nine were males, and eleven females. Only four of the persons attacked were previously in a state of health; twelve were suffering from severe laryngeal disease; ten were suffering or convalescent from typhus fever; four were recovering from pneumonia; two from acute cutaneous eruptions; and two from surgical accidents.

The following are the symptoms; we have slightly condensed them from Wunderlich.

The disease may begin as a common case of laryngitis, or as a laryngeal catarrh. There are hoarseness, pain, and a sensation of burning and pricking in the larynx, with an obstinate, dry, croup-like cough, and fever; these symptoms in a few days attain a degree of great intensity, and we likewise have perfect aphonia and much dyspnœa. The respiration is now audible at a considerable distance, and it may be easily seen that the act of inspiration is more interfered with than that of expiration. The thyroid gland and the whole neck are swollen. The patient complains that he feels as if a foreign body were in the larynx, or as if he were being strangled; and on examining with the finger, we can detect a tumour projecting behind the epiglottis. As the dyspnœa increases, the face becomes blue, the pulse small and irregular, and the skin cold. The patient throws himself into all positions, with the view of obtaining relief, bends back the neck, and struggles for air. Death either supervenes during a paroxysm of this sort, or else, as is more common, the patient sinks into a comatose state, the respiration gradually becomes slower, and the pulse smaller, and he dies without a struggle.

Recovery without operative aid can only be hoped for when the swelling begins to diminish after the first day or two, or when its progress has been slow, and it has never attained much size. When, in addition to the dyspnœa, there is the cyanotic coloration of the face, a spontaneous recovery is scarcely to be expected. As a general rule, the danger of the affection is proportional to the rapidity with which it comes on.

In reference to treatment, Wunderlich observes that:

"If we have to treat a vigorous person, and have reason to believe that the tumour is induced by hyperæmia, we must take a large quantity of blood from the arm, apply leeches freely to the neck, and then give an emetic or drastic purgative, so as to establish a derivative action to the intestines.

* Mémoires de l'Académie de Méd., 1845, t. xii. p. 82, published three years subsequently to the volume of the 'Guide du Médecin Practicien,' containing the article on 'Laryngite Œdémateuse.'

"This mode of treatment must be several times repeated during the first few days, till the impediment to the breathing begins to yield.

"When the patient was previously in a debilitated condition, (as, for instance, if he were phthisical or convalescent after fever, &c.,) or when for any cause we have reason to believe that there is no hyperæmia, and that there is merely a serous effusion, we may try the effect of strong derivative action to the throat and neck, as, for instance, vesication by boiling water, hot tincture of cantharides, &c. Strong emetics (sulphate of zinc and sulphate of copper) and drastic purgatives are also indicated.

"If the tumour be visible behind the epiglottis, we may attempt to scarify it with the lancet, or with the finger-nail, cut to a sharp point.

"When the danger of suffocation becomes very urgent, and the vesicular respiration weak and inaudible in some parts of the chest, and when, further, the coun tenance assumes a bluish red colour, we must no longer delay opening a new way for the respiration beneath the impediment. The best method is to cut through some of the rings of the trachea, beneath the cricoid cartilage. When that is impossible, we select the space between the cricoid and thyroid cartilages, and introduce and retain a canula, till the respiration can again be conducted through the larynx." (Wunderlich, vol. iii. p. 374.)

The next disease to which we shall advert is one of very common occurrence—namely, *acute bronchitis*. It is perhaps worthy of notice, that no definite idea was attached to the word bronchitis, although it had been long vaguely in use, till P. Frank published his 'Interpretationes Clinicæ' in 1812, and Badham his 'Essay on Bronchitis' in 1814; these pathologists being the first who clearly established the anatomical seat of this affection.

In his remarks on the etiology of this affection, we find that Wunderlich mentions the following diseases as having a tendency to induce secondary bronchitis:

Diseases of the air-passages themselves, extending to the bronchial tubes; as, for instance, coryza, laryngitis, and tracheitis, on the one hand, and pneumonia, tubercular disease, and cancer and gangrene of the lung, on the other. Chronic bronchitis must also be mentioned as predisposing to the acute form of disease.

Diseases which mechanically cause a stasis or congestion of the bronchial mucous membrane; amongst these we may especially place enlargement of the thyroid gland and heart-disease.

Acute exanthematous eruptions, especially measles, are commonly accompanied with more or less bronchitis.

Affections in which the blood is poisoned or qualitatively affected to any degree. Among the acute diseases of this class we may place typhus, pyæmia, putrid fever, glanders, &c., and among the chronic, lead-cachexia, arthritis, Bright's disease, degeneration of the liver and spleen, the dyscrasia of spirit-drinkers, marasmus, &c.

The following is a condensed sketch of the mode of treatment recommended by the same author. It must be adopted to meet the following indications:

1. The *indicatio causalis*, which, when the disease is once established, consists merely in avoiding fresh noxious influences, which might aggravate or prolong the disease, or might promote relapses, and in due attention to diet and general hygiène.

2. The indication to moderate the hyperæmia; general bloodletting when necessary; local bloodletting; cooling salts, as, for instance, nitre; and

drinks containing the acetates and citrates; derivatives, as, for instance, drastic purgatives, and counter-irritants applied to the skin. This indication can generally only be of essential use, either at the commencement of the disease or during an exacerbation.

3. The indication to favour and promote secretion. Abundance of mild warm fluids, nauseating doses of tartarized antimony, ipecacuanha, and squill; the inhalation of vapour; when there is much difficulty in getting rid of the secretion, we must have recourse to larger doses of the above medicines, to muriate and carbonate of ammonia, and to senega.

4. The indication to allay the cough, general irritation of the throat, &c. The cough can often be moderated to a great degree by a mere effort of the will. Large warm cataplasms or hot fomentations applied externally, and mild mucilaginous infusions, decoctions, and emulsions, are serviceable. When the cough is very hard and obstinate, recourse must be had to narcotics (morphia, prussic acid, hyoscyamus, stramonium, and aconite), to metallic remedies (as flowers of zinc), and to counter-irritants applied to the skin (as cantharides, croton oil, and tartar emetic).

5. The indication to limit too abundant a secretion. This is effected partly by expectorants, as senega, ammoniacum, the balsams, &c., and partly by medicines acting strongly on the kidneys, as squill, digitalis, juniper, and turpentine.

6. The indication to support the strength, especially in young children, weak persons, and the aged. This must be effected chiefly by diet. When there is much collapse, musk may be given to children, and camphor and wine to adults. Aromatic liniments should be rubbed over the chest, whatever the age of the patient may be.

Among the peculiarities of treatment required for special forms of bronchitis, we may observe that—

"In the catarrh of new-born infants and children at the breast, we should prescribe first an emetic (kermes) in several small doses, then mild laxatives (calomel and manna), diaphoretic and diuretic infusions, (Rad. graminis cum liquiritia), and sweet mucilaginous drink (containing marsh-mallow). When the cough is very obstinate and irritative, one or two drops of tincture of opium, and a blister. In cases of greater danger, benzoic acid, senega, balsam of copaiva, musk: when there is much fever, oxide of zinc. [We much doubt whether the safety of the "new-born infants" will be much promoted by giving them "one or two drops of tincture of opium."]

"In the catarrh of aged persons we must avoid purgatives and all depressing influences. An emetic should be prescribed at the commencement, and afterwards senega, and balsam of copaiva: when the affection has existed for some time, we may have recourse to carbonate of ammonia and benzoic acid; and when there is great exhaustion, quinine, wine, and camphor, must be ordered. . . . .

"In the catarrh of tuberculous patients, if they are still tolerably strong, local abstraction of blood must be prescribed; if they are much reduced, we must have recourse to narcotics. The treatment already recommended must be adopted, except that emetics, drastics, and strong diaphoretics should be avoided. The stronger expectorants, as polygala and lichen, should be tried at a comparatively early stage.

"In the catarrh of patients with heart-disease, we must decide from the general symptoms, whether there should be any local abstraction of blood. In addition to other treatment, digitalis should be given in an early stage of the affection, and emetics and diaphoretics should be avoided.

"In catarrh during pregnancy, the abstraction of blood, especially venesection,

gives great relief. Narcotics must frequently be had recourse to, in order to avoid the bad effect of the cough on the ovum. The bowels must be kept quite open by injections and the mildest laxatives, all drastic purgatives, emetics, diaphoretics, and powerful diuretics, being avoided.

"In catarrh with copious serous secretion, we must prescribe emetics, drastic purgatives, and powerful diuretics, and must have recourse to the stronger expectorants, as senega, ammoniacum, balsam of copaiva, arnica, carbonate of ammonia. When there is much perspiration, aromatic washes to the chest are very useful.

"In capillary bronchitis, venesection must only be had recourse to in adults, in primary attacks, and at the commencement of the disease; even under these circumstances its uses are very doubtful, and in opposite conditions the danger of such a practice is unquestionable. Local bloodletting is less dangerous, but never of much service. The repeated administration of emetics is the most serviceable treatment, but they are only of use when they produce actual vomiting; we must also apply irritating cataplasms to the soles of the feet, give warm, mucilaginous, sweet drinks, and be careful that the supine position is avoided. At a later stage, in order to facilitate the expectoration, we must also give stronger remedies, as senega and turpentine, and if the case becomes chronic, balsam of copaiva." (Wunderlich, vol. iii. pp. 399, 400.)

Wunderlich describes two varieties of *chronic bronchitis* — namely, chronic bronchitis with abundant secretion, and dry chronic bronchitis. The former either gives rise to dilatations and contractions or obliteration of the bronchial tubes, with pulmonary emphysema and œdema, and general dropsy, and finally (though often not till many years have elapsed) to suffocation from the non-removal of the sputa; or in an exacerbation it merges into acute œdema of the lungs, capillary bronchitis, or pneumonia, and in the course of a few days terminates fatally. The dry variety, on the other hand, commonly terminates in pulmonary emphysema, or when this is simultaneously present, it is developed to a greater degree: moreover, cirrhosis of the lungs and obliteration of the bronchial tubes, as well as œdema of the lungs, appear to be developed by the influence of dry chronic bronchitis. It is by these secondary diseases that it most commonly proves fatal, although it sometimes terminates in acute capillary bronchitis, or pneumonia.

Wunderlich gives an excellent history of *pulmonary emphysema.* In alluding to Dr. Ramadge's idea, that the production of emphysema cures pulmonary tuberculosis, he observes that he has seen very decided cases of chronic emphysema, in which tubercles and tuberculous caverns were present in the lungs. The following are his remarks on the treatment of this affection. The indications are:

1. The treatment of the accompanying catarrh, by mucilaginous drinks, narcotics, expectorants, and diuretics.
2. The occasional removal of the stagnated air by an emetic.
3. The avoidance of all accidental impediments to the respiration, such as tight-lacing, the accumulation of large fæcal masses in the colon, &c.
4. A moderate and uniform exercise of the respiratory muscles; moderate general exercise, and the avoidance of a sedentary life; no overexertion of the respiratory organs.
5. A good, pure, dry atmosphere, free from all noxious exhalations.
6. A diminution of the necessity for respiration (Laennec) by the use of opium.
7. An artificial augmentation of the contraction of the walls of the air-

cells; the breathing of a somewhat stimulating (mountain or sea) air, inhalations, antimony, strychnine, pectoral tonics (such as senega, polygala, lichen Islandicus), and electricity.

8. The avoidance of any cutaneous chill, and care to keep the functions of the skin in a state of activity by wearing flannel, by friction, stimulating washes, &c.

9. When there is a threatening of immediate danger, we must prescribe ether, camphor, and other stimulants. Recourse should never be had to general bloodletting, even when there is apparent congestion of the lungs; if there is much impediment to the return of the blood from the head, a little blood may be taken locally from the nape of the neck, or the temples.

Bronchial and pulmonary hæmorrhages next claim our attention. Every practical observer must, we think, agree with the opinion expressed by Wunderlich, that it is not always possible to draw a strict line of demarcation between hæmorrhage from the bronchial tubes and from the lungs. He adopts the following arrangement of this class of hæmorrhages:

1. Hæmorrhage in which the blood is for some time retained in the bronchial tubes, and finally discharged from them; the question whether the blood were originally effused in the bronchial tubes, or whether it came from the pulmonary cells, being put out of the question. (Bronchial hæmorrhage, in the more extended sense.)

2. Hæmorrhage in which the blood is entirely or partially retained in the uninjured pulmonary tissue, through a portion of which it is uniformly infiltrated. (Hæmoptoic infarctus.)

3. Hæmorrhage in which the blood collects in a cavity formed by the destruction of the tissue of the lung. (Pulmonary apoplexy.)

We shall briefly notice the etiology and pathological anatomy of these three forms of pulmonary hæmorrhage.

Blood may be effused into the bronchial tubes in various ways, and from numerous causes; it may arise from a perforation of an artery (aneurism) into a bronchus, from intense hyperæmia and inflammation of the bronchial tubes, from ulceration of the bronchial mucous membrane, or as a consequence of pulmonary hyperæmia, pneumonia, tubercular caverns, or gangrene of the lungs.

Besides the changes giving rise to the effusion of blood, the following anatomical disturbances are found to occur in the bronchial tubes. The effusion is observed to have taken place either in the sub-mucous cellular tissue, or in the canal, or in both simultaneously: in the first case, we observe larger or smaller patches of a blackish-red colour on the mucous membrane, which, when cut through, show that the sub-mucous tissue is swollen and infiltrated with blood. When blood has accumulated in the canal, as indeed usually occurs in fatal cases, we may find it either fluid or in clots.

Hæmoptoic infarctus—the second of these forms of pulmonary hæmorrhage—was found by Bochdalek to be the cause of death 76 times in 4500 examinations of the dead body in the Prague Hospital; so that, taking into consideration the cases which recover, we may regard this as one of the most common forms of interstitial hæmorrhage. It occurs as a consequence of intense hyperæmia of the lungs, terminating in

laceration of the vessel. Although the cause of the hyperæmia and of the infarctus may thus be regarded as one and the same, all forms of hyperæmia—even when of the same degree and the same intensity—do not equally predispose to the rupture of vessels and the induction of sanguineous infarctus. This peculiar tendency is most manifest in cases of heart-disease, especially when the cardiac affection has given rise to dropsical effusions, and in cachectic conditions of the system, as in cases of habitual intemperance, of diseased spleen and liver, of Bright's disease and of cancer. This variety of hæmorrhage is very rare in persons who are otherwise well and of strong constitution.

In this form of hæmorrhage, the blood is effused into the air-cells without any perceptible laceration of tissue, and remains there in a coagulated state. On examining the pulmonary tissue, we perceive one or more black or blackish-red patches, varying in size from a mere point to the magnitude of four cubic inches, and commonly situated in the lower lobes. These portions do not crepitate, sink when placed in water, and on great pressure nothing exudes but a little thick sanguineous fluid. They contrast most strongly with the other parts of the lungs, when these organs have comparatively little general tendency to hyperæmia. The bronchial tubes are usually filled with fluid, or coagulated blood, are redder than usual, and present an appearance of ecchymosis. If the hæmorrhagic infarctus has continued for some time without giving rise to a fatal result, the effused blood gradually becomes less dark, and assumes a brownish-red tint; there is a sanguineo-serous infiltration around the affected spot, and the blood becomes gradually resorbed: the spot itself, however, even in the most favourable cases, remains for a considerable time dark-coloured, flabby, and inelastic, and cannot receive the proper supply of air. The resorption of the blood is often only imperfectly effected; and in that case, the diseased spot, instead of softening, hardens, and presents a black, shrivelled appearance. In some cases, again, there is inflammation set up, and the whole becomes converted into a circumscribed abscess; and gangrene has been known to supervene on this affection. These two last-named terminations are, however, very rare.

Pulmonary apoplexy, that is to say, laceration of the tissue of the lung and the effusion of blood within it, is the rarest of the three forms of hæmorrhage from the respiratory passages. It is most commonly produced by the rupture of one of the larger pulmonary vessels, consequent on atheromatous deposition, aneurism, ulceration, gangrene, &c.; and is very seldom, if ever, caused by capillary hæmorrhage and hyperæmia.

The extravasated blood thus forms a cavity in the pulmonary tissue in which it collects. When the hæmorrhage occurs near the outer surface of the lung, the blood often bursts through the serous membrane, and accumulates in the pleural sac. Bricheteau has recorded a singular case (Archiv. Gén. de Méd. t. xii. p. 401), in which the blood made its way into the pleural sac, through five or six distinct openings.

We frequently meet with cases which seem almost equally pertaining to infarctus and pulmonary apoplexy; indeed, we believe that although extreme cases of each are well marked, the two affections so merge into one another, that no strict line of demarcation can be drawn between them.

The following are the principal modes in which severe cases of bronchial and pulmonary hæmorrhage may terminate:

(*a.*) In cure; the pulse, skin, respiration, &c., gradually becoming normal.

(*b.*) In pleuritis; especially in cases where the blood has been effused in the vicinity of the pleura, or has perforated that membrane.

(*c.*) In pneumonia, which is set up about the spot in which the effusion has occurred; it may either remain circumscribed, or gradually extend itself.

(*d.*) In œdema of the lungs.

(*e.*) In gangrene of the lungs; especially when the pulmonary vessels are plugged up with clots of blood or fibrin.

(*f.*) In tuberculosis.

(*g.*) In death; which may occur—(1.) Directly, by suffocation, or by the gradual supervention of coma, with or without anæmia; or (2.) Indirectly, by the secondary diseases to which we have already referred.

There is nothing in the remarks on treatment to call for special comment, and we therefore pass on to the next head, the

Exudations and Infiltrations. These are arranged under the seven following heads:

1. Œdema of the lungs.
2. Pneumonia.
3. Typhous exudation.
4. Pulmonary tuberculosis.
5. Cancer of the lungs.
6. Pigmentary deposits in the lungs.
7. Earthy deposits in the lungs.

Œdema of the lungs was never clearly recognised till the time of Laennec; and, indeed, it is to that distinguished physician and to Andral that we are indebted for most of the knowledge we at present possess on the subject. It is an affection common to all ages, and occurring more frequently as a secondary than as a primary disease. It may arise from a diseased condition of the blood, especially from too watery a condition of that fluid; occurring either slowly, in the form of "serous cachexia" and general dropsy; or suddenly, from the rapid suppression of watery discharges from the skin or kidneys, or the rapid absorption of serous effusions. Granular degeneration of the kidneys is the most frequent cause of the chronic alteration of the blood which induces pulmonary œdema.

Other lung-diseases, especially gangrene, pneumonia, and catarrh, may give rise to pulmonary œdema, which in these cases is often overlooked, its symptoms being partially masked by those of the primary disease. It may originate in disease of the heart or vessels. Any obstacle to the free return of the blood from the lungs, either insufficiency or obstruction of the mitral valves, or contraction or occlusion of the pulmonary veins, may produce œdema. That it may depend upon disease of the nervous system is obvious, from its supervening after section of the pneumogastric nerve, and from the frequency of its occurrence in cerebral diseases.

Lastly, it sometimes occurs without any apparent cause, and in aged and weakened persons it rapidly proves fatal. This is a disease which sometimes proves fatal even in a few hours. In this very rapid form there is sudden dyspnœa, the circulation is impeded, the brain overloaded with blood, the face livid, and a little bloody froth exudes from the lips of the dying patient. Commonly, however, the disease, even in its acute form, is not so rapid in its progress, and the following are the ordinary symptoms:

There is very painful dyspnœa, and a straining, spasmodic cough, which

brings up a little frothy and watery sputum, often tinged with blood. The thoracic movements are very much impeded. The respiratory sound is at some spots blowing, hissing, or almost whistling, while at others there is a finely vesicular, moist crepitation, especially at the posterior and inferior parts of the thorax, and sometimes (when the œdematous lung is firm and dense) there is bronchial respiration. The sounds elicited on percussion are generally almost normal, but sometimes rather tympanitic, and occasionally, especially in the lower parts of the lung, and towards the close of the disease, somewhat dull. The countenance is expressive of suffering, and has a blanched and dusky œdematous appearance, a livid tint supervening shortly before the fatal termination. If the cause of the œdema be not quickly removed, the patient dies from suffocation, with which coma is frequently associated.

Chronic œdema only gives rise to striking symptoms, when it affects a large portion of the lungs.

In regard to treatment, in the very rapid cases to which we have alluded, we must have recourse to powerful counter-irritants to the skin and to stimulating enemata. These cases are, however, seldom amenable to treatment. In the less acute and in the chronic cases, we must endeavour to remove the cause on which the œdema is dependent; thus, if it depend on suppression of the cutaneous transpiration, we must apply stimulating embrocations, hot fomentations, &c.; if on congestion or pneumonia, we must abstract blood; if on the rapid drying of chronic ulcers, we must dress them with cantharides; if on renal disease, we must direct our treatment to the kidneys, and so on. We must also attempt to establish a derivative action towards the intestinal canal and the kidneys, by drastic purgatives in the one case, and in the other, at first, by acetate of potash and nitre, and subsequently by squills and juniper. Emetics are only rarely of service. In chronic cases there is an abundant watery expectoration, which we must attempt to check by such means as will strengthen and give tone to the lungs.

For this purpose we must recommend a dry and moderately warm atmosphere, and such remedies as polygala, senega, lichen islandicus, and iron.

The subject of *pneumonia*, when taken in its general bearings, is so extensive, and is so fully discussed by all our authors, that we shall not attempt to review it in the present article. We shall shortly have occasion to direct the attention of our readers to several monographs that have recently appeared on this disease; and we can then introduce any remarks of Wunderlich, Canstatt, Valleix, or the authors of the Compendium, which we deem especially worthy of notice.

In his remarks on the etiology of *pulmonary tuberculosis*, Wunderlich notices the influence of age, sex, and constitution, of external causes which may give rise to this disease, of functional derangements of other organs, of different modes and habits of life, of the different seasons of the year, of climate, of contagion, and finally of local processes in the respiratory organs. As the subject is one of much importance, we shall endeavour to give a comprehensive abstract of his views.

Age.—Although pulmonary tuberculosis may occur at any age, it is extremely rare in the fœtus, and before the third or fourth month of extra-uterine existence. This is regarded as the first minimum of

frequency. About the period of first dentition, there is an augmented disposition to tubercular deposits generally, and to pulmonary tuberculosis; the bronchial glands are, however, more frequently affected than the lungs. At about the eighth year, the frequency of tuberculosis somewhat diminishes, and this diminution becomes more rapid at the age of puberty, the second minimum being reached at about the twentieth year. From this period the frequency rapidly advances, and attains its second maximum at about the thirtieth year, when at least one-half of the dead bodies that are examined present pulmonary tubercles, which are far more frequently the actual cause of death than the pulmonary tubercles found in the first maximum between the second and fourteenth years. From the thirtieth year the frequency diminishes, at first very gradually, but much more rapidly between the fiftieth and sixty-fifth years, without, however, attaining the minimum presented at about the twentieth year; in very advanced age, the frequency again slightly rises. The above conclusions are deduced from the statistical investigations of Guersant, Papavoine, Sir James Clark, Clendinning, Louis, Lombard, Briquet, and Cless.

Sex.—The evidence on this point is very contradictory, but the disease seems to preponderate slightly in the female sex.

Constitution.—An hereditary predisposition to this disease has been recognised from the earliest times. It is still undecided whether the father or the mother exercises the greater influence in transmitting the disease; when both are phthisical, or spring from families in which the disease prevails, the tendency is undoubtedly increased. Piorry found that of 269 cases of phthisis, sixty-three were unquestionably hereditary; and he further ascertained, that of the children of 174 healthy women, only sixteen were tuberculous. In ninety-five cases of phthisis investigated by Briquet, there were thirty-six in which an hereditary tendency was ascertained. It may be confidently expected that much important statistical matter on this and kindred points will be afforded us by the able staff of the Consumptive Hospital. It is commonly believed that parents suffering from any debilitating causes, and especially from scrofula or protracted syphilis, or marrying at too early or too late an age, are liable to engender phthisical children; but it is a matter of extreme difficulty to obtain trustworthy statistical evidence on these points.

We must omit, from want of space, any notice of Wunderlich's excellent description of the signs of a tuberculous diathesis. In his remarks on the peculiar form of the finger-nails, so commonly associated with phthisis, and formerly regarded as characteristic of that disease, he observes that it likewise occurs in emphysematous patients, and in cases of heart-disease; and further, that it often does not show itself till near the fatal termination of the disorder.

"Among the acute diseases, typhus presents a singular relation towards tuberculosis. Tubercles are very rarely found in the bodies of persons who have died from typhus; but when typhus attacks a tuberculous patient, it appears to induce a rapid softening and solution of the tuberculous deposits. Moreover, in convalescence after typhus, it not unfrequently happens that a widely diffused, very acute, and rapidly destroying tuberculosis is developed, which constitutes one of the most fatal of the sequelæ of typhus. Dysentery appears to resemble typhus in its relations to tuberculosis.

"Of all the acute exanthemata, measles the most frequently give rise to the

development of pulmonary tubercles, while scarlatina and variola appear to exert no influence of this nature.

"Rilliet and Barthez state, that of 314 cases of tuberculosis, twenty-two followed directly upon measles. As they found that of 208 bodies of vaccinated children, 138 or 66 per cent. were tuberculous, and that of ninety-five non-vaccinated bodies, only thirty or 31·5 per cent. were tuberculous, they infer that vaccination increases the disposition to tuberculosis.

"Moreover, the disposition to tubercular deposits appears to be augmented during convalescence from various acute diseases, and especially epidemic influenza.

"Only few chronic diseases are known, which heighten the disposition to tuberculosis, and in which tubercles are very frequently found. Of these, diabetes mellitus is the most striking; for here it is the rule, rather than the exception, to find tubercles in the lungs. They are also very common in mental diseases, and are not unfrequently met with in Bright's disease. Phthisis also sometimes supervenes in chronic skin-diseases." (Wunderlich, vol. iii. pp. 508, 509.)

The influence of climate on the development of phthisis is a subject on which much has been written, but regarding which little has been satisfactorily demonstrated. Bennoiston de Chateauneuf has calculated that of one thousand soldiers dying in the north of France, eighty-five were tuberculous; while of an equal number in the central parts and the south, seventy-three and eighty-two were thus affected. In Marseilles, one-fourth of the population is carried off by phthisis. It is common in the West Indian islands, in Madeira, in Rio Janeiro, in New Zealand, in Nice, Florence, Naples (where, according to Journé, three deaths in every seven result from phthisis), in Malta, Spain, Portugal, Calcutta, and Madras. Hence, no climate appears to afford an exemption from this malady. Wunderlich seems to incline to the belief that there is an antagonism between intermittent fever and phthisis; and hence that marsh-lands do, to a certain degree, afford a protection against this disease.

The following are the *local processes in the respiratory organs* which may give rise to tubercular deposits:

Frequent attacks of acute bronchial catarrh seem to predispose towards phthisis, or at all events to hasten the eruption of its symptoms: so, also, do epidemic catarrh and pertussis; while, on the other hand, chronic bronchial catarrhs seem to keep off pulmonary tuberculosis.

Pulmonary congestion is very favourable to the development of tubercle, especially when it frequently recurs and affects the upper lobes.

Hæmorrhage may give rise to the formation of tubercle in a secondary manner, by the retention of coagula.

Pneumonia is a very frequent cause of tuberculosis, the remains of the non-absorbed pneumonic infiltration being readily metamorphosed into tubercular matter.

Emphysema, on the other hand, has a tendency to exclude tuberculosis in the lungs, or at all events to check its extension.

Pleuritis with adhesions predisposes to tuberculosis, the metamorphosis commencing in the plastic exudation, and from thence extending to the lung. Compression of the lung, to a certain degree, but not altogether, excludes the development of tubercle.

The forms of tuberculosis described by Wunderlich are—

1. Miliary granulations.
2. Crude tubercles, or tubercular nodules.
3. Tuberculous infiltration.
4. Tuberculous exudation in the smaller bronchial tubes.

"The further metamorphoses of pulmonary tubercle, whether occurring as granulations, nodules, or infiltration, are—

"1. *Softening.*—This sometimes commences at the centre, sometimes at some other point, and sometimes at several points at once. The mass which was previously firm and dry, and of an almost chalky white, or very slightly pale yellow tint, begins to change to a deeper yellow, and to become soft and pulpy, till it is finally converted into a fluid containing fragments of solid tubercle, and which on a microscopic examination is found to present the elements of tubercle and a few granular corpuscles (pus-corpuscles). Thus the tubercle is converted into an abscess, sometimes only at particular spots, as in infiltration, but sometimes over its whole extent.

"Simultaneously with this softening process, there is generally a further extension of the tubercular deposit in the surrounding part; and as this additional tubercular matter also softens and becomes dissolved, the fluid contents unite with those in the first abscess, and the abscess thus enlarged finally opens (most commonly) into a bronchus, but occasionally (when the disease has been very widely extended) into the pleura, and thus discharges its contents. In this way abscesses become converted into open caverns. The tuberculous cavity varies in appearance, according as this process has been gradual or rapid. In the former case, the tubercular matter at the spot is usually completely fused, and the cavity, which can then thoroughly empty itself, has smooth and tolerably regular walls. The surrounding pulmonary tissue presents a bluish tint, is lax or callous (callöse), exhibits no cellular structure, and is strewed over with isolated tubercles. If, on the other hand, the progress of the abscess has been rapid, and it has opened very rapidly, we seldom find that the whole of the tubercular mass is dissolved. The cavity is then generally irregular, opening in various directions, and containing undissolved, jagged masses of tubercle. The adjacent tissue is in a state of infiltration. Many of these cavities sometimes communicate, and we may find a whole lung perforated with them. It is seldom that a large tuberculous cavity has a simple form; even when its walls are smooth, it is usually sinuous and presents compartments, partly because it is made up of the union of several cavities, and partly because some portions of tissue not thoroughly destroyed, run across or project into the cavity in the form of rafters, ledges, cords, and bridges. These are generally composed of an obliterated bloodvessel and some atrophied and compressed lung-substance, which in this state have resisted further destruction. It is rarely that any blood passes through such a vessel; and it is only when the disintegrating process has gone on very rapidly, that the walls of open vessels are destroyed, and that blood can in this manner escape. The contents of an abscess, previously to its opening, are usually yellow, greasy, and sometimes a little reddish. After an opening has ensued, but before the cavern has perfectly emptied itself, the contents sometimes present an external similarity to pus, or they may be more diluted and mixed with flocculi, and be either of a dirty red, a brown, a gray, or even a blackish tinge. In a cavern of this kind, we not unfrequently find free and detached, but not perfectly destroyed, portions of lung. When the contents are completely discharged, the walls of the cavern remain the seat of an ichoropurulent secretion, and are usually invested with a greasy pseudo-membrane; these walls may become the seat of gangrene.

"If an abscess breaks into the pleural cavity, its fluid contents are effused into the pleural sac, unless impeded by the presence of strong adhesions; and as there is usually a communication between the cavern and a bronchial tube, air enters with each inspiratory movement into the pleural sac. Hence there is developed a severe inflammation of the pleura with a plastic, purulent, and often ichorous exudation; and we have bulging of the side of the chest, and compression of the lung, in consequence of the entrance of the air. Perforation of the pleura is, however, comparatively rare, because the firmest adhesions usually occur at the spot where the tuberculous deposits make their way to the pleura.

"2. *Atrophy* may occur among the miliary granulations, converting them into

hard, very small, bluish gray, or black nodules, which are incapable of any further development, and which finally gradually disappear. Whether the larger tubercular deposits can undergo a similar metamorphosis is problematical.

"Various pathological peculiarities may be referred to the resorption and obsolescence of tubercle. Boudet ('Rech. sur la guér. nat. et spont. de la phthisie pulm., 1843,' p. 11) regards the change which tubercles undergo into an oval or elliptic form, as the commencement of resorption. Fournet ('Rech. clin.' II. t. 929, &c.) explains the transformation of the lungs at their apices into a black and often shrivelled mass, which is sometimes hard, and intersected by cellular and fibrous cords, and is covered by thickened and wrinkled pleura—a change of no rare occurrence, as any one with much experience in *post-mortem* examinations can testify—as representing the remains or cicatrices of old tubercles which have been resorbed in their crude state. In examining the bodies of persons who have died from other diseases, (once in a case of cancer of the stomach, and on another occasion in an old drunkard, who died from pulmonary infarctus,) I have sometimes found distinct chalky concretions in spots which have undergone this change, which seem to me to strengthen the view that these puckerings and shrivellings *may* depend on old resorbed tubercles; but whether in other cases they may not originate in a different manner, as, for instance, from obliteration of the terminal portions of the bronchial tubes, from shrivelled pneumonic infiltration, or even from insidious atelectasis, is a point which I cannot decide.

"3. *Cretification* is the ordinary mode in which crude tubercle and tuberculous infiltration are rendered innocuous to the organism. Cretification occurs in much the same manner as the atheromatous and chalky degeneration of plastic exudations. The conditions necessary for cretification are, doubtless, poverty of the blood, deficiency of the vital powers, and little motion of the surrounding parts; and it appears to begin to occur when softening commences. When the tubercle has become half-softened, the process stops, and there is a deposition of a preponderating quantity of salts (some of which are soluble, as the phosphate, hydrochlorate, and sulphate of soda, while others are insoluble, as the phosphate and carbonate of lime) and an abundance of crystals of cholesterin. The mass gradually dries, and there is left a chalky residue, which at first is triturable and sandy, but finally is studded with sharp spicula of the hardness of bone, and which neither exerts any disturbing influence on the adjacent lung-substance, nor on the organism in general. It is very probable that even this fragment is at length disintegrated and disappears. This fortunate mode of termination is, however, often frustrated by the circumstance, that while some of the tubercular deposits which are favourably situated for this process are healing by cretification, the disease is advancing in other parts of the lungs.

"4. In some very rare cases, the tubercle becomes *encysted* by a tough wall formed by the reaction that is established in the adjacent tissue. The tubercle is either in a state of crudity, or is calcified.

"5. The *cicatrization* of caverns can only occur when their contents are thoroughly discharged. The walls acquire firmness and toughness, and are invested either with a mucus-membrane-like coating, or with a partially ossified callus. One or more bronchial tubes open into the cavity. A cavern of this nature may remain for a long time without undergoing any apparent change, and may during this period secrete pus either scantily or copiously. Or its aperture may close, while the investing membrane gradually approximates to a serous coat, the contents become more aqueous, and the cavern itself becomes converted into a serous cyst. Or finally, the cavern may gradually diminish, till it at length disappears, and there remains in its place nothing but a cartilaginous, fibrous, or cellular cicatrix. All these modes of termination are comparatively rare, and only occur when the tubercles are few and scattered, and when the tuberculous dyscrasia has become extinct." (Wunderlich, vol. iii. pp. 517—519.)

Wunderlich's chapter on the symptoms of pulmonary tuberculosis is very

complete; we can, however, do little more than enumerate the different heads under which he arranges them. He considers

A. *The individual symptoms;* and,
B. *The course of the symptoms in general.*

First among the individual symptoms he places *the local sensations of the patient.* The pains which are so frequently felt under the clavicle on one or both sides, or under the short ribs, and which often extend along the shoulder and the arm, are usually caused by circumscribed pleuritic affections; but in the case of the arm, he conceives that they may sometimes depend on a tuberculous infiltration of the axillary glands. There is nothing in the *signs derived from percussion and auscultation* to detain us. In the remarks on the *signs derived from the cough,* he directs attention to the fact, that cases occasionally occur, even when there is abundant tubercular deposit and great destruction of the lung, in which cough is either altogether absent, or at most is very trifling, during the whole course of the disease. These cases are usually very rapid in their progress, although the absence of cough is sometimes observable in chronic phthisis, at all events for a long portion of its course. In other cases, again, cough is one of the earliest symptoms. Whenever a cough lasts for more than a month without diminishing in severity, and especially when it does not improve under the influence of warm weather, it should always be regarded with suspicion. A spasmodic cough coming on and continuing at a period when hooping cough is not epidemic, should still more excite our suspicion, whether it occur in children or in adults. In his observations on the *expectoration* he remarks, that although there is no constant peculiarity presented by the sputa in phthisis, even the absence of this excretion may in some cases be suspicious if a patient suffers from a very prolonged cough; it is, however, even a more suspicious circumstance if expectoration go on for a considerable time without any alleviation. A very copious expectoration may be dependent on chronic bronchitis, as well as on tuberculosis. Expectoration lasting for a considerable time and brought up with extreme and prolonged difficulty from the extremities of the bronchial tubes, is more indicative of tubercles than any other variety of sputa. According to our author, blood occurs in the sputa in about two-thirds of all the cases of phthisis, but is rarer in childhood and old age than in adult life. When blood is repeatedly seen in the expectoration, and at the same time there is no heart-disease, the case is extremely suspicious.—Passing over the signs derived from the *voice,* from the *conformation of the thorax and the movements of its walls during respiration,* and from the *condition of the brain and spinal cord,* from the *skin,* and from the *muscles,* we pause at the remarks on the signs derived from the *heart and vessels.* The pulse is usually found to be accelerated to ninety or more in the minute, even when the patient is in a state of repose, before there is any indication of hectic fever or of general disturbance of the health; its frequency is also liable to great variations from slight exciting causes. In advanced phthisis it ranges from 110 to 130, and towards the close of the disease it amounts to 160 or even more in the minute. Moreover, in phthisical patients we not unfrequently observe a tendency to powerful contractions of the heart and to true palpitation,

sometimes occurring when the patient is quiet, but at all events readily excited by any corporeal or psychical disturbance. Phthisis has thus been often mistaken for chronic disease of the heart.—There is nothing to call for special remark in reference to the signs deduced from the *intestinal canal*, from *peritonitis*, from the *urinary secretion*, or from the *function of the generative organs.*

We now proceed to the consideration of the symptoms in general. Tuberculosis of the lungs is a disease that varies singularly in its duration; "while it may prove fatal in a few days or weeks, it may, on the other hand, be a chronic disease extending over many years." Wunderlich describes three forms: (1.) *Acute tuberculosis;* (2.) *Acute phthisis* (*florid or galloping consumption*); and (3.) *Chronic phthisis.*

Acute tuberculosis is a disease to which English physicians have paid very little attention; instead of resembling pulmonary phthisis, in the ordinary acceptation of the phrase, it is rather an acute febrile disorder which often very closely resembles typhus. It generally occurs in young and robust persons, and usually without any apparent sufficient cause; or it may develop itself during the course of, or immediately after, a typhous fever or other acute disease, or even during the confinement following surgical accidents. It constitutes, according to our author, about one in every thirty cases of pulmonary consumption.

When the disease occurs in the primary form, it commences with shivering, or with sudden and intense dyspnœa, or with lacerating pains in the chest, or with general *malaise* and debility. When, however, it presents itself at the close of some other affection, it generally begins in an imperceptible manner, and it is only by the recurrence of the heats and chills, and by the non-improvement of the patient, that we are led to suspect the nature of the case. As the disease progresses, it frequently presents no special local symptoms, and merges into a kind of remittent or continued fever, with a rapid and variable pulse, great lassitude and depression, dull and obstinate headache, a sleepy and heavy expression of countenance, and slight wandering of the mind; the skin is at the same time either dry or perspiring very copiously, and often presents sudamina; the tongue is covered with a white, brown, or even blackish dry fur; the teeth and lips are coated with sordes, and there is either constipation or slight diarrhœa. Associated with these signs are thoracic symptoms of more or less intensity, as dyspnœa, frequency of the respiration, cough, which may be either dry and slight, or spasmodic, and accompanied with a little thin expectoration, occasionally containing blood, and pains in the chest. The stethoscopic signs are often unnoticed, from the circumstance that tuberculosis is unsuspected; if, however, the chest be examined, we may detect small bronchial respiration, and occasionally mucous rattles; and towards the close of the affection, the existence of one or more caverns may sometimes be established. As the disease approaches its fatal termination, the patient's faculties become more and more blunted, and at length he sinks into a comatose state. The disease may prove fatal as early as the twelfth day, or as late as the sixth week. We have, as yet, no evidence that it is curable, and indeed the diagnosis is seldom certain until it has been decided by a *post-mortem* examination. Waller, who has published an excellent memoir on this disease, in one of the early volumes of the 'Prager Vierteljahrschrift,' asserts that it may take the

form of typhus, of acute pulmonary catarrh, of gastric fever, of intermittent fever, or of delirium tremens.

Acute phthisis, according to Wunderlich, presents itself in nine out of every thirty cases of pulmonary consumption; while the remaining twenty cases are made up of chronic phthisis. In the former, death usually occurs between the second and twelfth month; and in the latter, the patient may live from one to twenty years, or even more, after the first appearance of the symptoms.

The terminations of phthisis next claim our attention. The disease may terminate in *recovery*, and often in so perfect a restoration to health, that the patient attains an advanced age. In the writings of Rogée, Hughes Bennett, Beau, Cruveilhier, and Prus, we find abundant evidence of the frequent occurrence of obsolete and cicatrized tubercles in the lungs of aged persons, who have died from causes in no way connected with pulmonary consumption.

The further progress of tubercle sometimes appears to be checked by *the formation of some other dyscrasia;* cancer and scurvy appear to possess this power. In the great majority of cases, however, the termination is *death*, which may result in any of the following ways:—

By gradual emaciation and debility, the vital powers being gently and slowly extinguished.
By rapid emaciation and debility in acute cases.
By the retention of the bronchial secretion, causing a stoppage of the tubes, great dyspnœa, and dreadful suffering.
By hepatization and acute infiltration of a greater portion of the lungs.
By œdema of the lungs, usually with considerable suffering.
By abundant hæmorrhage, causing fatal anæmia.
By œdema of the glottis, causing extreme dyspnœa.
By perforation of the pleura, and the entrance of air into the pleural sac.
By inflammation of the meninges, and effusion of serum into the ventricles, giving rise to coma.
By softening and attenuation of the walls of the stomach.
By perforation of the intestine and acute peritonitis.
By chronic peritonitis.
By gangrene of the lungs, and gangrenous bed-sores, with the symptoms of adynamic fever.
Finally, death may occur suddenly or unexpectedly, without any apparent cause being revealed by a *post-mortem* examination.

We extract from Valleix' 'Guide du Médecin Praticien,' the following diagnostic tables. He adopts the arrangement of Laennec and Louis, and divides phthisis into two periods or stages:

"I. *Positive signs of pulmonary phthisis at an early stage.*

A dry cough which has existed for some time; sometimes accompanied by mucous expectoration.
More or less discomfort in the respiration.
Pains in the chest.
Nocturnal sweats.
Debility and emaciation.
Dulness of sound on percussion under one or both clavicles.
Weakness or some alteration of the respiratory murmur.
Hæmoptysis in a considerable number of cases.

II. *Positive signs of pulmonary phthisis at the end of the first stage.*

The cough is more frequent.
There is expectoration of opaque, gray, or greenish sputa.
Hæmoptysis.
Night sweats.
Very frequently more or less diarrhœa.
More marked emaciation.
More extensive and complete dulness.
Blowing respiration, sub-crepitant *râles*, increased vocal resonance.

III. *Positive signs of pulmonary phthisis during the second period.*

Obstinate cough.
Sputa grayish and dirty-looking, opaque, irregular in form, and tinted or streaked with blood.
Increased emaciation and marasmus.
Extended dulness.
Tracheal and cavernous respiration.
Mucous *râles*, gurgling, and pectoriloquy.

The signs of very large caverns are, the cracked-metal sound (*bruit de pot fêlé* of Laennec), amphoric respiration, and metallic tinkling.

IV. *Distinctive signs between pulmonary phthisis in its early stage, and pulmonary catarrh.*

PHTHISIS.	PULMONARY CATARRH.
Cough dry.	Mucous expectoration.
Night sweats.	No night sweats.
Hæmoptysis.	No hæmoptysis.
Emaciation.	No marked emaciation.
Moderate obstruction to the respiration.	Much more obstruction of the respiration.
Dulness of sound under the clavicle.	The sound under the clavicles normal.
The respiratory murmur at those points altered.	The respiratory murmur normal at those parts.
The respiratory murmur normal at the posterior and inferior parts of the chest.	A sub-crepitant râle at the posterior and inferior part of the chest on both sides.

V. *Distinctive signs between pulmonary phthisis at the end of the first stage, and chronic pneumonia.*

PHTHISIS	CHRONIC PNEUMONIA
Is developed spontaneously.	Is the termination of acute pneumonia.
Is seated in the apices of the lungs and chiefly at their anterior part.	Is frequently seated in the inferior part of the lungs and almost always posteriorly.

VI. *Distinctive signs between pulmonary phthisis and dilatation of the bronchi.*

PHTHISIS.	DILATATION OF THE BRONCHI.
Emaciation commences from the beginning.	Very little emaciation notwithstanding the length of the disease.
Night sweats.	No night sweats.
Hæmoptysis a frequent symptom.	No hæmoptysis.
Frequent diarrhœa.	No habitual diarrhœa.
The stethoscopic signs in the upper lobes.	The physical signs may be *anywhere.*
Dulness or abnormal sound corresponding with the position of caverns.	Sometimes no dulness."

We now proceed to the important subject of the treatment of pulmonary tuberculosis, and we regret to state that none of our authors throw any

new light upon the question. Wunderlich truly remarks that no means are known by which we can with any certainty overcome the tuberculous dyscrasia, remove tuberculous deposits, or effect the diminution or cicatrization of caverns. He gives a list of substances which have been regarded as more or less certain specifics against tubercles, or the general dyscrasia which occasions them: in this list are comprised—Lichen islandicus, galeopsis grandiflora, the seeds of phellandrium aquaticum, polygala, cinchona bark, digitalis, lactucarium, hydrocyanic acid, dulcamara, hydrochlorate of ammonia, chloride of sodium, chlorine, iodine, iodide of iron, cod-liver oil, Dippel's oil, naphtha, phosphorus, mercury, antimony, arsenic, pitch, creosote, myrrh, and camphor. His observations on the general treatment to be pursued in the tubercular diathesis, and on the treatment of the different varieties of the disease, are thoroughly sound and practical, but contain nothing to detain us. It is, however, worthy of remark, that in his section on the treatment of phthisis, he makes no allusion to the use of cod-liver oil further than by simply naming it in the above list. Canstatt has the following paragraph bearing on this subject:

"Cod-liver oil and inunctions with fats and oils have been already mentioned under the head of *anti-phthisica*. I am enabled to speak favourably of both these modes of treatment from my own experience, and in several cases the fatal catastrophe of phthisis has apparently been averted by their use. At the present time there is a man alive with undoubted caverns in his lungs, who, four years ago, was suffering from colliquative diarrhœa, and whose life has unquestionably been prolonged by the use of the oil, which he has taken in enormous quantities. When the oil becomes very loathsome to my patients, I am in the habit of giving th m for a time poppy or almond oil." (Canstatt, vol. iii. p. 120.)

The value of oily and fatty inunctions is attested by several German physicians (Wunderlich among the number), as a means of checking the colliquative sweats, supporting the nutrition, and of diminishing the hectic fever.

Valleix and the authors of the Compendium speak in very doubtful terms of cod-liver oil. It seems to have had a fairer and more successful trial in this country than elsewhere. Ergot of rye is mentioned by Wunderlich as the most efficient remedy for hæmoptysis; and the extract of nux vomica, and the injection of starch, and nitrate of silver, are among the means recommended in the treatment of the diarrhœa which so often accompanies this disease.

Cancer of the lungs was so fully discussed a few years ago in the pages of the British and Foreign Medical Review (vol. xx. p. 430), that it is quite unnecessary to advert to it in this general sketch of the diseases of the respiratory organs: and, indeed, we may pass over the other diseases of the lungs without further comment.

We now arrive at *the Anatomical Disturbances affecting the arteries, veins, and lymphatic system of the air-passages*, which include contraction, occlusion, and dilatation of the pulmonary arteries and pulmonary veins, inflammation and rupture of these vessels, tuberculosis of the bronchial glands, &c. But as most of these affections are beyond the reach of art, we shall proceed to a more tractable class of disorders, the *Anatomical Disturbances affecting the Pleura.*

Pleurisy is very fully discussed by Wunderlich. It is one of the most

common forms of disease, for we open few bodies of adults, and scarcely any of aged persons, in whom there are not pleuritic adhesions. It may occur at any age, even during fœtal life, but severe cases seldom occur before the second year. It is most common in youth and middle life, and more frequent in the male than the female sex. It may be either a primary or a secondary disease. When it occurs as a primary affection, which is comparatively rare, it may often be traced to a blow, a penetrating chest-wound, or a fractured rib. Wunderlich considers it very doubtful whether other alleged causes—as a chill for instance—can induce the disease; for granting that pleurisy is most common during the colder part of the year, it is very possible that other disturbances may be established in the respiratory organs by the coldness of the atmosphere, which may be the precursors of inflammation of the pleura. Secondary pleurisy may most commonly be traced to pre-existing disease of the lungs: indeed, any pulmonary disorder which causes much hypertrophy in the superficial parts of the lung may induce it. Hence it is not only almost invariably associated with the graver pulmonary disorders—as pneumonia, pulmonary apoplexy, &c.—when they affect or extend to the periphery of the lung, but also with slighter and often almost unnoticed affections—as hyperæmia, bronchial catarrh, &c. The mere quiescence of a portion of lung, and the absence of the ordinary movement of the pleural surfaces on one another, is often sufficient to induce them to coalesce, in short, to give rise to inflammation. In pulmonary tuberculosis we almost invariably find numerous pleuritic adhesions; and in cancer, when it is superficial, the same is observed. The most severe and intense cases occur when a cavern opens and pours its purulent, tubercular, or ichorous contents into the pleural sac; generally, however, the hyperæmia which precedes this destructive process induces coalescence of the pleural layers and prevents this catastrophe. Morbid processes in other organs may excite inflammation in adjacent portions of the pleura: enlarged and hyperæmic bronchial glands, aneurisms, dilated veins, pericarditis, cancer in the pericardium, tumours in the mediastinum, and even diseases of the œsophagus, may, according to Wunderlich, exert a prejudicial influence of this nature. Affections of the walls of the thorax and of the diaphragm may also cause pleurisy; indeed, the close sympathy between the pleura and the outer wall of the chest is demonstrated by the utility of blisters &c. over the inflamed portion of pleural membrane. Moreover, the pleura sometimes becomes inflamed when there has been pre-existing, or is simultaneous, disease of other serous membranes; this is often seen in common peritonitis, and still more frequently in the puerperal variety, and in acute articular rheumatism. In pyæmia, and probably in other morbid conditions of the blood, a pleuritic effusion is of common occurrence; and finally, pleurisy often occurs as a sequela of the acute exanthemata, and in advanced stages of Bright's disease.

All parts of the pleura are not equally predisposed to inflammation. The products deposited on the pulmonary pleura are always less abundant than on the diaphragmatic and costal pleura. It is only between the lobes and at the roots of the lungs, that we find copious exudations on the pulmonary pleura. There is more abundant effusion laterally and posteriorly than anteriorly, and inferiorly than superiorly. These relations most probably depend on the difference of vascularity in the sub-serous tissues

at the different parts, and perhaps partly on the varying density of the serous coats.

We extract from Valleix the following diagnostic table:

"I. *Positive signs of acute pleurisy.*

Extended and complete dulness at the base of the chest, and absence of elasticity on percussion.
Respiration at that part weak, absent, or distant.
Thoracic vibration destroyed.
Respiration normal for a limited space between the inner border of the scapula and the vertebral column.
Egophony a little below this spot.
Displacement of these phenomena when the patient changes his position.
Cough not very violent, and either dry or accompanied with mucous expectoration.

II. *Distinctive signs of incipient pleurisy and of pneumonia.*

INCIPIENT PLEURISY.	PNEUMONIA.
On application of the hand the vibrations of the chest are diminished.	On application of the hand the vibrations of the chest are augmented.
Bronchial respiration over a limited space below the lower angle of the scapula.	Bronchial respiration wherever dulness occurs.
A bronchial *souffle* usually distant from the ear, with a ringing, silvery sound.	A bronchial *souffle* usually near the ear.
No *râles*.	Crepitant or sub-crepitant *râles*.
Egophony (a doubtful sign).	Bronchophony (a doubtful sign).
These phenomena sometimes change their place when the patient alters his position.	The seat of the phenomena is fixed.

III. *Distinctive signs of pleurisy with extensive superficial bronchial respiration and of pneumonia.*

PLEURISY.	PNEUMONIA.
The general symptoms are in no degree proportional to the widely-extended local symptoms.	The general symptoms are usually in a direct ratio with the extent of the local symptoms.
The bronchial respiration is usually more distant from the ear, and less strong at the inferior part.	The bronchial respiration usually becomes stronger as we descend.
There is either no expectoration, or it is mucous.	There are the characteristic sputa.
No *râles*.	Crepitant or sub-crepitant *râles*.

IV. *Distinctive signs of pleurisy and of pneumonia when the respiratory sounds are inaudible.*

PLEURISY.	PNEUMONIA.
Chest dilated, intercostal spaces effaced.	Chest not dilated.
General symptoms not proportional to the extent of the local symptoms.	General symptoms usually proportional to the extent of the local symptoms.
There is either no expectoration or it is mucous.	The characteristic sputa.

V. *Distinctive signs of pleurisy without effusion and of pleurodynia.*

DRY PLEURISY.	PLEURODYNIA.
More or less intense febrile disturbance.	Little or no febrile disturbance.
Pleuritic friction-sound.	No sign afforded by percussion or auscultation.

VI. *Distinctive signs of double pleurisy and of hydrothorax.*

DOUBLE PLEURISY.	HYDROTHORAX.
A primary affection.	A secondary affection.
Occupies the two sides of the chest successively.	Occurs in both sides at once.
General symptoms proportional to the extent of the local symptoms.	General symptoms have reference, and are proportional, to the primary affection."

(Valleix, vol. iii. pp. 523, 524.)

The treatment recommended both by the German and French writers is so nearly the same as that usually adopted in this country, that we may pass over this subject without remark. Wunderlich gives an excellent *résumé* of the conditions in which *paracentesis thoracis* or *thoracentesis* is advisable, basing his views on the cases and observations of Schuh, Skoda, Reynaud, Sedillot, Krause, Hughes and Cock, Roe, and Trousseau.

Pneumo-thorax was first described by Itard in 1803, in a thesis in which it is commonly believed he was assisted by Bayle; but until the time of Laennec, little was accurately known regarding the conditions under which air accumulates in the pleura, or the symptoms to which it gives rise; and if in his explanations of the phenomena he is somewhat in error, his description of the disease is at all events nearly perfect. Amongst those who have since devoted their attention to this subject, we may especially mention Piorry, Reynaud (who has collected eighty cases), Andral, Louis, Stokes, Saussier, Schuh, Chomel, Puchelt, Skoda, Castelnau, and Routier.

Our limited space forbids us from following him over the pathological anatomy and symptoms of this affection. Most patients with pneumo-thorax, he observes, die within the first ten days, or at all events in the course of the first month; and it is only seldom, and in very slight cases, that recovery ensues. Saussier has collected the results of 147 cases of pneumo-thorax, of which only sixteen recovered; and in none of these was there tuberculous perforation. Of eighty-four patients with pneumo-thorax from various causes, sixty-one lived less than a month, nineteen from a month to half a year, and four for a longer period.

There is considerable discrepancy of opinion regarding the treatment of this affection, and we must be very much influenced by the consideration of the causes on which the pneumo-thorax depends. In tuberculous pneumo-thorax, paracentesis can at most yield only temporary relief; but this should not be regarded as an objection to its use, if we ascertain from the physical signs that the non-affected lung is being seriously encroached upon. Sedatives and anti-spasmodics, especially opium, should be given very freely. Venesection, or the copious application of leeches, also gives decided relief, and seems to check the violence of the secondary pleurisy.

This section of Wunderlich's 'Handbuch' concludes with a sketch of the affections of the thoracic walls and the diaphragm, and of the thyroid and thymus glands.

Art. VI.

Cases in Midwifery. By the late John Green Crosse, M.D., F.R.S., Arranged (with an Introduction and Remarks) by Edward Copeman, M.D., F.R.C.S., Consulting Accoucheur to the Norwich Lying-in Charity, and Physician to the Norwich Magdalen.—*London.* 8vo. pp. 228.

There is no class of medical literature of more value, than that which contains personal experience. Works on clinical medicine, clinical surgery, and clinical midwifery, convey far more solid instruction to the student, than the most elaborate or classic dissertation on the theory and practice of any of these subjects. A bed-side education is the only certain foundation upon which a sound knowledge of medicine can be built, and the nearer the student can approach to it, even in his hours of studious retirement, the more benefit he will receive. We hail, therefore, with much pleasure, another contribution to obstetric literature of a clinical character, which is valuable, not alone because it is clinical, but also because it is the experience of so distinguished a man as Dr. Crosse, of Norwich. His high reputation extended the limits of his consultation practice considerably, and he was in the habit of reporting for himself such obstetric cases as he was called upon to relieve. The great pressure of an extensive practice did not give him time or opportunity to arrange them for publication. He therefore adopted the next best course, and confided his manuscript to a gentleman who was, as it were, trained up in his own school, and was perfectly conversant with his practice. He writes thus to Dr. E. Copeman, who has undertaken to arrange the collection:

"My dear Sir,—You are well acquainted with my habit of noting, during many years of an active professional life, all cases connected with the subject of Midwifery. With increasing and diversified occupations pressing upon me, there is no prospect of my turning these accumulated documents to any use; I am unable to determine whether they can be turned to any useful account by other hands; but confiding in your judgment, industry, and zeal, I commit these documents to you, to be dealt with in whatever manner you may consider best; remaining always,

"My dear Sir, yours, most sincerely,

J. G. Crosse.

"To Edward Copeman, Esq., Surgeon, Coltishall, Norfolk."

Dr. Copeman therefore undertook the task, and published the work before us. It is divided into two parts: one on Abortion, containing a brief account of 100 cases; a second on Parturition, which comprises a number of cases attended by Dr. Crosse in consultation; they amount to 161. Independently of these consultation-cases, Dr. Copeman has arranged in an introduction (to which we shall have presently to refer) 1377 labours occurring in Mr. Crosse's private practice, in which is shown:

1. The average age at which labour has occurred—namely, 30 years.
2. The number of children, male and female; males 737; females 657; total 1394.
3. The number of twin-cases, 17.
4. The average duration of labour, 8 hours.
5. The number of cases in each pregnancy—namely, 1st, 356; 2nd, 249; 3rd, 189; 4th, 155; 5th, 122; 6th, 83; 7th, 68; 8th, 48; 9th, 35; 10th, 27; 11th, 19; 12th, 13; 13th, 6; 14th, 2; 15th, 2; 16th, 1; 17th, 2.

6. Number of children, male and female, stillborn—namely, males 42; females 29; total 71.

7. The number of cases in which instruments were used, specifying how many times they were used in first, second, third, fourth, &c. labours. These were 84 cases; the forceps being used 25 times, the vectis 59. It is observed in a note, that "in five of the forceps-cases, the vectis had been previously tried without effect." These instruments were used in first labours 42 times, in second labours 11 times, and then in diminished frequency, being in sixth labours only twice, in tenth labours three times.

8. The presentations; of which 1320 were natural, 74 irregular, being 26 face to pubes, 5 face, 25 breech; the remaining 18 being, 1 breech and arm, 1 hand and arm, 1 knee, 1 hand, 6 feet, 2 feet in twins, 3 breech in twins, 1 hand and arm in twins, and two placenta-previa cases.

9. The number of deaths, and the names of the diseases which occasion them. There were 14 deaths, 7 of which arose from puerperal fever, 1 from hæmorrhage, 1 diffuse inflammation of cellular tissue of pelvis, 1 uterine disease, 1 hemiplegia, 1 influenza, 2 consumption.

Such is a general outline of the plan of the work. We shall now proceed to consider it a little more in detail. The first part contains several interesting cases of abortion, which convey a great deal of instruction, in respect both to the management and mismanagement of these cases. For instance, case 85 illustrates the injurious effect of extreme refrigeration, which, as the practice is so common in hæmorrhage, we shall relate in Dr. Crosse's words.

"Bailey, æt. thirty-five years. Several children. Delicate, nervous, emaciated, pallid. Miscarried at two months, but her surgeon was not convinced whether it were a miscarriage or purely menorrhagia; the loss was great, reducing her feeble system excessively. Mr. W. immediately adopted the refrigerating system, cloths dipped in cold lotion to the region of the uterus and labia; open windows and curtains; cold milk drinks only; no fire; besides which, aperients were given; lowish diet: and inf. ros. with acid. sulph. dil. After this system had been continued above a week, the menorrhagia still going on in a degree, I was summoned, owing to the distressing and threatening symptoms; hysteria, with ball in the throat; excessive difficulty of breathing; convulsive tremor of limbs; stammering, so that she could scarcely articulate; no locked jaw however; vomiting; diarrhœa; the state was really threatening; and the view I took was, that *the excessive refrigerating treatment was injurious and had caused the most distressing symptoms;* the windows were still open; only cold drinks had been offered; no fire allowed, though in October, and the nights frosty; I changed the measures, but gradually, with a view to restore warmth and power of system; closed windows; had a small fire in the room at night, allowed a covering of blanket; a starch opiate injection to quiet the bowels; a little warm tea, broth, jelly, ext. pap. alb. at night; an effervescing dose and a mild bitter as tonic; clean dry napkins. In eighteen hours there was scarcely a stain on the napkins; warmth of system had returned, and before it the hysterical symptoms had in a great degree vanished. The hysterical attacks returned a few times; and there was occasionally severe convulsive tremor of the limbs; but these ceased, correct speech was regained, and indeed recovery speedily established under a select nutritious diet and liq. cinchonæ." (p. 41.)

This case is very interesting, as showing the exhausting effects of a protracted though slight hæmorrhage, when following one of a more severe character. It illustrates the gradual loss of nervous power and consequent atony of the uterus, but is chiefly valuable in showing by contrast the

effects of the refrigerant and the stimulant treatment. We believe that there is nothing more misunderstood in the practice of midwifery, than the use of cold in arresting uterine hæmorrhage. Flooding suggests to us vinegar cloths to the hips and vulva, ice in the vagina, cold water dashed on the abdomen, cold drinks, cold air, and every mode of refrigeration we can think of; but it does not always occur to us that this may be carried too far, that the action of the heart may be reduced to a minimum, and the patient sink rather from extreme refrigeration than from actual loss of blood. The case just quoted is an instructive example of one error in practice. Case 90 is an excellent illustration of another.

"S——, æt. thirty-three. Seven living children. Eleventh pregnancy; between three and four months. Began to flow on a Sunday, continued to lose daily, and on following Friday the fœtus passed, but no membranes. The loss still went on, and on Sunday following a surgeon was sent for, who gave some general directions, but did not examine per vaginam. Daily loss continued until the patient was brought to the following state, fifteen days from the commencement of the loss, and ten days after the birth of the fœtus—viz., pulse scarcely to be felt; constant call for cold water to sip; emaciated body; the greatest prostration and almost syncope, clearly from continued loss of blood, till the system was nearly bloodless; putrid coagula lodged in the vagina, and as they escaped gave a most offensive stench in the room. Mr. —— had been there before me, but never examined per vaginam since first called, believing the old woman's account that the after-birth had passed away early this morning. The window was open; patient covered only with a sheet, *drenched in cold vinegar rags;* bed already spoiled from the moisture and hæmorrhage. On examining, I found the placenta projecting at os uteri—the external parts were enough relaxed, and I found it easy to introduce a finger into the os uteri and remove the placenta, which seemed to me to be still adherent in one part to the uterus; and indeed there is no other reason to think the placenta had continued in part adherent, as it was not putrid. I removed it with finger and thumb; *from this time the hæmorrhage ceased.* Brandy had been already ordered. I took care to place dry linen under her; to cover her lightly but sufficiently; got the stomach to bear a little milk-and-water, and a morsel of bread-and-butter; and by degrees, in the course of the next thirty-six hours, she rallied; the stomach was able to bear food better and better, and no doubt was felt as to her recovering from the alarming state to which she had been brought purely by loss of blood." (p. 43.)

Dr. Crosse justly remarks, "It is surprising that in this case the surgeon, a well-informed man, and experienced, did not examine per vaginam during the several visits he had made." This omission, on the part of the gentleman referred to by Dr. Crosse, seems to have been accounted for,—

"1st, from his never having, as he states, known a case of miscarriage fatal from hæmorrhage. 2nd, his impression that no manual assistance can be given at so early a period of utero-gestation, when abortion is impending, and attended by hæmorrhage." (p. 44.)

We need hardly say that reasons like these are too puerile to excuse such negligence, and we therefore quote the case as being equally instructive with the preceding.

Mr. Crosse also records in his collection some cases of abortion of a very obscure character, which have been met with occasionally, and always cause no little embarrassment to the practitioner to explain them. We shall quote three of them:

"Funnel, æt. thirty. Third pregnancy; two months. Loss for two or three days, then a mass passed having the form of the uterus, with a cavity enclosed by membranes, but *no fœtus*." (p. 32.)

"Wilde. Seven children. Flooding at three months. *No fœtus* to be found in the membranes, though these were entire and the liq. amnii clear. There were two or three small white bodies loose and floating in the liquor, which might be the detritus of the fœtus; for I can only explain this state by supposing that the fœtus dies and dissolves in the liquor; or I rather suppose this than that the fœtus should originally be wanting." (p. 34.)

"Blazeby. Four or five living children. Six or eight weeks gone. Slight loss recurring several times at intervals of several days each, then the loss continued and considerable, and when I was called it had been profuse, soaking the bed as well as bed-clothes. I found the ovum at os uteri, and was able with my finger and thumb (hand being in the vagina) to remove the ovum entire. . . . . The ovum showed signs of not being fresh, and on opening the membranes *I found no trace of fœtus or funis;* the contained liquor was turbid rather, and my explanation is, that from some cause the placenta became detached in part or principally from the uterus when the loss first appeared—that the fœtus died in consequence and putrefied away in the liquor, thus accounting for no trace of fœtus on examination." (p. 44.)

In these three cases no fœtus or trace of a fœtus was to be found within the membranes containing a liquor amnii. The question at once suggests itself, Were these true or false conceptions? and if true, What became of the fœtus? We know that in certain states of deranged menstruation a coagulum will form within the cavity of the uterus, and be retained so long as to have its characters quite altered; the fibrin, assuming the shape of the uterus, forms an outer layer or dense cyst which contains a clear fluid, the serum. The cyst has the rough exterior of the deciduous membrane, and is also internally lined by a thin false membrane very much like the amnion. In these cases the menses are generally suspended for one or two periods, and the mass is then thrown off just like an abortion. Such might be the explanation of these cases, but it is not probable that a man of Mr. Crosse's experience would be thus deceived; we can only conclude, therefore, that they were true conceptions, and if so, the absence of the fœtus might be more apparent than real. We are more disposed to think that the fœtus had never grown, than that, having grown, it was subsequently dissolved. We rather look upon these cases as instances of a total arrest of development in the fœtus, than agree in Mr. Crosse's explanation "that the fœtus died and putrefied away in the liquor." The change that we call putrefaction in the fœtus, is generally *pari passu* with a similar change in the membranes, of which we have no evidence here; and besides, in the two first cases the liquor amnii was quite clear. If we assume that the development of the embryo was arrested, that as a germ it retained its primitive form and size, while the membranes continued to increase with the uterus, we can readily conceive that the little gelatinous trace of fœtal existence might escape Dr. Crosse's notice. However this may be, we present these cases to our readers' notice as enigmas which we trust an improved knowledge of intra-uterine pathology will hereafter explain.

The advance that has been made in the pathology of the uterus reveals to us many causes of abortion only lately understood. Among them none stands more prominently forward than inflammation and ulceration of the neck of the womb. The following case proves that at a time when nothing

whatever was known of such causes, the acute mind of Dr. Crosse foresaw the probability of their existence.

"Page, æt. thirty-eight. Several children. Between two and three months. After some fatigue, profuse loss; and to know if the placenta were away, I examined. Did not feel it, but felt one lip of the os uteri very large, thick, prominent, rough on the surface, impressing me with the belief that an ulcerated tumour was situated at the os uteri; but no preceding pain, nor loss till within a few days, had occurred; general health quite good; all contradicting any preceding disease. But it suggested to me, that *disease of os uteri may be one cause of abortion.*" (p. 38.)

Had that much-abused instrument the speculum—abused equally by the hands of unscrupulous practitioners, as by the pen of malicious writers—been known to Dr. Crosse, he would have used it here, and what were only suspicions might have proved to be a certainty.

Part II. of Dr. Copeman's collection comprises a number of cases (chiefly labours) attended by Dr. Crosse in consultation. They are arranged under twelve heads, but may be included in four divisions —namely, 1. Diseases and displacements of soft parts complicating labour; 2. Difficulties and complications during labour; 3. Diseases occurring in consequence of labour; and 4. Diseases of infants. The first case of disease of the soft parts is described as "ulcer on the os uteri," . . . . "indicated by pain, and a foul, dark, stinking discharge." . . . "Her health was in a few weeks reduced by this painful disease, and she took to her bed. In August, the abdomen was found increased in size; she was emaciated and haggard from suffering and discharge." She miscarried at the end of August, and the night after this unexpected change "she was hurried, had a rigor, pulse 160, and died on the 6th of December." (p. 49.) No examination was allowed; had it been permitted we are sure Dr. Crosse would have recognised the malignant character of the ulcer, such, perhaps, as Sir Charles Clarke has described as the corroding ulcer of the uterus. We quote the case as a good example of the sense in which the term "ulcer on the os uteri" was generally used at a time when our knowledge of these morbid changes was extremely limited. The next case is so remarkable, that we cannot forbear quoting it at length:

"*Malignant Tumour of the Uterus.*—The most extraordinary and anomalous case which I have hitherto met with in the whole course of my practice is the following:—Some of the circumstances appear so monstrous, that credit would not be given to the narrator, except by those to whom he was known by the station he held, or the correct statements he had offered to the public on other subjects. Yet were I to publish this case, I could scarcely refer by name to other parties—fellow-witnesses of the dissection—for delicate reasons that must occur to every one. I was called up at two o'clock this morning by Mr. ——, to go to H——, a distance of seven miles, to a case of midwifery, attended with some unaccountable circumstances; and though the woman was dead, *the three surgeons* who had been with her wished me to go for their satisfaction, as they did not like to quit the house, leaving the woman undelivered, unless they could explain what had happened; and they moreover wished to inspect the body, which they feared would not be allowed if they once quitted the house.

"The history I learned was, that this woman, near forty years of age, had borne six or seven children; the last labour happened near two years ago, which was natural. She thought herself again pregnant and near her full time. A month ago or more, she had been blooded on account of pain on the right side of the body. She was a feeble woman, and these symptoms were then attributed by

Mr. H—— to the ordinary circumstances of pregnancy. She also became somewhat jaundiced, and continued so. She complained of labour-pain, as it was supposed, at noon yesterday (Saturday). Mr. H——, who had been engaged to attend, was sent for in the afternoon—he was from home, but arrived at five o'clock; and on examining, though there was very slight pain, he found *a soft tumour in the vagina, reaching nearly to the external labia, which he supposed was the liq. amnii distending the membranes.* Soon after, Mr. —— (who had been sent for on its being found that Mr. H—— was from home) arrived, and on examining, *thought the placenta was at the os uteri.* At seven o'clock, they left the patient for two hours, during which time she slept a little, and had not much pain. On returning and again examining, they believed *the placenta was presenting, and that it was a case for turning.* Mr. H—— ruptured the presenting part, or the part low in the vagina, expecting the waters to escape, *but only a little blood flowed:* he brought away several portions of solid substance, not unlike the placenta after being well washed in water. Some of these portions were bigger than a hen's egg, and were shown to me on my arrival. Having acted thus far under the impression that the case was a placental presentation, the same belief was entertained by both gentlemen present; and whatever was the position of the child (at one time the breech, at another the occiput, was thought to be felt), they believed turning would be the proceeding; so Mr. H—— having removed his coat and bared his arm, introduced it through the mass in the vagina; and believing he must reach the uterus, could not explain why he did not distinctly feel the child, but thought at one time he got the foot of the child; he, however, could not retain it; he got hold of something else, and no doubt used some force with the hand; but throughout the whole of this history I am unable to give a statement of what force was employed; it must be inferred from the subsequent part of the case. On withdrawing his hand, he brought away a firm whitish substance, which appeared like a bean. Mr. H——s *likened it to a bile-stone*—it was soft enough to be powdered readily. Mr. H——s next introduced his hand, but could not detect a fœtus; he, however, felt the spine—said he could feel a bag of such stones as the one brought away by his more active and more experienced companion; *neither of them, however, suspected that the hand had been introduced anywhere but into the uterus.* Numerous portions of the whitish soft mass, resembling by candle-light well-washed placenta, were brought away. The woman died half an hour after these attempts, though there had not been much loss of blood; a gradual loss, but no rapid flooding." (pp. 49—51.)

Dr. Crosse arrived three hours after the patient had expired, and found

"The corpse was lying on the bed, on her left side, doubled up in the usual position for examining during labour, with her clothes on; and the quantity of coagulated blood on the bed, reaching down to the feet, showed there had been no inconsiderable hæmorrhage. I introduced my hand into the vagina, felt lacerated parts, but could not distinguish fœtus or os uteri. My coat being on, I could not proceed far with the hand, and I did not wish it, preferring to leave the parts for internal inspection just as they were. On withdrawing my hand, portions of a substance, twice as big as a walnut, escaped, of precisely the same appearance and soft consistence as what had been removed during life. Enough of this mass, in detached pieces, was shown me, to fill perhaps a pint mug. Placing the corpse on the back, I made an incision in the course of the linea alba into the abdomen, extending from sternum to pubes; and as soon as the peritoneal cavity was opened, I discovered a quantity of coagulated blood, as much as eight or ten ounces, spread on a thin layer over the intestines. The uterus immediately presented itself—it contained an acephalous fœtus, plump and full grown in its limbs and body, and apparently not less than eight months old. The placenta was cut through in making a longitudinal incision into the uterus, answering to the linea alba, for it (placenta) adhered to the anterior and upper part of the uterus—the membranes were entire—only an ounce or two of liquor amnii in them—the os uteri open to the extent of a shilling, but *it had never been dilated*

nor meddled with in all the examinations which had taken place during the supposed labour; for the uterus lay high above the pubes and forwards, pressing immediately behind the anterior parietes of the abdomen. Pulling aside the uterus, and also the intestines, so as to examine the cavity of the pelvis, I found the whole of this cavity occupied by a morbid mass of tumour, soft and elastic to the touch, altogether in bulk sufficient to fill two quart mugs—some tumours were bigger than the largest orange, others the size of a walnut, all united together with serous or peritoneal membrane. ***Through the most compact of this morbid mass, in the centre of the pelvis, anterior to the rectum, was a lacerated opening leading into the vagina;*** and it was clear the hand of the accoucheur had made its way through this passage into the peritoneal cavity. One inexplicable circumstance during life was now explained—*the gall-bladder was ruptured* at its larger end—several small whitish biliary calculi were amongst the viscera near it—numerous others were in its cavity, one big as a nut, occupying the cystic duct—others varying in size from a pea to a bean; altogether I collected fifty-six of these calculi—*not including two or three removed per vaginam during life.* The liver was of a slate colour by candlelight, and particularly soft in its structure, so as to be easily torn with the fingers—kidneys and bladder healthy, as well as the surfaces of the peritoneum; but upon the spine, behind the mesentery, by the side of the aorta, there were large morbid masses, precisely like the tumour in the pelvis—soft—brainular. I took out the uterus and the morbid parts. On strict examination, and unravelling as it were the latter, I found that the great mass of these tumours had originated in the right ovarium; one single tumour of the same structure occupied the left ovarium. . . . The facts I deem to be made out are, that malignant tumours, forming in the right ovarium chiefly, medullary sarcoma or soft cancer in nature, growing rapidly as they are known to do, ... attained a large growth, filled the pelvis—descended into the vagina, carrying, I presume, the lining of the vagina before them—the tumours thus impacted in the pelvis and vagina would be pressed and fixed by the gravid uterus. Such was their position and such the circumstances when labour was supposed to be indicated by pain and forcing down; though I doubt, and *Mr. H——s expressed to me that he now doubts whether any real labour or contraction of the uterus to accomplish expulsion of a mature fœtus took place.* However, it was supposed to be labour —the mass in the vagina was mistaken for placenta presenting—*the loss of blood was not so great as to make active measures urgent, even under this impression* —but in examining, force enough was used with the finger to break through the membrane of the vagina; the hand entered one of these soft tumours; and, being pushed on in the idea of searching for the fœtus to turn it, readily broke through the peritoneal covering of the tumours, and was free in the cavity of the abdomen. In searching for the limbs of the child (monstrous as the narration must sound in the ears of every practitioner who hears it), the gall-bladder, which contained numerous gall-stones, was grasped and pulled at with sufficient force to rupture its coats—a calculus or two escaped—one was seized by the fingers of the accoucheur, and from the peritoneal cavity brought through the lacerated opening of the tumour and through the vagina so as to be examined externally. All this time the uterus remained untouched—the os uteri was over the pubes—the hand passed just anterior to the rectum and near the prominent lumbar vertebra. The curved posture of the patient, with knees drawn up to the sternum, pressed down the liver and gall-bladder. A second gentleman introduced his hand into the peritoneal cavity, so as to feel the bag of bile-stones and the liver, as he assured me afterwards. No wonder the woman should die shortly after such mischief was effected." (pp. 52—54.)

We have read of an ignorant midwife mistaking an inverted uterus for a second child, and pulling at it with such force, that she tore the womb out of the body.* A drunken surgeon was brought to trial at Derby for

* Medical Gazette, Sept. 9, 1842.

manslaughter, because he did the very same thing, having mistaken the inverted womb for the placenta.* We have been told of two sagacious practitioners who held a consultation in a case of protracted labour, and having decided that a very large tumour obstructed the delivery, agreed that it should be removed. The operation was performed and the tumour removed, which, when carefully examined, turned out to be the scrotum and testicles! We think the case just quoted may well follow these as a kind of climax of ignorance, of inexcusable ignorance, because the warnings that were given were so entirely disregarded. The presenting mass lay almost at the vulva; at first supposed to be the membranes, they were punctured, but no water flowed; then it was called the placenta, and several portions of a solid substance brought away, which they imagined to be fragments of placenta. It is not stated, however, that this tearing up of the placenta gave rise to any increase of hæmorrhage, nor that the os uteri was felt; but having detected some solid substance, they were uncertain whether it was the breech or the occiput of the child. Notwithstanding the absence of all these evidences, and although "there was no flooding urging the immediate proceeding for delivery," the gentlemen agreed to turn the child; the hand and arm were thrust through the morbid mass, behind the gravid uterus into the abdomen, as far as the liver and gall-bladder. The operator was no doubt surprised that he did not distinctly feel the child; he caught hold of the gall-bladder and thought he had the foot, used some force to bring it down, and succeeded in delivering a whitish substance, "very like a bile-stone." Our readers will be equally surprised that when the gentleman got his hand into the abdomen among the intestines, and found that he had extracted something very like a bile-stone, he was not enlightened, and perhaps be still more astonished to read that "his more active and more experienced companion" then introduced his hand just in the same way, said he could feel *a bag of such stones* as the one brought away, and yet neither the one nor the other *ever suspected that the hand had been introduced anywhere but into the uterus!* Such ignorance as this is inconceivable in any man who has obtained a licence to practise—we will not say midwifery—because, unfortunately, there is no such licence required, but simply a licence to practise surgery. Here is the important fact, that should be indelibly impressed on the mind of every one who is anxious for the honour of his profession, that two surgeons, having, we presume, their diploma to practice on her Majesty's subjects, absolutely had their hand and arm in the abdomen of this unfortunate woman, felt her liver and tore her gall-bladder, and yet never imagined that they were anywhere else but in the cavity of the uterus. If they knew nothing whatever of midwifery, if they had never attended a labour in their lives, yet as surgeons they must be supposed to know something of anatomy, something of the sensation communicated to their fingers when touching the liver, the gall-bladder, and the intestines: however, it appears that their knowledge of anatomy did not extend so far, and consequently the woman was sacrificed to their ignorance.

No doubt many of our readers will at once exclaim—such gross mismanagement should have been exposed; it was a case for the inquiry of the coroner, perhaps for a trial for manslaughter. Such a course would no

* Lancet, vol. ii. 1844, p. 383.

doubt have all the justification that these frightful facts could give to it; but we must ask, *cui bono?* If Dr. Crosse had so acted, these unfortunate gentlemen would of course have their reputation ruined, their practice destroyed, their families beggared: in their own neighbourhood their names would hang like skeletons on a gibbet, a terror to all practising surgeons; but would it prevent another ignorant surgeon from committing a similar mistake? We doubt it exceedingly. Had the case been brought before a court of justice, we can imagine the judge, in his charge to the jury, stating that this case, although a melancholy proof of the ignorance of the medical profession, was not manslaughter; there was no proof of any want of attention on the part of the surgeon; he sought the best assistance he could get to relieve his patient; there was no evidence of any other intention than to save her life, and therefore he could not be found guilty of any other charge than gross ignorance. We think Mr. H—— would be acquitted, and the effect would be, as it has happened in other trials, that the law would seem to shelter ignorant practitioners. But had the gentleman been found guilty and punished, we still ask, *cui bono?* The example might frighten, but it would not communicate knowledge to, other ignorant surgeons; they might be, for a time, more cautious, but when the excitement subsided they would return to their former habits, and perhaps commit another awful mistake. We are much more disposed to trace such a want of knowledge to its true source, and to inquire how such persons could have obtained diplomas to practise: how it happened that men of such calibre were given an authority which was practically a deception on the public, and were allowed to call themselves surgeons. We think that if the law were a little altered in this respect, and if, instead of permitting these examples of ignorance to escape either scot free or only with a slight punishment, it looked, beyond the accused, to the college they came from, and placed it at the bar for giving a diploma that was not justified, such a course might perhaps limit the number of ignorant practitioners, and more effectually prevent the recurrence of such abuses. This novel mode of correcting the evil we have described, may perhaps take our readers by surprise; but possibly on reflection they may agree with us, that a heavy fine levied upon those who grant the right to practise to an incompetent person, would have a far more useful effect than the ruin of the hapless being who obtained his diploma and fancied himself a surgeon.

We believe, also, if such were the law, Midwifery would not stand in its present isolated position, neither admitted for examination by the physician nor recognised by the surgeon: being a kind of orphan, or rather foundling, which neither will acknowledge, though begotten of both, and which would perish but that it is sheltered by the compassionate apothecary. If such a trial as we have supposed took place, if Colleges were brought to the bar of public justice, we can imagine the interest that would be excited; we confess that we might ourselves be drawn into court to hear the cross-examination. If the defence of the physicians were, that they only gave authority to practise medicine, we can fancy a queen's counsel demanding proof that the practice of midwifery is not, in many respects, the practice of medicine. Or if the surgeon should say, "they do not qualify midwives," we can picture to ourselves a powerful special pleader levelling with his thunders the assumed distinction between an operation

in surgery and an operation in midwifery. If such took place, we think that the foundling would be received, and a proper provision made for it; but as matters are, we cannot blame Dr. Crosse for keeping this case quietly at rest in his case-book, rather than expose the unfortunate delinquents.

In the chapter on "Displacement of the Soft Parts Complicating Labour," we regret to find another, but not quite so bad an instance of mismanagement. It is a case of "inversion and protrusion of the bladder through an opening into the vagina." Mrs. C——, æt. twenty-nine, had a labour ten years previously, that required instruments to effect delivery. "After this labour, which was protracted several days, *the patient never retained her urine, it always dribbled away.*" Her present labour took place prematurely at the seventh month, and she was delivered in two hours of a dead fœtus:

"The nurse waited an hour and a half, and then sent for a surgeon, on account of the placenta not coming away, and her not feeling it, but something irregular, on examining. Mr. R—— came, *broke the cord*, but did not get the placenta—two other surgeons were called by him, but could not explain the case. At 3 p. m. Mr.—— saw the case, and at 6 p. m. requested me (Dr. C.) to meet him." (p. 62, 63.)

The cause of all this difficulty was a swelling, the size of a very large pear, prolapsing so as to be visible between the labia. The os uteri could be felt behind it.

"The patient's condition was very bad—rapid small pulse—tender abdomen—anxious countenance. There was no reason to suppose ruptured uterus; and the practice seemed clear—to sooth by anodynes, to support the patient by bland food, and to leave the placenta to come away of itself." (p. 63.)

These judicious directions were followed, the placenta was expelled the next morning naturally, but the patient showed signs of peritoneal inflammation, of which she died the day following. A *post-mortem* examination was made, which, like many others, produced some unpleasant disclosures. It was found that—

"The uterus was healthy in size and every other appearance, except its peritoneal covering. Just behind the pubes and anterior to the neck of the uterus, it seemed *the peritoneum had been injured, if not torn through; which happened no doubt within a few hours after the expulsion of the child, from rough usage to ascertain the nature of the presenting tumour, and to remove the retained placenta.*" (p. 63.)

This, then, was the first evidence of mismanagement, of ignorance, and of violence. The peritoneum torn through by the fingers, which intended to enter the cavity of the uterus, but mistaking their way passed up just behind the pubes, and *anterior* to the neck of the uterus. The peritoneum was evidently healthy (and in this respect differs from the preceding case), but such was the force used that it was torn through. The second evidence of mismanagement applies to the woman's first labour:

"The prolapsed tumour was the bladder itself inverted through the large opening into the vagina, formed by sloughing after the first labour—consequently the lining membrane of the bladder was the covering of the tumour presented to our view between the labia." (p. 63.)

A vesico-vaginal fistula, so large as to admit through it an inversion of the bladder!

These examples are valuable, not for the purpose of demonstrating the

ignorance of some surgeons who practise midwifery, but as proofs of the importance of correcting the very imperfect obstetric education which surgeons at present receive. The Royal College of Surgeons in England do not examine in midwifery, and merely require a short summer course of sixty lectures as evidence of obstetric knowledge. The College of Physicians exclude it altogether. The Apothecaries' Company alone have given it their attention. The same laudable zeal for the improvement of medical education that has raised them so far above their original position, has led them to require, in their examinations, a knowledge of midwifery, and to demand, both in respect to lectures and practice, a more extended study of it. By them much has been done, nevertheless much is still wanted; and we trust that the instances we have quoted may have a useful effect, in proving the importance of a more formal obstetric examination than at present exists. We hope they may expose the fallacy of an error, that we fear too generally exists—viz., that parturition being a natural process requires not the aid of an accomplished physician, nor of a skilful surgeon; that its management is the business of midwives, and requires no more knowledge than they possess. These examples give the best reply to such absurd reasoning, and trumpet-tongued proclaim the inexcusable neglect of an adequate obstetric education.

Dr. Crosse's next chapter, on "Difficult Labour requiring Vectis or Forceps," is extremely interesting, as pointing out, not only the use, but the abuse, of these instruments. His observations confirm in a great degree our suspicions, that both are far too frequently and too often employed, not on the principle of necessity, but of convenience. A case of labour that might terminate naturally in twelve hours, may be finished in six by the forceps or vectis; the sufferings of the patient are over, the child perhaps born living, and the practitioner crowned with the applauses of friends, who admire the skilful operator, and imagine, what perhaps they were previously told, that the woman never would have got well without this admired operation. These temptations are great, and only to be corrected by the enlarged experience of such men as Dr. Crosse, who know the opposite side of the picture, and who have too many painful instances of mischief from such meddling brought before them not to know the truth, who are all aware that this seeming triumph has only a short existence, and that although the patient may, at the time of delivery, escape any recognised injury, nevertheless, that her after-life of ill-health too often is dated from her last confinement. We gladly, therefore, quote Dr. Crosse's protest against such practices:

"The use of instruments," he observes, "to hasten labour may be sufficiently discouraged in books, and the cases in which they are strictly required clearly pointed out in lectures; but amongst the circle of practitioners of midwifery, under my own observation, I have the means of knowing that there are many *who indiscriminately employ them against all rule, reason, or propriety.* I refer more particularly to the vectis; with which I am told delivery is easily expedited without any injury to the mother. This may be generally stated with truth by those *who, always armed with this weapon of offence, employ it on every occasion that they approach the bedside of a female, when the child happens not to be born before their arrival;* because a good pelvis, relaxed parts, and an advanced position of the child's head (the state of things in a great majority of patients at the time the accoucheur is sent for), enable him to effect

delivery with a single pain, and so little force as shall leave all parties safe and uninjured. But such, however, are not the circumstances that call for the employment of the vectis; though, I believe, it is better fitted to these cases than to any other, having in many instances of real difficulty seen it attempted to be used, and relinquished for the forceps, *which never disappoint the experienced man.*" (p. 80.)

These sentiments are most valuable, because they bear the stamp of experience, and are free from all spirit of controversy; they are the private opinion of one who had a full opportunity of ascertaining their correctness. With regard to this frequent use of the vectis, Dr. Crosse speaks with just reprobation.

"The act of parturition is a painful, a most unaccountably painful process, and to lessen the suffering by shortening its duration (instruments never lessen it in any other way than by shortening it, as they invariably increase the degree of suffering at the moment) must be the anxious wish of every humane mind. *But let those who are forward to interfere with the regular efforts of nature, be sure that they are not considering their own personal convenience, instead of acting from feelings of humanity, and under the guidance of sound experience and good reasoning.* I can readily believe that if the vectis be used when the pelvis is large, the maternal parts relaxed, and the fœtus properly placed, it may at once complete delivery, which would be delayed many hours from the pains being slight, and occurring at distant intervals; and that mother and child may be uninjured, (except that both must suffer some pain from the means employed,) and even where some difficulty has to be overcome, the experienced in the use of the vectis may succeed without injury to the mother; but the fœtus whose sense of touch is fully developed, though it has no means of expressing pain but by a movement of its limbs, suffers whenever either the vectis or forceps is used, and is brought into the world exhausted, still, or lifeless, requiring great care for its revival. I wish those who are forward to use the vectis, and who argue that it does no injury, inflicts no suffering upon the child, would sleep in an iron night-cap once in a week, and tell us what sort of repose they get. Let the advocates for the use of instruments, vectis or forceps, to expedite labour that might be naturally terminated without them, ask themselves what are the effects of a vectis or end of a spoon used as a scoop to remove fæces from the rectum, and how they would like to have such an instrument employed to expedite the delivery of a costive motion? . . . . This comparison is sufficiently just, and puts in its proper disgusting and inhuman light, the practice of certain accoucheurs of employing instruments upon every occasion where labour is a little delayed, so as to give them the opportunity. Happy for the patient, whom such practitioners are engaged to attend, if a circumstance, that needlessly alarms her and all around, happen—viz., the child being born before the accoucheur arrives." (pp. 81, 82.)

As illustrations of his forcible and just arguments, Mr. Crosse quotes two instances, in one of which the forceps did such injury, that a bowel escaped per vaginam, and on dissection, *post-mortem*, the exact mischief was ascertained by other surgeons, *who hushed up the business.* In another, the forceps were used *after six hours' labour!* "The patient died of flooding after the birth of the child, before another practitioner could be got to the spot." Would that we could enumerate the many instances of such abuses which we doubt not exist, but which for obvious reasons cannot be ascertained, unless indeed the death of the patient is the consequence; and even then, unless it is too obvious, it may be very conveniently "hushed up." When we find men, the most distinguished for practical experience, condemning the too frequent use of these instruments—when we find the recorded results of their practice confirming the justice of their opinions, (we may readily conjecture why they found it necessary,

like Dr. Crosse, thus to protest,) we may imagine the number of "hushed-up" cases that come within the reach of their observation; their language sufficiently expresses the painful feelings which such instances excited, and which account for the energy of their warnings. Dr. Blundell's remark, "a meddlesome midwifery is a bad one," has become a proverb; but unfortunately, like other proverbs, is too little attended to. The motives assigned for meddling are too powerful; the convenience of terminating labour, and the credit of a skilful operation, are temptations that we fear too often seduce the practitioner from an honest and straightforward course; and, as Dr. Crosse observes, lead "certain accoucheurs to employ instruments upon every occasion where labour is a little delayed, so as to give them the opportunity." When we find in the great European Lying-in Hospitals, the forceps used not more than once in 200 cases, we can judge how frequently such assistance is absolutely required, and can estimate the practice of those who employ it in every eighth or tenth labour. In these establishments, the motives to which we have alluded are no inducements to interfere with natural labour, and form, therefore, a better criterion to judge by, than the private practice of individuals. In the great hospital at Vienna, Boër used the forceps only once in 251 births. In the Maternité at Paris, Boivin and La Chapelle, in 42,000 cases, delivered with the forceps only 173 cases, or one in 246. Collins, in the Dublin Lying-in Hospital, employed it once in 574 cases; and Dr. Ramsbotham, in the Maternity Charity of London, the largest district charity in the metropolis, uses the forceps but once in every 535th case.* In this Charity, the practice of which can be best compared with private practice, the mortality was only one in 306 cases, a most favourable return, inasmuch as the Registrar General's Reports give an average of about one in 150, as the proportionate mortality in childbirth in London. The result, therefore, of this extensive range of experience establishes the safety of a practice in which instruments are very seldom used; it proves that *they are not necessary* once in at least two hundred labours; necessity is the only argument that we can admit as a justification for their employment; and therefore we must, with Dr. Crosse, protest against the abuse he so ably points out. Dr. Copeman, in his Introduction, gives a series of tables, in which the results of private practice are shown. They prove a remarkable difference in the frequency with which these instruments are used by different practitioners. Mr. Waddington, of Margate, uses the forceps only once in 449 cases; Mr. Mantell, of Lewes, once in 401 cases; Mr. Earle, of Cromer, once in 135 cases; Dr. Toogood, of Bridgewater, once in 75 cases; and Dr. Copeman has used the vectis once in about nine and three-quarter cases.

We confess, that we were greatly surprised to find that Dr. Copeman interferes with labour so frequently as the above numbers indicate, in this respect departing so completely from the precepts of Dr. Crosse; and we could not explain this constant use of the vectis, until we found that the position in which Dr. Copeman was placed compelled him to employ it much oftener than was necessary.

"The gentleman with whom I (Dr. Copeman) entered into partnership had, during his whole professional life, employed the vectis, and that only; and *I found it absolutely necessary to my success in midwifery to use it also.*" (p. 84.)

* Medical Gazette, Nov. 7, 1851, p. 793.

He further observes,

"I do not desire to advocate the use of instruments when they are not absolutely necessary, being fully aware of the *possibility* of their causing much severe and sometimes irreparable mischief; I can therefore only say in justification, that in some instances, *I have been implored to use the vectis by patients who have before experienced its beneficial effects;* that I have never seen a patient die whom I had subjected to its operation; and that I have never witnessed, in my own practice, the occurrences so frequently attributed to the abuse of instruments—viz., ruptured perineum, inflammation, and sloughing of vagina, vesico-vaginal fistula, or any such prejudicial effect." (p. 17, 18.)

Dr. Copeman, then, having had the misfortune to be allied to a partner who was evidently very unscrupulous in the use of the vectis, was obliged, at the risk of losing his practice, to follow this vicious example; his patients imploring him to deliver them as they had been delivered before. Finding that he could do so with impunity, he was encouraged to proceed; and as he has not yet met with any accident, seems inclined to adopt his predecessor's practice. The passages we have quoted will explain the reason that Dr. Copeman operates in every ninth case. As Dr. Copeman uses the vectis (an instrument which, we agree with Dr. Crosse, is perfectly useless in a really difficult labour), we infer that he has been fortunate in the class of cases that have formed his practice; they offered no real difficulties; but the patients were intolerant of their pains, and were easily and safely delivered by the vectis. We would caution Dr. Copeman against the deception of such success; an accident may happen, and if so, necessity cannot be offered as a plea in justification; he becomes responsible for all the consequences of this imprudent meddling, although they may not manifest themselves for months afterwards.

"Whilst therefore," observes Dr. Copeman, "it is incumbent upon us to qualify ourselves for emergencies, let us at the same time never cease to remember that our best judgment and kindest feelings should be exerted to the utmost to discover in *how many* cases instruments may be avoided, and *how few* there are in which they are imperatively demanded." (p. 85, 86.)

With such a disposition for inquiry, we have no doubt that when the second edition of this little work arrives, we shall find a very different result from Dr. Copeman's more enlarged experience; we think for nine we shall read ninety, as the proportionate number, and perhaps an acknowledgment of the errors into which a temporary success in his early career had betrayed him.

Dr. Crosse performed embryotomy eight times,—in four of these operations the death of the child had been previously ascertained, so as to remove the great objection to this operation,—in two, the head was above the brim of the pelvis, in consequence of deficient space in the conjugate measurement,—one was a face-presentation, and one was thus delivered because the forceps failed; but, observes Mr. Crosse,

"I had to regret not having used Naegle's long forceps, a little curved, as I think I might have succeeded, and given a chance of the child being born living." (p. 98.)

We fear that not a few of these operations are performed, because of the ill success of the attempt to deliver with a badly-contrived forceps. Every practitioner in midwifery selects his favourite forceps, which is to

suit every variety of case where such an operation is required. If, on the contrary, he would not confine himself in this way; if he would seek rather to adapt his forceps to the case, than his case to the instrument that he happens to have at hand, his success would be greater, his failures not so frequent, and perhaps the injury done to the soft parts by its application would not be so common an occurrence.

Dr. Crosse relates one case of spontaneous evolution, very imperfectly however:

"The patient in her third pregnancy, groaned a month prematurely of a dead child, the arm presenting. Mr. H— was with her eight hours, unable to turn or to bring the *child forth arm first.* At length, declining to examine for a short time, he found the feet had come down, and the child was soon born, spontaneous evolution having taken place." (p. 105.)

If this be the same Mr. H— whom we have before introduced to our readers as having delivered his patient of something "very like a bile-stone," it will account for the notion that he had of "bringing the child forth arm first." The skill with which the former was accomplished justifies the belief, that he would not have failed to drag away the child even in this unusual manner. Our readers, however, will agree with us, that the woman had a most fortunate escape by the provident interposition of Nature. Dr. Copeman quotes a case that had occurred in his own practice:

"The os uteri was fully dilated; and the child was now discovered to be lying across the pelvis, *with the back presenting;* but I could feel neither shoulders nor hips with the finger, so as to be able to ascertain in which direction the head was situated. Neither could I feel spinous processes or ribs, but determined the nature of the presentation more from the breadth and flatness of the presenting part; and a mark on the skin made with my finger-nail, indicated after birth the correctness of the diagnosis. As the contractions of the uterus were but slight, and occurring at long intervals, I took the opportunity of sending for my partner, that he might witness the case, and be at hand to assist if required; meanwhile the patient took some nourishment, and was encouraged to submit with fortitude to the proposed operation of turning. During this time she sat up in the bed, and said it was the easiest position. In less than two hours, Mr. Evans arrived; the patient was then properly placed in bed, and I prepared to turn without further delay; but to my surprise I found the pelvis filled; there had been several good pains just before, *which had forced the back of the neck and shoulders downwards at the right side of the pelvis.* I feared valuable time had been lost, and that turning would now be very difficult to accomplish. I at once endeavoured to pass my hand over the right side of the child towards the pubes, but in doing so I felt the child recede, and therefore confined myself to raising the child's pelvis with my flat hand and fingers; whilst the pains forced down the occiput, the head descended, and delivery was quickly completed. I think if I had waited a little longer, spontaneous evolution would have occurred, and the child have been born head foremost, even without manual interference. The child was a full-grown male, lively and vigorous; the placenta followed easily.—Both mother and child did well." (p. 106, 107.)

Dr. Copeman quotes this as a case, that if left to itself would have been spontaneous evolution; but he evidently mistakes the sense in which this term is generally understood. Had the child presenting the back so changed its position as to bring the head into the pelvic cavity, the evolution would certainly have been spontaneous, but it would not have been the "spontaneous evolution" of Denman, Douglas, and all obstetric

authorities. The case is remarkable as being the very unusual presentation of the back in a full-grown child, and also in the success with which Dr. Copeman pushed up the hips and buttock of the child, and brought the head into the pelvis—a feat which the older authors used always to recommend to be done in cross-births, but which being exceedingly difficult to accomplish, the attempt was given up for the present more usual practice of bringing down the buttock and feet, and so delivering it. Dr. Copeman's success we attribute to the extremely wide pelvic cavity, which admitted a full-grown child to be so doubled up in it, and which of course could present no impediment to the passage of the head. As a rule of practice, however, we would caution Dr. Copeman against adopting it in the next back-presentation he meets with, lest he might find it not quite so easy to bring the head into the pelvic cavity as in the case related.

Under the head of Hæmorrhages there are many interesting cases that we must omit, but two of them are too important to pass by. One is a case of transfusion; every means to arrest hæmorrhage and revive the patient had failed.

"When seeing the patient must soon die, and not wishing to delay the trial till jactitation was present, I injected six ounces of blood into the right cephalic vein, which I took from her husband's arm. Whilst this was doing, the patient became more distressed—the pupils dilated—purplish pallor of face—pulse no longer perceptible—death within an hour after. The transfusion, though done readily, and as far as I can judge, in a right manner, not only failed to rally, but really seemed to have a bad effect." (p.115.)

The case also illustrates another professional error.

"The mistake here," Dr. Crosse observes, "was in the surgeon not calling advice as soon as he suspected a placenta prævia; when artificial delivery, so clearly unavoidable, so urgently called for, might have been *timely* and successfully adopted." (p. 116.)

Another case of retained placenta proves the mischievous effect of ergot of rye, which we quote because this medicine is so constantly given in these cases. Mrs. J. L—, after three days' labour, was delivered of a dead child.

"The placenta not following, and the funis being somehow broken off, Mr. R——, after an hour or so, tried to remove the afterbirth with his hand, but could not, and says he found hour-glass contraction. He sent for Mr. W——, who arrived at 8 p. m. Before Mr. W——'s arrival, but I know not whether before or after the birth of the child, an infusion of ʒi of ergot, in two doses, had been given. Mr. W—— examined soon after his arrival; but removing his hand to take off his coat that he might proceed to introduce the full arm to extract the placenta, the patient would not let him again proceed—and thus things rested, the patient not having perceptible afterpains, until I arrived at half-past five on Saturday morning. I proceeded to introduce my left hand (after spending an hour in the house, hearing the history of the case, and examining the patient), and *never felt such pressure as the uterus made on my hand;* so much as to make me complain, and to convince me that I could not long bear it—by perseverance, and also fortitude I may say, I got the hand to the fundus uteri, where the placenta lodged, *directly at the very fundus.* I could just feel the placenta—it could scarcely be called hour-glass contraction, because all the lower three-fourths of the uterus were contracted, leaving only the cavity at the fundus." (p. 121.)

Dr. Crosse with extreme difficulty removed a small placenta, and remarks :—

"It would seem that this was partial contraction of the uterus, in an extreme

degree, shutting up the placenta at the fundus. The case for ergot is when there is feeble contraction, usually accompanied by flooding more or less. Was ergot called for here? and might it not rather have increased the contraction by which the placenta was retained? I think the ergot had this effect, and was not called for." (p. 121.)

Most assuredly it was not; there is no greater abuse in practice, than giving ergot in this indiscriminate way;—a placenta is retained by irregular contraction of the uterus, ergot is given, and the irregular contraction is increased;—the head of the child is delayed in a pelvis too narrow for it to pass; the pains are suspended, and ergot is given; they return with violence and rupture the uterus.

We trust that these examples may have their proper effect in limiting the abuse of this medicine. The cases really requiring it are few; those in which it is improperly given are of every-day occurrence.

We cannot dwell longer on this little work, but shall merely quote one more instance of *mala praxis;* a case of lacerated os uteri.

"The patient forty-four years old. In her several former labours had long continuance of pains, but always regular and good—began to groan well the preceding evening; at two in the morning the membranes protruded, and *were ruptured by a surgeon,* who afterwards found os uteri very thick; and using his finger, *he tore it at one part, so that the end of his finger entered the substance of the neck of the uterus.* Violent pains continuing without progress for four hours, I was called—it seemed a case in which the state of the os uteri presented all the difficulty to delivery—it was swollen, and one and a half or two inches thick on the side next the pubes; on the right side the edge of the os uteri was lacerated, and *my finger went into the opening. The pelvis was sufficiently large—the child's head not impacted in it or fixed at all,* but driven down at each pain and rising up afterwards—pains violent and forcing. In such a state of os uteri I did not wish to use instruments, so I waited five hours, violent pains continuing all the while. There was very little change; the woman impatient and getting exhausted. By introducing my whole hand I could feel an ear; so I applied the forceps, and after a few pains effected the delivery of a fair-sized boy, whom I revived from the still state he was born in, and left him well. The membranes should not have been ruptured, and the os uteri had been meddled with too much." (p. 138.)

Such an example of ignorant meddling as this may well follow those we have related. A woman who had several former labours regular and good, but tedious, had fallen into the hands of a gentleman imbued with the doctrine that the membranes and contained liquor amnii are useless in labour, and therefore should be ruptured. The waters being discharged, the cervix and os uteri became congested; consequently the surgeon "afterwards found the os uteri very thick;" he thereupon, intending to adopt, we presume, another rule of artificial dilatation, *thrust his finger through the substance of the cervix,* so that Dr. Crosse's finger "went into the opening." Had this woman been in charge of a timid midwife, who would have let her alone, she would have got well, as in her former labours; but being in the hands of a more adventurous surgeon, she was indebted to the superior judgment and prudence of Dr. Crosse for her escape.

With this case we shall conclude our notice of this interesting collection;—a work that is valuable, because it gives the experience of a most eminent provincial surgeon; more valuable, inasmuch as that experience is so candidly and faithfully related; but most valuable, as it exposes

the present state of the practice of midwifery, even in a district where we should least expect deficiencies. We have not scrupled to notice these examples of *mala praxis*, and even to give them a prominence that would be unnecessary, were it not for the worse than apathy that exists respecting an improved education in midwifery. The sophism that we have alluded to—that because parturition is a natural process, no great amount of knowledge is required—has had a most mischievous effect in retarding its progress; and we believe that, as in the case of the Monument at London-bridge, it is only when several lives have been lost through negligence, that proper means will be adopted for the public safety. It is because we think that the injuries which result from an ignorance of midwifery must be put conspicuously forward,—must be thrust, as it were, before the eye of the professional public—that we have felt it our duty thus to notice the misadventures narrated in this excellent work. When one surgeon forces his finger through the substance of a thickened cervix uteri for the purpose of dilating it; when another tears his way through a healthy peritoneum, passing his hand along the front of the uterus, in place of into its cavity; when two surgeons in succession force their way through a morbid mass into the abdomen, tear the gall-bladder, deliver gall-stones, and yet never imagine that their hands are anywhere but in the cavity of the uterus; when such things are read of, it is time to reflect a little on the causes of such gross mismanagement, and to remove the evil that obviously exists.

ART. VII.

1. *Traité Elémentaire d'Hygiène Privée et Publique.* Par A. BECQUEREL, Professeur Agrégé à la Faculté de Médecine, &c.—*Paris*, 1851. 8vo, pp. 640.

An Elementary Treatise on Public and Individual Hygiène. By A. BECQUEREL, &c.

2. *Handbuch der Hygieine für den Einzelnen wie für eine Bevölkerung.* Von DR. FR. OESTERLEN, Professor der Medicin in Heidelberg.—*Tübingen*, 1851. 8vo, pp. 838.

A Manual of both Personal and Public Hygiène. By PROF. OESTERLEN.

3. *Report of a General Plan for the Promotion of Public and Personal Health, devised, prepared, and recommended by the Commissioners appointed under a Resolve of the Legislature of Massachusetts, &c.* Presented in April, 1850.—*Boston, U. S.* 8vo, pp. 544.

4. *First Report of the Committee of Public Hygiène of the American Medical Association: with an Appendix, containing Sketches of the Sanitary Condition of the Cities of Concord, Portland, New York, Philadelphia, Boston, Lowell, Baltimore, Charleston, New Orleans, Louisville, and Cincinnati.*—Philadelphia, 1849. 8vo, pp. 654.

5. *Southern Medical Reports: consisting of General and Special Reports on the Medical Topography, Meteorology, and Prevalent Diseases in the following* [*ten Southern*] *States, &c.* Edited by E. D. FENNER, M.D., of New Orleans. Vol. I., 1849. 8vo, pp. 472.

6. *Report on the Present State of Certain Parts of the Metropolis, and on the Model Lodging-Houses of London.* By R. D. GRAINGER, Esq. Ordered by the House of Commons to be printed, 13 June, 1851. Royal 8vo, pp. 60.

7. *Summary of Experience on Disease, and Comparative Rates of Mortality.* By WILLIAM LEE, Esq., Superintending Inspector. Official Report to the Board of Health. 1851. 8vo, pp. 78.

FEW medical practitioners can neglect the principles and practice of General Hygiène, if they would succeed in attaining or maintaining a good position. Individual exceptional instances may occur, but we are satisfied that the thoughtful and intelligent men in the profession will agree with us in the general proposition we announce. This circumstance arises from the great and increasing interest taken by the public in matters relating to the public health and to personal hygiène, and from the necessity the layman feels himself under of appealing to the professional man for information. A very brief act of reminiscence will indicate to the senior practitioner, how greatly things have changed in this respect since he was young. Twenty years ago "the privy odour" was only thought of as a nuisance to the sense of smell, to be coarsely joked about, or, at most, abominated as merely an odious stench; now it is seen to have important relation to the origin and intensity of disease, especially of the febrile class, both by the profession and laity;—and so also with other matters.

The theory and practice of Hygiène constitute, therefore, a *practical* question, and this, whether we consider the subject in its relations to professional success in a pecuniary sense, or to professional success in combating disease. Whether the majority of the profession be or be not wearied with their unavailing appeals to local boards of health, or boards of guardians, or to even higher authorities, is another question. We believe many have felt that they have laboured in vain, and have therefore experienced bitter disappointment and that helpless lassitude which want of success excites. To them this subject may be distasteful; nevertheless, although the race is not always to the swift, nor the battle to the strong, we have the same high authority for a more encouraging observation—namely, that no effort to do good is unavailable. "Cast thy bread upon the waters," said *the* wise man, "and it shall be found after many days."

The list of hygienic publications before us (and it might be made far more lengthy) is a convincing proof that the important subject of the *prevention of disease* has lost nothing of its interest, or rather, that the medical profession in the four most enlightened nations of the world is true to its high mission. Ever so brief a statement of the various facts and conclusions advanced by the writers under notice, would only be rendered wearisome by dry details, and would be unsatisfactory from its condensed form. We are therefore compelled to select a topic for illustration from the large mass, and we cannot think of one more important than the *Hygiène of Cities.* The wider spread and greater prevalence of epidemic diseases, concurrently with the rapid increase of civic populations, cannot have escaped the observation of the most unobservant. Even the sweating sickness has re-appeared on the Continent of Europe, having proved very fatal in several districts of France. With a closer aggregation of mankind in cities, there is undoubtedly a greater development of numerous causes

of disease; and it is to these that we would briefly recal the attention of our readers.

We must premise, however, a few words respecting each of the publications before us. M. Becquerel's treatise is eminently French in its arrangement and details, but it contains little that is novel, or not to be found in the works of Metard and Lévy. It is concise, comprehensive, portable, and moderate in price, and is therefore suitable to the library of the union officer or medical inspector. Professor Oesterlen's may be looked upon as a companion work to Professor Becquerel's. The arrangement is nearly the same; the general principles are not different, but the subjects are looked at from different points of view. They are treated in a less scholastic and generalizing manner; they are illustrated more extensively by special facts and statements, and the handling is altogether more practical and more suitable to every-day work. To the German scholar this work may also be recommended as a book of reference and a storehouse of precise facts, such as English literature cannot afford.

The American works are of a different character; they are English; that is to say, facts are observed, deductions drawn, and special applications advocated. The admirable method of the French, and the laborious research and minuteness of the German, writer are wanting; but there is a homely origin and a tone of earnestness running throughout the publications, which more than compensate for this want. In some respects the American works are amusing, from the glimpses they give us of professional doings and habits of thought in the New World, or interesting, from the information they convey as to the social condition of the people. Apropos of quackery and orthodox practice, we find the following in a report on the topography, climate, and diseases of Madison county, Alabama, by S. G. Bassett, M.D., of Huntsville, in Dr. Fenner's volume:

"There are thirty physicians in the county, who do an aggregate of about thirty thousand dollars of good practice annually; to which may be added ten or twenty per cent. for *charity*. There are six irregular or nondescript practitioners, who claim an aggregate practice of six thousand dollars annually. . . . . We have also a German root-doctor, a homœopathist, a steam-doctor, and several negro faith-doctors. I have no means of ascertaining the amount of their practice, though it is certain that the last who comes generally does a respectable amount for a longer or shorter time, which depends upon his individual tact and church-membership."

The same vivacious writer gives the following illustration of medical practice, thirty or forty years ago, from the traditional archives of the village:

"Case.—A man was knocked down on an election-day, and lay senseless. Dr. H—— forced his way through the crowd, and seized the wrist of the patient. (The radial artery dividing high up, dodged the doctor's thumb by passing over the back of the hand.) Having no history of the case, he pronounced it 'apoplexy.' 'Is he dangerous?' said the sheriff. 'I pronounce him a dead man,' said the doctor. 'Did you say that in reference to me?' asked the patient, rising and reaching for his smashed hat!"

We are not sure, however, that an English medical work has not to answer for these and other amusing escapades of Dr. Bassett; for there is internal evidence that the writer is acquainted with 'Medical Bibliography, A and B,' by the late learned and witty James Atkinson, of York; a work *sui generis* in medical literature, whether we consider the research it

displays or the wit, humour, and copiousness of the writer; and Dr. Bassett seems to have taken his tone from this curious book.

The "introductory address" of Dr. Fenner is creditable to his judgment and feelings. In common with his brethren in the north, he laments the sad neglect of Public Hygiène by the state, and the fearful destruction of human life thence resulting.

"From the earliest settlement of the Southern States by the white man, their historian had to note the prevalence of the most destructive diseases. If the thousands who have fallen into untimely graves could rise from the dead and stand before us, how appalling would be the sight! In view of this great destruction of human life, we might readily suppose that the diseases to which man has been exposed in the south had been objects of special attention, and that the profession which is devoted to their study and management had received every possible encouragement and support—moreover, that every physician who had witnessed the sufferings of his fellow-beings, and discovered any better method of relieving them, had not only been richly rewarded for his services, but urged by every noble impulse to transmit his observations to those who were to follow him. But how different are the facts! The governments organized by the people of this region to provide for their welfare and safety, have deemed the preservation of human life either *beneath their notice or beyond their comprehension;* whilst the physicians, left to take care of themselves, and bereft of all the customary stimulants to ambition, have spent their toilsome lives in obscurity, and carried with them to the tomb whatever useful knowledge they may have gathered from the lessons of experience." (p. 8.)

Dr. Fenner's volume contains thirteen Reports on the medical topography, epidemics (especially yellow fever and cholera), and medical charities of New Orleans; four from Alabama, three from Georgia, four from Mississippi, one from Memphis (Tennessee), three from South Carolina, and two from Texas. There are also practical communications in medicine, and surgery, and miscellaneous articles; the whole constituting an interesting volume.

The American Medical Association occupies in the United States a far more important position than any *single* medical society or association in England. The attention of the British profession has already been drawn to the excellent code of ethics of this body, as well as to its transactions. The Report of the Committee on Public Hygiène is an interesting indication of a high tone of professional morals, and contains a mass of valuable data. The general views entertained by the committee may be gathered from the subjoined extract from the Report on the Public Hygiène of Boston and Lowell, by Dr. J. Carter:

"The diseases which are most fatal among us are those that are the most easily prevented by hygienic prophylactics, yet they are increasing, and so subtracting from the valuation of human life in our midst, that in our state, our cities, and villages, longevity is taking the inverse ratio of their increasing density of population. In this particular we are falling into the rear of other nations. The dwellings of the masses and the factories of the few seem less cared for than our prisons. Municipal authorities dictate the mode of building for the protection of property, but not for the protection of life. Typhus, dysentery, and struma, visit the offending, especially in dense urban localities . . . . The germs of malignant disease incubated in these nests, extend their infectious toils to those who visit to counsel and succour, as well as spread over juxtaposite localities . . . . Between the last two annual meetings of the medical society of this state, seven of its members died in this city (Boston). Four of these died of fever contracted while on

professional duty. The best testimony informs us that near 4000 deaths and over 75,000 cases of sickness might have been avoided in Boston within the last three years; and that over 800 deaths and 16,000 attacks of sickness might have been prevented in Lowell within the same time. (p. 532.)

In January, 1848, the Massachusetts Medical Society presented a memorial to the State Legislature, calling its attention to the sanitary proceedings of "some of the European governments;" to the fact that sanitary surveys had been made by public authority, by which means "many of the causes of disease have been revealed, and many of them are proved to be removable;" and requesting the legislature to "cause a similar sanitary survey to be made of the State." This request was supported by the various cogent reasons which have been continually adduced in England on behalf of an improved system of public hygiène. About the same time a petition was presented to the same body, with a similar request, and adducing similar arguments, by the American Statistical Association. This petition appears to have been drawn up by Dr. Jarvis, of Dorchester, Massachusetts, the chairman: it is a lucid document, and embodies many important statistical facts bearing on the hygiène of the State, some of which we shall make use of. The American public is also indebted to Dr. Jarvis for an article on public hygiène, in the form of a review of recent English reports and publications bearing on the sanitary question, and published in the American Journal of Medical Science. It is an able abstract of the more important facts collected by the Royal Commissioners, or found in the Health of Towns Reports.

The Commonwealth of Massachusetts, in accordance with these steps, appointed three commissioners to prepare and report a plan for a sanitary survey of the State, embracing a statement of such facts and suggestions as they may think proper to illustrate the subject. Lemuel Shattuck, an able statist, of Boston, as chairman and reporter, Nathaniel P. Banks, of Waltham, and Schiel Abbott, were nominated to this duty. They appear to have taken as their model the Report of the Royal Commissioners for inquiring into the sanitary condition of towns and populous districts in Great Britain, embodying at the same time the facts collected by the Statistical Association. In addition to a voluminous and interesting report, a bibliography of hygiène is given in the form of a catalogue of works suitable for sanitary libraries. We feel satisfaction in observing that our own labours have been appreciated by the commissioners of Massachusetts, and that we occupy a place in their catalogue as "an able advocate of sanitary reform," and as "the leading medical review of the world."—We subjoin the general conclusion to which their inquiries have led them.

"We believe that the conditions of perfect health, either public or personal, are seldom or never attained, though attainable; that the average length of human life may be very much extended, and its physical power greatly augmented; that, in every year within this commonwealth, thousands of lives are lost which might have been saved; that tens of thousands of cases of sickness occur, which might have been prevented; that a vast amount of unnecessarily impaired health and physical debility exists among those not actually confined by sickness; that these preventible evils require an enormous expenditure and loss of money, and impose upon the people unnumbered and unmeasurable calamities—pecuniary, social, physical, mental, and moral—which might be avoided; that means exist, within our reach, for their mitigation or removal; and that measures for prevention will effect infinitely more than remedies for the cure of disease." (p. 10.)

We shall return to these American publications shortly, but we would here take the opportunity of calling the attention of Dr. Jarvis, Mr. Shattuck, and the members of the American Association generally, to our article on "Sanitary Reform," published in the first number of this journal, and also to Mr. Neison's paper therein quoted. We think, upon re-consideration, they will doubt whether the "average age at death" is so useful and certain an index of the sanitary condition of a population, as Mr. Chadwick (whose example they follow) conceives it to be. And although the inspectors of the general board still adopt the formula, we are not the less satisfied that it is a mistake.

We have next to call our readers' attention to the report by one of these inspectors (Mr. Grainger), on the sanitary state of certain parts of London. It is a remarkable document in every way—partly, because it describes a state of things almost incompatible with modern civilization—partly, because it demonstrates how much good modern civilization may effect. He asks his readers to look on two pictures—namely, the model lodging-houses of London, and the populous districts of London. During the prevalence of epidemic cholera in 1848-49, these localities were examined very carefully by inspectors. Mr. Grainger states:

"The uniform evidence of every medical man engaged during that period, whether as an inspector, visitor, or poor-law surgeon, showed that, in all parts of the metropolis, the dwellings of the industrious classes were in a most deplorable condition; that they were filthy, unwholesome, and neglected; that they were deficient in all the arrangements demanded by decency, comfort, and health; that there were in many localities, houses utterly unfit for human habitation; that the water-supply was miserable, both as to quantity and quality, the privies foul and overflowing, and the scavenging grossly neglected; and that as the direct consequence of all this, there was extreme misery, sickness, and mortality." (p. 4.)

Some improvements have been made since the inspection referred to; but how much yet remains to be done, may be gathered from the results of Mr. Grainger's recent inquiries. Of Agar Town, St. Pancras, he observes:

"This district has the aspect of one of the most neglected parts of the metropolis; from the appearance of the roads, it might indeed be supposed that one was suddenly transported to the poorest locality of some decayed country-town. In many places, the roads are in deep ruts, filled in with mud and filth; in various directions are large heaps of dry mud, which have evidently been long accumulated; here and there were deposits of manure, either lying by the side of the road, or within the small enclosures belonging to the cottages, all adding to the contamination of the atmosphere and general discomfort." (p. 7.)

The ditches, sewers, and drains reek with miasm; the stench being described by the inhabitants as "enough to knock them down." But more horrible descriptions follow: here is one, from the purlieus of St. Giles:

"At the top of a narrow alley in Church-lane, I saw an old hole full of excrement, and sickening to the smell. In this alley are four small dark huts, or single rooms, black with filth, crowded with children and adults. In one of these hovels, in which I could not stand upright, was a poor woman, cowering over a few embers, in the cold stage of ague; a girl with fever; and, standing at the door, a man who was attacked with typhus last January, had been three months ill in the workhouse, and although discharged was still ill and ailing, and his eyesight so much

affected from the fever that he cannot go up any scaffolding, and consequently is unable to get work." (p. 9.)

Another may serve to illustrate the supply of water (?) to the inhabitants of Jacob's Island:

"On the first day of my visit (April 28, 1851), the water in the mill-stream was low, so that the muddy banks were uncovered to a considerable extent. Many privies overhang the stream, and I saw a large quantity of excrement lying on the mud; the water was filthy and green, and poisonous gas was bubbling up on the surface. I found on this and a subsequent visit, that many of the poor are still compelled to use and even DRINK THIS HORRIBLY POLLUTED WATER. . . . . Some of the poor people stated, that on complaining to their landlord of the want of a proper supply, they were told that *the water of the ditch was good enough for them.*" (p. 11.)

"The Potteries," Kensington, were inspected by Mr. Grainger, and he thus describes the condition of a putrid pool there:

"Standing by the side of the National School (!), I found the stench arising from 'the Ocean' most oppressive; the water is saturated with putrid organic matter, proceeding partly from the numerous pigsties, which empty their contents into the pond, and partly from the many dead animals which are thrown into it. I counted the bodies of twelve pigs, and the schoolmaster informed me, that he had seen twenty dead animals at one time in 'the Ocean.'" (p. 12.)

In Jennings' Buildings, Kensington, the privies are in a frightful state, that is, where there are such things at all.

"It is impossible, by any description, to convey an idea of the horrible condition of the few privies that are provided for this large population. Many of the houses are altogether unprovided. One young married woman said, there was no privy for the house where she lived, and, consequently, she was obliged to use the common convenience described below, adding, that 'it was a shameful thing.' Another person said he lived in Cooper's Gardens; that there was a privy for the back rooms, but that it was locked up; and he had a front room. Some time since, it appears, a public privy was provided; but, owing to gross neglect, it is in a most disgraceful state, the whole area being, at the time of my visit, covered deeply with excrement, and emitting so foul a stench as nearly to induce vomiting. To this place there is no outer door, and, in this horrible state, men, women, and children are compelled to resort to it in common. In another part was a second privy, open, and in a most foul condition; in other courts and roads excrement was lying about, producing altogether a most loathsome appearance, and poisoning the atmosphere with that peculiar stench, which, when permanently present, is always accompanied with a large amount of disease and mortality." (p. 13.)

This is a state of things not peculiar to London. In all the reports of the Board of Health, to whatever part of the United Kingdom they relate, the bestial state of the privies, and their noxious influence on the health and morals of the people, are placed by the inspectors in a prominent point of view.

Turning to the Model Lodging-houses, we find the picture reversed. Instead of sickness, health; instead of filth, cleanliness; instead of a very high rate of mortality, a rate below the average of the whole country. Out of a population of mechanics, &c., amounting to 1082 in four establishments, the deaths were one per cent. per annum; while the average mortality of England, including all ranks, is more than double, or above two per cent. Infantile viability is so much diminished by defective hygiène, that

the number of deaths under ten years of age may be considered as a fair exponent of the sanitary condition of a district. In Old Pancras Buildings, the average annual mortality was somewhat more than two per cent. for the three years ending May 31, 1851; but then there was a large infantile population. In 1841, the number of children living in the metropolis under ten years of age, was in the proportion of 21·7 per cent. on the total population, and the mortality on an average of seven years 5·2 per cent.; but in the Metropolitan Buildings the proportion of children was nearly treble, or 62·3 per cent., while the mortality amongst them was less than one half, or 2·5 per cent.; so that if the children in these Buildings had suffered in the same proportion as the mass of children in the metropolis, the deaths would have been 56 instead of 28. This test of salubrity is very decisive; but it is the more decisive when we rem mber that the *general* infantile mortality includes *all* ranks; whereas, in these Buildings, the population is purely of the working class, amongst whom the infantile mortality is elsewhere excessive, the mean duration of life at "the Potteries" being only 10 years! The *absence of fever* from these exemplar structures is a remarkable circumstance. Mr. Grainger has ascertained from evidence received, that in six out of seven of these model establishments, including the lodging-houses for single men, and containing about 957 persons, there has not been a single case of typhus; whilst in the Metropolitan Buildings, opened upwards of three years, and having an average population of about 550, there has been only one case, and that hardly to be considered as true typhus. Mr. Grainger remarks on these facts:

"Now, if, for the sake of illustration, the 1507 of the working population of the model buildings, several of which are situated in the most crowded and unhealthy parts of London, and where, as in Church-lane, fever prevails, be contrasted with the working population of Liverpool, about *sixty* cases of low fever ought to have occurred annually." (p. 31.)

This improvement in the health of a population is a constant result of the appropriate means. It has been proved in the Report of the General Board of Health on the Epidemic Cholera, that in the metropolis every efficient sanitary improvement has been followed as directly as cause and effect by a corresponding decrease of sickness and mortality. There is no exception, Mr. Grainger asserts, to this rule; it applies to public institutions of every kind, to prisons, to hospitals, to lunatic asylums.

This glimpse at the condition of the mighty metropolis of the British race under its two aspects, will enable us to contrast that condition with the state of provincial towns, cities, and districts. Now, for the purposes of this comparison and inquiry, Boston, New York, or New Orleans, are as much provincial cities as Liverpool, Manchester, or Bristol. This is true, at least, in all *essential* respects, for the difference of climate is not so great as to modify the results. Let us take a glance, then, at the provinces, in this wide meaning of the term, and begin, first, with the facts accumulated in Dr. Fenner's "Reports." It appears that a Board of Health exists in New Orleans, whose principal duty is "to make an annual report to the several councils, as to the health of the city for the preceding year, and to suggest means for improving the same." It appears that the powers of the Board are even more limited than of boards in this country, since it

has not even the power to obtain the means of intimating correctly the actual sanitary condition of the city; but they have enabled it to come to the conclusion that the average annual mortality amounts to 5·99(!) per cent. in a population of about 105,347. The inhabitants of New Orleans, as of all other cities and districts, "with a reputation abroad for perennial pestilence, boast at home of unparalleled salubrity." The average annual temperature for nine months in the year inside the houses, is 72·81°; while the least reflected temperature it is possible to obtain is 80·40°. The results may be readily inferred, and the unparalleled insalubrity comprehended, by the intelligent reader, from the following description of the sewage and cleansing of the city.

"The removal of street and backyard filth, according to an ordinance of the city councils, and the strong and urgent recommendation and even entreaty of this board, has been essayed in vain; in fact, the proper and effectual cleansing of the city, its effectual drainage, so far as to ascertain how these conditions influence the health and well-being of the inhabitants, HAS NEVER YET BEEN TRIED! The offal from the houses has not been removed from the streets, at the periods directed by the board; the filth, scraped up into masses, has been suffered to lie for days, often until washed into the gutter by rains, or scattered by carts and drags, the gutters left choked up by filth." (p. 92.)

Add to this, that the mighty Mississippi is prevented overflowing the city by a bank "the Level," seven feet higher than the city level; that marshes surround it; that one half of the streets are not paved; that there are *no sewers;* that the open gutters often present the most disgusting aspect; that there are a considerable number of vacant plots of ground in the city, many of which are lower than the level of the streets, and during wet weather, contain stagnant water, which breeds myriads of mosquitoes; and that stagnant water is to be found under many houses;—and we have a picture of a locality in which the mortality from fevers must be frightful. But the report of Dr. Fenner shall speak for itself on this point.

"The New Orleans Charity Hospital is probably the most extensive fever hospital in the world . . . . It appears from the records, that in a period of *nine years,* from the 1st of January, 1841, to the 1st of January, 1850, there were admitted into this hospital 73,216 patients; of which number were admitted for *all the different forms of fever* 33,381 (and among these last) for *intermitting fevers,* 17,217."

During 25 years, the average mortality of yellow-fever cases in the hospital was 44 per cent.

In the face of this fearful amount of absolutely preventible disease, we think it psychologically curious to find the following passage in reference to England in the report of the New Orleans Board of Health.

"The towering pride of the loftiest power that intellectual man has ever seen, even now totters to its fall from an oppression in Ireland, nay, under her very nose in "happy England" and India, the like of which defies the records of history, and the naked facts of which stagger the credulity of the present day. But it is thus that an all-seeing eye avenges oppression and punishes misconduct, and it is thus that He gives lessons to nations and warns them of the consequences of their misdeeds."

We presume this was written with special reference to the famine and pestilence of Ireland. Poor John Bull! Thy ten millions sterling, thy lavish private subscriptions, thy unresting efforts to ameliorate and improve thy people, undertaken with a sincerity and earnestness un-

questionable, cannot save thee from this reproof—a reproof, too, from a people who thus treat their fellow-men, when suffering from a disease induced by "a deficiency of suitable nutriment."

"To cure the habit of dirt-eating, many (slaveowners) resort to the cruel methods of preventing the indulgence of it by tin masks for the face, *iron gags, chaining on plank floors, &c.*" (Report of Dr. J. B. Duncan.)

We have many abuses to assuage in England, but we can come into court with clean hands and say, the meanest pauper and labourer amongst us experiences, when suffering from the consequences of a "deficiency of suitable nourishment," no treatment like this. We thank Dr. Duncan for protesting against it.

Passing from the south to the north, we note the condition of Boston, the Athens of the United States. In sixty-five miles of streets there are only twenty-five miles of sewers, and those so very imperfect as to be little else than elongated cesspools. Dr. Buckingham, one of the physicians to the Boston Dispensary, states :

"Speculation has built up houses and streets long before the new ground has become solid, and at such slight elevation, that waste water can run off only with the tide's fall, and on its return the water is, in some places, to be heard under the houses, and instances are not unknown where the water regularly comes up so as to be seen from the rooms below. I have seen the tide pouring into a back yard, from all four sides, to the depth of a foot, and have known men to sail around their kitchens in pursuit of their dinners, and coast along the shores of their cellars in tubs for their winter's wood."

The poor in Boston (and even more in Lowell, in which "the Acre" rivals St. Giles's) suffer from over-crowding and defective conveniences much in the same way as in London. Dr. Buckingham states :

"Almost all the houses are provided with privies, into which chamber-slops are thrown. There are some houses, however, unprovided with these buildings; in such cases, as well as in some others, excrement is thrown into the yard, and even under the lower floor of the houses. The dwellings of the poor are mostly filthy, often from neglect on the part of the occupants; as often from neglect on the part of the landlords, who get large rents, and do not provide suitable drains, privies, yards, &c. The number of families in a house varies with the number of rooms. I have found from six to forty or more in one house of two stories; eleven or more in one room constantly, and eight in one bed (men and women). This latter was in a physician's house."

The experienced hygiènist would easily predicate from these data the class of zymotic diseases to which the people of Boston are most liable. We shall have no difficulty in crediting the statement of the Massachusetts Sanitary Report :

"It is proved that the people of this State are constantly liable to typhus, cholera, dysentery, scarlatina, small-pox, and the other great epidemics, and to consumption and the other fatal diseases which destroy so many of the human race in other parts of the world."

Referring to the statistical tables, we find that the greater number of deaths take place during the hot months of August and September; and that infantile cholera, diarrhœa, and dysentery (the diseases caused by sewer-poison), are the causes of the increased mortality.

We ought not to omit the mention here of circumstances which take

away a great deal of the blame, which, without explanation, might be attributed to the intelligent citizens of Boston. It is well-known in England that the sanitary condition of our town populations has been very much lowered by the immigration of large numbers of the Irish peasantry. We need not enter into any special details touching these people; their habits and conduct are too well known in both hemispheres. We have only to say that they form a large proportion of the population of Boston. During the four years ending with 1849, the population of Boston increased by about 23,000, the whole of which is attributed to foreigners, three-fourths being Irish labourers, and the remaining one-fourth from all nations, and of all professions and occupations. Hence it is obvious the terms "foreigner" and "Irish labourer" are nearly synonymous in the Massachusetts Sanitary Report. Of the births in 1849, 62 per cent. were the children of foreigners; of the 1066 vagabond children in the city in January, 1849, 90·3 were foreigners; of 1170 dram-shops, 70 per cent. were kept by foreigners. "More than three-fourths of all the arrests by the night-watch and police in Boston, and nearly three-fourths of all the commitments to the county jail, and of the cases before the police and municipal courts, were those of foreigners." Of the Dispensary patients 88 per cent. were foreigners; so, also, 97 per cent. of the inmates of the Boston almshouses. Of the total number of deaths from cholera, 81 per cent. were foreigners; of the entire number of deaths from all causes, 59 per cent. were foreigners; and, finally, there has been actually a *decrease* in the American population, while the foreign element has increased so much as to amount to *nearly one-half*, or 45·62 per cent. A significant diagram pointedly indicates the unhappy people amongst whom all this misery, physical and moral, concentrates. It gives the mortality at different ages, and shows that while of the healthy population of England, 82 per cent. survived the age of ten years, and 50 per cent. of the general population of Boston attained the same age, the proportion among the "Catholics" was only 34 per cent.! We also find it stated in the petition of the American Statistical Association, that while in Massachusetts the deaths of children under five years were 33 per cent. of the total deaths, they amounted to 61 per cent. among the "Catholics" of Boston. We mention these facts, because we shall presently refer to the preventible agencies recommended, and shall have to point out the circumstance, that *ignorance is the great source of evil.*

An interesting general fact of very great importance is noticed in the Sanitary Report of Massachusetts, and in the "Summary" of Mr. Lee—namely, that the sanitary condition of towns has rapidly deteriorated for some years past. The reporters of Massachusetts, justly premising that the extent to which epidemic, endemic, and contagious diseases prevail as causes of death, is the great index of the public health, show, by an interesting table, that they have gradually increased in Boston in fatality, until they have *doubled* in thirty years; or, in other words, have risen from 15·85 per cent. during 1810-20, to 21·32 per cent. during 1820-30, to 27·56 per cent. in the next decennium ending 1840, and, finally, for the ten years ending 1849, reached the average of 31·59 per cent. These facts are the more pregnant with warning, because, even in Massachusetts, nearly the whole of the increase of the population is to be found in cities and villages,

and not in the rural districts. "The tendency of our people," the Report observes, "seems to be towards social concentration: and it is well to inquire what will probably be the consequences of these central tendencies." On this point the petition of the American Medical Association contains some striking facts. Thus, while twenty-four cities and towns of Massachusetts have increased, in twenty years, 102 per cent., the rest of the country increased only 20 per cent.; and while twenty-two cities of the United States increased, in the same time, at the rate of 155 per cent., the rest of the great republic increased only 72 per cent.

The deterioration of the health of several English towns is pointed out by Mr. Lee in a very conclusive series of tables, comprising Reading, Gainsborough, Ely, Selby, Wisbeach, and Norwich. He attributes it to the cesspool system, &c.:

"The cesspool and open-privy system, the surface gutter, and stagnant pools and ditches, perform a cumulative work, that of saturating the whole subsoil of towns. Perfect saturation, when the whole mass in any case would become a dungheap, is but a question of time. In every place I have visited, the *earth* is tainted, and the evaporation from its surface poisons the *air*, which the inhabitants are compelled to breathe; in most towns the accumulated filth already percolates to the wells, and pollutes the *water*, which the people are compelled to drink." (p. 47.)

With the object of showing more distinctly the injurious operation of civic miasm, Mr. Lee compares the mortality of towns with that of the adjoining rural districts; he also arranges the deaths from "preventible" and "inevitable" diseases under distinct headings. The tables are very elaborate, and must have cost the compiler an immense amount of labour; we subjoin the conclusions to which they lead:

"Statistical analysis proves, that the *inevitable* mortality of the inhabitants of this country is probably not greater than the proportion of ten to a thousand of the population per annum.

"The analysis of vital statistics proves that the existing annual mortality in differently conditioned parts of the same town, varies from 17 to 45 in a thousand; that the deduction of the deaths arising from preventible diseases, from the whole deaths, leaves an inevitable mortality of only 11 per thousand; and that this is nearly a constant quantity in healthy and unhealthy districts." (p. 64.)

It follows from these remarkable results of statistical inquiries, that the populations of towns lose at least one-half the attainable period of life. Whether full reliance is to be placed upon the figures or not (and figures are much doubted), there can be no doubt of the *general* principles, however the minute accuracy of the details may be questioned.

The remedies.—Mr. Lee lays considerable stress on the *pecuniary loss* inflicted by these high rates of mortality, with the view of demonstrating that sanitary improvement is a measure of economy as well as of duty. We doubt much, whether this constant putting-forward of the money argument (sound as it is) has had a favourable influence on public opinion. It has deadened the enthusiasm of those likely to guide the public mind, and has failed to convince the hebete judgments of those to whom it is mainly addressed—namely, guardians of the poor, and other civic authorities. We should prefer that active legislation be defended on the broad ground of Christian duty, and as a large question of morals. It is true that "godliness has the promise of the life which now is" has been asserted by high authority, and adduced as a persuasive to a Christian life; but the

argument is ever placed subordinately to those of a higher nature. So also, in this case, the hope of reward for well-doing may earnestly be held out to encourage effort, but not otherwise.—The principal means that have been recommended as adapted to abate the evils demonstrated, may be traced to government *employés;* it is not, therefore, surprising that the agency of officials is involved. Two plans are before us—namely, first, the recommendation of the Massachusetts Sanitary Report; and, secondly, the circular issued to town-councils by the General Board of Health, in February last. In the latter, the Board, in pursuance of the Public Health Act, § 40, direct what duties shall be performed by "Officers of health." These are comprised under thirty heads, and require the "officer" to take all practicable means to obtain the earliest information of the occurrence of cases of fever, small-pox, measles, or other diseases of the zymotic class; to visit the locality affected, make a "house-to-house visitation," and give such warning or instructions, and take such measures, as the circumstances may seem to require. In case of the disease being small-pox, he is to look after the unvaccinated, and urge on the guardians the duty of seeing that all such are vaccinated. If the locality be overcrowded, he is to give instructions for the removal of the sick, or the diminution of the overcrowding; and if a death occur in a room occupied as a living or sleeping-room by the survivors, he is to give instructions either for the removal of the corpse, or of the inmates. In *all cases* of death, he shall give instructions to the survivors as to the care of the body, as to the sort of coffin, as to the antiseptic precautions, and as to the removal and interment of the corpse. He must examine the locality of epidemics, with a view to the discovery and removal of preventible causes; inspect schools in which small-pox &c. have broken out, and officiate accordingly. Under similar circumstances he must inspect factories, "and inquire and examine whether there be symptoms of the disease amongst the workers, and give such instructions," &c. That he may attain to all possible sources of knowledge, he is to "maintain a constant communication with the inspector of nuisances for the district, . . . with the ministers of religion, the registrar of his district, and the Union medical officers;" and is to appoint and notify convenient times at which he will personally attend daily at his office to receive information. He is also to find out the consumptive, to note the pallid, feeble, or sickly in schools, factories, or workshops; he must superintend "works of amendment" recommended by him; perform all the duties imposed upon him by local boards of health as to inspection &c. of slaughter-houses, lodging-houses, cleansing of cesspools, &c.; he must report noxious smells and atmospheric pollutions of all kinds, and collect evidence for courts of law as to nuisances. If a person die without having a medical certificate, he is to inquire into the cause of death, and if violence be suspected, collect evidence to aid the investigations of coroners or magistrates. He must attend all meetings of the local board, keep "a diary" and "a register of complaints," and he must make a quarterly report, and prepare annually another report, full of all kinds of statistics, such as Mr. Chadwick delights in. The instructions, unfortunately, do not say when he is to eat or sleep, or enjoy a day's holiday—a grievous omission, as with such multifarious duties we do not see how he could possibly find time for either.

This brief abstract sets forth very imperfectly the nature of the duties sketched out by the General Board of Health; but we need hardly observe to those acquainted with "the local authorities," and the habits and feelings of the people of England, that the scheme, however well devised, is at present quite impracticable, and must remain so until there is *a sound public opinion*, and the people perfectly comprehend the importance of public hygiène, and the value of preventive measures.

While we write, a vigorous attempt is being made by the General Board of Health to expurgate the filthy localities of our towns, suppress cellar-dwellings, and reform the lodging-houses, by a special application of Lord Ashley's rider to the Public Health Act, taken in connexion with two admirable Acts passed during the last session of Parliament—which are among the most important *moves* that sanitary legislation has yet made, one of them being for the imperative regulation of the common lodging-houses at present in existence, by the local authorities; the other for enabling local boards of health and other local authorities to borrow money for the establishment of such well-ordered houses, with the approval of the Treasury and of the General Board of Health. How urgently such changes are needed for the moral as well as physical improvement of our labouring population, will be apparent from the following extracts from the 'Notification' recently issued by the General Board of Health.

"The Report of the Commissioners of Inquiry into the State of Crime, and the Means of establishing a Constabulary Force, showed, by evidence received from every part of the country, that the common lodging-houses form, in most cases, the chief hotbeds of crime in county towns; and it subsequently appeared, from similar evidence set forth in the First Sanitary Report, that overcrowded and filthy lodging-houses are the places where epidemic disease usually first breaks out, and is the most obstinate and fatal.

"The evil influences of such houses are not confined to common mendicants or tramps, but seriously affect large numbers of honest and industrious persons, such as wayfarers and handicraftsmen in search of work or in temporary work; sailors returning to or from their ships or waiting for them; poor emigrants, labourers in husbandry, such as those who go hop-picking, or are seeking general harvest work, and who, not having proper lodgings in farms or in villages (where two families are now frequently huddled into one cottage), are driven into sheds, or obliged to encamp in fields, and, as was found during the prevalence of the last visitation of Asiatic cholera, were crowded into the worst description of common lodging-houses in towns. Whilst the attention of the Union medical and relieving officer is constantly required for the relief of the sickness created by the condition of these houses, and the consequent helplessness and destitution among this class of lodgers, the reports of inspectors and chaplains of prisons show that the vigilance of the police and constabulary is constantly called for in these places, and that the first deviation from courses of honest industry, in a large proportion of the instances of delinquency by young persons of honest parentage, is due to the same influence.

"Among the worst of the places in which some of the labouring classes are driven to herd together are, in many parts of the country, the beershops. Hitherto, beershops have not been under any magisterial or other supervision: but, under the Common-lodging Houses Act, whenever a beershop or a part of the house in which a beershop is kept, is used as a common lodging-house, it is to be regulated under that Act.

"The investigations into the health of town populations, the measures taken for the relief of severe epidemics, and the house to house visitations of the clergy and home missionaries, have also disclosed the fact that some of the worst forms of human misery exist amongst the comparatively settled labouring classes of towns crowded together in cellar-dwellings.

"Hitherto the excuse or defence of local administrations for these admitted states of misery and depravity, and for the continuance of a condition of the poor disgraceful to a Christian community, has been the want of power or means either to regulate the existing lodging-houses or cellar-dwellings, or to make other and suitable provision for the accommodation of their inmates. To attempt to diminish the overcrowding of common lodging-houses by ejecting the lodgers without providing fitter habitations, would, it has been said, and often very justly, only increase the number of miserable outcasts, who are found sleeping in out-houses or in the streets. To eject families dwelling in cellars, without any suitable provision for housing them, would be to force them to overcrowd upper rooms, and a doubly crowded upper room might be even worse than a less crowded, though damper, cellar-dwelling." (p. 1—3.)

This has been, in fact, the result of the well-intentioned attempt to put down cellar-dwellings in Liverpool, for which purpose a local act was obtained a few years since.

"When proceedings were taken to execute that Act, it was found that it involved the unhousing of a population of between twenty and thirty thousand people, without any provision whatsoever for their reception. The board have been informed that under these circumstances the partial attempts to execute that Act have been attended, as might be expected, with an increase of the evil of overcrowding the upper tenements; and that the want of other provision is proved by the continual occupation of the police in the re-ejection of persons who have been driven back into cellars declared to be unfit for human habitation." (p. 3.)

It is considered by the General Board of Health, however, that as the local authorities are now themselves bound to provide the accommodation required, the time has come for enforcing the provisions of the Health of Towns' Bill; and accordingly they impose upon churchwardens and overseers of the poor the duties there marked out for them. To let or occupy as a dwelling *any* vault, cellar, or underground room, built or rebuilt after August 31, 1848, or not let or occupied before that date, rendered the person so letting or occupying liable to indictment from the time of the passing of the Act; and the letting or occupation of such cellars &c. as were previously in use, became illegal a year after that date; or, in districts under the Public-Health Act, in six months from the application of that Act. The 'Notification' concludes with the following admonition:

"The Board would forewarn the members of local boards and others, that the due execution of the provisions of the law will need especial attention and support against the opposition, indirect as well as direct, which it is matter of experience will be raised against them by owners of the worst-conditioned houses, who in most towns are found in array against the introduction of the Public-Health Act, or the application of its provisions, on the representation that they will eventually increase local expenses, whereas, when properly executed, they are found to diminish them. It is moreover necessary to state, that the evils are too large to be completely met by the legislative provisions referred to, and that they will require all the aid which may be rendered by voluntary associations or by private exertions, in providing means for arresting the moral and physical deterioration of the population by ill-constructed and overcrowded dwellings." (p. 10, 11.)

In company with this document, the Board has issued an Appendix, containing a selection from the Reports made through Dr. Lyon Playfair and Mr. Smith of Deanston, on the state of the cellar-dwellings and common lodging-houses in Lancashire up to 1848. As these have been for some time before the public, we need not now add any lengthened extracts from them; but we may cite a few short passages which will

serve to indicate the physical and moral condition of the occupants of these worse than bestial dens.

"Some time ago I visited (says Mr. Holme of Liverpool) a poor woman in distress, the wife of a labouring man; she had been confined only a few days, and herself and infant were lying on straw in a vault through the outer cellar, with a clay floor impervious to water. There was no light nor ventilation in it, and the air was dreadful. I had to walk on bricks across the floor to reach her bedside, as the floor itself was flooded with stagnant water. This is by no means an extraordinary case, for I have witnessed scenes equally wretched." (p. 2.)

"I know numerous instances of families (says Dr. Playfair) whose united wages amount to 40*s*. or 50*s*. per week, yet possessing only one sleeping room, so that the grown-up members of the family, male and female, sleep together often in the same bed.

"J. S. has a father and mother who live with him; he and his wife sleep in one bed, his father and mother in another, his two grown-up sisters in a third, his brother, a lad of 19, and a young man lodger 'who is courting one of his sisters,' in a fourth, all in the same room. J. S. does not know, or will not tell, how much they all make, but thinks it 'a good lot,' as his wife and sisters and brother are at factory, himself on a print-ground, and his father a labourer.

"Mr. Holland gives me the following instance in the case of one of his dispensary patients:—D. F. is a widower with one sleeping apartment, in which sleep his adult son and daughter. The latter has a bastard child, which she affiliates on the father, he upon his son, and the neighbours upon both." (p. 4.)

"These common lodging-houses (says Mr. Rawlinson, in his Report on Sunderland) are a prolific source of crime, vice, and human misery. They are frequented by vagrants, thieves, tramps, and beggars. Savages have more regard for the rules of morality than the outcasts who frequent them. Men, women, and children crowd the same room, the old with the young; and the most vicious leaven the whole mass. Males and females, strangers to each other it may be, occupy adjoining beds, almost touching, but without the slightest division or curtain of any kind. The males habitually strip themselves naked before lying down, even in the presence of young girls and children, and women are confined surrounded by such inmates. It is considered that 800 or 1000 cubic feet of breathing space is requisite for health; criminals in their cells have this allowance; but in many of the common lodging-houses there is frequently less than 100 cubic feet of space to each occupant. I have seen a room packed until there was only 35 cubic feet of air for each person; the atmosphere was almost suffocating; how it could be breathed a whole night, and the inmates live, seemed a wonder. These places are hotbeds for fever, vice, crime, and pauperism." (p. 18.)

We anticipate that these objects will be more attainable than those aimed at by the appointment of "officers of health," because self-interest and the desire for popularity will render local boards willing, if not efficient, co-operators with the General Board. We fear, however, that even these glaring evils will be more than a match for the ignorance and inertness of civic hygiènists. It is well known that the whole question of public hygiène is distasteful to them, because they cannot act much without betraying their ignorance; and to be convicted of incapacity would be a fearful hurt to their vanity. We trust, therefore, that the members of our profession will not shrink from the duty which we consider to be specially incumbent upon them—that of ceaselessly urging upon the civic authorities to exercise the powers with which they are now armed, for the improvement of the public health and morality.

The recommendations of the Massachusetts Commissioners are fifty in

number; the majority are identical with those made by our own Health of Towns' Commission; but there are some which are worthy of notice here. In recommending the formation of a General Board of Health, they propose that it be constituted of *seven* persons—namely, two physicians, a chemist or natural philosopher, a barrister, and two of other professions or occupations having special hygienic qualifications, and at the same time of practical experience in the common business of life. In recommending the establishment of local boards of health, they suggest the appointment of a "City physician," who should perform the duties of the surgeon to the jail and the Union medical officer with us, as well as of the inspector of nuisances; of a "Port physician," to superintend the quarantine arrangements at Boston; and of five "consulting physicians" to constitute a medical council for the city in all cases of emergency. It is recommended that an annual grant be made by the State for the purchase of books for the use of the General Board, and by each city and town for their respective local boards. The Board of Health of Philadelphia has already commenced a sanitary library. It is proposed that the Census and General Registration be placed under the same management. But the most important fact of all is, that the instruction of youth in the principles of physiology and hygiène is already made imperative under certain restrictions, in the Schools of Massachusetts. We quote the provisions of the Act passed April 24th, 1850.

"Sect. 1. Physiology and hygiène shall hereafter be taught in all the public schools of this commonwealth, in all cases in which the school committee shall deem it expedient.—Sect. 2. All school teachers shall hereafter be examined in their knowledge of the elementary principles of physiology and hygiène, and their ability to give instructions in the same."

The commissioners, in calling attention to this law, recommend that the scholars be required, on the first day of every month, to make a sanitary examination of themselves and their associates, by filling up a printed schedule, under the direction of the teacher. The commissioners recommend that insane and idiotic persons be placed under the protection of the Board of Health; all cemeteries and funerals to be under their management, as well as the passengers by merchant-ships; quarantine; the sanitary regulation of emigrants. They further note the effects of the smoke nuisance; the patent-medicine swindlers; and the adulteration of food, drink, and medicines, with the view of placing the remedy in the hands of the local boards of health. The extent to which drugs are adulterated is almost incredible.

"The subject was brought to the attention of Congress, and on the 26th June, 1848, 'an act to prevent the importation of adulterated and spurious drugs and medicines' was passed. Under this act special examiners are appointed to reside in the various ports of entry, to carry the law into execution. Dr. W. J. Bailey, the examiner at New York, has reported that during ten months, ending April, 1849, about 90,000 pounds of various kinds of drugs have been rejected and refused admittance at the custom house in that city alone! Among these were 16,989 pounds of rhubarb; 3253 pounds of opium; 34,570 pounds of spurious yellow bark; 12,483 pounds of jalap; 5058 pounds of iodine, and large quantities of various other articles." (p. 222.)

In Massachusetts the punishment for adulterating drugs is one year's imprisonment, or a fine of four hundred dollars; and for adulterating any

wine or other drink with any substance injurious to health, one year's imprisonment, or a fine of three hundred dollars.

In addition to these general recommendations, the Commissioners advise that institutions be formed to educate nurses for the sick; that a class of medical practitioners be educated to practise as preventive advisers as well as curative; that physicians keep records of all their cases (for which a form is supplied); that clergymen of all denominations make public health the subject of one or more discourses annually; that each family keep records of its physical and sanitary condition, for which suitable schedules are prepared; and that parents and others apply a knowledge of the laws of health to the management of infancy and childhood.

Of all these more or less valuable suggestions, the most noteworthy are those which recommend that the people be made practically acquainted with physiology and hygiène. In relation to these important subjects, it is emphatically true that "the people perish for lack of knowledge," and it is (we think) equally true, that unless the lack be supplied, the best directed efforts of the public authorities will be comparatively useless. The intelligent Americans fully recognise the great principle, that a free people can only be governed by guiding their judgments, and establishing a sound public opinion. With them this recognition is the inevitable necessity of their form of government and their political institutions. Hence the strenuous efforts to educate every citizen, and hence their numerous and well-established schools. The more restricted institutions of this country do not render this great necessity so apparent; but, in reality, education is with us also become absolutely necessary to the safety of our institutions, and to the maintenance of our position amongst nations. Although we cannot but regret that our American brethren have anticipated us in making hygiène a part of their established system of education, we feel that it is not too late to profit by their example, and to be encouraged by their success. In 1843, of 150,000 scholars in the public schools of Massachusetts, only 416 were pursuing the study of human physiology, while 2333 were studying the less important branch of algebra. The Hon. Horace Mann, Secretary of the Board of Education, made some very valuable remarks on this incongruity, in his able Report for that year. As a proof how much may be done by an earnest and intelligent functionary, we quote the following extract from a letter sent by Mr. Mann to Dr. S. Curtis, the reporter on the Public Hygiène of Massachusetts to the American Medical Association. It is dated April 12th, 1849.

"A vast change has taken place in the public mind since that time; perhaps on no other subject a greater. *All the teachers just out of our normal schools are well-grounded in the elements of human physiology. They introduce it wherever they go.* It has been prominent at all our teachers' institutes. I have lectured upon it hundreds of times to teachers and school associations. My estimate is, that not less than 15,000 of the 170,000 children in our public schools attend to this subject."

These are encouraging facts for the consideration of British statesmen and philanthropists. Whatever may be the fate of the parent country, we have no fears that the transatlantic stock will degenerate, with so able and earnest and wise a corps of labourers, and such a vast scheme of physical and moral advancement of a free people, as is comprised in the application of medical science to every branch of its social economy.

With these remarks we must close this article; not, however, without recommending the Report of the Massachusetts Commissioners (for which that State is mainly indebted, we understand, to Mr. Shattuck) to our readers, and especially to those officially engaged in sanitary operations.

ART. VIII.

1. *Anæsthesia, or the Employment of Chloroform and Ether in Surgery, Midwifery, &c.* By J. Y. SIMPSON, M.D., F.R.S.E., Professor of Midwifery in the University of Edinburgh, Physician-Accoucheur to the Queen in Scotland, &c. &c.—*Philadelphia*, 1849. pp. 248.
2. *A Treatise on Etherization in Childbirth, illustrated by Five Hundred and Eighty-one Cases.* By WALTER CHANNING, M.D., Professor of Midwifery and Medical Jurisprudence in the University at Cambridge, U.S.—*Boston*, 1848. pp. 400.
3. *On Anæsthesia and Anæsthetic Substances generally.* By THOMAS NUNNELEY, Esq., F.R.C.S.E., Transactions of the Provincial Medical and Surgical Association.—*London*, 1849. pp. 381.
4. *Traité de la Méthode Anæsthétique appliquée à la Chirurgie et aux differentes Branches de l'Art de Guérir.* Par le Docteur E. F. BOUISSON, Professeur de Clinique Chirurgicale à la Faculté de Médicine de Montpellier, Chirurgien en Chef de l'Hôpital Saint Eloi, &c.—*Paris*, 1850.

Treatise on the Anæsthetic Method applied to Surgery and to the different Branches of the Healing Art. By Dr. E. F. BOUISSON, Professor of Clinical Surgery to the Medical Faculty of Montpellier, Chief-Surgeon of the St. Eloi Hospital, &c.—*Paris*, 1850. pp. 560.

5. *Ether and Chloroform: their Employment in Surgery, Dentistry, Midwifery, Therapeutics, &c.* By J. B. FLAGG, M.D., Surgeon-Dentist, Member of the Rhode Island Medical Society.—*Philadelphia*, 1851. pp. 189.
6. *On the Inhalation of the Vapour of Ether in Surgical Operations; containing a Description of the various stages of Etherization.* By JOHN SNOW, M.D.—*London*, 1847. 8vo, pp. 88.

THE time appears to have come, when, from the accumulation of facts and the lessons of experience, we may be enabled to arrive at satisfactory conclusions generally, with regard to the whole subject of Anæsthesia. Since the discovery of the anæsthetic powers of ether, a prodigious amount of labour has been devoted to the investigation of the properties of this substance and of the other anæsthetic agents which have been since discovered, to the best modes of administering them, to their physiological and toxicological action, and to the still disputed questions connected with their use in midwifery. It is not possible here to review all the works of importance which have appeared on Anæsthesia, still less the host of essays and pamphlets, many of them of a controversial character; but we have placed at the head of this article the titles of such as appeared of most importance; and many others will incidentally come under our notice.

The work which stands first is an American reprint of the various publications on this subject which have issued from the pen of Professor Simpson. The second work contains a very complete account of the various questions connected with the practice of Anæsthesia in midwifery. The paper of Mr. Nunneley contains the fullest investigation which has been published, of the physiological properties of the various anæsthetic agents. The work of M. Bouisson is the most complete treatise on the whole subject that has yet appeared. The little work of Dr. Flagg is valuable as showing the present state of opinion in America, with regard to the comparative merits and disadvantages of ether and chloroform; for it appears that many of our trans-atlantic brethren have returned to the use of the former substance. And the various publications of Dr. Snow, while containing a large amount of information upon the whole subject, treat most fully of the instrumental means of applying anæsthetic agents.

We shall divide our present article into three parts:—We propose, 1st, to speak of the history of anæsthesia; for there are several rectifications yet to be made; 2ndly, we shall treat of the physiological and toxicological action of anæsthetic agents; and, in the 3rd place, of their natural history and comparative merits. We shall devote a subsequent article to the discussion of the purely practical matters connected with the subject. Meanwhile, in the present article, many practical points will necessarily be alluded to.

1. The history of great discoveries exhibits a kind of uniform outline. Minerva does not, except in fable, spring at once from the head of Jupiter. One human being has rarely in himself sufficient power both to excogitate a great principle, and to carry it out in its details and practical applications. We almost invariably find two classes of persons engaged in these cases;—those who originate the ideas, and those who make them useful; and as mankind only pay in general for actual value received, the latter class are sure to receive the more substantial reward. History, therefore, should embalm the memory of the former.

The practice of Anæsthesia appears to have existed in very remote times. The root of the mandrake, to which so many extraordinary properties were ascribed by the ancients, is distinctly stated by Dioscorides and Pliny to have the power of throwing patients into such a deep sleep, as to render them insensible to the pain of surgical operations. In the middle ages, the vapours of somniferous plants appear to have been used for the like purpose. A secret agent was used to render a king of Poland insensible during a surgical operation, towards the close of the seventeenth century.* But the art of inducing anæsthesia had so completely fallen into desuetude, that the announcement of the application of vapours for this purpose, as recently made from America, deserves the entire merit of a discovery. It is true, that it was known how compression of a limb may induce insensibility below the part compressed; and insensibility to pain during the mesmeric state had been much canvassed. It was also

* See Dr. Simpson's paper 'On the History of Anæsthetic Agents,' read before the Medico-Chirurgical Society of Edinburgh, (Edinburgh Monthly Journal of Medical Science, for 1847, p. 451,) and Dr. Silvester 'On the Administration of Anæsthetics in Former Times,' (Medical Gazette, for 1848, vol. vi. p. 513.) The King of Poland referred to must have been Augustus the Second, the rival of Leczinski and enemy of Charles the Twelfth; and we have read that this prince had really lost part of his foot by some disease or operation.

generally known that little sensibility to pain existed during intoxication.

Sir Humphry Davy, about the beginning of the present century, has the credit of having been the first to propose the application of nitrous oxide gas to the removal of pain in surgical operations. "As nitrous oxide, in its extensive operation, appears capable of destroying physical pain, it may probably be used with advantage in surgical operations, in which no great effusion of blood takes place."*

The idea of Davy was not put into practice until the year 1844. In the autumn of that year, the late Mr. Horace Wells, of Connecticut, was led to use nitrous oxide gas as an anæsthetic agent, not, according to his own account, from the expressed idea of Davy, but from analogical inference from the effects of great excitement in producing insensibility, from what has been observed in drunkenness, and so forth. He says,

"I accordingly procured some nitrous oxide gas, resolving to make the first experiment on myself, by having a tooth extracted, which was done without any painful sensation. I then performed the same operation for twelve or fourteen others, with the like results. This was in the fall of 1844. Being a resident of Hartford, Connecticut, I proceeded to Boston, in December of the same year, in order to present my discovery to the medical faculty; first making it known to Drs. Warren, Hayward, Jackson, and Morton; the last two of whom expressed themselves in the disbelief that surgical operations could be performed without pain,—both admitting that this *modus operandi* was quite new to them, and these are the individuals who now claim my discovery."†

Mr. Wells states, that he remained in Boston several days, in order to administer the gas in a case where an arm was to be removed; but the operation was postponed. The only public exhibition of the agent which he seems to have made, was in a case of tooth-drawing, where he was not successful, owing, as he says, to the boy being removed too soon. So far, the statement of Mr. Wells is supported by undeniable evidence.

Drs. Jackson and Morton, of Boston, had been both cognizant of the researches of Wells. Morton had received the information direct from Wells, whose account is, that the former having gone to Jackson to get some gas prepared, received the ether instead, as less troublesome to prepare, or rather at hand. As far as we can judge, from the report on the respective claims of Morton and Jackson, laid before the American House of Representatives, Feb. 23, 1849, and from that of the Massachusetts General Hospital at the annual meeting, Jan. 1848, Morton had several conversations on the subject with Jackson, after the idea was suggested by Wells. Whether Jackson or Morton first proposed to employ the ether is uncertain. Jackson, however, seems to have considered it hazardous, and of doubtful utility.‡ Morton, after trying the ether on himself, first used it in the extraction of a tooth with success on the 30th September, 1846. After this, and other successful applications of the kind, the first surgical operation of a general character—viz., removal of a tumour under the influence of ether—was performed at Mr. Morton's

* Remarks on Nitrous Oxide.

† A History of the Application of Nitrous Oxide Gas, Ether, and other Vapours, to Surgical Operations, by Horace Wells, Hartford, U.S., 1847.

‡ An Address delivered before the Students at the Castleton Medical College, on the History of the Original Application of Anæsthetic Agents, May 17th, 1848, by E. Smilie, M.D., Boston, 1848.

suggestion at the Massachusetts General Hospital. As to Dr. Jackson's merits, the following is decisive:

"But to settle the whole matter, and it might have been done in the outset, to the satisfaction of every candid mind, after Dr. Morton began to use the ether in his practice, and for some weeks, it is well known to a large number of our most respectable citizens, that Dr. Jackson clearly and distinctly washed his hands of the whole thing. He, on many occasions, as is well known to his friends, disclaimed all connexion with the discovery or use of ether in surgery."*

The following extract from the Report of the trustees of the Massachusetts General Hospital, quoted also by the Committee of Congress, is very decisive :—

"1st. Dr. Jackson does not seem at any time to have made any discovery with regard to ether, which was not in print in Great Britain some years before." [That is to say, he was aware of the soothing effect of ether-vapour when inhaled in some chest affections.]

"2nd. Dr. Morton, in 1846, discovered the fact, before unknown, that ether would prevent the pain of surgical operations, and that it might be given in sufficient quantity to effect this purpose without danger to life. He first established these facts by numerous operations on teeth, and afterwards induced the surgeons of the hospital to demonstrate its general applicability and importance in surgical operations.

"3rd. Dr. Jackson appears to have had the belief that a power in ether to prevent pain in surgical operations would be discovered. He advised various parties to attempt the discovery; but neither he nor they took any measures to that end, and the world remained in entire ignorance of both the power and the safety of ether, until Dr. Morton made his experiments.

"4th. The whole agency of Dr. Jackson appears to consist only in his having made certain suggestions which led or aided Dr. Morton to make the discovery; a discovery which had for some time been the object of his labour and researches."

It is clear that no vague idea of the soothing properties of ether, when inhaled in asthma or bronchitis, should be put in the scale. In the works on toxicology, as in those of Orfila and Christison, indications of the insensibility of animals when submitted to ether may be found. The inhalation of ether, too, as a remedial measure, was distinctly stated in treatises on Materia Medica, as for instance in the *Dictionnaire de Matière Médicale et de Thérapeutique* of Merat and Delens in 1831. Richard Pearson, of Birmingham, had prescribed ether as a remedy in phthisis, either alone or with extract of conium, in the way of inhalation, about the year 1795; and Beddoes, in his work on Factitious Airs, published some observations of Thornton, in which ether-vapour was found beneficial, not merely in phthisis, but was stated to be efficacious in preventing the pain of an inflamed mamma. According to Mr. Robinson, the late Dr. Woolcombe, of Plymouth, had been in the habit of causing his patients to inhale ether as a sedative and anæsthetic in pulmonary complaints.†

A work by Chambert "*Sur les Effets Physiologiques et Thérapeutiques des Ethers*," quoted in an American journal before us,‡ describes the experiments of a M. Dauriol, in 1832. This experimentalist had tried inhalations with ether containing extracts of conium, henbane, or stramonium, on animals, and states distinctly that they were thus made insen-

* Some Account of the Letheon, by Edward Warren, Boston, 1848.
† A Treatise on the Inhalation of the Vapour of Ether, by J. Robinson. London, 1847.
‡ Littell's Living Age, March 18, 1848.

sible to operations. No knowledge, then, of merely general facts, such as he may well have possessed, is sufficient to give to Jackson the credit which he has so generally received in regard to the ether discovery. But Dr. Jackson, who seems to have a wonderful name as a scientific man in America, sent a letter to the Academy of Sciences of Paris, claiming the discovery as his own, without making mention of any one else, except in the most subordinate degree. A sealed letter from Jackson, dated Boston, November 13th, 1846, was opened on the 18th of January, by M. Elie de Beaumont, before the Institute. In this communication he says, "I ask permission to communicate through you a discovery which I have made, and which I believe important, for the relief of suffering humanity."* The consequence is, that, on the Continent at least, Jackson has got the undeserved credit of having made a discovery which he at first ridiculed. One French writer has proposed that a world's testimonial should be got up to the "illustrious Jackson!"† But it seems very doubtful whether another party is not entitled to the merit of having first used the vapour of ether to relieve pain in surgical operations on the human subject. This is the Dr. Smilie to whose paper we have already referred. This gentleman, while residing at Derry, N. H., before 1844, used inhalations of an ethereal tincture of opium to relieve irritation of the bronchi, and in the spring of 1844 he operated on a patient under the anæsthetic influence of these inhalations, having opened a sinous abscess without the least pain being caused. He however laid aside the practice, from fear, as he says, of dangerous consequences.

After the performance of the first operations, the use of ether rapidly spread in America. The first intimation of the discovery was conveyed to England in a letter from Dr. Bigelow, of Boston, to Dr. Boott, of London, dated November 28th, accompanied by a report containing an account of several important operations, with such comments as the early experience seemed to warrant. On the 19th December, the agent was used by Mr. Robinson in a case of tooth-drawing; and on the 21st, by Mr. Liston, with complete success, in two cases, one of amputation of the thigh, and another of removal of the great toe-nail. And the rapid spread of the practice of anæsthesia in England showed how willing the majority of medical men, in the present day at least, are to adopt improvements calculated to relieve human suffering. In France the ether discovery was applied to practice before the communication of Jackson had been made public. Information of the success of Liston soon penetrated there; and the discovery, says M. Bouisson, with true French gratulation, "received the glorious extension which useful truth finds in this country of science and enthusiasm." M. Malgaigne was the first in France to apply anæsthesia in practice. He communicated some satisfactory results to the Academy of Medicine on the 12th January, 1847. The further progress of the *currus triumphalis* of ether does not come under our scope at present; nor shall we here speak of the various kinds of apparatus that were invented to facilitate the inhalation.

* Bouisson, p. 49.

† De l'Insensibilité produite par le Chloroform et par l'Ether, par le Dr. Sedillot. Paris et Londres, 1848, p. 25. Dr. C. T. Jackson, the claimant of the ether discovery, must be distinguished from Dr. J. Jackson, also of Boston.

But soon a cloud began to appear on the hitherto brilliant horizon. Experiments on animals showed that etherization continued beyond a certain point, might terminate in death; and it was not long before cases occurred in the human subject, in which death, although not immediate, might be attributed to the use of ether. M. Jobert, at the sitting of the Academy of Medicine, of February 16th, stated that in two cases in his wards where death had occurred after operations, he was inclined to believe the ether not unconnected with the fatal result. In the 'Medical Gazette' of March 5th, there is a case of lithotomy related by Mr. Nunn, where also the ether was supposed by the operator to have something to do with the death of the patient.

The next stage in the progress of anæsthesia is an important one; we allude to the application of it to midwifery. The merit of this is entirely due to Professor Simpson, of Edinburgh, who observed, about the beginning of February, that the uterine contractions were not suspended under the influence of ether; and very shortly afterwards proposed the use of anæsthesia generally in midwifery.

A more decided case of fatal etherization was about to occur. On the 9th of March, at Grantham, a Mrs. Parkinson had a tumour removed while in a state of partial anæsthesia. She died two days after the operation; and a coroner's jury returned a verdict of "died from the effects of ether." The *post-mortem* appearances were not very decided—fluidity of the blood, and slight congestion of the lungs and brain, being the principal appearances observed. One or two similar cases, where death took place some time after the use of ether, are recorded; but we are not aware of any cases of *instant* death from the use of this agent, similar to what have occurred from chloroform, if we except one which occurred at Auxerre, in August, 1847. A man aged 55, operated on for cancer, was rendered quite insensible by ether, when suddenly, just as the operation had commenced, the face turned violet and then livid; the pulse became low, and then ceased to beat; and he was found to be quite dead in ten minutes from the commencement of the inhalation. The *post-mortem* appearances were chiefly congestion of the posterior portions of the lungs with frothy mucus in the air-tubes, which were stained of a dark colour, and darkness and fluidity of the blood. A case occurred in America, where phrenitis was alleged to be produced; and one in England, where alternate fits of delirium and depression, and death in three hours, were ascribed to the use of ether; and numerous after-consequences of an unpleasant and dangerous character, as convulsions, severe headaches, &c., began to be noted as among the occasional effects.

Whilst considerable controversy still existed with regard to the merits and dangers of ether, and especially concerning the application to midwifery, the world was startled by the announcement of a new agent, to which was ascribed both superior power and an almost harmless character. This was Chloroform.

It may be premised that after the first introduction of sulphuric ether, numerous experiments were made, principally on animals, and especially in France, with other allied bodies; as muriatic, nitric, acetic, and formic ethers, &c. These and other bodies had all been experimented on at a very early period, and found to possess more or less of the general

anæsthetic properties, without offering any specific advantages. On the 8th of March, 1847, M. Flourens, in a communication to the Academy of Sciences, related some experiments made by him on animals with the vapour of chloroform.

"It was especially by the observation of the effects of muriatic ether, that he was led to try the new body known by the name of chloroform. At the end of some minutes, of six in a first experiment, and of four in two others, the animals submitted to the operation were distinctly *etherized.* The spinal cord was then exposed; the posterior roots of the spinal nerves were found to be insensible; of five anterior roots only two preserved their motive power, the three others had lost it. Thus it is incontestable that the discovery of the anæsthetic properties of chloroform is due to a French philosopher . . . . Unfortunately he confined his operations to animals."*

After the proposition to employ chloroform by Professor Simpson, a kind of claim was set up in favour of Mr. Jacob Bell, who used the agent, according to the 'Medical Gazette,' as far back as 1847, under the name of chloric ether. But it is by no means clear what the substance formerly called chloric ether was. The name appears to have been given to muriatic ether, to the Dutch liquid, and to an impure solution of chloroform in alcohol. Dr. Hayward has recently stated that what is called chloric ether in America consists of one part of pure chloroform and nine parts of alcohol.

Thus the entire merit of applying chloroform in the human subject should rest with Dr. Simpson. The physiological properties of this body given in the liquid form, had been described by Dr. Glover from experiments on animals as early as 1842; its analogies with the other bodies, which, like it, have been since found to possess anæsthetic properties, pointed out, and the remarkable effects produced by the whole group much insisted on.† With this exception and the experiments of M. Flourens, chloroform may be considered almost unknown till Dr. Simpson issued his first publication on the subject, dated 15th of November, 1847.‡ From this it appears that Dr. Simpson had long entertained the idea of finding a substitute for ether superior to the original substance, that he had made inquiries of several chemists what fluids might be sufficiently volatile to be respirable, and that Mr. Waldie first suggested chloroform, while Dr. Gregory recommended the Dutch liquid. After some trials on himself and friends, Dr. Simpson applied the new agent in surgical operations, and in midwifery, and became satisfied of its superiority to ether, as he states in his first pamphlet—"1st, because less chloroform is required; 2nd, because its action is more complete and persistent; 3rd, because it is more agreeable; 4th, because it is less expensive, and more portable, and does not require any apparatus for its application."

The substitute for ether was as rapidly made to replace its forerunner, as this itself had been brought into use; and all for a time was again *couleur de rose;* when a fatal accident occurred, which gave rise to much discussion. We allude to the well-known case of Hannah Greener, which

* Bouisson, op. cit., p. 77.

† 'On the Properties of Bromine,' &c., (Edinburgh Medical and Surgical Journal, for July and October, 1842.) See section on the Chloride and Bromide of Olefiant Gas, Chloroform, Bromoform, and Iodoform.

‡ On a New Anæsthetic Agent.

occurred in the practice of Dr. Meggison, at Whickham, near Newcastle.* Sir John Fife and Dr. Glover, who were employed by the coroner to examine the body and report on the case, attributed the death of the deceased to the chloroform used; and the jury returned a verdict accordingly. Professor Simpson maintained at the time, that the death was really caused by the injudicious forcing of brandy-and-water into the mouth when the girl was unable to swallow; that, in fact, she was choked or drowned, and not poisoned. But the majority of medical men evidently took a different view; and it has since been shown that the morbid appearances in the case, although very similar to those of ordinary asphyxia in many respects, were yet in others decidedly more like what are found in animals, and have since been found in human beings, undoubtedly poisoned by chloroform. And unfortunately the fatal cases which have since occurred, and are sufficiently numerous in themselves, however small a proportion they may bear to the vast number of patients who have been put under the influence of chloroform, can hardly leave any doubt that this girl was really poisoned by chloroform, not unduly or improperly administered.

These fatal cases of death from chloroform, recorded and established on reasonable grounds, seem to us to be twenty-three in number: we exclude cases of determined suicide, like a recent case at Vienna, or cases of death from the excessive use of chloroform, like that of the druggist's apprentice at Aberdeen, or from the undue use of the agent, as in the case of Dr. Adams, and others, as they only show the danger which may arise from the excessive use of chloroform, and the impropriety of taking it habitually for purposes of intoxication—positions which nobody doubts; and we confine the enumeration of these deaths solely to patients who may be believed on *reasonable grounds* to have died from the fair and legitimate employment of the agent.

It may be stated that no list of the number of deaths up to the present time has been published; and that some deaths are recorded in certain journals, and omitted in others. Dr. John Warren, of Boston, United States,† and M. Bouisson, have published lists up to the date of their works, and which, although pretty full, do not exactly correspond; the following is however, we believe, a pretty correct enumeration. For the sake of brevity, we insert little more than the particulars required to verify the facts, and omit the *post-mortem* appearances, which, as we shall afterwards see, have a general correspondence. The cases are in the order of their occurrence up to the present time.

1. Hannah Greener, aged fifteen, was greatly afraid of respiring the chloroform,—only about two drachms were used,—insensibility was produced in half a minute, when the removal of a great toe-nail was commenced; death occurred in about two minutes.

2. Case at Cincinnati, aged thirty-five, apparently in good health; was chloroformed for about a minute for extraction of a tooth; death occurred in from five to ten minutes.

3. Patrick Coyle, chloroformed for fistula; he inhaled for about a minute, and almost instantly expired.‡

* January 28, 1848.

† Effects of Chloroform and of Strong Chloric Ether as Narcotic Agents, Boston, 1849.

‡ The case of Coyle is given by Dr. Warren alone.

4. Mdlle. Stock, Boulogne, aged thirty, subject to palpitation, and chlorotic; inhaled about fifteen or twenty drops; operation, opening of an abscess in hip; death almost instantaneous.

5. Daniel Schlyg, aged twenty-four, had a thigh fractured by a ball during the days of June, in Paris, and was in a state of profound depression; he inhaled the chloroform for about three minutes, amputation at the hip-joint was performed, and death occurred in three quarters of an hour.

6. Walter Badger, aged twenty-three, had heart and liver disease; he inhaled chloroform for about a minute, previous to the extraction of a tooth; the operator, Mr. Robinson, of London, was absent for less than a minute to seek more chloroform, and found his patient dead on his return. The inhalation took place from an apparatus.

7. A young woman of Hyderabad was chloroformed for amputation of middle finger of left hand; about a drachm of chloroform was used; death almost instantaneous.

8. John Griffith (Warren) had chancres and hæmorrhoids; inhaled about three drachms, and died in about ten minutes, during the excision of the hæmorrhoids.

9. Abbey Pennock (Warren) inhaled about three drachms in two applications, to relieve the pain of toothache, and died almost immediately after the second application.

10. Example by M. Malgaigne. A man, one of the wounded of the three days of June, had the humerus broken by a ball, and was a good deal weakened by hæmorrhage and gangrene of the wound; was chloroformed, and the humerus disarticulated; new inhalations were taken during the search after the ball, and he died during the last incision.*

11. Charles Denoyers, aged twenty two, was affected with white swellings of the left wrist, and chloroformed at the Hotel Dieu, of Lyons, for five minutes before and during cauterization of the tumours; death took place shortly after the commencement of the operation.

12. Case of M. Roux; removal of a scirrhous breast; death before quitting the amphitheatre.

13. Case described by M. Guérin, as having taken place at the Bicêtre. A man affected with a lesion of the thigh was chloroformed for amputation at the hip-joint; death took place before the end of the operation.

14. Case at Govan; a young man chloroformed for removal of a great toe-nail; death almost immediate.

15. Case of M. Barrier, of Lyons; a young man, named Verrier, aged seventeen, inhaled from six to eight *grammes* of chloroform for about six minutes, for an amputation of the finger, and died in about half a minute afterwards.

16. At the Hospital of Madrid, (Bouisson,) a child of twelve years being chloroformed for an amputation of the leg, was seized with a tetanic spasm, and died in a minute and a half.

17. By M. de Confevron, of Langres. Madame Labrune, aged thirty-three, of a nervous temperament, was chloroformed for the extraction of a tooth; almost instant death.

* Dr. Warren gives 10 fatal cases, but inserts the cases which we have omitted for the reasons above given. This last case is our 9th; and our 11th is the 7th of M. Bouisson, who, like us, excludes suicidal cases, and such like. He gives 15 cases; his last is our 17th.

18. Mr. Solly's case; a man, aged forty-eight, apparently in perfect health, inhaled rather more than a drachm of chloroform from an inhaler for about three minutes; then a toe-nail was removed; and death took place in about six or seven minutes from the beginning.

19. Case at Leeds, Robert Mitchell; chloroform applied during an attack of delirium tremens by Mr. Teale; death about an hour afterwards. (Hardly a clear case.)

20. Case at Shrewsbury; a girl named Jones; only a small quantity of chloroform was given before proposed extirpation of the eyeball; almost instant death, as if from apoplexy.

21. Case at Berlin; a young lady died during the extraction of a tooth, from the alleged effects of about half a drachm of chloroform.

22. Case at Guy's Hospital, referred to in 'Med. Gaz.' for June, 1850.

23. Aschendorf's case; a child of one year old operated on for a nævus under the influence of chloroform; only about nine drops were used; death on removal from the table.

These cases are independent of at least five well-authenticated deaths from needless and careless use of chloroform in this country alone, of one case of determined suicide at Vienna, of numerous cases, where dangerous symptoms have been observed, or death obscurely traced, and perhaps of some unrecorded death during surgical operations.

Two modes of applying anæsthetic vapours otherwise than by the lungs have been proposed; the 1st is the rectal method; the 2nd, local anæsthesia. M. Pirogoff, of St. Petersburgh, first proposed the rectal method in Europe; but he had been anticipated by Mr. Crawford in India,* who had ascertained that in this way animals could be affected; and the phenomena of etherization by the ordinary way produced. M. Pirogoff proposed to administer the vapour in this way by means of a syringe included in a tin vessel filled with hot water, so as to volatilize the agent. "The advantages of rectal inhalation are, according to M. Pirogoff—1st, that it does not in any way affect the respiratory organs; 2nd, that it renders the etherization altogether independent of the will of the patient; 3rd, that it gives rise to no excessive excitement, and determines an *etherism* much more difficult to extinguish, and consequently more favourable in long and severe operations."† And he maintains that this mode is peculiarly useful in spasmodic affections of the intestines; and in some diseases of the urinary and genital organs. It seems to us indeed reasonable to suppose, that in some of the class of affections indicated, the rectal method may be found beneficial; although further experience has removed all probability of its ever being used generally in surgical operations, &c.

Mr. Nunneley has the merit of having first pointed out the local physiological effects of anæsthetic agents, at the Leeds meeting of the Provincial Medical Association in June 1848.

2. *Of the Physiological and Toxicological Action of Anæsthetic Agents.*—Here we must be understood to speak of the principal anæsthetic agents; as some bodies have been added in particular works to the group, which differ very widely in their physiological properties from ether and chloro-

* Medical Gazette, vol. iv. for 1849. † Chambers, op. cit. p. 242.

form. The external phenomena of anæsthesia and local anæsthesia, and internal experience of patients who have been etherized (*a*); the effect on particular systems, organs, and functions (*b*); the *post-mortem* appearances and toxicological action (*c*); and lastly, the *modus operandi* (*d*) may be considered.

(*a.*) The external phenomena produced by etherization* do not differ materially in the lower animals from what are observed in man, making allowance for the lower degrees of intelligence in the animals. In man we observe that at first there is for the most part a little cough, with expectoration of mucus and flow of saliva, and some impediment of the respiration; that then the respirations take place without difficulty, and become strong and deep; the pulse becomes quickened and the eyes injected. With these early symptoms there are often irregular movements of the limbs, and expressions of various kinds are uttered; sometimes a patient will try to put away the sponge or instrument used, but more generally he is anxious to retain it. If the inhalation goes on, the face generally becomes distended, the eyes brilliant, and turn in different directions, often upwards; soon the eyelids droop; very often now there are laughter and incoherent expres ions; the pulse begins to be slower, and a general insensibility, with muscular relaxation, follows. This is the true surgical period of anæsthesia. If this period be surpassed, and the etherization be pushed to its utmost limits, the respirations become stertorous, the face livid, the pulse slow and weak, and death may take place. To sum up; temporary excitement, then stupefaction or disorder of the intellectual powers, insensibility and death, are the three great visible and observable stages of etherization pushed to its utmost limits.

The periods occupied by the first two stages may vary in every case. Almost instant insensibility may be produced, especially by chloroform, which agent has also, in some rare cases, occasioned instantaneous death. The insensibility may last for from five to ten minutes, or a much longer period, and may be prolonged by a skilful management of the inhalation for hours. The symptoms are often far from succeeding each other in regular rotation. Tetanic convulsions are sometimes observed. Varieties are less common in the lower animals than in man, owing doubtless to their simpler intelligence. Escape of urine and fæces sometimes takes place towards the latter periods of etherization, and is very common in animals poisoned by chloroform vapour. Frothing at the mouth is also observed in similar cases, and whining and other expressions of pain at the beginning.

The general experience of individuals under the influence of etherization, putting aside exceptional cases, is really very simple. At first there is irritation of the mouth and air-passages; then a sense of heat in the chest, optical illusions, flashes of light, ringing in the ears, &c.; these are followed by an irresistible desire of repose. Finally, there is total loss of sensation and suspension of the intellectual powers, or sometimes a dreamy state with certain hallucinations.

The fact that during an operation the patient often exhibits signs of feeling, as wincing or calling out at the use of the knife, and afterwards declares that he has felt nothing, has led to the discussion of a curious and

* Etherization is used as the more general term.

interesting question—namely, whether actual pain is felt and afterwards forgotten, or to what the phenomena alluded to are to be ascribed. This question has been ably discussed by M. Bouisson. He observes, that it has "naturally arisen from the kind of contradiction which exists in certain patients, between the symptoms of pain observed, and the denial of any such sensation by the patients themselves afterwards." He establishes the following categories, under which he ranges etherized patients.

1. Those who suffer and preserve their intelligence; 2. Those who lose all sensibility with their intelligence; 3. Those who preserve their intelligence without suffering; 4. Those who in spite of the loss of their intelligence appear to suffer.

"The patients of the first class are those who have scarcely respired the anæsthetic vapours, or have improperly respired them, or in whom the slowness of the effect leads us to suppose a peculiarity opposed to the influence of the agent; or perhaps the operation has been commenced too soon, and the ether has only had time to exercise its primary action on the organism; it has produced excitement, but has not destroyed the ordinary relations between the sensibility and intelligence" . . . . the former "has been rather exalted than abated."

The patients of the second category are in a better condition. They enjoy the benefit which the surgeon always seeks to obtain, and this benefit they owe to the temporary but complete loss of sensibility and intelligence.

"The state in which the patients are plunged is that which is to be desired for the use of surgical applications. In the patients of the preceding category, pain is experienced on account of the exalted impressionability of the organism; in the others it is annulled to the wished degree, especially when to the extinction of the psychological sensibility is joined that of the proper sensibility of the nervous centres whence the reflex power emanates."

Some remarkable facts have been observed with regard to the third class of M. Bouisson. Persons have preserved their intelligence, and been aware of what was going on around them, although totally insensible to pain. The following curious experiment, related by Dr. Flagg, may be quoted here as bearing on this part of the subject:

"It had so frequently happened in my practice that my patients had expressed to me their utter impossibility of doing any way different from what I directed them to do; as if, for example, they attempted to get out of the chair, my saying, 'Sit still' to them, was a law too potent for them to think of breaking; or if they inclined to laugh, my suggestion of 'perfect quiet and calmness' was as instantly obeyed; it occurred to me to propose the following experiment. One of the ladies was to administer the ether to this gentleman, with the express understanding that he was to obey any directions she might give him under its influence, if by possibility he could do so. The ether was inhaled sufficiently to secure a painless operation had it been necessary to extract a tooth; and the lady immediately asked him, 'What o'clock is it?' His hand was carried to his vest pocket to remove his watch; he hesitated a moment, and then carried his other hand to the same place. He soon changed his hands very rapidly in the vicinity of his watch, but did not take it out. Whilst doing so, the lady requested him to *sing*. The gentleman was known to possess considerable musical powers, but the noise he made resembled more the howling of a wolf than anything else I could liken it to. He then whistled almost as much of a tune as *he sang*, but soon stopped; and, getting up, made castanets of his fingers, waltzed around his chair, and resumed his seat. The ether passed off. The gentleman put his hand to his head and exclaimed, 'Oh, heavenly!'

"When entirely restored to himself, he gave this explanation. He knew everything that had transpired. When the lady asked him 'the time,' he was not prepared for the question, and felt somewhat confused, but recollected that he was to obey this lady. In feeling for his watch, it occurred to him as he could not see it would be folly in him to take it out, and he moved his hands about to indicate that fully. When requested to sing, he was equally taken by surprise, if not more so than at first; for he was not quite clear in his mind that he had complied with the lady's first requirement, or that she fully understood him. Still he felt a binding obligation to do his best. *What to sing* became now a serious question with him, and the noise he made was trying his voice. He was aware that there was no regularity or gradation of sounds, but could not help it. In his anxiety to obey, and finding he could not sing, he attempted to whistle. Here he was as much at a loss for a *tune* as at first, when a lucky expedient presented itself: 'If I dance and keep time,' he thought, 'she must be satisfied that I have done all in the way of music it is possible for me to do.' And this gentleman attributes a large share of his happiness to being able to accomplish the dance. He likewise asserts that he knew of no one being present but the lady who administered the ether."*

As regards the fourth class of cases, M. Bouisson observes:

"In other individuals etherized and submitted to surgical operations, the phenomena are differently manifested. The intelligence is plunged in torpor, the sensibility appears benumbed, when only superficial stimulants are applied; but during the course of a surgical operation it seems to awaken, sometimes with the appearance of a greater vivacity, sometimes preserving its ordinary expression. . . . .We should believe in the suffering of the patient, but when all is over and he has recovered his intelligence, and is interrogated, he is astonished at having undergone the operation."

A few days ago we were present at the excision of a mamma where we applied chloroform. The patient gave a loud scream at the first incision, but afterwards lay perfectly still and insensible, and on recovering her senses, declared that she had felt nothing whatever during the operation. Some parties, as MM. Blandin and Longet, have maintained, that in such cases the patient only forgets the sensation of pain which he has really experienced, and that this is owing to the enfeebled state of the intelligence, which renders it unable to retain the impression which it at the moment actually received. Many of the phenomena of dreams have been quoted in support of this view. We often at the moment of waking have the greatest difficulty in putting together the odd and sometimes painful impressions which we have received during sleep, and after a few instants can render no account of them. On the other hand, it may be observed, that etherized patients can generally give an account of any dreams they may have had; and if of their dreams, why not of any feelings of pain, if such had been really experienced? M. Bouisson sums up as follows: that those patients "who declare that they have not suffered pain in any way, either in fact or in a dream, have really been exempt from pain in the ordinary sense of the word." If we consider that the complete exercise of sensibility in man (perception) requires an act of the intellect, that the perception of the sensation implies the conjunction of the sentient and understanding entity; that the action, as it were, of the human soul can be suspended in given circumstances and conditions, and that etherization can effect this most powerfully; that the impossibility of perceiving is a

* There is some resemblance between these phenomena and those described by electro-biologists.

necessary consequence of the state of torpor which exists, and that to forget presupposes the sensation of pain to have been once felt;—upon all these grounds he maintains that the phenomena which appear to indicate pain in the cases referred to are really of a reflex character. In our opinion this conclusion is not quite sufficiently warranted. It may be true of the mere writhings and convulsions which we often see, but not of the more reasonable expressions of pain, so to speak, which are occasionally uttered.

Patients under the influence of anæsthetic agents sometimes exhibit movements, or utter expressions indicative of erotic sensations, and lascivious dreams are occasionally experienced; but phenomena of this kind are, on the whole, of rare occurrence.

The production of local anæsthesia is an idea, as Dr. Simpson has shown, by no means confined to modern times.* Recently the application of cold for this purpose has been a good deal spoken of. In the human subject, or in the higher animals, but little effect can be produced by the local application of anæsthetic vapours, or the fluids from which they are disengaged, if we except the local effects produced by such agents as prussic acid, which we would carefully distinguish from the true anæsthetic substances. Nor does it appear that local anæsthesia in the human subject is capable of any considerable practical application. Mr. Nunneley, indeed, before operating on a difficult case of artificial pupil, applied the vapour of chloroform to the eye, for about twenty minutes, and succeeded in rendering the parts nearly insensible. But it is evident that the local effects are too slight to realize any important practical application. Dr. Simpson has obtained some results from the local use of chloroform in particular, which tend to throw light on the general action of anæsthetic agents. The creatures in which the local action was most remarkably shown, were annelida, crustacea, and fishes:

"Nothing could be more curious and satisfactory than the experiments alluded to, on the production of local anæsthesia by the local application of chloroform vapour to different parts of the body of the earth-worm. The resulting degree of local anæsthesia in the part exposed is generally, in the course of two or three minutes, most complete, as regards both sensation and motion; in fact, after being sufficiently exposed, the chloroformed portion of the animal is quite flat and flaccid, does not move under any irritation, and can be doubled, and twisted up on itself, like a piece of loose wetted cord. If the part paralyzed by the chloroform be small, it will be dragged along by the movements of the other unaffected portions of the worm. Generally, in the course of a few minutes, it gradually regains its powers of motion, and its irritability and contraction under stimuli."

It should be observed, that the part of the animal exposed to the anæsthetic vapour is thrown, for a minute or two, into excessive and even violent excitement, manifested by its motions, before the local torpor and insensibility are induced. In centipedes, chloroform applied by a brush to the head, or some of the middle rings, or the tail, always anæsthetized and paralyzed the part to which the application had been made. Mr. Nunneley produced complete paralysis of the individual limbs of frogs and toads, by immersing them in the vapour of chloroform. He also rendered the limb of a rabbit sufficiently anæsthetic in the same way, to enable it

* Dr. Simpson on Local Anæsthesia, Provincial Medical and Surgical Journal, July 12, 1848.

to bear mutilation without pain. It is unnecessary to enter into a further detail of experiments of the kind. It is evident that the local action of anæsthetic agents is greater, as the nervous system of the animal experimented on is less highly developed; and when the nervous system of an animal is localized in a particular part, *to that part the action of the anæsthetic agent may be more strictly confined.*

(*b.*) Under this head may be noticed the more special action of anæsthetic agents on the nervous system, the respiration, and circulation, the animal temperature, and some of the functions of organic life.

According to the experiments of M. Serres, when the peripheral nerves, —as, for example, the sciatic nerve of a frog,—are exposed to the action of ether, the sensibility below the point to which the ether is directly applied is destroyed, while that of the parts above remains intact, precisely as in cases of destruction or section of the nerve at a given point; and this is accompanied with loss of contractility in the muscles supplied with nerves from the same point. This double result is attributed by M. Serres to the solvent action of the ether on the fatty principles of the nervous matter, and the consequent destruction of the organization on which the due performance of the functions depends. We cannot subscribe to this chemical explanation, especially after what we know of local anæsthesia. The effect takes place before there is time for the chemical action supposed. No doubt, the prolonged steeping of a nerve in ether will lead to some slight chemical action of the kind. M. Longet has determined two stages in this species of direct etherization. 1st, Where the insensibility to pain below the point exposed to the ether exists; but where the galvanic current directed from above may produce muscular contractions; and, 2ndly, where both sensibility and contractility are entirely destroyed. These effects, and the local anæsthesia just spoken of, are analogous to the effects produced by the local application of substances, like opium, belladonna, prussic acid, &c., which should be separated from the group of true anæsthetics.

The sensibility of the skin in anæsthesia appears to be lost in a part, in the inverse ratio of its natural sensibility to pain. It is said, that the skin over the temporal region retains its sensibility longest.

The effects of anæsthetic agents are sometimes so gradual, as to admit of our tracing their action successively on the different nervous centres; so that Longet, Flourens, and others, have compared the results thus obtained to those derived from vivisections, as a means of isolating the functions of the nervous system, and localizing them in particular organs. Thus, in the progress of anæsthesia we observe, first, disorder of the intellectual faculties; and then, in animals particularly, incapacity of sustaining themselves on their limbs, and other phenomena which may be ascribed to an action on the cerebellum, according to the views of M. Flourens with regard to the functions of that organ:

"However, if, before pushing the etherization further, we pinch them strongly in a sensible part of the body, they cry and are agitated, without however arousing themselves to oppose, in an efficacious manner, the violence to which they are subjected. M. Longet sees in this impotence a proof of the etherization of the cerebral lobes, and different parts of the encephalon, as the cerebellum, corpora quadrigemina, optic thalami, and corpora striata, while there is still integrity of the annular protuberance, and medulla oblongata." (Bouisson.)

If the etherization be continued after the suspension of the intellect, the derangement of the movements, and the loss of voluntary power, the sensibility is extinguished, and the reactions against pain—or rather against impressions which are calculated in ordinary circumstances to occasion pain—are only instinctive or reflex, until at last every kind of evidence of sensation disappears. The complete loss of sensibility is attributed to the etherization of the annular protuberance, and those parts connected with it, which are regarded as essentially instrumental in the perception of sensations—the perceptive centre, or centres, of general sensibility and tactile impressions. M. Longet performed the following experiment:—He took three rabbits or dogs, exposed their sciatic nerves, and concealed the bodies carefully from the eyes of the spectators. The nerves in the three were successively pinched and torn, and every time there was great excitement, and plaintive cries were heard from each of the animals. The unanimous opinion of all present was, that in these three cases there incontestably was pain. Now, of the three animals, one was etherized to the first degree (etherization of the cerebral lobes); the second retained no other part of its encephalon but the annular protuberance and medulla oblongata; and the third, except the wound in its thigh, was quite intact. After this, in the second, the protuberance was cut off; and although the animal continued to live and respire, it remained calm, and did not utter the least cry under the scalpel and the pincers. The conclusion to be drawn appears sufficiently obvious.

The spinal cord is more easily experimented on, than the parts of the nervous system within the cranium. M. Flourens has exposed the spinal cord in animals completely etherized, has cut and pinched both anterior and posterior roots, and even the spinal cord itself, without producing either signs of pain, or any motion whatever.

The part of the nervous system most essential to life is the medulla oblongata. It is closely connected with the nerves of the respiratory organs; and may be regarded as the *ultimum moriens* of the whole nervous system: and it is likewise the last part of this system to submit to the influence of anæsthetic agents. When it is at length paralyzed, death is inevitable. M. Flourens seems to have proved from experiments on animals that this is really the case.

If these views be correct, then, we may conclude the action of etherization on the different nervous centres to be successive and progressive; and that it goes first to the cerebral lobes and the cerebellum, then to the annular protuberance and centres of perception of common sensation, and afterwards to the spinal cord, and at length to the medulla oblongata. Thus the animal at first loses intelligence and the equilibrium of its motions; it then loses sensation and motion; and when it has lost these, if a strong action of the anæsthetic agent be continued, it may lose life as well.

In an able paper by Mr. Dunn, nearly the same view is taken of the progressive action of anæsthetic agents on the nervous centres, to that just given.* Adopting the opinion that the thalami, being in commissural connexion with the *posterior* segmental ganglia of the cord, are with these ganglia the great centres of tactile and common sensation, and that the

* Medical Gazette, Aug. 1851.

corpora quadrigemina "are the seat of simple emotional impulses readily excited into action by sight, and quite independent of the cerebrum," he endeavours to show that the paralysis, for a time, of these centres of common sensation and tactile and visual impressions, and emotions to which they give rise, is essential for the completion of anæsthesia.

But although the action on the nervous centres is generally progressive, it is not always so; and perhaps some of the cases of sudden death, or other untoward accidents of a less formidable character, may be owing to deviations from the regular course, arising from causes which at present we are unable to detect. In actual practice it is frequently impossible to trace the progressive development of the phenomena in a gradual manner; and this is especially true of the rapid anæsthesia often induced by chloroform.

The only other action on the nervous system to which we think it necessary to allude, is that which the vapours of ether or chloroform, when inhaled, have been supposed, on good analogical grounds, to exercise on the extreme fibres of the pneumogastric nerve in the lungs. The first feeling of heat and irritation is probably owing to the contact of the vapour with them, because the direct application of ether and chloroform to other nerves causes pain at first, and then paralysis; afterwards these nervous filaments may lose their power, and hence may result accumulation of mucus, imperfect hæmatosis, and cessation of the *besoin de respirer*.

The respiratory functions are considerably affected in anæsthesia. M. Bouisson has observed that the respirations, which, at the first, were twenty-two, became twenty-five at about the third minute, and nineteen at the sixth. And something like this appears to be generally the case—that is to say, the respirations, which at first are more rapid, in the end become slower. The expired air at first contains rather more carbonic acid than in the normal state; afterwards it contains less. This fact is in accordance with the views of Magnus and others, and the experiments of Edwards on the products of the respiration in animals obliged to respire pure hydrogen; and also with the now generally admitted theory, that the oxygen inspired does not effect its combinations in the lungs, but in the course of the circulation; so that, in fact, the inhaled chloroform or ether vapour absorbed into the blood expels the ready-formed carbonic acid, and at first it expels a larger quantity; but as the changes in the blood do not go on so rapidly afterwards, the quantity of carbonic acid in a short time undergoes diminution.

Auscultation is not without its possible use in some cases of etherization; the nature of the changes going on in the lung might thus be detected in suspicious cases.

The pulse, as has been said, is at first increased in frequency during etherization; and then it becomes slower. The same is true of its force. It has sometimes been observed to rise to 170 or 180, and to fall again to 60 or even 40. The heart's action, at first augmented, may afterwards become exceedingly enfeebled. There is reason to believe that several of the deaths from chloroform have arisen from a genuine syncope. The heart in animals poisoned by chloroform or ether vapour, has been found flaccid and empty, at other times distended with blood. M. Gosselin, in

some recent experiments, as Dr. Glover long ago, found that liquid chloroform injected into the jugular almost immediately paralyzed the heart. Although great pains have been taken to examine the state of the blood in etherization, but little has been determined, except, perhaps, that if the action of the agent be much prolonged, the arterial blood acquires a dark hue. (Researches of Amussat and others.)

The temperature, at first increased, soon shows a remarkable diminution. Thus, the external temperature being 16·5 of the centigrade, and the temperature of a dog under the influence of ether being 40° at the end of the first three minutes, it gradually sunk until death at the thirty-fifth minute, when it was only 37·5. In an experiment related by Dr. Glover, where the chloride of olefiant gas (Dutch liquid) was injected into the jugular of a dog, very remarkable lowering of the temperature was observed. "The respirations gradually became slower, the heart's action imperceptible; the animal heat declined rapidly; towards the third hour, a little before death, the temperature was 63° in the axilla, 72° in the groin, and 82° in the rectum, the temperature of the room being 42°."

Etherization has some slight effect upon the abdominal organs; M. Mandl has observed that in dogs profoundly etherized, the peristaltic action of the intestines is diminished. Vomiting is not a very uncommon effect either during or after etherization. Diarrhœa occasionally follows. The saliva is very often secreted in larger quantity; and abundant sweating sometimes occurs. The urinary secretion does not seem to be affected.

(*c.*) It may be inferred from the fatal cases recorded, that much uncertainty exists with regard to the action of anæsthetic agents on different individuals. In many of the fatal cases which have occurred, it is quite impossible to detect an adequate cause either in the quantity of the substance employed, the mode in which it was used, or the known condition of the patient. Are we reduced to use the word idiosyncrasy to cover our ignorance?

Some circumstances have been observed in individuals, which tend to modify the action. As a general rule, there may be assumed to be less danger in adults, than in the very young or the aged. Children are easily etherized, except when they resist much; in them there is less excitement, more profound anæsthesia, and greater relaxation of the limbs. We have, however, in practice met with great exceptions. Abatement of the animal heat, too, is remarkable in young subjects. Young animals of all kinds die more readily under the action of anæsthetics, than those of more advanced life. But children, almost newly born, have been successfully etherized. M. Guersant has a high opinion of the value of anæsthesia in children. In old men the effects of etherization are longer in passing off; and the danger of sanguineous congestion is greater. The action is more prompt in women than in men; and they are, as might be supposed, more subject to hysterical and erotic symptoms. Fatal accidents have been more common in them.

The effects of anæsthetic agents on animals are much more powerful in the lower animals than in man. Dr. Marshall Hall, some time ago, remarked that from the action of chloroform vapour on animals, no one could conceive it could be used with such impunity on the human subject. Can the habitual use of alcohol have any effect in rendering human beings less

liable to this action? It is certain that it is very difficult to etherize drunkards; and almost every human being, either personally or from inheritance, may be supposed to have some antidote-power of the kind.

If we take for a basis the *post-mortem* appearances observed in the first fatal case of poisoning by chloroform in the human subject, we shall find in the statements since recorded as to other cases of the kind, as well as in what has been found in animals poisoned by ether and chloroform, the strongest general resemblance. It is not necessary to quote the whole of the account of the *post-mortem* examination of Hannah Greener. The principal appearances were, great congestion of the lungs, which "were mottled with patches of a deep purple, bluish, or scarlet hue;" much bloody froth in the pulmonary tissue, reddening of the epiglottis, and general congestion of the air-passages, dark fluid blood in both sides of the heart, and some congestion of the membranes of the brain. In almost all the recorded cases of poisoning by chloroform we find similar appearances, except that in some cases the heart was flaccid and empty; and in some few, the lungs were not engorged.

Mr. Wakley states that in all his experiments, where the animal died, the heart's action continued for some time after cessation of the respiration. He thus sums up the results of his *post-mortem* examinations. After insisting on the dangerous consequences likely to ensue from the use of chloroform in such cases as that of the late Mr. Liston, where it had been tried without benefit, he says—"The examinations prove that blood almost black had collected in the heart and lungs, and the great vessels connected with these organs, to a degree of intensity which was probably never surpassed. Anything of the kind more striking, probably, was never witnessed in *post-mortem* examinations."* In several of Dr. Bennett's experiments on animals, the lungs were found quite healthy. "On examining the bodies of the pigeon, rabbit, and dog, thirty hours after death, it was found that the right cavities of the heart were distended with blood, which was firmly coagulated. The lungs and other organs were perfectly healthy." Nevertheless, he relates such facts as the following:

"A strong rabbit was killed by causing him to inhale two drachms of chloroform. On its first application he struggled a little; but speedily lost sensation. The inhalation was continued, and the respirations became accelerated. In two minutes there were slight spasms of the hind legs. There were convulsive heavings of the chest, which in thirty seconds more ceased. The heart's action, however, continued; but in three and a half minutes from the commencement of the experiment these also ceased. The animal was dead. On examining the body four hours afterwards, the right side of the heart was found inordinately distended with dark, firmly coagulated blood. Both lungs were intensely injected, and of a dark colour. The brain and other organs were healthy."

In another experiment on a rabbit, where a drachm of chloroform was injected down the throat, and had got, apparently, into the windpipe, "both lungs were dark coloured, condensed, exactly resembling the liver of the same animal, except at the margins, where a fringe of spongy lung surrounded the organ."†

In Mr. Nunneley's experiments with anæsthetic agents on animals, there was likewise found much congestion of the lungs in the greater number of cases.

* Lancet, Jan. 1, 1848.

† See his Report on the subject, Edinburgh Monthly Journal, Jan. 1848.

It is very extraordinary how this enormous congestion of the lungs can so suddenly arise. This, indeed, is one of the curious pathological phenomena, of which we have been made aware through the practice of anæsthesia.

From the symptoms and *post-mortem* appearances, then, it seems to us that death may take place in more ways than one, from the action of the chief anæsthetic agents. In some cases the fatal effect seems to arise from stoppage of the circulation through the lung, either through some action on the minute nervous filaments, or from constriction of the capillary vessels by the direct action of these agents; when, the heart's action continuing, the congestion and accumulation of blood in the right side of the heart might be accounted for. And *as the general rule, the heart's action continues, however feebly, after the respiration has ceased.** In other cases, the cause of death appears to be syncope — a sudden cessation of the heart's action. In the case of death which occurred at the Hôtel Dieu at Lyons, under M. Barrier, it is particularly noted that some respiration continued after the pulse ceased; and in this case the heart was found flaccid and empty, and the lungs, although of a dark colour, do not appear to have been much loaded with blood.

We know, also, that chloroform is capable both of congesting the lungs enormously, and, in some instances, of paralyzing the heart, when injected into the jugular. Under these circumstances, there seems no reasonable doubt that the poisonous action of chloroform may be manifested either on the lungs or heart in the first instance; and we may be unable to determine what shall direct the action primarily on one or on the other organ. Of the power of a sudden check to the circulation through the pulmonary tissue, and suspension of the hæmatosis, there can likewise be no doubt. But may not the chloroform, being absorbed into the blood, exert a directly poisonous action on the brain? Dr. Glover killed a dog, almost instantaneously, by injecting chloroform into the carotid in the direction of the circulation, and apparently from such direct action on the brain. In the case of Madame Labrune, related by M. De Confevron, where only about a *gramme* of chloroform was used, the fatal effects

"Were manifested in eight seconds, and the operator remarked constant winking of the eyelids. The patient repulsed the dentist's hand, making signs that the effect was not complete. She then made four or five fuller inspirations. At that instant, M. De Confevron removed the handkerchief, and only took his eyes off her for the instant occupied by placing it on the table; but in this brief instant, he found the patient's face turned pale, the lips discoloured, the features altered, the eyes turned upwards, the pupils horribly dilated, the jaw closed, the head drawn backwards, the pulse could not be felt, the limbs were all relaxed, and a few inspirations at long intervals were the only indications of life."†

Here the membranes of the brain were found greatly congested, and the veins at the base of the skull gorged with dark fluid blood; the ventricles also contained much serum.

In some cases of alleged poisoning by ether and chloroform in animals and men, air has been found in the veins and heart.

It is very evident that many of the appearances found in chloroform

* Bouisson, op. cit., p. 300. Wakley, as quoted.
† Medical Gazette, vol. ix. 1849, p. 952.

poisoning are nearly identical with those exhibited in ordinary asphyxia, and especially in drowned persons. It has been observed, in comparing the appearances presented by drowned with those of chloroformed dogs, that in the former "the congestion was more livid, not so florid; in patches not so uniform; and there was much froth and apparently water in the lungs. The larynx and epiglottis not so vascular."*

The remarkable power of penetrating the tissues, which chloroform and all its allied bodies possess, was long ago observed. Dr. Cogswell many years ago demonstrated this of the curious body iodoform. He detected this substance after death, by its iodine, in nearly all the tissues of the body; and in the secretions and excretions. In animals poisoned by liquid chloroform, or chloride and bromide of olefiant gas, injected into the stomach, the smell of the poison can be detected in the muscles after death.†

It appears to us that Dr. Snow has too rashly denied the possibility of chloroform being used by thieves to produce a sudden insensibility.‡ Used for such a purpose, it would not be given sparingly, but a handkerchief soaked in some ounces of it would probably be applied at once; and what with the exclusion of atmospheric air and the filling of the lungs with the vapour of chloroform, we see no reason why almost instantaneous insensibility should not be produced. Experience tends to confirm this.

The precautions to be taken against fatal accidents resolve themselves, 1st, into the purity of the agent employed; 2nd, into a care that the patient has plenty of atmospheric air along with the vapour; and if there is reason to apprehend danger from the symptoms, after discontinuing the inhalation, it does not seem that anything more can be done than merely dashing cold water on the face, and practising artificial respiration. Mr. Nunneley has shown that ammonia, galvanism, and other remedial means proposed are useless or prejudicial.§

(*d.*) The *modus operandi* of anæsthetic agents in producing temporary insensibility, has of course been a matter of considerable dispute. We might, in the first place, re-open the whole general question respecting the operation of remedial and poisonous agents by sympathy or absorption.

Some have endeavoured to explain the matter, by likening the action of anæsthetic agents to ordinary intoxication. But although there is some analogy, there is also a good deal of difference. In ordinary intoxication the intelligence never remains after complete insensibility to pain is produced; and again, the anæsthesia is rarely complete; and almost always very slight in comparison with the amount of mental disorder. And alcohol acts more powerfully when it is taken into the stomach, less powerfully when its vapour is inhaled; while the reverse is the case with the true anæsthetics.

It appears to us that anæsthetics may act in more than one way;—thus, 1st, they may produce a sudden, temporary, and partial asphyxia, or a

* Lancet, vol. i. 1848, pp. 441, 442.

† Chloroform may be detected in the blood or tissues by the following means:—Place the blood in a sand-bath, pass the resulting vapour through a tube heated in the centre to a red heat, and lined at its extremity with a paste of iodide of potassium and starch; its open end being also covered with paper moistened with the same mixture. If chloroform be present, the paper will be tinged blue. This process depends on the decomposition of the chloroform at a red heat. The simple distillation of the chloroform would often be better.

‡ Medical Gazette, Feb. 1850. § See his paper, pp. 378—381.

modification of that state; 2nd, they may destroy the power of the nerves to carry impressions to the centres of common sensation, or the power of the brain to perceive them. The experiments of Flourens on the spinal cord of animals under the influence of chloroform, already stated, would seem to confirm the idea of the nerves being rendered incapable of carrying impressions to the centres; although the same effects would result from insensibility of the centres themselves. Then the experiments of the same philosopher and others, and the reasoning of Mr. Dunn, appear to show a regular progressive action on the nervous centres. We believe that the action of anæsthetics is not simple, but compound, and that they produce the result through the combination of the modes just stated. Sufficient arguments in support of this view may be found by the reader himself in the course of this article.

M. Robin endeavours to make out that ether and chloroform received into the circulation in sufficient quantity "prevent the combustion of the blood and its conversion from venous to arterial, and that their anæsthetic effects proceed from this condition and constitute a true asphyxia."*

3. *Anæsthetic Agents and their Comparative Merits.*—We have said that shortly after the discovery of the anæsthetic properties of ether, attempts were made, especially in France, to ascertain how far similar properties were possessed by other bodies. Flourens, Chambert, Simpson, and Nunneley, have particularly distinguished themselves in this inquiry. The list of bodies given by the last is the most extensive, and we shall therefore allude more especially to his researches. The substances tried by him, the effects of which are detailed, are the following:—chloroform, alcohol, spiritus vini of the pharmacopœia, sulphuric ether, spiritus etheris nitrici, nitric ether, acetic ether, chloric ether, chloroform and spirit of wine mixed, hydrochloric ether, hydriodic ether, hydrobromic ether, Dutch liquid, oleum ethereum, heavy oil of wine, aldehyde, two or more of these mixed, iodoform, olefiant gas, light carburetted hydrogen, coal gas, benzoyle, camphor, naphtha, oil of turpentine, creosote, protoxide of azote, hydrocyanic acid, coneine, hydrogen, carbonic acid, carbonic oxide, bisulphuret of carbon, suphuretted hydrogen and bromoform. To this list should be added formomethylal, of which mention is made by M. Bouisson, and with which we ourselves have made some experiments, and acetone.

This extensive list admits of classifications. We would arrange the groups in the following order:

1. Protoxide of azote, $N\,O$.
2. Carbonic oxide, $C\,O$; carbonic acid, $C\,O_2$
3. Light carburetted hydrogen, $C\,H_2$; olefiant gas, $C_2\,H_2$
4. Ethyle series:—Alcohol, $C_4\,H_5\,O + H\,O$; sulphuric ether, $C_4\,H_5\,O$; nitric ether, $C_4\,H_5\,O + N\,O_5$; acetic ethers, $C_4\,H_5\,O + C_4\,H_3\,O_3$; hydrochloric ether, $C_4\,H_5\,Chl.$; hydrobromic ether, $C_4\,H_5\,Br.$; hydriodic ether, $C_4\,H_5\,I$; formic ether, $C_4\,H_5\,O + C_2\,H\,O_3$
5. Acetyle series:—Aldehyde, $C_4\,H_3\,O + H\,O$; Dutch liquid, $C_4\,H_3\,Chl. + Chl.\,H$; and doubtless the corresponding iodide and bromide.
6. Formyle series:—Chloroform, $C_2\,H\,Chl_3$; bromoform, $C_2\,H\,Br_3$; iodoform, $C_2\,H\,I_3$

* Medical Gazette, Feb. 1850.

7. Compounds of methyle; pyroxylic spirit or wood naphtha, $C_2 H_3 O + H O$; rock naphtha, $C_4 H_5$; coal naphtha; formomethylal (uncertain composition); methylal, $C_2 HO, HO + 2 C_2 H_3 O$.

8. Turpentine, $C_{10} H_8$; benzoyle, $C_{12} H_6$; camphor, $C_{10} H_8 O$; creosote, $C_{14} H_9 O_2$

9. Bisulphuret of carbon, $C S_2$

10. False anæsthetics, as sulphuretted hydrogen, prussic acid, coneine, &c.

11. Mixed bodies, as oleum ethereum, chloric ether, &c.

12. Acetone, $C_3 H_3 O$.

Mr. Nunneley's great and praiseworthy object, in the laborious series of experiments on animals which he relates in the paper before us, was to ascertain how far the anæsthetic properties of bodies corresponded with their chemical relations. This is a subdivision of a general investigation into the relation between the natural history of bodies and their action on the living systems of men and animals; an investigation which has enabled us to state, both with regard to plants and to chemical groups, that such a connexion or relation does exist, and thus to frame one of the grandest laws bearing on physiology, toxicology, and therapeutics. The anæsthetic properties of bodies offer no exception to this law.

It will be observed that almost all the true anæsthetic agents are compounds of carbon. The protoxide of azote is an exception; and its properties, contrary to what we might anticipate from the observations of Wells, are very different from those of ether and chloroform. It may be premised that there seems no reason for doubting the purity of the protoxide employed by Mr. Nunneley. He says that his experiments are quite sufficient to show

"That nitrous oxide never could be employed as an anæsthetic, and that the inhalation of it is not altogether so harmless as is generally stated. Probably, when inhaling it in the ordinary way as a laughing gas, almost as soon as it begins to produce its effect, very little more is taken into the lungs, and insensibility is not very frequently occasioned, but only its first effect, the drunkenness and excitement, though even a very small quantity will quickly stupify some persons; these experiments, however, clearly prove that with animals the state of insensibility when safe passes off so quickly as to be practically useless, while if it be rendered more profound or more prolonged, it is highly dangerous. Moreover, they indicate its action to be very uncertain: that of three animals of the same species, and similar as to age, size, and sex, with the same quantity of gas, one was killed in five minutes, while the others, after being exposed to the gas for nearly three times as long, showed hardly any symptom of being affected by it."

The *post-mortem* appearances, too, were very different from those found in animals killed by the true anæsthetics.

It seems to us that an immense difference exists between the true anæsthetic agents and such bodies as hydrocyanic acid, coneine, and the like, where the anæsthetic action is only subordinate to one destructive of life. It is very clear that if by the administration of a poison we destroy the natural sensibility, a kind of secondary anæsthesia may be induced. If we were to call these poisonous agents anæsthetics, there is hardly a single deleterious substance which might not be ranged in the list!

Of the less known bodies in the list, none seems so worthy of attention as the Dutch liquid. This beautiful substance is made by causing chlorine and olefiant gas to come in contact; they instantly form a heavy, colour-

less, and volatile liquid, which in smell and taste considerably resembles chloroform. The result of some of Mr. Nunneley's investigations would seem to point to the conclusion, that this substance is quite as powerful as chloroform, and more free from dangerous and disagreeable effects. The animals were quite as soon and as completely etherized by the one body as by the other. Mr. Nunneley has used it in surgical operations with perfect success. On the other hand, Dr. Simpson and Dr. Snow have tried it, and consider it a dangerous preparation. We have ourselves used it with perfect safety, but some tendency to nausea was left afterwards. The enormous cost of its preparation, judging from the price we were charged for it by a London druggist, would effectually prevent its use.

The analogous body, bromide of olefiant gas, is a remarkably agreeable substance to the taste and smell; animals are easily etherized by it, without the least irritation or other untoward symptom, except some laborious breathing; they readily came round, but all died in a few hours.

None of the ethers have any advantage over the sulphuric. The formic ether might be supposed to unite the good properties of ether and chloroform, but it is found to create too much irritation. Aldehyde, which is a true anæsthetic, is exposed to the same objection. Of the bodies analogous to chloroform, bromoform in all probability, and iodoform certainly, are not sufficiently volatile.

The light carburetted hydrogen possesses feeble anæsthetic properties. These are enjoyed more powerfully by olefiant gas, but the latter is only capable of producing insensibility when it begins to be dangerous.

Common coal gas seems to be a very effectual anæsthetic; animals are quickly brought under its influence, and it appears to be both safe and manageable.

Carbonic oxide and carbonic acid act as powerful narcotic poisons, as is well known, and cannot therefore be justly ranked with the anæsthetic bodies.

The chloride of carbon very much resembles chloroform and the Dutch liquid in physiological action.

Of the compounds of methyle, the naphthas do not possess much anæsthetic power, resembling, in this respect, the analogous body, alcohol. The coal naphtha is the most powerful. The substance called formomethylal, procured from the reaction of peroxide of manganese, sulphuric acid, and wood spirit, is considered by M. Bouisson to be an anæsthetic agent of very considerable power. This body is an ethereal, colourless liquid, possessed of an agreeable aromatic odour. It boils at about 42° cent. Its specific gravity is 0·855. It is very inflammable, and burns with a luminous flame. Liebig considers it a compound of oxides of formyle and methyle. In some experiments performed by M. Bouisson, animals were readily brought under its influence, and without any unpleasant effects.* We have ourselves performed several experiments on animals with this substance, and can corroborate his opinions. But in preparing the agent, two formidable explosions occurred—fortunately in the temporary absence of parties from the laboratory; and there is reason to believe that these accidents may arise from some unknown cause, and require considerable precautions to obviate them.

* Op. cit. pp. 94—98.

The bisulphuret of carbon is less effectual than chloroform or the ordinary ethers; it is evidently more dangerous, and infinitely more disagreeable.

Acetone appears to us to be a feeble anæsthetic agent, and was found by Dr. Simpson to cause considerable irritation of the chest, manifested by cough and dyspnœa.

The class of bodies represented by benzoyle, turpentine, camphor, &c., admit of being summed up in a few words. They possess the feeblest anæsthetic powers, or these are accompanied with such irritant qualities as to render their use quite inadmissible.

From what has been said it may be gathered, that of the numerous class of substances which we have cited, there are only three or four likely to be of practical use; and the principal question at present is with regard to the relative merits of ether and chloroform. Hitherto, ether has been decidedly less fatal than its rival; on the other hand, it is less prompt in its action, and more apt to cause inconvenience to the patient at first. In America, many practitioners have returned to its use, preferring it decidedly to chloroform. Thus, Dr. Hayward states:

"I have not seen convulsions follow its exhibition, nor any delirium, except a slight and transitory kind, such as arises from intoxicating liquors. I confess that I was much surprised to learn, by carefully watching its effects, to what a small extent, and for how short a time, it disturbed the functions of the nervous system, and how rare it was to find headache among the consequences of its inhalation.

"The only objections of which I am aware to sulphuric ether as an anæsthetic agent, are, its pungent odour, which is offensive to some persons, and the no inconsiderable degree of irritation which its inhalation produces in the air-passages; this irritation, I am confident, may be in great measure prevented, by proper attention to the mode of its exhibition, and the quality of the article used. Admitting these objections to be as great as they have been said to be by those who have urged them with the most earnestness, they do not, in my opinion, counterbalance the advantages; and I have no hesitation in saying that I should give it the preference over any other article with which I am acquainted, that is used for the purpose of producing insensibility."*

Dr. Warren evidently prefers ether;† and Dr. Flagg goes so far as to say that "chloroform has now ceased to be used in this section of the country, except to be mixed in very small proportion with the ether, and occasionally by local application."‡ The author just named prints a correspondence with several medical men in the United States, from which we may gather that ether has never at any time been superseded there by chloroform, and that the former substance is still preferred by many.

No great advantages are attainable by mixing these substances, or by using the mixture called chloric ether, or any of the solutions of chloroform. It would seem more reasonable, if chloroform be considered too powerful, to take care that a sufficient quantity of air be inhaled along with it, than to use any other kind of dilution.

Dangerous consequences or fatal effects have been ascribed to the impu-

* American Journal of the Medical Sciences, July, 1850.

† See Etherization, with Surgical Remarks, by John Warren. Boston, 1848.

‡ Op. cit. p. 35. On the other hand, Professor Gross, of New York, decidedly prefers chloroform. See Edinburgh Monthly Journal, Feb. 1851.

rities mixed with ether and chloroform. The only admixtures of this kind likely to be present in chloroform, are certain oily compounds of indefinite composition, alcohol, and muriatic acid, or free chlorine. Dr. Gregory is inclined to attribute the remarkable success which has attended the administration of the agent in Edinburgh to the excellence of the preparation. He says:

"In London and elsewhere chloroform has been extensively sold so bad, that I have examined specimens which did not contain half of their bulk of chloroform, in some cases not a third or a fourth part, and I have seen one which hardly contained any at all. But to make up for this, they were rich in poisonous oils, and often in free hydrochloric acid."*

Of the three kinds of impurity, the oils are probably the most dangerous. From what we know of the feeble anæsthetic powers of alcohol, and the use of the substance called chloric ether in America, we should not be disposed to regard the adulteration with alcohol as of any great consequence, except as weakening the anæsthetic effect. Either chlorine or muriatic acid must of course produce very great irritation, and, if in quantity, render the chloroform irrespirable. Oil of vitriol perfectly pure and colourless is a very delicate test for the oils, leading to a precipitation of their carbon and a brown discoloration. The ordinary process for purifying chloroform, was to agitate it with sulphuric acid, and then distil from lime or barytes. Instead of the latter part of this process, Dr. Gregory proposed to leave the chloroform in contact with, or to filter through, peroxide of manganese. But this process, although yielding a perfectly pure chloroform, has been found to furnish a body more liable to decomposition than that prepared by the process formerly in use.†

Dr. Fleming never uses chloroform, without first examining it by litmus paper and water, and, if at hand, a solution of nitrate of silver.

"If the former remains unaffected by the vapour, and some of the specimen, dropped into a test-glass containing either of the latter fluids, occupies the bottom of the glass in a transparent globule, I am satisfied that it is genuine, or, at all events, suited for practical purposes. But if, on the contrary, the litmus paper is reddened or bleached, and the globule appears opalescent, or like a muddy lens, I reject it as adulterated, and unsafe for use."‡

These simple tests have the merit of easy applicability to practice.

As the strength of sulphuric ether is of great importance to its anæsthetic action, this agent, if it be preferred, should previously be tested by the pharmacopœia tests, the specific gravity, the action on litmus, and the action of muriate of lime; and, if necessary, it should be well washed, and treated with dry subcarbonate of potass, and distilled after being treated with chloride of calcium.

In our next number, we propose entering fully into the various applications of Anæsthetic agents, in the practice of Medicine, Surgery, and Obstetrics.

* Edinburgh Monthly Journal of Medical Science, May, 1850.
† Christison, in Edinburgh Monthly Journal for September, 1850.
‡ Etherization in Surgery, by Dr. Fleming. Dublin, 1851, p. 22.

ART. IX.

Lectures on the Principles and Practice of Surgery. By BRANSBY B. COOPER, F.R.S., Senior Surgeon to Guy's Hospital, &c.—*London*, 1851. 8vo, pp. 964.

MR. BRANSBY COOPER is well known, both to the public and to his professional brethren, to be a surgeon of large experience, much skill, and disciplined judgment; but authorship is certainly not his *forte;* and whoever may happen to have no better materials for forming an opinion of his powers than such as are supplied by the volume before us, might very readily come to a conclusion respecting the writer directly the reverse of that which his very great professional attainments would really warrant.

The title-page of Mr. Cooper's work does not convey an altogether correct idea of its contents. If it did, we should dismiss the volume with a very few remarks; since a mere course of Lectures on Surgery, such as are delivered in the class-room, must of necessity be a somewhat meagre performance. As Mr. Cooper himself very reasonably says, in an introductory discourse at the commencement of the volume,

> "The time placed at the disposal of a lecturer in a public school of medicine—such as our own, for example—is far too limited to permit of the subject being treated fully; the teacher is therefore compelled to seize the most salient points only, furnishing to the student a well-considered and carefully-arranged sketch, to which he must himself supply the details, the filling-in, as it were, from knowledge derived from reading and other means of study within his reach." (p. xii.)

If, then, the present book corresponded to the foregoing description, if it purported to be merely a sketchy and imperfect outline, we should deal with it as such. But Mr. Cooper's Lectures are not published in the shape in which they were delivered. The author himself says, in the preface to the work: "I have spared no pains in its preparation, and in its present form a considerable addition of matter has been made to that originally given in the Lectures." Mr. Cooper, in fact, (again to quote his own words), offers the volume as one "of a practical character, embodying the experience of twenty-five years, during which he has occupied the position of Surgeon to Guy's Hospital," and also as furnishing "a useful compendium of surgery, in which the student may meet with a clear account of the practice of that science, established, not only on the author's experience, but likewise upon the best acknowledged authorities." The publication of a compendium of surgery, written by the senior surgeon of Guy's Hospital, and offered as a guide, "not only to the student, but also to those who have entered upon the practice of their profession" (p. ix.), is a rather noticeable event in the medical department of the republic of letters; and we are therefore, in a manner, constrained to inquire in what manner the successor of Cline and of Sir Astley Cooper has executed his self-imposed task.

In attempting to do so, however, we encounter a difficulty which we almost despair of surmounting. The structure, if we may use the phrase, of the present work, is peculiar. Whatever it may profess to be, it is *not* a *compendium of surgery.* It does not contain anything like a complete

account of the anatomical characters, natural history, or treatment, of any one surgical affection; not a few very important surgical affections are merely mentioned, and literally nothing more than mentioned; and many others are passed by in absolute silence. Nor is this all, for the subjects that are touched upon, though often treated with much diffuseness and a copious expenditure of words, are yet very commonly discussed after a fashion so fragmentary, unmethodical, and confused, that our author is not unfrequently betrayed into point-blank contradictions of statements antecedently made. These are the radical faults of the work; and obviously it would be only possible to show that they pervade it throughout, by adducing cumulative evidence co-extensive with the work itself; but such a criticism, it is plain, would rival, or rather exceed, its subject matter in bulk. But, on the other hand, in confining ourselves within reasonable limits, we incur the risk of seeming to assign insufficient grounds for our opinion of Mr. Cooper's treatise. We must, however, of necessity, select the latter alternative, and rest content with exhibiting a few samples of the material of which it is constructed.

In the first two lectures, Mr. Cooper touches discursively upon the physiology and pathology of the blood and the bloodvessels. It may be that these lectures are intended to give a kind of Hunterian colouring to the work, or possibly the "filling-in" of this part of the "sketch" has been designedly omitted, with the view of stimulating the student, at the outset, to independent inquiry. If the former hypothesis be the true one, the contrast, thus inevitably provoked, between our author and the "prophet of surgery" (as Mr. Cooper terms Hunter, p. 2), were much better avoided; but if the latter supposition is correct, we freely admit that full scope is left for the investigations of the inquisitive learner. To point out the omissions in these chapters would be nearly equivalent to writing an essay on the subjects Mr. Cooper professes to treat. We can only adduce a very few instances, by way of sample, of Mr. Cooper's matter and manner. We cannot, of course, make any objection to the grave dictum that "we must consider the human body not as an inanimate but as a vital mass" (p. 5), and we willingly believe that certain statements of Mülder, respecting the protein compounds, are attributed (at p. 10) to Müller, purely through inadvertence, the more especially as Oken's views of the homology of the bones of the cranium are in like manner transferred (at p. 416) to a certain Professor Okie, whoever he may be. But when we read, at p. 10, that "albumen, *without undergoing any change*, constitutes a considerable portion of many structures," we would gladly learn what are the structures so composed, and by what process of investigation Mr. Cooper made this discovery, the merit of which, we believe, belongs solely to himself. Your purely practical man, very possibly, may deem this not a matter of sovereign importance, but he will scarcely fail to shrug his shoulders at the following extract from the section entitled 'Pathology of the Capillaries.'

"If there be increased flow of blood in the arteries, or a retardation in the circulation of the blood through the veins, the capillaries must necessarily be distended with blood, and a state of hypertrophy and hyperæmia be the result. For a time, these vessels may be capable of sustaining this condition without the occurrence of any serious morbid changes . . . . . but if the exciting cause

of this state of hypertrophy be not subdued, whether the fault depends upon the arteries, veins, or capillaries themselves, these latter vessels will soon lose their tonicity: they will be congested and dilated, the blood itself impeded in its course, and effusions of other than the appropriate constituents will be thrown out, and the part may then be said to be inflamed. Serum is the constituent usually exuding through the coats of the capillaries under *this* condition, which very generally results from venous congestion, as is seen in œdema of the face, from pressure on the large veins of the neck; of the upper extremity, from pressure on the axillary vein in diseases of the axillary glands; and in ascites, from obliteration of the inferior cava." (p. 19.)

It is not very difficult to guess what Mr. Cooper *intended* to say in the foregoing passage; but let us look for a moment to what he *does* say. The loose use of the word "hypertrophy" we pass by; but then it is directly implied, indeed, stated in so many words, that a part may be said to be *inflamed*, when it is the seat of serous effusion consequent upon an obstruction to the venous circulation. That Mr. Cooper did not mean to convey that idea, is clear enough from the examples he gives, *not* of inflammation, but of dropsical effusion from impeded venous circulation; yet even here something calculated to mislead the student is negligently allowed to slip in. Ascites is coupled with "obliteration of the inferior vena cava," one of the very rarest causes of that affection. We should like to know in how many of the cases of ascites observed by Mr. Cooper, obliteration of the inferior cava, so familiarly spoken of, existed.

In connexion with the pathology of the arteries, Mr. Cooper devotes a few lines to the consideration of the "hæmorrhagic diathesis," and says it is

"Incumbent on a surgeon whenever he experiences any difficulty in restraining bleeding, especially if it occur from a slight cause, to investigate the constitutional condition of his patient, and to correct the diathesis if it be present." (p. 18.)

The soundness of this precept we do not question; for Mr. Cooper says, "I have known persons, the subjects of this disease, (which, by the bye, is frequently hereditary,) bleed nearly to death from the drawing of a tooth, or some slight incised wound;" and we must presume, that he succeeded himself in "correcting the diathesis," seeing that he pronounces it to be a duty "incumbent" upon other surgeons to do so. This we merely notice by way of illustrating the provoking abruptness with which Mr. Cooper frequently abandons a subject, at the very point where we should wish to hear more. We cannot but regret Mr. Cooper's reserve in omitting to mention the means whereby we may succeed in correcting a constitutional condition, which is as unmanageable and formidable when it does exist, as it is fortunately rare.

We pass on to the lecture on *Tetanus*, a malady which, Mr. Cooper says, "is as appalling, and unfortunately as incurable," as hydrophobia. As "incurable," in one sense, possibly it may be; but certainly it is far from being so fatal, as the tolerably numerous cases of recovery scattered through the records of medicine abundantly testify: and indeed Mr. Cooper himself (p. 79) relates a case of recovery from tetanus resulting from the injection of a hydrocele. Mr. Cooper also mentions two cases (among others) in his own practice, which proved fatal, one in five and a half, the other in forty hours; and it is worthy of note, that the symptoms of tetanus did not supervene until the seventh and eighteenth days

respectively, after the receipt of the injuries upon which they depended. Mr. Cooper says, these "facts are contradictory of the opinion of many surgeons, that tetanus but rarely comes on as the result of wounds, after suppuration has set in." (p. 79.) We doubt that such an opinion is entertained by any surgeon of repute; but, at all events, it would be easy to reckon up cases by scores that very decidedly refute it. These two cases, however, (the first of them especially,) are somewhat unusual, in that they proved so very rapidly fatal, although so many days had elapsed from the infliction of the injuries upon which the tetanus depended; and in this they are exceptions to the general rule, which Mr. Cooper omits to mention, that tetanus usually runs its course the more rapidly, the earlier after the injury the symptoms come on. It is curious, too, that Mr. Cooper does not mention in his account of the disease the remarkable remission of the symptoms that occasionally occurs previous to its fatal termination; although he gives the following very marked instance of the event, which, it will be seen, seems to have taken him completely by surprise. A man was admitted into Guy's Hospital, with very violent symptoms of acute tetanus of two days' standing:

"I ordered (says Mr. Cooper) a blister to be applied to the whole length of the spine, gave him calomel and opium, and a purgative enema. The next morning, about sixteen hours after the blister had been applied, all his symptoms had subsided; he could open his mouth easily, and with feelings of exultation, I dictated to my dresser while he wrote out every detail of the case. My triumph was, however, but of short duration, for on desiring the patient to turn in his bed that we might dress his blister, in making the necessary muscular exertion to change his position, he was seized with spasm, and died in an instant." (p. 81.)

A case of tetanus is detailed, in which Mr. Cooper administered the vapour of ether, both by the rectum, and by inhalation in the ordinary way. The malady terminated fatally; but we think it well to extract Mr. Cooper's summary of the influence exerted by the treatment, which may rather encourage to a further trial of this remedy, or of its analogue, chloroform.

"I was induced to continue the use of the etheral vapour, to the very last, in consequence of the comfort it appeared constantly to afford to the patient, so as to lead him frequently to ask for an inhalation. I was also warranted in persisting in the inhalation, for, although it did not relieve the spasms to the extent I had hoped, it certainly diminished the patient's sufferings, and prolonged his life to a period considerably beyond the time to which patients usually live under an acute attack of tetanus." (p. 81.)

In another case of tetanus, which supervened on the seventeenth day after an injury of the leg, Mr. Cooper amputated the limb immediately that the symptoms of tetanus became decided; and on the following day "all the tetanic symptoms had disappeared"—unfortunately, suppurative phlebitis with purulent infection set in, and proved fatal a fortnight after the operation, "but without a single symptom of tetanus after the amputation of the limb." Mr. Cooper simply records this case, without giving any opinion respecting the expediency of performing amputation when the first symptoms of tetanus came on—a practice which, despite some few instances of success, is generally rejected by surgeons; and which certainly appears, in most cases, to have hastened rather than retarded death.

Mr. Cooper has had the advantage of considerable experience in military

surgery; and we therefore turned to the chapter on "Gun-shot Wounds," with the intention of giving a summary of his views respecting that important class of injuries; but an instance or two will serve to show how far the student will find satisfactory information, upon this head, in the present work.

Mr. Cooper describes the characters of *gun-shot wounds* as follows:

"The wound made by the entrance of the ball is *small*, and its lips are inverted, discoloured, and valvular, while the opening through which the ball has made its escape is *much larger*, with an inverted and ragged edge." (p. 92.)

Further on we discover the following passage:

"It has been said, that the hole by which it (the bullet) enters is smaller and cleaner than that by which it leaves the body, which is ragged and more gaping. This does not, however, seem to be correct; the opening by which the ball enters seems to be generally somewhat the *larger* of the two. But, in fact, there is so little difference between them, that unless the direction of the shot were previously known, it would be impossible to say by which opening the ball entered, or by which it left the body." (p. 99.)

In the former of the two foregoing extracts, Mr. Cooper unreservedly adopts the account ordinarily given by surgeons, who have written upon gun-shot wounds. But after having composed four pages, he seems to have changed his mind, and gives an equally unqualified adhesion to an opinion that has latterly obtained some currency, chiefly, we believe, on the authority of M. Gerdy. Now, suppose this question to arise in a court of justice, what figure would the young surgeon make who leant upon Mr. Cooper's authority? How would he reconcile, under the torture of cross-examination, the conflicting statements of the senior surgeon of Guy's Hospital, as they are nakedly put in the present work? We make not the slightest doubt that Mr. Cooper could readily supply what he has omitted, and could most perspicuously point out the circumstances that may influence the respective magnitudes of the orifices of exit and entry of gun-shot wounds, as well as the characters which, irrespective of the relative size of the orifices, enable us, in most cases, to distinguish the opening at which the ball entered from that through which it issued. We have only quoted the foregoing passages to exemplify the easy, negligent inaccuracy with which Mr. Cooper writes; and it is solely with the same view that we now proceed to make the following extracts relative to *gun-shot wounds of the chest:*

"If, during the treatment, the patient faints from bleeding, the surgeon may *safely* pass his finger into the wound *to search for an extraneous body;* and, perhaps, it may be necessary carefully to enlarge the wound with a probe-pointed bistoury for its extraction." (p. 103.)

But subsequently, when treating of fractures of the ribs, Mr. Cooper returns to gun-shot wounds of the chest, and leans towards a very opposite doctrine, for we are told that—

"Surgeons (including Mr. B. Cooper himself, be it remembered) have recommended, that, if such an accident have occurred from a gun-shot wound, or from any cause by which it is probable that an *extraneous substance* may have entered the chest, *advantage should be taken of the syncope* produced by the copious bleeding, *to seek for* and remove the source of irritation; but I should advise great caution in following this treatment; for there is every reason to dread inflammation of the pleura after officious interference in wounds of the chest." (p. 246.)

First, then, a certain practice is said to be *safe*—and, subsequently, we are cautioned that the practice in question is perilous, because of its liability to excite inflammation of the pleura. We do not stop to inquire which of these two conflicting precepts is the correct one: it is enough for our present purpose to place them in juxtaposition.

It would be impossible to indicate, even in the most cursory way, the numerous important omissions in the chapter on "Gun-shot Wounds"—omissions the more to be regretted, as of the seventeen and half pages devoted to the subject, nearly five are occupied with a narrative of two cases, the interest of which is scarcely sufficient to justify their insertion, when many matters of much greater importance are scarcely so much as mentioned. In one of these cases, a ball passed through the chest between the base of the lung and the diaphragm; penetrated the diaphragm; made its way

"Between the stomach and transverse arch of the colon, and, without wounding a single viscus, lodged in the muscular parietes of the abdomen." (p. 106.)

Mr. Cooper thereupon asks—

"Who could have believed the ball could have taken the course it did, without wounding the lungs, or some of the abdominal viscera?" (p. 106.)

No doubt it is a curious circumstance how a ball may make its way between important parts without wounding them, but the well-attested facts of the kind on record are sufficiently numerous to prepare any tolerably well-read surgeon for such an occurrence.

In the chapter on *Suppuration* and *Abscess*, the diagnosis of the formation of matter is represented as being unattended with the slightest difficulty; or, at least, the possibility of the occurrence of any difficulty occurring is not so much as hinted at. And, no doubt, when all the symptoms mentioned by Mr. Cooper exist, the detection of matter will be easy enough, although some sufficiently important and familiar signs are omitted; as, for example—pointing of the skin in superficial and pitting on pressure in deep-seated, suppuration. But it is a very serious omission, that the student would rise from the perusal of the present work without so much as suspecting that the diagnosis of matter, lying deep beneath a strong aponeurosis or a thick layer of muscles, is often extremely obscure, and may constitute a most embarrassing practical difficulty. But to pass from what is not said to what is said—Mr. Cooper, after stating that cellular membrane is very prone to suppuration, and is the seat of most "common abscesses," thus proceeds:

"Hence, matter is very frequently diffused over large surfaces, and leads to most mischievous results, by separating this universal connecting medium from important organs of the body." (p. 127.)

It is very true, that matter often does become diffused through the cellular tissue, though "common abscess" is not just the case in which that event is very likely to occur; but we are somewhat surprised to find that Mr. Cooper fails to give the slightest information as to how the matter of "common abscess" is circumscribed, and its infiltration through the cellular tissue guarded against. Neither does he explain how the pus of abscess—in the liver, for example—may make its way to the surface, without becoming diffused *in transitu*. A most curiously inappropriate illustration is, indeed, incidentally given to exemplify the process; but

that illustration, it will be seen, is calculated to convey as erroneous an idea of the pathology of abscess as could well be conceived. The passage runs thus:

"Whatever may be the situation of an abscess, it generally directs itself towards the surface. *The mutual pressure between the viscera and the walls of the cavity, which is shown by the facility with which the former escape when those walls are accidentally wounded, is sufficient to prove that it would be difficult for pus to make its way into a serous cavity:* although it is not common, it is, however, well known that such a result may occur, and that abscesses may burst and pour their contents into either of the great cavities." (p. 129.)

Amidst the account given of acute abscess, such as it is, some passages are interspersed, pretty much at random, respecting what, Mr. Cooper says, Chelius and other surgeons have termed "cold abscess" (p. 126). The term "cold abscess" by no means originated with Chelius, having been in use among French surgeons before Chelius was born, to designate the affections called chronic abscesses by English surgeons; and it is to these abscesses, we presume, that Mr. Cooper refers, inasmuch as he speaks of their containing thin unhealthy pus, of the serious consequences that often result after they open, and of their frequent connexion with diseased bone. We must, however, confess that we are not quite certain what it is Mr. Cooper really means; for he subsequently, when speaking of the treatment of abscess, expresses himself thus:

"In cases of *encysted tumours*, in which, *instead of pus*, a kind of *serous secretion* is thrown out, corresponding to what is sometimes called a '*cold abscess*,' the seton may be desirable. . . . . I have successfully treated such cases by the introduction of a seton, but *cannot think* that this disease should be designated *abscess*." (p. 130.)

But, immediately after intimating this sagacious doubt, Mr. Cooper proceeds to relate, at length, two cases, which, he says, "may be regarded as instances of what is termed cold abscess,"—but which are simply cases of serous cysts of the neck, long since published by Mr. Cooper, in 'Guy's Hospital Reports,' as cases of "Hydrocele of the Neck," and again mentioned, at p. 432 of the present volume, as examples of this latter affection, being thus made to do duty a third time. Now all this is a very fair average example of the loose, confused puzzling way in which Mr. Cooper writes. We cannot pretend to guess why serous cysts and chronic abscesses are first confounded together—why a grave opinion is next hazarded, that the two diseases are not identical—and why, finally, after the expression of that opinion, two cases of serous cysts are adduced "as instances of what may be termed cold abscess." Be this as it may, we have given the substance of everything said by Mr. Cooper, that can be in any way interpreted to apply to chronic abscess, with the exception of some observations respecting psoas and lumbar abscess, to which we shall now briefly refer.

Of the *symptoms* of psoas abscess Mr. Cooper says nothing whatever; but he tells us, it requires "considerable knowledge and close investigation to distinguish it from hernia." (p. 134.) Very well; and if so, the student will look with some anxiety for an account of the signs by which the distinction is to be effected. This brings us to an excellent example of the compendious manner in which Mr. Cooper disposes of the differential diagnosis of surgical disease. The problem is to discriminate psoas abscess from hernia—here is the solution:

"The history of the case" (no history of the *disease* having been given), "the appearance of the patient, and the strict examination of the swelling, must be the means employed for forming a just diagnosis of the two diseases." (p. 134.)

Mr. Cooper's habitual abstinence from enlarging upon the diagnosis of surgical disease, is the more provoking, inasmuch as he occasionally breaks bounds and trespasses upon the domain of the physician. We may as well give the result of one of these excursions, which runs thus: "In pneumonia, intense heat of the skin is *the great pathognomonic sign* of the disease"!

Mr. Cooper recommends a method of treating psoas abscess, which, though it "in some respects has proved as ineffectual as all others, certainly has tended to lengthen life beyond the period at which it would have terminated had the abscess been opened." He first endeavours to amend the health of the patient by means of nutritious food, porter, strict rest, tonics, and iodine; under this treatment the patient's general condition usually improved, but at length:

"The dresser, perhaps, informs me that the abscess increases in size; I walk from the bed, or perhaps order cold to be applied to the swelling, hesitating as to what is best to be done, knowing that directly the abscess is opened, constitutional symptoms will set in, and from that moment the patient will begin to sink. I therefore always refrain from evacuating the abscess until the tumour on the thigh is shut out from the cavity of the abdomen by an adhesive inflammation; and this object I have attempted to effect in some cases by applying a little pressure, such as that produced by a weak truss, at the point of communication between the thigh and abdomen. The evidence of this object being attained is the impossibility of pressing the fluid into the abdomen, and the diminished impulse of the femoral tumour on coughing. (p. 135.)

It is scarcely worth while observing that a psoas abscess does not communicate with "the cavity of the abdomen;" but it is certainly quite a new fact in the pathology of chronic abscess, that adhesion can be thus readily produced between the opposed surfaces of the sac. We must own that we entertain a strong doubt whether the communication between the two portions of the abscess is really obliterated by the pressure of the truss employed by Mr. Cooper. But if the fact be really so, the application of the principle to external chronic abscesses, in which either the whole or a considerable portion of the opposed surfaces could be brought into contact, after the evacuation of the pus by a valvular opening, can scarcely fail to prove most valuable.

We can touch on one or two points only in the lectures on Diseases of Bones. And first we may observe, that tubercle of bone is not so much as mentioned; no trifling omission, it will be readily admitted. Mollities ossium and rachitis *are* mentioned, but their anatomical characters and symptoms are completely omitted; and it would almost appear as if Mr. Cooper imagined he was advancing something novel in expressing an opinion that these two diseases are not identical, when he says (p. 193): "I believe that a distinction *ought to be made* between rickets and the condition I have described as mollities ossium." But this is mere negligence; the same kind of negligence that makes Mr. Cooper say (in the passage just quoted) that he has "described" mollities ossium, although he gave no "description" whatever of the disease beyond writing down its name; and the same kind of negligence that makes Mr. Cooper

say (at p. 217) that hydatids constitute "a very rare variety of *cystiform* disease," and then, in the very next page, flatly contradict himself in the following words: "hydatids are *not* to be classed in the category of *cystiform* tumours."

We shall here copy *all* that Mr. Cooper says upon the subject of deviations of the spine; with the view of enabling our readers to form their own opinion, from an unmutilated extract, how far his description of surgical diseases is sufficient and satisfactory:

"In the practice of your profession your attention will frequently be directed to the treatment of distortions of the spine resulting from the softening of the bones, and I feel, therefore, that I am bound to give you my opinion of the mode of treatment to be adopted. I repudiate the use of all apparatus and mechanical treatment of any kind for straightening a crooked spine, which confine the patient to the recumbent posture, and prevent by exclusion from air and gentle exercise the due performance of the natural functions, unless there be pain upon pressure in any particular point of the spine indicating inflammation and probably ulceration: under these circumstances the recumbent posture must be maintained so long as the above indications remain; but I believe blisters, moxas, setons, leeches, and other local means, avail but little.

"The disease is a constitutional defect in healthy nutrition, and can be remedied only by the improvement of the general health, although it is true, the application of simple mechanical means may aid this object so long as it does not interfere with the natural and vital functions of life. Use common sense, therefore, and bear in mind that however the preponderance of physical disturbance to the natural functions of the spine may attract your attention, this state is only a result of a general constitutional deterioration, and that without the improvement of health no good can be effected. The remedies must (to be useful) be directed to the removal of the cause of the disease, and not to the effects. You should do all you can to strengthen the assimilative powers; and as there is every reason to believe that the nutrition of bone is most at fault, means should be adopted, as far as diet and medicine can avail, to remedy the evil. Such diet, for instance, should be enjoined as contains most phosphate of lime: beef and mutton, and what is termed secondary bread, are therefore advisable: and phosphoric acid should at the same time be prescribed, for the purpose of its union with the lime, rendering it capable of being more easily absorbed. Bottled porter will also assist in improving the constitutional powers of the patient; and care should be taken that the bowels are not relaxed so as to carry off the lime too quickly, instead of leaving it to be taken up by the absorbents of the intestinal canal. The physical treatment indicated is to support the weight of the trunk by the most simple mechanical means competent to relieve the affected bones; and such muscles should be put into gentle action as have a tendency to counteract the unnatural direction the bones may have acquired from the influence of the existing abnormal causes, viz. the undue disposition of the weight and altered muscular action.

"Patients labouring under this affection should be frequently in the open air, and should be permitted to take gentle exercise; but must avoid most cautiously the slightest fatigue either of body or mind: riding in an open carriage, or sailing on the sea, is the very best kind of recreation which can be adopted.

"Tonics of different kinds,—iodine, and other alterative and sedative medicines,—may be indicated: but I cannot lay down a general rule to direct your choice of any particular remedies: your own judgment, and the experience derived from a knowledge of your patient's constitution, should influence your selection.

"This is all the advice I can give you as to the treatment of distorted spine; and although folios have been written on this subject, I can cull from them nothing beyond what I have just told you; unless, indeed, I entered into a detailed description of the various apparatus employed, the different gymnastic exercises recommended, the formulæ for the favourite medicines prescribed, and the recital

of numerous cases illustrative of the too often imaginary efficacy of hypothetically infallible nostrums." (pp. 194, 195.)

The *pathology* of spinal distortions, it will be observed, is comprised in the three words—"softening of the bones." Of the *causes* little more is said than that they are "various," and that their appreciation is essential for the scientific treatment of the patient. And as to the directions for the *treatment*, we most willingly admit that they are judicious, so far as they go; but whether Mr. Cooper has succeeded, as he professes to have done, in culling all that is valuable from what has been written upon deviations of the spine, and embodying it in the foregoing extract, we leave our readers to judge.

The history of *Injuries of the Head* (including fractures of the cranium, concussion and compression of the brain, hernia cerebri, &c. &c.) is compressed into eight and a half pages. The omissions, consequently, must be very numerous; but we find one piece of information which would be very interesting were it correct. With reference to the condition of the brain in concussion of that organ, Mr. Cooper says, at page 225: "Mr. Colles, of Dublin, states that in the cases (of concussion) he had examined, it appeared to him that the brain was diminished in size, and that it no longer completely filled the cavity of the cranium." Such a confirmation, by so competent an authority, of Littré's celebrated observation (to which, by the way, Mr. Cooper does not refer), would be very important; but we do not happen to know where Mr. Colles has made the statement attributed to him by Mr. Cooper. We do know, however, that Mr. Colles, in a report of his lectures, published after his death by Mr. Coy, is represented as expressing very great doubt that any such diminution of the size of the brain has ever been observed in concussion.

Four lectures are devoted to the consideration of *Dislocations*. We must of necessity limit ourselves to a very few points under this head, and select those that can be noticed within the briefest compass. At pages 323-4, it is said that a knowledge of the nature of the injured joint facilitates the diagnosis, prognosis, and treatment; and by way of illustrating this general position, Mr. Cooper observes—"In the hinge and trochoidal joints, he (the surgeon) would know that from their naturally limited degree of motion, the displacement, unless the injury were complicated with fracture, could only take place in *two* directions;" yet at pages 343, 349, and 366 respectively, Mr. Cooper describes dislocation of the elbow in *three* directions, and of the wrist and of the knee in *four* directions, severally. Now, the wrist happens to be one of the hinge-joints, in which the rule previously laid down by Mr. Cooper does hold good; for we doubt that a well-attested case of *lateral* dislocation of the wrist has been recorded. We may also observe, that Mr. Cooper does not mention the extreme rarity of dislocations of the wrist; and, so far as the dislocations backwards and forwards are concerned, we probably find the explanation of that omission by referring to his account of fractures of the lower extremity of the radius, where it is said:

"Fractures of the lower part of the radius are often misunderstood, and may be taken for incomplete dislocation of the wrist; but the power of restoring its normal conformation by a slight force, together with the crepitus, is sufficient to distinguish this accident from dislocation." (p. 268.)

Now the surgeon who depends upon the signs here given to distinguish fracture of the inferior extremity of the radius from dislocation of the wrist, will most frequently fall into error; for a slight force is not always sufficient to restore the natural conformation of the joint, and, what is more important, crepitus is most commonly absent. Indeed, it is the frequent absence of the ordinary signs of fracture in these cases, that led to the long prevalent belief of the frequency of dislocations of the wrist. With reference to the knee-joint, Mr. Cooper is of opinion that dislocation of the tibia forwards upon the femur is a much rarer accident than displacement of the same bone backwards. It is curious how authorities differ upon this point. Some, as Duverney, have even denied the possibility of dislocation of the tibia forwards; while others, as Boyer, dispute the occurrence of the displacement backwards. If we appeal to statistics to decide between conflicting authorities, it appears from the cases collected by Velpeau, that the displacement forwards is the more frequent of the two; for in twenty-one cases of complete dislocation of the knee, eight only were backwards, while thirteen were forwards.

Mr. Cooper thinks that "the strength of the ligaments &c. almost precludes the possibility of dislocation of the sternal extremity of the clavicle backwards from external violence." (p. 332.) And dislocation of the sternal extremity of the clavicle upwards, he says, "can hardly take place." (p. 333.) The two dislocations here in question no doubt are of rare occurrence, the latter more especially; but there are at least nine authentic cases of traumatic dislocation of the sternal extremity of the clavicle backwards upon record; and MM. Sedillot and Baraduc have each published a case of the dislocation upwards (one complete, the other incomplete), which establish the occurrence of the displacement in that direction beyond all doubt.

Mr. Cooper discusses dislocations of the shoulder with a brevity that renders some of his statements very obscure; but so far as we can gather his meaning, much of what he says is open to objection:

"The head of the humerus (we are first told) may be dislocated in three different directions—namely, downwards into the axilla; downwards, *forwards*, and inwards, upon the venter of the scapula; and backwards and outwards upon the dorsum." (p. 337.)

But nineteen lines further down it is said—

"Of the three kinds of dislocation to which the humerus is liable, that downwards into the axilla is the most common, that *backwards* and inwards next," &c.

Here, then, the species of dislocation, which a moment since belonged to the class of dislocation *forwards*, is now ranked among the dislocations *backwards*. Nor is this an error of the press; for the same statement is thus distinctly repeated, in italics as we copy it, at p. 340—"*Dislocation of the humerus, backwards and inwards, upon the venter of the scapula.*" Any doubt that might exist as to the variety of dislocation Mr. Cooper means to refer to here, is removed by his adding—"in this dislocation the head of the humerus is thrown beneath the coracoid process." Consequently, in sub-coracoid dislocation of the humerus, the head of the bone, according to Mr. Cooper, is displaced backwards! So far as we can collect, Mr. Cooper denies the existence of intra-coracoid dislocation of the

humerus, therefore we cannot say in what direction he would describe the head of the bone as being thrown in that displacement. As to the account given of the *symptoms* of the several varieties of dislocation of the humerus, it will be enough to quote the following passage from what is said respecting "dislocation into the axilla."

"Some have stated that the head of the displaced bone may be felt in the axilla; this can only be in very thin people, and is productive of very great pain; and as it leads to no useful object, I hold it unnecessary to seek for this additional proof of dislocation." (p. 338.)

It may be well to observe, in connexion with this extract, that nothing whatever is said respecting the differential diagnosis of dislocations of the humerus; nor is the slightest intimation given to the student, that such accidents as fractures of the neck of the acromion process, and of the anatomical and surgical neck of the humerus, possibly might be, and, the second especially, actually have been, confounded with dislocation of the humerus. We have, perhaps, said enough to convey a fair idea of Mr. Cooper's chapters upon Dislocation. We may, however, add, that he

"Doubts the possibility of complete displacement of the astragalus (forwards) happening, excepting under such circumstances as would render immediate amputation unavoidable." (p. 377.)

Certainly this, though somewhat surprising, needs no comment.

Next comes the portion of the work devoted to the *Surgery of Regions*, and we are induced to extract the following passage from the section upon "The Posterior or Occipital Region" of the cranium, because it is convenient from its brevity, and at the same time admirably illustrates some of the most characteristic peculiarities of the present work:

"A diarthrodial joint connects the occiput and atlas at this region, forming an articulation in which *anchylosis* sometimes occurs; *this* must be regarded as one of the diseases of the region." (p. 390.)

The few preceding words constitute the beginning, middle, and end of what is said respecting that formidable and not very unfrequent disease, caries of the first two cervical vertebræ; for we venture to conjecture that Mr. Cooper had that affection floating in his mind, in some loose, vague kind of way, when he wrote the passage just quoted. True it is, *anchylosis* is the antecedent to the word *this*, which we have printed in italics; and therefore, according to strict grammatical construction, *anchylosis* of the occipito-axoid articulation is the disease actually referred to. We do not, of course, mean to dispute that anchylosis does constitute a disease; although, in this particular instance, above all others, perhaps, it may practically be regarded as the termination, by cure, of a most dangerous malady: but is it not extraordinary that the history of the antecedent malady is completely ignored, and its rarest termination alone noticed?

The chapters upon *Injuries of the Abdomen* are a decided improvement upon the preceding portions of the work; but are by no means free from many important omissions, and present several of those damaging slips which Mr. Cooper so frequently makes through sheer inadvertence. The rambling diffuseness of our author's style renders it very difficult to find passages of manageable length for quotation; the annexed one, however, may answer our purpose, but we must premise it by observing, that

Travers's celebrated experiment, (which Mr. Cooper does not mention,) in which the continuity of the intestinal canal of a dog was restored after the gut had been circularly constricted by a ligature, gave rise to a speculative conjecture, that a similar result might possibly be obtained in the human subject. Of this purely hypothetical proceeding Mr. Cooper thus speaks:

"Some have recommended that the upper extremity of the intestine should be passed into the lower, and that a ligature be then applied around the whole. This produces contact of the peritonæal coat of the intestine above and below the ligature, and as adhesive inflammation is set up, an effusion of plastic matter soon covers the ligature, and re-establishes the continuity of the external part of the canal; the ligature itself, and the constricted portion, ultimately sloughing off internally, and being conveyed away with the excretions. It has been objected to this operation, that, in bringing the severed ends of the intestine together, a serous is presented to a mucous surface, and that these two structures are not fitted for union; but it is not intended in this operation that they shall unite: the union is caused by the effusion of the plastic matter from the external surfaces above and below the ligature, and from serous to serous membrane, the whole of the intestine included in the ligature being destroyed, and sloughing away. M. Jobert has proposed, as an improvement in the above operation, to invert the inferior extremity before the superior is introduced. In that case, two serous membranes are brought in contact, and the union may take placé at once between them; but, under these circumstances, the invaginated portion would not be included in a ligature, but retained in position by suture." (p. 451.)

The student, after perusing this passage, would scarcely suspect that we should be guilty of outrageously violent *mal*practice in adopting the practice in question; for Mr. Cooper seems—but, of course, only seems—to consider it both rational and feasible. We need hardly say, that M. Jobert proposed his method of invagination as an improvement on that of Ramdohr, and not to replace a proceeding, which no one ever had the wild hardihood to practise on the human subject.

Mr. Cooper appears to lean towards the opinion, that it is preferable, when a wounded intestine is completely divided,

"To establish an artificial anus, and leave nature to her own efforts for the ultimate restoration of the patient; and this (he adds) does not indeed appear to be so difficult a process as may be supposed, particularly if nature be judiciously assisted by the art of the surgeon." (p. 452.)

In the practice which Mr. Cooper here seems to recommend—we say *seems*, for he does not express a decided opinion,—we may be allowed to express our concurrence. Our reasons for this opinion were assigned in the pages of one of our predecessors, (Brit. and For. Med. Rev. vol. xxiii.) and we need not repeat them here.

Mr. Cooper passes over the treatment of *Artificial Anus*, consequent upon wounds of the intestines, very superficially indeed. The medical treatment he recommends is very judicious, but the only description of operative interference mentioned, is the destruction of the "Eperon" by means of Dupuytren's enterotome. But the prominence,—nay, the very existence—of the "Eperon" is governed by the amount of the protrusion of the intestine from the abdomen, and the extent of the loss of substance it sustains; and, consequently, however important a part the "Eperon" may play in artificial anus resulting from sphacelated hernia, its presence, to any prejudicial extent at least, must be exceptional in artificial anus

from wounded intestine, and therefore, the enterotome can be scarcely ever applicable in such cases.

Within a very few pages we find another example, to add to those already adduced, of the unceremonious way in which Mr. Cooper contradicts himself:

"When the urinary bladder is ruptured, if the lesion involves any portion covered by peritonæum, death rapidly follows, and medical treatment is completely unavailing, as the patient dies without rallying from the state of collapse, and, consequently, antiphlogistic means cannot be had recourse to." (p. 458.)

This is very positive and precise, but let us contrast it with the following passage in the same page:

"Even where the peritonæum has undergone lesion, there may be reasonable hope of recovery, if the quantity of urine extravasated be not large, and if judicious means be employed to prevent, or combat, or subdue peritonitis." (p. 458.)

We had only intended to place Mr. Cooper's conflicting statements side by side; but, in reference to the former of the two quotations just made, we must remark, that so far from death always rapidly following rupture of the bladder involving its peritoneal coat, it is, on the contrary, a very remarkable circumstance, that, in many cases of that accident, the symptoms of peritonitis set in rather insidiously, and the patient may live a week or more. As concerns the second extract, we think it goes too far in its contradiction of the first; for, we believe, we are warranted in saying, that there is scarcely more than a single instance on record, of ultimate recovery from rupture of the bladder including its peritoneal coat.* Dupuytren, indeed, concluded from the *post-mortem* appearances in one case, that recovery might possibly ensue after this accident; and perhaps it might. But there is a long distance between a "reasonable hope" of cure, and a conjectural possibility of such a favourable event, unsupported, we believe, by a solitary instance of its occurrence.

We purposed to examine, somewhat in detail, the chapters upon *Hernia*, but we shall merely copy the following novel diagnostic sign of congenital hernia:

"The diagnosis of the congenital character of the hernia may be formed to a certain extent, from the testicle constituting a second tumour immediately below the true hernial protrusion. (p. 491.)

It is the prevailing opinion, that in ordinary inguinal hernia, the testicle forms "a second tumour" (that is to say, if we can term the *testicle* a *tumour*) below the hernia; while in congenital hernia, on the contrary, it is commonly thought that the testicle is, in a manner, confounded with the tumour; so that it is difficult, or may even be impossible, to distinguish it. Mr. Cooper, however, appears to think otherwise.

We have now examined somewhat more than half of Mr. Cooper's work, and we prefer devoting our remaining space to a few extracts, which may give a more favourable idea of the author's performance than what we have hitherto said is calculated to convey; but before doing so, we must say a very few words upon another point.

Mr. Cooper refers very sparingly to the opinions and practice of other

* This one instance was transferred to our pages from Mr. Rynd's valuable "Observations on Strictures," p. 48. (See Brit. and For. Med.-Chir. Rev., vol. iv. p. 173.)

surgeons, and he occasionally carries this reserve so far, that he seems to lay claim to what does not rightfully belong to him. At p. 863, for example, Mr. Cooper describes a method of amputation, that he has "been lately" in the habit of performing, which is absolutely identical with M. Sedillot's "mixed method," combining the flap and the circular operations. And again, at p. 947, Mr. Cooper proposes to perform tracheotomy in certain intractable cases of syphilitic disease of the larynx "with the object of placing the inflamed and ulcerated mucous membrane in a condition in which it can remain in a perfect state of rest;" and adds, "I acknowledge that I have never myself performed the operation of tracheotomy with this view; though I once advised its adoption;" yet this operation was, long since, not only proposed for the attainment of this object, but successfully performed, by Professor Porter, as may be seen by referring to his very valuable work on the Larynx.

We have already said that it is no easy matter to select passages from the present volume, that are at once of moderate length, and, at the same time, fairly represent the writer's views. We therefore prefer extracting, as fully as our space allows, from what is said upon one or two subjects, to multiplying shorter quotations. And we shall first copy the following observations respecting the necessity of thoroughly ascertaining the condition of the parts, when decided symptoms of strangulated hernia exist, even though the surgeon encounters during the operation a state of things, which might, *primâ facie*, justify the supposition that a tumour of another description had been mistaken for a hernia. The passages bearing upon this point are scattered over twenty-five pages, but we shall here connect them together in one extract.

"On dividing the tendon of the external abdominal oblique muscle, instead of exposing the hernial sac, we may discover a varicose condition of the spermatic veins, a hydrocele of the chord, an undescended testicle, or a cyst, perhaps, connected with the ovarium; or a steatomatous tumour may conceal the hernia from view. It is, therefore, necessary in all cases to be prepared for such contingencies, and even when they are met with, if the symptoms of hernia are present, whatever the concomitants may be, the malady must not be referred to them, but careful examination further made, to ascertain, beyond all question, the presence or absence of a visceral protrusion. In a former lecture, I mentioned the case of a surgeon, who, in seeking for hernia, found hydrocele of the chord; without further investigation, he set this down as the origin of the symptoms, and allowed the hernia, which, in fact, existed, to remain constricted until the life of the patient was sacrificed. I therefore again urge the necessity for pushing inquiry in such cases to the most extreme limits. I have myself met with considerable difficulty whilst operating in a case of congenital bubonocele, from mistaking a nondescended testicle for a hernial sac: very lately, my colleague, Mr. Cock, in operating in a case of bubonocele, found a second tumour beside the protruded intestine, and upon dividing the stricture a quantity of viscid fluid escaped. He was consequently led to consider it as an encysted tumour, proceeding probably from the ovarium. The unexpected occurrence did not, however, prevent the completion of the operation; as the hernia was reduced, and the patient recovered. Psoas abscess sometimes makes its way into the inguinal canal, and when concomitant with bubonocele, pus might escape during the operation. A case occurred some time ago at Epsom, in which, with all the symptoms of hernia, a tumour was discovered in the abdomen; the tumour was, however, distinctly fluctuating; it was therefore punctured, and pus let out: the evacuation of the matter satisfied the surgeon; he set down all the symptoms to the account of the abscess, and did not seek for any-

thing further. As the opening of the abscess did not, however, relieve the patient (the symptoms of hernia remaining), in a short time he died, and upon post-mortem examination a hernia was found in addition to the abscess from which the matter had been discharged." (pp. 491, 492.)

"Supposing an enlarged gland be exposed, it should be removed, and the investigation continued; for it is very probable that, under the circumstances described, a hernial tumour may yet be discovered behind the enlarged gland. Sir Astley Cooper mentions a case, in his published lectures, of a patient being admitted into Guy's Hospital, with a strangulated femoral hernia, to which he had had a poultice applied for three days, under the supposition that it was a bubo. When the operation was performed, the intestine was found in a state of gangrene, and the patient died. Another case is mentioned, in which a surgeon not only poulticed, but also opened a femoral hernia, believing it to be abscess, and the patient died two days after. I witnessed the same mistake in Norwich, several years ago; but in that case the patient survived, and an artificial anus proved only a temporary inconvenience." (pp. 503, 504.)

"The operation for strangulated hernia may be complicated by the co-existence of a hydrocele: and in a case in which we may have carefully dissected down to the tumour, an escape of fluid, and the sudden disappearance of the swelling, may lead to the supposition that a hydrocele has been mistaken for a hernia. Such a conclusion ought not, however, to prevent further investigation; the finger should be passed upwards to the ring, to examine if there be not some other tumour; as it may be that a hernia in a distinct peritoneal sac, may be placed in front of the tunica vaginalis, or may, indeed, have passed down into it; in either case the hernia, unless it were liberated, would lead to the destruction of life. A patient consulted Mr. Colman, of Norwich, on account of a swelling in the right inguinal region, suffering at the same time from all the symptoms of hernia, which would not yield to the usual remedies, nor could the tumour be reduced. Mr. Martineau was called in consultation, and the operation for strangulated hernia was determined upon. Upon opening the membranous covering to the tumour by careful dissection, a quantity of fluid made its escape, which satisfied the mind of Mr. Colman that the swelling was not a hernia, and therefore he desisted from further exploration. Mr. Martineau, however, strongly recommended him to continue his search: but he neglected to do so, and the patient died without any abatement of the symptoms. On a post-mortem examination, it was found that a hernia existed at the internal ring, within the peritoneal covering of the round ligament, which had also contained the water that had been evacuated, constituting a hydrocele of the round ligament, complicated with hernia—a very rare disease. There can be no doubt that further exploration should have been made in this case, as the liberation of the strangulated intestine would have offered a fair chance of the patient's recovery." (pp. 515, 516.)

We take the following passages, respecting the operations of Lithotrity and Lithotomy respectively, from the chapters upon Stone in the Bladder:

"Our attention must next be devoted to the consideration of the circumstances under which the operation of breaking down the stone, technically termed lithotrity, is to be preferred to the operation of lithotomy. This subject has occupied the attention of many eminent surgeons, and lengthened dissertations have been the result. I think, however, that very few words need be said on the subject. There can be no doubt that there are cases in which lithotrity is infinitely preferable to lithotomy; and it is equally true, that there are many others to which it is wholly inapplicable: nor are the means of judging between these two conditions at all difficult; and the circumstances which should regulate the choice are few, and easily appreciated. In cases of small stone, when the bladder is capable of containing about six ounces of water for a considerable time, and the urethra is of normal size, I believe the operation of lithotrity may always be recommended; and not even a moderately diseased state of the kidneys need prohibit this operation,

although in lithotomy the same extent of disease would involve a considerable increase of danger: indeed, in appropriate cases, the operation of crushing the stone is comparatively so simple, that there is a just hope of the morbid condition of the kidneys becoming improved after the removal of the calculus, unless they have undergone some organic change; and even then improvement may be expected from the removal of so great a source of irritation." (p. 581.)

"Although circumstances may arise that may render the operation of lithotrity unsafe, it does not necessarily follow that that of lithotomy should be performed; for, as I have already mentioned, it is the duty of the surgeon first fully to ascertain that the general health of the patient renders him fit to be subjected to such an ordeal. In the first place, I should say the state of the urine should be strictly investigated; and if it be found, by the application of heat or nitric acid, to contain a large quantity of albumen, free from the colouring principle of the blood, the patient must be considered totally unfit, at least at the time, to be exposed to the hazard of the operation. The state of the heart and lungs, of the abdominal viscera, and more especially of the liver, should each be the object of close investigation;—I say especially of the liver, on account of the liability of that organ to disease, and not unfrequently to a fatty degeneration, concomitant with which the vital powers are invariably much diminished, and the subject of the complaint rendered incapable of sustaining the reparative efforts of the constitution. Such cases are not, however, to be despaired of: medicinal means should be employed, change of air and scene recommended to the patient, and he will probably be restored to a condition in which he may be able to undergo the operation with reasonable prospect of a successful result. When none of these cogent reasons exist for delaying the operation of lithotomy, there is a previous preparation which the patient ought invariably to be submitted to, and for which no general plan can be laid down, as it must in every instance be regulated by the constitutional peculiarities of the individual. Thus, in plethoric habits, it may be necessary to have recourse to depletion, and both bloodletting and purging may be requisite; indeed, I have generally found them more efficacious when conjointly employed than when either has alone been resorted to. In cases of great obesity, it may also be necessary to reduce a patient before the operation can be safely performed; and my friend, Mr. Green, some years ago had a patient from Manchester, who, when he first consulted him, weighed twenty-six stone: he was reduced, by strict dietetic discipline, to nineteen stone, and was then successfully subjected to the operation of lithotomy.

"It is also highly desirable to prepare the mind of the patient, as well as his body, for what he has to undergo; and for this purpose it is proper to describe to him the position in which he will be placed during the operation,—for I have not unfrequently seen a patient, particularly in the better class of life, who had heroically made up his mind to submit to the operation, and walked firmly up to the table, completely give way on learning the constrained position in which he was to be placed. It is also of great use to prevail on the patient, for a few days before the operation, to remain occasionally for five or ten minutes in the attitude of grasping the soles of his feet, in order to accustom the muscles to so unusual a position; and this not only diminishes the terror, but also the distress, which would otherwise produce a great increase of excitement during the operation.

"The night previous to the operation, a purgative should be administered, and in the morning an enema given, for the purpose of securing a complete evacuation of the rectum. In persons of very irritable habits, it is also advisable, after the action caused by the enema has ceased, to inject about thirty drops of laudanum, in an ounce of gruel, into the bowel, with the view of lessening the muscular irritability of the parts, and also to produce a beneficial sedative effect after the operation has been performed. Let me here suggest that the surgeon himself should be certain that all these preparations have been effected before the appointed period for the operation, so that when the time arrives, the patient's mind may not be kept in agitation by unnecessary delay." (pp. 587—589.)

"In the course of my practice I have performed the operation of lithotomy on 134 different individuals, and my average of success has been fourteen out of fifteen. A large number of these were, however, young persons, and in them the danger is far less than in patients who have attained the age of puberty.

"I cannot help thinking that the operation of lithotrity is fast superseding that of lithotomy; and although the above statistics would encourage us to look forward with some confidence as to the result of the latter operation under commonly favourable circumstances, I do not consider that it ought ever to be adopted, and the danger incurred (which must always to a certain extent be present), in any case in which the safer operation of lithotrity can be employed." (p. 610.)

We must conclude with extracting the two following cases of entrance of air into the veins: the first of which is perhaps the most remarkable instance of the event that has yet been recorded:

"The following case occurred to Dr. Willis, of Barnes:—On the 25th of March, 1848, Dr. Willis performed the operation of putting a seton into the neck of a man suffering from chronic laryngitis: the seton needle was inserted about two and a half inches above the superior edge of the sternum: at the moment of its entrance, Dr. Willis heard a peculiar hissing sound, so that he thought he had opened a subcutaneous abscess communicating with the windpipe; a glance at the face of the patient told, however, of a more serious accident—the sound was produced by the entrance of air into a small vein that had been punctured in the operation. The man was deadly pale; he fainted, then became rigid and convulsed. Dr. Willis immediately compressed the orifice, to prevent the ingress of more air, and sent off to Putney for Dr. Cormack. Upon consultation it was decided on to try the effect of bleeding from the arm; the pulse, which was almost imperceptible, then became good, and the patient appeared to be in many respects relieved. Mr. Syme was now sent for; he concurred in the treatment that had been adopted, and suggested the application of warm fomentations to the feet, and the administration of a little wine: the patient did not, however, rally after this time, but continued in the same state until four o'clock in the afternoon, when he died.

"After death the body was examined: it was found that the jugular veins and the large vessels of the neck were uninjured: but the right auricle and pulmonary artery were distended with frothy blood, and the lungs were emphysematous.

"A young lady at Camberwell, about nineteen years of age, had a tumour in the neck; she was placed under the care of Sir Benjamin Brodie, and he determined upon its removal. After commencing the operation, he found that the internal jugular vein passed directly through the abnormal mass, and that it would be impossible to remove the latter without wounding the vein. He therefore placed a ligature upon the vein, above and below the points where his incisions were to be made in extirpating the tumour. In separating the tumour from its attachment to the surrounding structures, a small opening was accidentally made in the vein, unfortunately a little below the proximal ligature; a loud whizzing sound was immediately heard; the patient became suddenly insensible, her face was pallid, and there were present all the symptoms described as attendant upon the entrance of air into a vein: indeed, in this case the symptoms at the time were so urgent, that at one moment it was thought that the patient was dead; but after persevering in the use of stimuli, dashing cold water on the head and face, application of ammonia to the nostrils, and its administration internally, she began to show signs of returning animation, and, what is very remarkable, after a short time recovered without suffering from any of the distressing and protracted symptoms generally attendant upon this accident." (pp. 811, 812.)

We now take our leave of Mr. Cooper. The blemishes of the present work wholly result from want of diligence and method in digesting the copious stores of accurate information possessed by the author; and when next we meet him in print, we trust that no similar grounds for complaint will occur.

ART. X.

Translation of the Pharmacopœia of the Royal College of Physicians of London. 1851. *With Notes and Illustrations.* By RICHARD PHILLIPS, F.R.S., L. & E., F.G.S., late President of the Chemical Society, Curator of the Museum of Practical Geology, &c.—*London*, 1851. 8vo, pp. 567.

As we have so recently reviewed the new Pharmacopœias, and other works professing to be based on them, and as the present volume is but a new edition of a work well known to the public, and highly and deservedly appreciated, it will not be requisite for us to devote any lengthened space to its consideration, and we shall content ourselves with noticing some of the principal differences between it and its predecessor. The present edition of the 'Translation of the Pharmacopœia' was entrusted, on account of the lamented death of Mr. Phillips, which took place last May, soon after the appearance of the new 'London Pharmacopœia,' to Mr. Denham Smith, a former pupil of the author's, who thus speaks of himself in the preface:

"For the grateful task of editing this, the author's last work, I possessed both the requisite leisure and a long-standing acquaintance with his scientific views and methods of investigation; and most amply shall I be rewarded should I be deemed to have discharged this duty without detracting from the well-established reputation of the work."

A portion of the volume, it appears, was in the press at the time of the author's death, and for its completion the editor was supplied with copious manuscripts and explanatory notes by his widow.

Although a few foot-notes have been introduced by the editor, chiefly on points which he has himself investigated, yet throughout the work, in the remarks which are appended to the various preparations, no distinction has been made between Mr. Phillips's labours and his own; and we are therefore led to infer that the substance of these has been chiefly derived from the former source. The size of the book has been increased by 175 pages; this increase being due chiefly to the more full description of the chemical composition of the various substances of the Materia Medica; but partly, however, to the retention of many of the old processes of the 'Pharmacopœia' of 1836. In the former edition, although the chemistry of many drugs was given, yet, on some points, the information was very meagre, and on others altogether wanting. This defect has been completely remedied in the present edition. As an example we will turn to *Enema Tabaci.* In 1836, the only observations were: "A very drastic enema, recommended by some in cases of hernia, but with doubtful success." In the present edition, however, a rather full account is given of the composition of Tobacco, of the chemical properties of its active principles, and the adulteration to which the drug is liable; and as the subject of tobacco has recently acquired some additional interest to members of the medical profession, from the late case of poisoning with its active principle in Belgium, by Count Bocarmé, and as these remarks will at the same time afford a good idea of the mode in which this part of the work has been executed, we shall lay them before our readers.

"Tobacco is the leaf of a plant belonging to the natural family *solanaceæ;* the fresh leaves contain nicotina, nicoticium or tobacco camphor, bitter extractive, gum, chlorophyll, albumen, gluten, malic and citric acids, lignin, salts of potash, lime and ammonia, silica and water, to which some analysts would add tannin and gallic acids, a yellow oil and resin, a substance analogous to morphia, an orange-red colouring matter, and nicotic or tabacic acid, the existence of which is, however, somewhat doubtful. Of these principles, the most active are nicoticium, and *nicotina,* which exists in large quantities in tobacco, and may be procured by digesting the extract of the leaf with spirit, treating this tincture, when concentrated, with solution of potash, and afterwards with ether, which dissolves the crude nicotina. To this solution powdered oxalic acid should be added, which combining with the nicotina forms a heavy liquid; this, after treatment with fresh ether and subsequently with potash, must be heated in the presence of hydrogen gas at a temperature of 284°, which drives off all the volatile impurities. When the temperature is raised to 356°, pure nicotina distils over. This liquid, colourless, with an acid smell and taste, does not solidify at 20°, boils at 474°, when it decomposes; it exhibits alkaline reactions, combines with several acids to form salts, and also with some salts to form double salts, is soluble in water, alcohol, ether, and in oils. It dilates the pupil of the eye, and is a most active poison. Nicotina, according to the analysis of Melsens, which has since received ample confirmation, although it has been proposed to double the formula, consists of

Twenty equivalents of carbon	$6 \times 20 =$	120 or	74·08
Fourteen equivalents of hydrogen . . .	$1 \times 14 =$	14 or	8·64
Two equivalents of nitrogen	$14 \times 2 =$	28 or	17·28
Equivalent		162	100·00

Formula $C_{20} H_{14} N_2$

"*Nicoticium* exists in dried tobacco; it is solid, volatile, and insoluble in water, but dissolves in ether and the fixed alkalies; it does not appear to be so poisonous as nicotina.

"*Adulterations and Tests.*—Sugar, treacle, honey, various vegetables, liquorice, salts of various kinds, and earthy matter; but it is not necessary here to describe the proper tests, as any adulteration of the tobacco used for making the enema is not likely to materially affect its action."

It will be observed, on comparing the two editions, that considerable alterations have been made in the diagrams; but we find nothing in them very special to remark upon, they being simply those every day employed by chemists and writers on materia medica, instead of the rather peculiar forms previously made use of.

With regard to the chemistry of the work, we may remark that it has been completely, or nearly so, brought up to the present time; and although there are a few errors, yet it has been, in our opinion, very greatly improved. There is, however, one point, connected with this part, on which we must make a few comments; and this is, the introduction of most of the old formulæ for the preparation of substances now placed in the lists of the materia medica, and for which no processes are now given. When reviewing the little work of Dr. Lane, in our last number, we made some remarks on this head, and stated that we considered that they unnecessarily enlarged a work professing to be only a compendium of Materia Medica. Now, without for one moment wishing to compare the above-mentioned work with the one before us, still we must say, that although a knowledge of some of these processes is useful, and the very faulty ones are here omitted, yet as the book professes to be a 'Translation of the Pharmacopœia with Notes

and Illustrations,' these old processes were not required; for many of them are certainly not such as are employed by the manufacturer, while of such as are known to be in use, the details are easily obtained from the very many works on materia medica and pharmacy which abound both in this country and abroad. A few processes have been omitted, and others substituted for them; thus, Berzelius's method of obtaining veratria has taken the place of the old and faulty one of the last Pharmacopœia; we notice also that, under Infusum Valerianæ, a detailed method of preparing valerianic acid from the root has been given, but the process of preparing this acid from fusel oil, now officinal in the new 'Dublin Pharmacopœia,' and which is the only one practicable (the yield from the root being too small to make it repay the cost of its production) has been only mentioned. This process of the 'Dublin Pharmacopœia' we have already had occasion to refer to, in our notice of that work in our last number.

Under the head of Medicinal Uses, the alterations are by no means important; and being written by non-professional gentlemen, are, as we should expect to find them, very short, and occasionally show evidence of a want of medical knowledge, as, for example, when, under Liquor Morphiæ Acetatis, it is stated that "the advantage which they (the salts of morphia) seem to possess over opium, is chiefly ascribed to the absence of narcotin." Now we know that this principle is devoid of all power of producing the disagreeable effects which sometimes follow the use of the crude drug.

We had occasion, in our review of the new pharmacopœias, &c., to speak of the strength of the Tincture of Opium; and we then objected to the commonly received statement, that nineteen minims of the tincture were equivalent to one grain of opium, asserting that its strength was much greater. Perhaps, in the work then under notice, the statement could scarcely be considered a fault, the same having been asserted by so many writers on materia medica and pharmacy, and amongst others, by the late Mr. Phillips,—it being, indeed, from his assertion, contained in a former edition of the present work, that the statement which we affirmed to be erroneous was derived. As it is of much importance to the medical profession and the public that correct views on this point should be entertained, we shall make a digression, in order to discuss it.

Our readers will find that in 1836 Mr. Phillips makes the following remarks under Tincture of Opium:—

"Its specific gravity I find to be about 0·952 when properly prepared with proof spirit, as directed in the Pharmacopœia; about nineteen minims contain one grain of opium; this was proved by boiling down the tincture, and also by determining the quantity of opium left undissolved. It will appear from what has already been stated that proof spirit is a much better solvent of opium than cold water; for the latter dissolves less than three-sevenths of the opium, whereas proof spirit, as I found in preparing the tincture, dissolves more than two-thirds of it."

In the present edition the same observations are made. As the last edition was published "by permission," and therefore authorized by the College, it is not to be wondered at that the members of the medical profession, as well as others occupied in the dispensing of the preparation, should have received the statement as correct; many, perhaps the majority, without even remarking upon the difference in the amount of opium ordered to be used in the formation of the tincture, and the strength of the prepa-

ration indicated in the authorized translation of the Pharmacopœia ; yet, if only a simple calculation is made, the discrepancy must appear very great. We observe that Mr. Phillips bases his assertion on the fact that nineteen minims of properly prepared tincture yield, on evaporation, one grain of solid extract, and that a certain amount of matter, which he calls opium, is left undissolved. If this calculation be correct, it must necessarily follow that *the undissolved and the dissolved portions are of the same medicinal strength ;* but on consideration this appears very improbable, since we know that the salts of morphia, which seem to possess the chief active or medicinal virtues of the drug, together also with most of the so called resinous matters which, next to the morphia-salts, appear most active, are dissolved by the proof spirit ; whereas the greater part of the *narcotin* is left in the dregs, together with certain insoluble impurities. We were therefore disposed to consider Mr. Phillips's statement as erroneous ; and we found, on referring to many works on materia medica, that although in some, as in those of Dr. Pereira, and the late Dr. A. T. Thomson, the same statement was made, yet that in many others objections were raised to the calculation above alluded to ; these works were those of Dr. Christison, Dr. Neligan, Mr. Squire (the two latter works being under review at the same time), and also Dr. Royle's 'Manual.' Moreover, we had reason to believe that when the statement of Mr. Phillips was followed, it was often coincided in somewhat unthinkingly ; whereas those who objected to it, did so from having had the subject brought more especially under their notice. We imagined that much thought could not have been expended on the subject by those agreeing with Mr. Phillips, from finding that when certain *other* preparations of opium were spoken of, and the strength given, the calculations were based on the quantity of opium used, and not on the amount of matter dissolved ; for a proof of this we have only to refer to the remarks made by some of the authors above alluded to, on the Tinctura Opii Ammoniata of the 'Edinburgh Pharmacopœia,' and also to Mr. Phillips's own remarks on the Tinctura Opii Camphorata, which are based on the supposition that the whole of the active principles of the opium are taken up in each case, and that the medicinal strength is therefore not to be reckoned by the quantity of solid matter dissolved. Since the appearance of our last number, we have found that the view we then held on the subject is also taken by writers on the other side of the Atlantic ; for Dr. Wood, in the new edition of the 'United States Dispensatory,' remarks, that—

"The dose, equivalent to a grain of opium, is about thirteen minims, or twenty-five drops. Mr. Phillips, in his translation of the London Pharmacopœia of 1836, states that, by evaporating the tincture, and also by determining the amount of solid matter left undissolved, he found the preparation to contain one grain of opium in nineteen minims ; and this quantity, therefore, is given as the dose equivalent to a grain of opium. But this mode of calculation is obviously fallacious, as the portion of the drug dissolved is much more active than that left behind by the menstruum."

The only difficulty that appeared to us to attend the estimation of the strength of the tincture, lay in the somewhat uncertain state of our knowledge of the activity of the dregs ; especially as we found it remarked by some writers that morphia had been obtained from the residuum, the quantity, however, not being given. This difficulty, however, has been recently quite

removed, as will appear from a paper by Dr. Garrod contained in the December number of the 'Pharmaceutical Journal,' which was read and discussed at the November meeting of the Pharmaceutical Society. Dr. Garrod, for the purpose of endeavouring to determine whether or not Mr. Phillips's statement was correct, and what was to be considered the real strength of the tincture, made certain experiments, which confirmed the assertion of Mr. Phillips as to the amount of extract obtained on evaporating the tincture, and also as to the amount of undissolved matters; but at the same time these experiments proved that Mr. Phillips's inference with regard to the strength was completely erroneous, since it was found that *the residue was inert even in half-drachm doses;* and that whilst it contained most of the narcotin (a fact well known), also some meconic acid, the quantity of morphia was so small as to be scarcely appreciable, as might be anticipated from the fact just stated. Dr. Garrod also pointed out a possible source of the morphia which *may* be found in the dregs—viz., that a portion of the tincture of opium might remain adherent to them. The residue employed in his experiments was first washed with a little cold water to remove such extraneous matters, and was obtained from the tincture of opium prepared in the ordinary way at University College Hospital, with only seven days' maceration, according to the directions of the present 'London Pharmacopœia.' The conclusion arrived at by Dr. Garrod, as to the strength of the tincture, was, that if estimated with regard to the ordinary opium as used by dispensers, 1 grain was contained in 12 minims, or 5 grains in a fluid drachm; but if compared with dry opium, then 1 grain was contained in about $13\frac{1}{2}$ minims, or $4\frac{1}{2}$ grains in the drachm; the inference before arrived at by Dr. Christison, Dr. Wood, and many others.

The *error*, for we now feel additional confidence in thus designating it, is not confined to works on materia medica and pharmacy, but is also met with in others devoted to medical jurisprudence and toxicology. Thus, in the works of Dr. Alfred Taylor, Mr. Phillips's statement is not simply tacitly assumed to be true, but this gentleman has made experiments for himself; and finding that the tincture only yields $\frac{1}{20}$ of its weight of solid residue, he asserts that 20 minims are equivalent to 1 grain of opium instead of 19 minims. We find it difficult to conceive how Dr. Taylor can come to this conclusion, in face of the evidence which he himself brings forward on the other side; for we observe, in his work on Poisons, page 579, that even hot water poured on powdered opium in a bottle extracted almost all the meconate of morphia, and that but *very faint traces* of the salt could be found in a subsequent infusion: and again, in the following page, it is asserted that 3 grains of the extract of opium is equivalent to 5 grains of crude opium; and yet, when thus allowing that even the *watery extract* is stronger than the crude drug in the proportion of 5 to 3, he still makes the statement that the amount of *spirituous extract* yielded by the tincture is to be the indication of the actual quantity of the drug contained in the preparation. What would be thought if he asserted that a pint of vinum ipecacuanhæ was the equivalent of only 100 grains of ipecacuanha, because, as stated by Dr. A. T. Thomson, 100 grains of soluble matter are contained in it, whereas 600 grains of the root are used in forming this amount of the wine? And would not such a mode of calculating the strengths of other tinctures and wines be considered absurd?

We allude to the occurrence of this statement in the works referred to, because we consider it by no means unimportant, that when called upon to give evidence in cases of poisoning by laudanum, the medical man, on oath, should make near approximation to truth, and not assert, in a case of poisoning with half an ounce of tincture of opium, that an equivalent to 12 grains only of solid opium has been taken; since, provided the tincture is properly prepared, the person has, to all intents and purposes, actually swallowed as much as 20 grains of the substance which is always referred to by the name of opium, and which any one, without any extraordinary exercise of his mental faculties, can readily distinguish from the alcoholic extract.

In conclusion, to return to the work before us, we may say with confidence, that we consider the new edition of Phillips's Pharmacopœia very carefully executed, and equal in all respects to the former. Its additional size is perhaps scarcely equalled by the increase of practical information on the different pharmacopœia preparations; but the sudden and lamented death of the author will readily account for this circumstance. We have little doubt but that the work will maintain the well-earned reputation of former editions, and will be considered, as hitherto, the almost indispensable companion of those engaged in the practice of pharmacy and the study of materia medica.

ART. XI.

Mikroskopische Anatomie, oder Gewebelehre des Menschen, &c.

Microscopical Anatomy, or Histiology of Man. By Dr. A. KÖLLIKER, Professor of Anatomy and Physiology in Wurzburg. Volume Second: Special Histiology. First half; on the Skin, Muscles, Bones, and Nerves. —*Leipsic*, 1850. 8vo, pp. 554. With Four Plates, and 177 Wood-engravings.

THE title of this work contains a brief but significant recognition of a most influential truth in the science of organization. It imports that the intimate structure of organized beings lies beyond the reach of our unassisted eye, and yet may be faithfully brought within our cognizance by the art of the optician. No intelligent anatomist will now dispute this position; yet we remember the time when we could not have introduced a book with such a title to the notice of our readers, without having to combat a very general feeling of distrust of microscopical inquiries, leading men neither of mean authority, nor generally tainted with prejudice, to reject the microscope altogether as a means of anatomical research. That we have now no need to begin with "an apology for the microscope," is due mainly to the signal improvements made of late years in the construction of the instrument, to the great extension of its employment among various classes of scientific inquirers, and to the education in its use which they have derived from their own practice, or from the training of experienced instructors. The value of the kindred gift of optical science, the telescope, was much earlier appreciated. It is true, that the indications of the telescope can be, to a certain extent, verified by a nearer approach to such objects as are within our reach, a test which cannot be

applied to the microscope; but its more speedy rise into confidence must be ascribed, in a great measure, to its early application to the practical purposes of life, and to its consequent extensive employment by men engaged in ordinary vocations, as well as by philosophers. Hence the evidence soon acquired of its utility, and the demand for skill on the part of its constructors, readily responded to, in extending its power and correcting its errors. The microscope, of much more restricted use, long remained a defective, and, in its "compound" form, a most fallacious instrument. It is doubtless true, that in the hands of the first micrographers it cast a new and, for the most part, truthful light on the minute structure and hidden operations of the animal and vegetable organism; but its early promise was not maintained, and, with a few eminent exceptions, the microscopic observations of later years, dissonant and irreconcilable, offered a convenient ambiguity for futile physiological speculation, but were looked upon by sober inquirers as the fantastic creations of optical illusion, and utterly devoid of scientific value. From such indiscriminate condemnation, however, the microscope has now been rescued. Within the last twenty years the construction of the compound instrument has been signally improved, and its aberrations corrected; and scientific investigators have not been slow to avail themselves of its now more trustworthy services. The use of the microscope is no longer confined to a few, supposed to be initiated in its mysteries; every enlightened anatomist and physiologist now judges for himself respecting its indications. Nor are the possession of the instrument, and the skill to use it, restricted to men engaged in those scientific or professional pursuits in which its utility has been specially approved; for it has passed into the hands of many, who, like Leeuwenhoek and some others in earlier times, have been led to take an interest in the examination of various natural objects, simply because these furnish interesting materials for its employment. These causes have not only spurred on the ingenuity of the optician towards the perfecting of the instrument, but have contributed to give greater certainty and a more convincing degree of congruity to its revelations; and to this important revolution in the history of the microscope, must in great part be ascribed the no less striking advances in the knowledge of animal and vegetable structure and development, which have been made during the past quarter of a century.

But let none suppose, from what has preceded, that the book before us is a mere description of anatomical microscopic objects, and its author a *dilettante* micrographer. On the contrary, it is a most elaborate treatise on the textures of the human body,—their structure, chemical constitution, vital properties, and mode of development,—in which micrography performs its fitting part, but is ever kept in its due subordination; and no name has been more frequently or honourably associated with the progress of physiological anatomy for the last decennium, than that of Professor Kölliker.

The plan adopted by the author is to treat his subject under two divisions, general and special. The first will comprehend the history of the structural elements of the body, from the simple cell up to the complex fibre and tube, with their mode of development, their distribution in the body, and their combination into higher structures; but all in a

general point of view. This will form the first volume, which it has been deemed advisable to make the last in order of publication. In the special division it is intended to consider the several organic systems in succession—namely, their structural constituents, as well independently as combined in more or less complex organs, and also the organs themselves, both individually and in their mutual relations. It is expected that this division will occupy two volumes, the entire work thus extending to three.

The volume now produced, the second in order of place though the first to appear, contains the Cutaneous, Muscular, Osseous, and Nervous systems, and their dependencies. The author informs us that, in the execution of his task, he has above all things endeavoured correctly to determine every matter of fact; and, with this view, he has made it his duty to submit every point to actual examination by himself, so far as this was practicable. Whatever, after such scrutiny, he has found to be securely established, is so set forth. Doubtful matter has been critically discussed with a view to elucidation, or detailed as it stands without comment; since it seemed better to expose deficiencies, than to hide them by ill-founded speculation. Next to accuracy he has aimed at completeness, striving not only to bring his subject into a clear light, but to expose it to view on all sides. After a careful perusal of the volume, it appears to us that the author has, throughout, steadily and successfully adhered to these guiding principles; and this declaration will best serve to express our opinion of the general character of the work. Our further duty of making our readers more specially acquainted with its contents will be best fulfilled, by gathering from a vast amount of matter, valuable no doubt, but unsusceptible of condensation, what appears to possess the most interest, either as the fruit of new discovery, or as throwing further light on questions of moment. As, however, the contents of the first three chapters will afford ample materials for our selection on the present occasion, we shall defer our examination of that on the nervous system (consisting of 160 very full pages, and almost forming a treatise of itself) until the next convenient opportunity.

The subject treated of in the First Book is the *Cutaneous system*, under which head are included the true skin and cuticle, the subcutaneous areolar and adipose tissues, the nails and hairs, and the cutaneous glands.

In the corium, or cutis vera, the author describes a papillary and a deeper reticular portion; but admits that such division, though adopted by various anatomists, does not naturally exist, and he makes use of it merely for convenience of description. The length of the papillæ, on most parts of the surface, he finds to be from $\frac{1}{33}$ to $\frac{1}{22}$ of a line. On the palm, sole, and nipple, where they are mostly of the compound variety, their length is from $\frac{1}{20}$ to $\frac{1}{10}$ of a line, which is also their measure on the matrix of the nail and the labia minora. On the face they are reduced to from $\frac{1}{66}$ to $\frac{1}{40}$ of a line; and here they at parts disappear altogether, or are replaced by slightly elevated reticular ridges. Spots occur also in other parts of the body, where the papillæ are very scanty or altogether wanting. He can find no basement or limitary membrane on the surface of the corium in the adult; still as a fine film exists in that situation in the embryo, he thinks we may assume its presence at later periods, though then blended with the subjacent part of the corium.

Of greater present interest are the author's observations on the smooth or non-striated Muscular fibres of the integument. He has discovered such fibres, not only in the dartos, where they have already been recognised, but in the subcutaneous tissue of the penis and perineum, in the nipple and areola of the breast, in the hair-follicles, (to be afterwards referred to,) and in the corium itself. In the subcutaneous tissue of the scrotum and penis, including the prepuce, and of the forepart of the perineum, these fibres are strongly developed. They form well-marked bundles, somewhat flattened in shape, and surrounded with a fine inclosure of filamentous tissue and nuclear fibres. These bundles join with each other to form reticular superimposed layers, which are also connected at various points, and are especially well-marked under the skin of the scrotum, in the so-called tunica dartos. The general direction of the bundles in these parts is longitudinal, but they are crossed here and there, especially in the penis, by strong bundles running transversely. The muscular layers are separated from the parts beneath by a stratum of simple lax areolar tissue, but towards the surface they are immediately applied to the delicate corium, with its hair-follicles and sebaceous glands. These fibres also form bundles in the nipple and areola, but without the inclosing sheaths; they here belong chiefly to the corium itself; a smaller share, especially in the areola, lie in the subcutaneous tissue. In the areola they are disposed circularly; in the nipple they are partly circular, partly parallel to the axis, and form a close network, through which the milk-ducts pass. Smooth muscular fibres exist also in the superficial part of the corium wherever there are hairs, but are absent from that of the palms and soles, and other hairless parts. They are in bundles, like those of the dartos, and seem to be invariably connected with the hair-follicles. They would appear to spring from the superficial part of the corium, and then to slant downwards to be fixed into the hair-follicles below their sebaceous glandules.

The operation of these muscular fibres is sufficiently conspicuous in the corrugation of the scrotum and integuments of the penis, as well as in the condition of the skin termed cutis anserina, in which the muscular fibres protrude the hair-follicles with which they are connected, whilst they retract and depress the intermediate cutaneous surface. The same thing is seen in the erection of the nipple and contraction of the areola, which, according to the author's observation, will sometimes shrink in child-bearing women to near half of its original diameter, when the nipple is erected. It is interesting, moreover, to know, that in addition to the natural stimuli to which the contractile parts in question are obedient, electricity, applied by means of the magneto-electric apparatus, operates powerfully and unmistakably in causing contraction of the areola, and giving rise to decided cutis anserina within a certain space round its point of application.

In his description of the Subcutaneous Adipose tissue, the author calls attention to the fact, that in the innermost layers of the dartos, usually regarded as entirely destitute of fat, cells occur, which he reckons as undeveloped forms of fat-cells. They lie in rows along the vessels, and do not occur in clusters; some of them are filled with granules, others with clear fluid and a nucleus, thus resembling the fat-cells of the embryo, which are at first pellucid vesicles, then acquire granular, and at last oily contents.

Fat-cells, wholly or partially deprived of their oil, such as are often met with in emaciated or dropsical adults, invariably present a nucleus; and hence Professor Kölliker infers that it is persistent in all cases, though usually hidden. He has occasionally observed the nucleus in well-filled adipose cells from the marrow, and from muscles. He has also found in dropsical areolar tissue, fusiform and stellate cells containing a nucleus, with usually but a few fine fat-granules, also other granuliferous cells of much smaller size than ordinary fat-cells; and he has seen so many transitional forms between these apparently anomalous cells and true fat-cells deprived of their oil, that he is induced to regard the former as metamorphosed states of the latter.

Our readers must be aware of the interest, as well as the doubt, which attends the question, as to the mode of termination of the Nerves in the skin; and will therefore be glad to learn what light so acute and accomplished an observer as the author has been able to throw on this difficult point of anatomical inquiry. In accordance with previous descriptions, the author finds that in the superficial part of the corium the nerves form a plexus, becoming finer and closer as it approaches nearer to the surface, so that its branches at length come to contain but one or two fibres each. In this situation, the nerve-fibres undergo actual division, and for the most part into two branches separating at an acute angle. From this finest part of the plexus, fibres pass up into the papillæ, and form arches or loops at their summits. The division of the nerve-fibres (at least after they lose their dark borders) may be seen with no great trouble in the skin of the mouse, but to discover it in the human skin is a matter of much difficulty; nevertheless, the author has succeeded in finding four decided examples of it in the skin of the glans. The loops in the papillæ, first admitted on the authority of Gerber, confirmed by Purkinje and Krause, but looked for in vain by equally competent observers, may, the author states, be traced with comparative ease, provided the skin has been previously steeped in a weak solution of caustic soda. What may be the relation of these loops to the branching fibres,—whether both legs of a loop are derived by division from the same fibre, or come from different fibres,—he is unable to determine; and he is in equal doubt as to the reality of the supposed termination of single fibres in the human skin by abruptly truncated ends. This last remark, of course, does not apply to the ends of cutaneous nerve-fibres which are inclosed in the Pacinian bodies.

Passing over an elaborate account of the development of the corium, and a critical discussion of the theoretical explanations which have been offered of the well-known facts discovered by E. H. Weber, concerning the distribution of the sense of touch in the skin, we come to the Epidermis, which the author divides, as usual, into the deep or Malpighian, and the superficial or horny layers. The former he describes as consisting in its deepest part of elongated, nucleated cells, resembling those of columnar epithelium, placed perpendicularly on the surface of the corium, in immediate contact with it, and consequently without the intermediate layer of blastema with nuclei, described by many writers. The perpendicular cells usually form only one, but in some places two or three strata. Confirming Krause's statements, he finds that in the skin of the negro and the dusky parts of the European skin, the dark colour is chiefly due to cells stained

throughout (except the envelopes) of a deep brown, and having brown coloured nuclei usually of a still deeper tint. Distinct pigment granules occur in a few cells, but in most the contained matter is not granular, and the colour is uniformly diffused through it. The darkest cells are nearest to the corium, and the colour fades or vanishes altogether in the more superficial layers. The Malpighian layer is not thicker in the negro than in the European, but its darkest-coloured cells have a deeper tint, and the colour extends further towards the surface of the cuticle, so that even the horny layer retains a tinge. For much interesting matter concerning the effects of physical and chemical agents on the cuticle, we must refer to the work itself; but we may notice Donders's method of displaying the cellular structure of the epidermis, nails, and similar tissues, which we have often employed with advantage. This consists simply in immersing the substance for a quarter or half an hour in dilute solution of caustic potash or soda, by which treatment the tissue swells up, and its component cells, though previously flattened and compacted together, become distended, and display themselves in a marvellously distinct and beautiful manner. The effect may be produced more speedily (in two or three minutes) by using a strong solution of the alkali, and then adding water.

Not being able to discover free nuclei in the epidermis, the author is disposed to think that its cells are multiplied by endogenous formation, although he frankly acknowledges the paucity of facts tending directly to decide the question. He is, moreover, of opinion, that in the full-grown person the constant growth and desquamation of the cuticular covering is brought about by the constant abrasion of the exposed surface; he thinks that the natural tendency of the cuticle is not to grow constantly, but merely to maintain its integrity through means of the plasma, or material of nutrition, furnished by the corium; but as a constant abrasion and loss at its surface are caused by foreign contact, a constant restoration, and therefore a continual growth, is necessary in order to maintain its integrity.

The account of the structure, growth, and development of the Nails is founded on a critical comparison of received statements with the results of careful examination by the author himself; it contains nothing remarkable on account of its novelty, but the subject is handled with his usual acuteness and perspicuity.

The subject of the Hair is treated at great length, occupying no less than fifty-eight pages, in which we meet with a good deal of curious and interesting matter. First, as regards the fibrous part of the hair, the author has succeeded in resolving the irregular, jagged fibres of which it is said to consist, into flattened cells of a fusiform outline. The colour of the fibrous part he ascribes, not only to oblong patches of pigment granules, but also to colouring matter of less intensity diffused through the substance. Very slender elongated nuclei are also discovered by means of reagents, whilst specks or marks of another description in the fibrous substance are occasioned by minute, irregularly-shaped cavities containing air. These lacunulæ are best seen in white hairs, in which there is no risk of deception from pigment-specks. Viewed with transmitted light they appear dark, but brilliantly white with reflected light. When a white hair has been boiled in water, ether, or oil of turpentine, these cavities are filled with the liquid,

and then become quite pellucid; but when a hair thus treated is dried, the air quickly finds its way again into the lacunæ, and they resume their original aspect. The clusters of fine silver-white granules in the medulla or pith of many hairs, which are dark with transmitted light, also consist of air-particles. The presence of air in feathers, and in the pith of the spines and hairs of quadrupeds, is well known; it appears also to have been noticed in the medulla of human hairs by Van Laer, and to have been previously assigned as the cause of whiteness in grey hairs by Roulin; still the granular masses in question have hitherto been very generally looked on as clumps of minute fat molecules, though their real nature may be easily demonstrated, as Professor Kölliker shows, by treating the hair in the way mentioned above. The root of the hair, usually swollen into a bulb or knob, is excavated to receive a papilla rising from the bottom of the hair-follicle, not only in the case of the vibrissæ, and other stout hairs of mammalia, but, according to the author, in the hairs generally, both small and large, of man as well as quadrupeds. The papilla is commonly of an ovoid shape, and is attached to the bottom of the follicle by a narrow base, or a sort of pedicle. He distinguishes three coats in the follicle; the outer one is composed of areolar tissue, with longitudinal bundles of fibres, and is the only one which receives vessels and nerves; the middle coat reaches as high as the entrance of the sebaceous ducts; it is composed of circular fibres, having the aspect of non-striated muscular fibres, but as they have not yet been perfectly separated into nucleated fibre-cells, and as nothing has been proved as to their contractile property, the author refrains from deciding on their real nature. The innerm st coat is homogeneous, and corresponds with the membrana propria, or basement-membrane of analogous structures. These three coats belong to the dermic part of the follicle; its epidermic lining, or "root-sheath of the hair," is now well known to microscopic anatomists. Some years ago, however, Henlé described a homogeneous perforated membrane as interposed between the outer or Malpighian, and the inner or horny layer, of which the root-sheath, like the cuticle generally, consists; and this—as to some it will no doubt appear—rather curious than significant matter, has been the subject of much controversy among German microscopists; several maintaining that the perforated membrane is merely a layer of somewhat flattened cells without nuclei, in which perforations are produced by accidental laceration, which is the opinion adopted by the author.

Next comes a description of the mode of development of the hairs in the embryo, founded on original observations. In substance it is as follows. The hair-rudiments appear as little pits in the cutis vera, filled with cells of precisely the same nature as those of the Malpighian layer of the cuticle with which they are continuous; so that it might be correctly said that the hair-rudiments are formed of little processes of the Malpighian layer, which sink down into the corium. A homogeneous limiting membrane next appears, inclosing the collection of cells, and continuous above with a similar simple film which at this time lies between the Malpighian layer of the cuticle and the cutis vera. The hair-rudiments now lengthen, and swell out at the bottom, so as to assume a flask shape. Cells are deposited outside the limitary membrane, which are eventually converted into, or give place to, fibres; and thus the dermic coats of the follicle are produced. But whilst

this is going on outside, the cells within the follicle undergo changes. Those in the middle lengthen out conformably with the axis of the follicle, and constitute a short conical miniature hair, faintly distinguishable by difference of shade from the surrounding mass of cells, which are also slightly elongated, but transversely with regard to the follicle. The papilla makes its appearance at the swollen root of the little hair; and the residuary cells contained within the rudimentary follicle form the root-sheath, the inner layer of which, lying next to the hair, is soon distinguished by its translucency from the more opaque outer layer that fills up the rest of the cavity. The young hair, continuing to grow, at last perforates the epidermis, either directly, or after first slanting up for some way between the Malpighian and the horny strata. In the former case it may, perhaps, be aided in its progress by the harder inner layer of the root-sheath, which accompanies the hair, and makes way for it through the cuticle. The author further thinks it not improbable that the eruption of the hairs is facilitated by the general desquamation and shedding of the superficial part of the epidermis, which occurs from time to time during fœtal life; more especially as the period of most thorough desquamation begins at about the same time as the first eruption of hairs.

A shedding of the first-formed hairs, or lanugo, is known to take place before birth; but, according to the author, only to an inconsiderable extent. On the other hand, he has observed that the infantile hairs are entirely shed and renewed within a few months after birth; those of the general surface first, and afterwards the hairs of the eyebrows and head, which he finds in process of change in infants about a year old. The new hairs are generated in the follicles of the old, as previously seen by Heusinger and Kohlrausch in quadrupeds; but the author describes the steps of the process somewhat differently from his predecessors. He conceives that an increased growth of cells takes place in the soft hair-knob and in the adjoining part of the root-sheath (its outer layer); the growing mass protrudes or lengthens out the lower end of the hair-follicle into a process, at the bottom of which is found the generative papilla, now, by the interposition of the new cell-growth, withdrawn from the root of the hair. The newly-formed mass of cells, occupying the lower or prolonged part of the follicle, and resting on the papilla, is gradually converted into a new hair with its root-sheath, just as happens in the primitive process of formation in the embryo; and as the new hair lengthens and emerges from the follicle, the old one, detached from its matrix, is gradually pushed nearer to the opening, and at last falls out, its root-sheath having previously undergone a partial absorption.

Concerning the Sudoriparous glands of the skin, Professor Kölliker observes that in the larger sort, such as those of the axilla, the tube is rarely simple, being more usually parted by repeated dichotomous division into several branches, which, also, before ending, give off short cæcal processes. In rare cases, the branches anastomose. He distinguishes the glands into those with thick and those with thin walls. The latter consist of a delicate fibrous outer coat, and a lining of polygonal epithelium-cells in two or more strata; the former possess, in addition, a layer of smooth or plain muscular fibre-cells disposed longitudinally between the fibrous coat and the epithelium. In neither variety, at least when fully

formed, could he discover a homogeneous membrana propria (basement membrane). The thick walled ducts, with muscular layer, are best marked in large sweat-glands, such as those of the arm-pit, the root of the penis, and the nipple. In most other places, the coats are thin, and without muscular fibres; but intermediate forms present themselves in certain parts, with partially developed muscular coats confined mostly to the part of the gland tube next its blind extremity.

The secretion of the sweat-glands generally is a thin, pellucid, colourless fluid, without definite particles; but in the axilla and areola of the breast, it is for the most part tenacious in consistence, and contains granules, pale or variously coloured, together with nuclei, and sometimes even cells or their fragments; and cells containing similar-coloured particles are seen in the epithelial lining of the gland-tube.

Scarcely any observations have hitherto been recorded on the mode of development of the sudoriparous glands, and accordingly the author's account of the process is in a great measure new. He states that the rudiments of these glands, when first recognisable in the embryo, have much the same appearance as those of the hairs, and, in like manner, consist of processes of the Malpighian layer of the epidermis, which pass down and are received into corresponding recesses of the corium. They are formed throughout of cells collected into a solid mass of an elongated pyriform, or rather club shape, continuous by its small end with the soft layer of the cuticle, and elsewhere surrounded by a homogeneous limiting membrane, which is prolonged above, between the corium and cuticle. The subsequent changes consist in the elongation of the cellular process, the formation of a cavity along its axis, at first without an outlet, the prolongation of its canal through the epidermis to open on the surface, and, in the meantime, the coiling-up of the gradually lengthening gland-tube into a compact ball, and the twisting of its excretory duct as it proceeds to its orifice. The original homogeneous membrane of the duct becomes thickened and is continuous with the surface of the corium, whilst an epithelium appears within, consisting of several layers of pale, polygonal or rounded cells. The ceruminous glands in the auditory passage are known to consist of a tube coiled into a rounded or oval ball, like the sweat-glands; and the investigations of Professor Kölliker show such a further correspondence between the two, in structure and in mode of development, as to lead him to regard the ceruminous glands as a mere local variety of the sudoriparous, which, as already noticed, present specialities in particular regions of the body both in structure and secretion.

The description of the Sebaceous glands of the skin, of their varieties in different parts, and of their mode of development, occupies a considerable space, and bears evidence of much careful investigation on the part of the author. His observations on the development of these glands are in a great measure new. He finds that they sprout like little buds from the sides of the hair-follicles, and are at first, in fact, excrescences of the external or Malpighian layer of the root-sheath, and are composed entirely of nucleated cells. Each little process soon assumes a flask shape and is at first solid; but in due time a group of cells containing fat-particles appears in its centre, and gradually extends itself along the axis of the

pedicle until it penetrates through the root-sheath, and the fat-cells thus escape into the cavity of the hair-follicle, and constitute the first secretion of the sebaceous gland. They are soon succeeded by others of the same kind, and the little gland is established in its office. Additional sacculi and recesses, by which the originally simple cavity of the gland is complicated, are formed by budding-out of its epithelium, as the first was produced from the epithelial root sheath, and are excavated in a similar manner.

It is thus shown that the rudiments of the hair-follicles, sweat glands, and sebaceous glands, are all derived from the same source. They all originally appear as solid, bud-like excrescences of the soft Malpighian layer of the epidermis, for the outer stratum of the root-sheath must be regarded as such; these grow down into the corium, in which recesses are formed to receive them, and which, of course, yields the material requisite both for the maintenance of their secreting function and for the production of new cells for their further growth.

The Second Book is devoted to the *Muscular system.* Under this title the author treats of the muscles with striated fibres, which serve for the motions of the skeleton, the skin, and the organs of sense, including, as dependencies or accessories, the tendons with their sheaths, the synovial bursæ, sesamoid bones, and fasciæ.

His description of the striated Muscular fibres agrees with that most generally received. They invariably possess a sarcolemma, and the nuclei which appear in them are attached to the inner surface of that homogeneous sheath. The history of the fibrillæ is reserved for the general part of the work; but the author takes occasion, in the meantime, to state his belief in the actual existence of such longitudinal elements, relying on the comparative frequency and facility with which fresh muscular fibres may be split longitudinally, and on the fact that the individual fibrils may be seen with perfect distinctness in the striated muscles of insects, whilst their tissue is quite fresh and uninjured, nay, even while they are actually contracting.

A fact of some interest respecting the striated muscular fibres has recently come to light—namely, that in some situations they divide into branches and also join together. The author, in 1849, described such divisions and anastomoses in the muscular fibres of the frog's heart; about the same time the ramification, and in certain cases also the anastomosis of striated muscular fibres were observed in some of the invertebrata by Leydig and Hessling. Since then, the author, in association with Corti, has found such anastomoses in the heart of man and various animals, and in the lymphatic hearts of the frog, and thinks it probable that they occur in the cardiac muscular fibres of all the vertebrata. In the tongue of the frog also, the muscular fibres, as they approach the surface, divide into numerous but not anastomosing branches, by which they are attached to the under surface of the mucous membrane. In regard to the last-mentioned fact, we may remark, that similar observations on the muscles of the frog's tongue had been made in this country by Dr. Augustus Waller, before the publication of the present work. Professor Kölliker, having since had his attention drawn to Leeuwenhoek's account of the muscular tissue of the heart of the duck, ox, and codfish, is disposed to consider the present as an instance of the rediscovery of a fact, pointed out long

before, but neglected or forgotten. But although we have long been aware of the statement of Leeuwenhoek, we must confess we always looked on it as referring to the subdivision and conjunction of fasciculi (composed of several fibres), and not of the fibres themselves; and on again carefully perusing his description, and comparing his magnified figures with what he states to be the natural size of the object, we see no reason for coming to a different conclusion.

On the subject of the connexion of the muscular fibres with those of tendons, the author observes that where the two kinds of fibres correspond in their direction, a muscular fibre gradually losing its cross striæ becomes continuous with a tendinous fasciculus of equal size; the muscular fibrillæ seeming to pass continuously into the filaments of the tendon, some sooner, others later, so that the connexion does not take place throughout the thickness of the fibre in the same transverse plane. The tendinous bundle, at first equal, or nearly equal in thickness to the muscular fibre, gets thinner as it recedes from the fleshy tissue. The sarcolemma does not surround the end of the fleshy fibre as a closed tube, but passes on to the tendinous bundle, where its further disposition is doubtful. When, on the other hand, the fibres of a muscle are inserted more or less obliquely into the side of a tendon, whose fibres take a different direction, the muscular fibres are attached by rounded or blunt conical ends to the side of the tendinous fasciculi; they are completely invested by their sarcolemma in the form of a closed tube, and are received into shallow pits or dimples in the tendon, whilst the connexion is further secured by the continuity of the areolar tissue of the muscle with the investing areolar tissue of the tendon.

Respecting the synovial bursæ and sheaths, the author had previously shown that they do not constitute perfect sacs, unless, perhaps, in some cases, where they lie between muscles. In those situated under tendons, or lining tendinous sheaths, the membrane is wanting over considerable portions of the gliding surfaces. On such naked parts, there is of course no epithelium; but the fibrous tissue immediately, or even for some depth beneath the surface, is intermixed with cartilage-cells of various forms, and converted into fibro-cartilage.

The question of the existence and mode of distribution of lymphatics in the muscular tissue, has hitherto received contradictory answers from the few anatomists who have inquired into the matter. Knowing the difficulty, as well as the indecisive character of attempts to inject lymphatic vessels in muscle, Professor Kölliker has tried to determine the point by microscopic investigation; and is disposed to conclude that small muscles are destitute of such vessels, and that the few lymphatics which seem to issue from some of the larger muscles, belong to their areolar sheath and its larger subdivisions. The lymphatics alleged to have been injected in the diaphragm by Fohmann and Arnold, he regards as belonging to its serous covering.

Our readers are doubtless aware of the doctrine of what is called the "looped" or sling-like terminations of the ultimate fibres of the nerves in muscular tissue, as also of the recent observations—held by many to be subversive of this doctrine—which have been made on the muscles of the pike by Müller and Brücke, and on those of the frog by Wagner; from which it appears that the nerve-fibres on approaching their termination in

a muscle undergo division, and at last end, not by loops, but by exquisitely fine, free extremities. But although there is no doubt of the truth of these statements, as applied to the muscles and nerves of cold-blooded Vertebrata, and of many Invertebrata, still Professor Kölliker has found that in the muscles of Man and other Mammalia, the looped or sling-like arrangement of the ultimate nerve-fibres, as described by Valentin and Emmert, holds good; and that their division and termination by free ends is exceedingly rare, and probably exceptional. In reference, therefore, to the muscles of man and mammalia, he rejects the inference from analogy, as inconsistent with actual demonstration, and adheres to the earlier view. He has further noticed that in the mammalia, the plexus by which a nerve is distributed in a muscle, is usually confined to a very small and limited portion of the muscle, and he denies that either in mammalia, fish, or amphibia, the ultimate nerve-fibres penetrate the sarcolemma. A mammalian muscular fibre is usually only crossed by a nerve-fibre, and is rarely long in contact with it; so that much of the muscular substance can have only an indirect connexion with the nerve. Nerves of small size accompany the branches of bloodvessels within muscles, but do not reach the capillaries; though destined for the vessels, these nerves sometimes communicate with the proper muscular plexus. The larger tendons have only such vascular nerves; the smaller tendons, the fasciæ, and tendon-sheaths, as well as the synovial bursæ of the muscular system, are, according to the author, altogether devoid of nerves.

The development of muscular tissue is given according to Schwann; the nuclei of the original formative cells are supposed to multiply by endogenous formation. The enlargement of muscles after birth, and probably also before, is ascribed to growth, in all dimensions, of its original fibres, without multiplication of them by fission or interpolation of new ones; the fibrillæ are of course increased in number.

The author calls special attention to the elasticity of the muscular tissue in the living and in the dead state. In the former condition, the property in question is not great in intensity, but remarkably perfect in its nature; so that while a living muscle, not in action, yields to a slight stretching force, it returns again completely to its original condition. After death, on the other hand, the elasticity is increased in force, so that stretching is more strongly resisted, but the shrinking after such forcible extension is less complete. He ascribes cadaveric rigidity to the occurrence of this change in the elastic property of muscle. It is known that vital contraction of the muscular tissue is accompanied by swelling of the fibres and approximation of their striæ; the same phenomena ensue in shrinking from mere elasticity; and the author does not look on the existence and modification of these striæ as essentially connected with the exercise of vital contractility. Similar changes occur in the fibrillæ, as indeed the striæ of the fibre are caused by corresponding markings of the fibrils. The cross marking of the fibrillæ, or "varicosity," as the author (employing a commonly-used but not very appropriate term) denominates it, may either be the result of original construction, or be caused by alternately greater and less consistency in successive parts of the fibril, by reason of which they are unequally affected when it undergoes changes in length, either from vital action or mere elasticity. The author adopts the latter

opinion; but we must wait for the appearance of the general part of the work, in order to learn more explicitly his views on the constitution of the fibrillæ. We must confess, however, that in so far as we have been able to comprehend them from what he has revealed in the present chapter, they do not appear to us readily to harmonize with the apparent structure of the fibrils, as seen, either in the stretched or the relaxed condition, under the microscope. Nevertheless, we agree with him, though on different grounds from those above stated, in attaching but secondary importance to the structure in question, as regards the essential conditions of vital contraction.

The Third Book, which extends to 126 pages, treats of the structure and development of Bone, together with cartilage and fibro-cartilage, and the ligaments and synovial apparatus of joints. In his account of the structure of bone, besides a very complete and clear exposition of known facts, the author gives the results of his own very numerous micrometric determinations. Concerning the ultimate structure of the proper osseous substance, he expresses himself with doubt, but seems disposed to assume, as most in harmony with the observations of himself and others, that it consists of an intimate mixture of inorganic and organic materials in form of firmly cohering granules. He has not been able to perceive a reticular fibrous structure in the lamellæ of bone, either before or after removal of the earthy part. It may be as well, however, here to remark, from our own experience, that the fibrous character of the lamellæ of softened bone is apt to be obliterated by the hydrochloric acid employed, which, in this respect, acts like acetic acid; and to counteract this effect, it is advisable to macerate the decalcified bone for two or three days in water, and then immerse it in alcohol. In regard to the lamellæ, the author further notes, that when viewed with transmitted light in a cross section, each of them appears to consist of a lighter and more homogeneous layer and a darker granular one, marked with short transverse striæ. The appearance here described has been already represented by Dr. Baly, in a figure which he has given in his translation of Müller's Physiology, and is well known to us. Much care is bestowed on the description of the lacunæ and canaliculi. Various writers profess to have seen nuclei contained in the lacunæ of perfectly-formed bone; and, on a careful search, Professor Kölliker was able to discover them in the osseous lacunæ in a person of eighteen years of age. The bones were first decalcified, and then fine sections were boiled, from one to three minutes, in a dilute solution of caustic soda. All the lacunæ, in this case, appeared to contain round or oblong pale greyish nuclei. They were brought into view, also, by boiling the softened bone for some time in water. He has often observed single instances of lacunæ containing nuclei, in recent as well as softened bones of other individuals; but he thinks that further inquiry is necessary, in order to determine whether the appearance is constant, and continues in more advanced periods of life.

On the subject of the marrow, we are informed that the author's observations—on this point conforming with the previous chemical determinations of Berzelius—tend to show that fat-cells occur very sparingly in the reddish and more fluid marrow of the cranial diploe and bodies of the vertebræ. Whilst, however, he thus finds a paucity of fat-cells, he has

observed in the marrow of those bones, and in that of the ribs and sternum, nucleated cells containing no fat, and, though here occurring normally, resembling those he had previously met with in the articular ends of the long bones affected with hyperæmia. In the bones first mentioned, the cells referred to exist regularly and constantly, not only in man but in quadrupeds; they are wanting in the long and short bones of the limbs, unless in hyperæmia of the marrow, as above stated; in the scapula, innominate bone, and facial bones, they occur in variable proportion.

In treating of the tissues which serve to connect the bones, or which enter into the formation of joints, the author (correctly, as we think) states that the thin layer interposed between the bones at the sutures is not cartilage, but consists of fibres like those of ligament with numerous nuclei. The yellow or elastic, and many of the ordinary, ligaments are attached to the surface of the bones immediately, and without the intervention of periosteum; in such cases, as well as in tendons similarly connected, of which there are many, it is not uncommon to find the white fibrous tissue mixed with cartilage-cells at the place of its junction to the osseous surface. The specialities of structure observable in the intervertebral, pubic, and sacro-iliac symphyses, and in the rib-cartilages, are given in a detailed description, which cannot well be abridged and presented here.

Good representations are given (figs. 97 and 98) of the rudimentary or crude kind of ossification, which encroaches on the deep part of articular cartilages where they adjoin the bone. The true nature of this process, which forms, or at least helps to form, Mr. Toynbee's imperforate "articular lamella," has been already recognised (see Todd and Bowman's 'Physiological Anatomy,' vol. i. p. 90—Sharpey, in 'Quain's Anatomy,' 5th edit. p. clviii.) It consists in a mere calcification of the cartilaginous tissue. Earthy molecules are first deposited in the matrix, giving it a granular and opaque aspect; as these increase in number, the calcified substance becomes compact and transparent. The cartilage-cells, surrounded and more or less encroached on by the earthy deposit, remain as cavities in the hard tissue, inclosing their nuclei and other existing contents, and presenting a certain resemblance to the lacunæ of bone. We may remark, however, that the resulting hard tissue is very different from ordinary bone. On steeping it in an acid, the residuum is not a soft organic basis, having a lamellar structure, and yielding gelatin, but merely the original cartilage now decalcified, and not to be distinguished from the adjoining cartilaginous tissue which had undergone no change. The cells, too, even in the hard state of the tissue, can scarcely be said to present canaliculi; and when the cartilage is freed from its earthy impregnation, they perfectly recover their original aspect, which is very different from that of the lacunæ, in the decalcified gelatiniferous matrix of ordinary bone. Professor Kölliker remarks that the calcified matrix of cartilage is for the most part fibrous, and this accords with the observations lately published by Dr. Leidy of Philadelphia (American Journal of Med. Sci., April, 1849), who finds that the matrix of articular cartilage contains extremely fine filaments running between the elongated groups of cartilage-cells, in a direction perpendicular to the surface of the bone to which it is attached. The calcified stratum is bounded towards the rest of the cartilage by a dark granular

and tolerably even border. The true, or at least ordinary, bone is readily distinguished by its Haversian canals, concentric lamellæ and canaliculated solitary lacunæ; it terminates towards the calcified cartilage by a deeply and irregularly sinuous limit.

In the account of the Synovial Membranes, given in this part of the work, a remark of some moment occurs respecting the odd little processes which grow from the edges of the vascular Haversian fringes, found not only in joints but in synovial sheaths and bursæ. The appendages in question, often pedunculated, are destitute of vessels, as stated by Mr. Rainey, who, as far as we are aware, first pointed out their existence. Besides epithelium, inclosing a little filamentous tissue, they often contain, according to Professor Kölliker, two or three cartilage-cells; and he states, that by enlargement, consolidation of their substance, and separation from their attachment, they give rise to the loose cartilage-like bodies occasionally found in joints.

Returning to the soft textures in more immediate connexion with the bones, we find the bloodvessels of the osseous tissue described much in accordance with existing statements, with this addition, that the coats of the arteries in the finer Haversian canals are reduced to an epithelium with a layer of filamentous tissue containing nuclei, and in some cases, to a homogeneous membrane with nuclei; the coat of the vessel in either case being in immediate contact with the parietes of the canal.

The statements of Purkinje and of Pappenheim, affirming the existence of nerves in the periosteum, are confirmed by the author. The nerves seen in this membrane, it is true, are for the most part destined for the subjacent bone, but some are for the periosteum itself. They are wanting in certain parts of the periosteum; still there is probably no bone without periosteal nerves on some part or other of its surface. These nerves, like the vessels, lie in the outer layer of the periosteum, and are derived, at least in part, from the larger nerves of the bone itself. In the main nervous bundles, the diameter of the primitive fibres mostly ranges from $\frac{1}{6000}$ to $\frac{1}{3000}$ of an inch; but in their further progress they are reduced, partly by real division and partly by gradual attenuation, to a diameter of from $\frac{1}{10000}$ to $\frac{1}{7500}$. No terminal loops could be seen; but in two cases there were observed, apparently, but not certainly, free pointed terminations. Adjoining the articular ends of many bones, the nerves appeared more abundant, ramifying and conjoining in the vascular areolar tissue over the proper periosteum, but not presenting any divisions of the primitive fibres, or any terminations which could be discovered.

The nerves distributed to the bone itself are thus described by Professor Kölliker:

"The nerves of the osseous tissue probably exist in all bones except the sesamoid and auditory, but they do not present the same arrangement in all cases. In the larger cylindrical bones they are disposed thus: Firstly, they enter the bone along with the nutritious vessels, as one, or—where two foramina exist—two considerable nervous offshoots conspicuous to the naked eye; and having reached the medullary cavity, spread out in the marrow, in company with the vessels, though not always attached to them; they then extend towards the ends of the bones, repeatedly branching, but, so far as I have been able to see, forming but few anastomoses. Secondly, all these bones, as I find in opposition to Engel, have also many finer nerves distributed in their apophyses, which enter into the spongy

substance along with its numerous bloodvessels, and branch out in the marrow. Lastly, very delicate nervous filaments pass even into the pores of the compact tissue of the diaphysis, along with the fine arteries which penetrate it, and they doubtless branch out there, although I have not yet succeeded in finding them in the interior of the compact part. The small cylindrical bones of the hand and foot agree with the larger; only, from there being no medullary canal, their numerous nerves are not so regularly separated into apophysal and diaphysal. Among the short bones, I have found the vertebræ extremely rich in nerves, especially in their bodies. These nerves enter from behind in company with the arteries and veins at that part (*venæ basi-vertebrales*), also in front and laterally along with the vessels, and spread out in the marrow of the spongy tissue. I have also observed nerves in the astragalus, calcaneum, navicular, cuboid, and internal cuneiform bones; the larger bones receiving several filaments, the smaller at least one. I have not yet examined the carpal bones with this view, but I do not doubt that they also possess nerves. In the scapula and innominate bone the nerves are very numerous. They for the most part enter with the larger vessels at the points formerly indicated, partly on the surface and partly in the vicinity of the articular cavities. Nerves can also be demonstrated without difficulty in the sternum and the tabular bones of the skull. As regards the latter, I have seen nerves in the new-born infant which entered the occipital and parietal bones by the emissary foramina, which at that time also contained an artery; and in the parietal, frontal, and occipital bones of the adult, microscopic nervous filaments may be discovered, sparingly, no doubt, but still here and there, along with the small arteries; they enter at the surface of the compact substance, and probably penetrate as far as the diploe.

"From these as well as from previously existing observations, to be noticed hereafter, it is plain that the bones are richly supplied with nerves, and it only remains to inquire respecting their source and their mode of termination. As regards the first question, various nervous branches proceeding to bones, as will appear in the sequel, have been already traced by preceding inquirers to cranial and spinal nerves as their source; thus, the diaphysal nerves of the femur, the tibia, and humerus, have been ascertained to come from the crural, tibial, sciatic, and perforans Casserii, and so also a nerve to the frontal bone is contributed by the supraorbital. On the other hand, there is only one spot in which an osseous nerve has been traced from a sacral ganglion of the sympathetic, so that it would seem as if the sympathetic had no essential share in furnishing these nerves. This supposition, indeed, derives confirmation from microscopic examination, which shows that the structure of the nerves of bone, both in their trunks and at their terminations, entirely resembles that of the sensitive branches of the spinal nerves. Thus, in the trunk of the diaphysal nerve of the humerus, one-third of the fibres range from $\frac{1}{2400}$ to $\frac{1}{2000}$ of an inch, the remaining two-thirds from $\frac{1}{6000}$ to $\frac{1}{3000}$; in its larger branches the prevalent size of the fibres is from $\frac{1}{6000}$ to $\frac{1}{4000}$, some, however, reaching $\frac{1}{2000}$, and in the finest ramifications they are only from $\frac{1}{10000}$ to $\frac{1}{7500}$. The proportions are similar in the principal nerves of the scapula, femur, and tibia. The fine nerves of the compact substance of the long bones have fibres measuring from $\frac{1}{7500}$ to $\frac{1}{6000}$, whilst a nervule in the parietal bone, itself only $\frac{1}{1000}$ of an inch in diameter, was found to be entirely composed of fibres measuring from $\frac{1}{3000}$ to $\frac{1}{2300}$. The larger of the nerves belonging to the vertebræ consist chiefly of fibres ranging from $\frac{1}{6000}$ to $\frac{1}{4000}$, but contain some as large as $\frac{1}{2500}$; whilst in their branches, as well as in the smaller independent truncules, the fibres are found to measure only from $\frac{1}{10000}$ to $\frac{1}{6000}$. In any case, therefore, it is clear that the majority of the osseous nerves are not branches of the sympathetic, in this respect agreeing with the periosteal nerves, with which they can often be shown to communicate, and which can also be traced from the nerves of the extremities; at the same time it must not be forgotten that the nerves going to the bones contain many fine fibres, which, here and there, are probably derived from the sympathetic.

"How the nerves of bone terminate, I have not yet been able to discover; and the reason will be readily understood by those, who know how difficult it is to trace them in the spongy substance and marrow. This much only can I say, that the nerves in the medulla at last give off minute branches consisting of one or two fine fibres and more or less neurilemma. It is further worthy of note, that in two cases I have found Pacinian bodies on osseous nerves before their entrance into the bone—viz., in the diaphysal nerve of the tibia, on which there was one of these bodies, placed about two lines from the point where the nerve entered the nutritious foramen, and on the largest nerve of the metatarsal bone of the great toe, which had two such corpuscles connected with it, also near its entrance. The ganglia said to have been observed on osseous nerves before their entrance into the bones, such as those mentioned by Gros, and those found by Engel in the perichondrium of the larynx, I have never been able to discover." (p. 338.)

The author remarks that there is still a dearth of observations respecting the nerves of the accessory structures connected with the skeleton. As to the ligaments, Engel could see no nerves in them, whereas Pappenheim states that he found nerves in a large proportion of human ligaments, and Halbertsma has traced the interosseal nerve of the leg down into the syndesmosis between the tibia and fibula. The author has not, as yet, been able to see nerves in the human ligaments, unless in the interosseous membrane of the leg; but he has no doubt that, like the tendons, they receive a sparing supply along with their bloodvessels. He is satisfied that the human cartilages are in the adult quite destitute of nerves; but doubts whether they are equally so whilst ossification is going on and vessels exist in them. He has seen very distinct, though minute, nerves in the cartilage of the nasal septum in the calf. In accordance with the previous statements of others, he finds numerous nerves in the articular capsules, but adds that these nerves belong principally to the fibrous capsules and the lax areolar tissue, exterior to the synovial membranes. From what he has observed in the knee-joint, it would appear that nerves also exist in the synovial membrane, but only sparingly, and they are most distinct in the large, vascular, Haversian fringes.

The subject of *Osteogeny* is treated of in great detail, occupying no less than thirty-seven pages. Two modes of ossification are recognised—namely, that which proceeds in cartilage, and that which takes place in a tissue different from cartilage. This distinction was adverted to by Ruysch, and afterwards more explicitly asserted by Dr. Nesbitt, towards the middle of the last century. But while the fact that the ossification of certain bones began and proceeded in membrane was recognised by several, though by no means by all subsequent writers, the author thinks that the histiological character of the soft ossifying tissue in question was not determined, until Dr. Sharpey, in 1846 (this date is supplied by ourselves), and he himself, in 1849, showed that it was made up of fibres, like those of areolar tissue, with interposed cells, similar to those occurring in ordinary blastema. Those bones which are originally represented by cartilages, he names *primary;* the others, *secondary.* The latter class comprehends the tabular bones of the roof of the skull—namely, the frontal, parietal, squamous part of the temporal, and upper angle of the occipital; also the tympanic ring, the nasal, lachrymal, malar, palatine, superior and inferior maxillary, the vomer, the external pterygoid plate, and sphenoidal turbinate bones. The remaining bones of the skeleton belong to the first or

primary class, and the so-called "primordial cranium" is constituted by those cranial and facial bones—not enumerated above—which are ossified in cartilage, together with the hyoid bone; it includes, also, certain cartilages or portions of cartilage which do not ossify (e. g., in the nose), and others which disappear.

But whilst bones which begin in cartilage extend themselves at their extremities and edges by the growth and ossification of the primary cartilage, they grow at their *surface* by the second mode of ossification. It is in this latter mode, for example, that the cylindrical bones increase in circumference.

In his account of the intra-cartilaginous ossification, the author describes the precursory changes in the cartilage as they are now pretty generally understood, coinciding in opinion with Mr. Tomes, that the longitudinal grouping of the cells is due to their endogenous multiplication in that direction. The invasion of the pellucid, faintly-striated lines of matrix between the cell-groups by granular deposit of earthy matter, is represented also in the manner already known. He conceives that the cartilage-cells also become partially calcified, and form lacunæ; and describes the most recently formed bone—that, namely, which immediately adjoins the cartilage—as perfectly compact and solid in structure. At a little distance from the limit where ossification is advancing, this originally compact osseous tissue becomes pervaded by intercommunicating cavities hollowed out in its substance by absorption. These excavations, at a still further distance from the ossifying margin, augment in size and become filled with a reddish substance, the fœtal marrow. A great part of this cavernulated bony structure is removed by further absorption, to make way for the medullary canal, in bones where that exists; but it remains towards the ends, and in the epiphyses, to constitute the cancellated tissue. This description accords in part with our own conception of the process; but we are not prepared to admit that the advancing bone is at first actually compact. The author infers that the cartilage-cells are calcified, and the newest osseous tissue thereby consolidated, from observations he has made on rickety bones; but in studying the normal process of ossification, it has always appeared to us that the bone when first formed has an areolar or loculated structure, corresponding in arrangement with the calcified intercellular matrix of the cartilage, the areolæ or loculi being of an oblong or fusiform shape, and enclosing the groups of cells, which are little, if at all, encroached on by the calcifying process. These cells are next absorbed from the primary bony loculi, which then open into each other, both longitudinally and laterally, by absorption of a portion of their party-walls. When they are traced at greater depths from the ossifying border by longitudinal and successive transverse sections, more and more of these cavities are found to have coalesced in the way described, and larger spaces are thus opened up. But a further step is necessary to complete this as yet rudimentary cancellated texture. The walls of the cavities become lined with new osseous matter, formed by the vascular blastema which has now been deposited within them, and laid on in layers observed to be more numerous as the structure is traced further from the ossifying limit, and constituting the concentrically laminated walls of the cavities of the cancellated tissue, so well seen in a section of decalcified

bone. The formation of this *secondary deposit* has been described by Todd and Bowman, Tomes, and Sharpey. The last-named observer has also pointed out the finely reticular structure of the lamellæ, and regards the process as precisely of the same nature as the sub-periosteal ossification, and that of the vault of the cranium, to be presently referred to.

In reading Professor Kölliker's account of the formation of the cancellar structure, we were disappointed at finding that no notice was taken of the gradual lining of the walls of the cavities by laminated osseous deposit, as above described; and we had almost arrived at the conclusion that he was not aware of the fact, until we met with a passage, nine pages further on, in which he thus alludes to it:

"It must not be forgotten . . . . that also in the cancellated substance which arises in cartilage—e. g., in the apophyses—secondary deposits appear to exist, similar to those of the Haversian canals and of that part of the spongy tissue which is formed by periosteal deposition, only not so much developed." (p. 373.)

It has long been known that growing bones increase in thickness by deposition of bony matter under the periosteum, while absorption goes on within; and it has very generally been understood that the superficial deposit takes place in cartilage, which is supposed to grow in this direction as well as at the ends of the bone. But according to the author, it has been more correctly shown by Dr. Sharpey (1846) and himself* that the sub-periosteal ossification does not take place in cartilage, but in a layer of soft blastema, containing fibres and oblong granulated cells, a material, in short, which might not inaptly be compared to fibrous or areolar tissue in an immature condition. Some recent writers, who oppose this view, regard the sub-periosteal deposit as merely a particular form of cartilage, or, at least, of fibro-cartilage. But if the presence of granular cells be held sufficient to confer the character of cartilage, every tissue which arises from a cellular blastema—and nearly all thus arise—must be regarded as having passed through a cartilaginous stage; and to maintain, on the other hand, that because a tissue containing cells undergoes ossification it is therefore cartilage, would manifestly be assuming the point at issue. But the substance in question differs from cartilage, not only in histiological character, but also, as far as inquiry has gone, in chemical nature; for, from experiments by Dr. Scherer and the author on the growing bones of the calf, it appears that the ossifying sub-periosteal tissue yields only gelatine, at any rate no chondrin. The periosteum itself, strictly speaking, does not undergo calcification, only the fibro-cellular tissue underneath it.

Professor Kölliker's account of the formation of the compact osseous tissue by this sub-periosteal growth, and of the lamellæ of the Haversian canals, from prolongations of ossifying blastema connected with, and similar to, the sub-periosteal fibrous layer, agrees with that given by Dr. Sharpey, being only more detailed on certain points. The same remark applies to his description of the ossification of the tabular bones of the skull, which, as already said, are formed in membrane by the ossification of a fibro-cellular non-cartilaginous blastema. But Professor Kölliker has made the interesting observation, that in some cases, when a bone commences in a non-cartilaginous blastema, a certain amount of real cartilage may be

* Zweiter zootomischer Bericht der Univ. Würzburg, 1849.

subsequently generated at some of the borders or angles of the bone, and may undergo ossification in the usual way. A most remarkable example of this kind is afforded by the lower jaw, in which, though the chief part is formed in fibro-cellular tissue, the condyle is ossified in superadded cartilage. The angle of the jaw, too, is cartilaginous in the calf; and the anterior ends of the originally separate halves of the bone are completed in a half-fibrous half-cartilaginous mass.

The medullary spaces, as soon as they appear, become filled with fœtal medulla. This, according to the author, consists originally of merely a fluid, with many roundish cells, containing one or two nuclei and granular matter. In process of time, these cells are developed into areolar tissue, bloodvessels, fat-cells, and nerves. He has, moreover, observed, in the soft tissue within the growing Haversian canals, peculiar cellular bodies, of a flattened figure, with a round, oblong, or jagged outline, and of firmly granular substance, containing from three to twelve or more vesicular nuclei and nucleoli. They measure from $\frac{1}{1200}$ to $\frac{1}{600}$ of an inch or more. Professor Kölliker is unable to decide on the nature and purpose of these bodies; but on the whole is disposed to look on them as masses of cytoblastema, in which nuclei are generated, growing afterwards into cells, and eventually forming medullary fat-vesicles, or becoming incorporated in the osseous substance, and giving rise to lacunæ.

The mode of origin of the osseous lacunæ and canaliculi is, as our readers must be aware, a much-vexed question. Schwann supposes each canaliculated lacuna to be formed by a cell, which sends out ramose processes, like certain kinds of pigment-cells. Todd and Bowman think it is not a cell but a nucleus, which branches out to form the lacuna; the substance of the nucleus being afterwards absorbed. According to Henlé, earthy matter is deposited on the inner surface of the wall or envelope of a cell, leaving certain points uncovered, and the deposition thus continues towards the centre, which remains vacant as the lacuna, whilst channels corresponding with the uncovered spots, and reaching from the circumference of the now partially calcified cell to the central lacuna, are left to constitute the canaliculi. This process corresponds with that whereby the so-called "pore-cells" or "dotted cells" are partially filled up with solid matter in the hard tissues of plants. Professor Kölliker adopts the last-mentioned explanation, but with an addition of his own. In support of it, he lays great stress on an observation he has made on the ossification of the cartilaginous ends of growing bones affected with rickets. In such cases, the cartilage-cells are calcified before the adjacent part of the matrix; so that the successive stages of the process, as exhibited in cells of different states of advancement, can be readily traced; and he thus noticed, in perfect conformity with Henlé's view, that the hard matter is first deposited at the circumference of the cell-cavity, and then extending inwards, gradually fills it up, except a residuary vacuity in the centre, with small radiating channels, constituting a lacuna and canaliculi. We must confess that we attach less significance than the author does to this observation. Possibly, nay, very probably, lacunæ may be formed from cells; but it is clear, from Professor Kölliker's own showing, that very few indeed of the lacunæ found in a full-grown bone can have arisen from *cartilage-cells,* seeing that but an inconsiderable

part of its osseous substance is formed from cartilage. Again, if we look at a transverse section of the shaft of a bone, and imagine a line drawn round a lacuna so as to circumscribe it, together with its canaliculi, and thus represent the circumference of the cell in which it is supposed to be formed, it will be obviously seen that the included space is not filled up by concentric strata, nor even by amorphous deposit, but is crossed by numerous Haversian lamellæ, which would also intersect the circumference of the imaginary cell. The intercommunication of the canaliculi of neighbouring lacunæ is another difficulty which has occurred to Professor Kölliker himself; and to obviate this latter objection, he has eked out Henlé's hypothesis, by supposing that after the lacuna with its canaliculi have been produced within a cell in the way above described, the latter are subsequently lengthened out and extended through the surrounding hard tissue by absorption. Thus propped up, the hypothesis may meet both objections; but Schwann's explanation being more simple, will probably be found more satisfactory, especially when we keep in mind that the blastemal cells, from which nearly all the lacunæ in a bone must be formed—supposing these arise from cells at all—have never been seen to undergo the partial concentric calcification observed in cartilage-cells, to which so much (as we believe) undue importance has been attached.

We still consider the question of the formation of the canaliculated lacunæ as by no means settled; and with this remark we take leave of the subject, and, for the present, also of Professor Kölliker's most able and elaborate work; in which, copious as it is, the most accomplished anatomist can yet scarce open a page without finding instruction.

Art. XII.

1. *History of a Successful Case of Ovariotomy.* By E.W. Duffin, Surgeon. *With a Description of the Morbid Anatomy of the Sac.* By Robert Lee, M.D., F.R.S., &c. &c.
2. *An Analysis of* 162 *Cases of Ovariotomy, which have occurred in Great Britain.* By Robert Lee, M.D., F.R.S.

(From the 'Medico-Chirurgical Transactions,' Vol. XXXIV. *London,* 1851.)

The interest which has been recently excited in the profession on the subject of Ovariotomy, has induced us to anticipate a little the notice we are accustomed to bestow on the 'Transactions of the Medico-Chirurgical Society,' for the purpose of bringing under the consideration of our readers at the earliest moment possible, the remarkable statistical paper which Dr. Lee has contributed to the volume that has just appeared. As connected with it also, and as ushering in the valuable series of papers which we shall take the first opportunity of analyzing, we shall now give some account of the paper which stands first in order in the present volume. The operation of Ovariotomy has now been so frequently performed in this country and elsewhere, that we presume a simple record of a successful case would not have found its way into the 'Medico-Chirurgical Transactions,' except as displaying some circumstance of unusual interest or importance; and accordingly it is to the Supplement of Dr. Lee that we

must look for an explanation of the insertion of Mr. Duffin's paper, as the case itself differs little from many others that are familiar to the profession.

On the 23rd of August, 1850, a woman, 38 years old, applied to Mr. Duffin on account of an ovarian tumour which distended the abdomen to the size that it attains in the last month of pregnancy. The patient was in good health, and was very urgent for the removal of the disease, in consequence of severe neuralgic pains which it occasioned in the right thigh, from pressure on the sciatic nerve, as was afterwards proved. It is not stated whether she had ever borne children. The surgeon in this case quickly made up his mind both as to the nature of the tumour, and what to do with it; for on the fifth day from his first seeing the patient, he opened the abdomen, and fortunately meeting with no adhesions, quickly relieved the sufferer from the burden which had oppressed her for between seven and eight months. Following the plan which, as will be seen hereafter, has been attended with considerable success, he first examined the nature and connexions of the tumour through an incision in the abdominal wall, of sufficient size to admit the forefinger; and there being nothing discovered to contraindicate the removal of the disease, this opening was enlarged to the extent of nearly three inches; and the contents of the sac, amounting to 130 fluid ounces, having been evacuated through a trocar, the cyst was drawn through the aperture. The root of the tumour contained three large arteries and a considerable vein, which were secured by a double ligature passed through its centre, and tied on each side; another ligature was then passed round the whole neck, and the cyst removed by dividing its attachment, which was about an inch and a half in length. The ligatures were stitched in the wound, with the view of preventing the cut surface from receding into the pelvis and setting up inflammation when it commenced to suppurate; and this contrivance was found to answer its purpose in every way. The wound closed by the first intention, reopened on the ninth day, and on the fifteenth the ligature and slough escaped. In less than three weeks after the performance of this formidable operation, the patient was moving about again, and has since returned to her usual employment.

Dr. Robert Lee carefully dissected the tumour, and has recorded some particulars not generally noticed. The cyst was composed of three layers, —a peritoneal covering, a middle fibrous coat, and a thick membranous sac, in which the fluid had been contained. The peritoneal coat retained the usual characters of that membrane. The middle coat was composed of several layers, to which arteries and nerves, with ganglionic enlargements, were distributed. And the internal coat presented the interesting appearance of being composed, like the Graafian vesicle, of two perfectly distinct membranous layers.

The following paragraph we extract in Dr. Lee's own words, as they probably best explain the peculiarity which he discovered in the formation of this multilocular cyst. It will at once be observed that this account differs from that given by Dr. Hodgkin of the usual development of compound cystoid growths; whilst it accords with the views of Rokitansky:

"Imbedded in the middle coat, near the root, is another and much smaller cyst, with a lining membrane which presents, on the inner surface, precisely the same appearances as those seen on the inner surface of the larger cyst. The lining

membrane of this smaller cyst is likewise composed of two distinct layers, like that of the larger cyst and the Graafian vesicle, both before and after the escape of the ovum. From the preparation it is seen that a thin stratum of the middle coat is interposed between these two cysts, and that they are independent of each other. But the smaller cyst, though not adherent to the outer surface of the larger, has grown so as to encroach on the cavity of the latter, the lining membrane of which [the] smaller cyst has protruded before it. From this dissection it is obvious that the smaller cyst did not grow from the inner surface of the larger, nor from its outer surface; but that, in the progress of development of the smaller cyst, it pushed before it a portion of the lining membrane of the larger, and thus acquired the layer of reflected membrane from the inner coat of the larger cyst by which it is invested.

"At the base or root of the great cyst in the middle fibrous coat, between the outer surface of the smaller cyst and peritoneum, there is a group of small multilocular cysts, which contained similar fluid, have all the same structure, and bear the same relation to one another, as the two cysts above described. These multilocular cysts have evidently been formed independently of each other; but in the progress of their growth and enlargement, some of them have encroached upon the cavities of those cysts with which they were contiguous, and in the same mechanical manner have acquired reflected portions of their membranes." (p. 8.)

The present race of Obstetricians may fairly claim the distinction of having set the profession by the ears, more frequently and with greater violence than any other class of practitioners. In the instrumental department, the Speculum and the Uterine Sound, and in the operative one, the Cæsarean section and the extraction of diseased Ovaria, have each engaged, on several occasions, the angry attention of the Royal Medico-Chirurgical Society. Whether the manner in which these disputes have been conducted, can claim the merit either of contributing to the advance of truth, or of increasing the respectability of our calling, constitutes a serious question, which we shall not, at present, attempt to determine. Those who do not practise midwifery may well be excused, if they suspect a little exaggeration, and a little lack of calm good sense, when they observe the eminent men who are arranged on either side holding diametrically opposite opinions upon the value of particular instruments, the use of which can hardly be so difficult to settle as some disputants would have us to believe. Other departments of the medical profession are not, however, so pure, that we feel justified in holding up any one of them as a pattern for the rest; for it is very certain that jealousy and partizanship are vices not confined to the champions of the science and practice of obstetrics.

On the present occasion, Dr. Lee has contributed a most valuable addition to the statistics of an operation which interests every class of the profession—the physician and surgeon, equally with the obstetrician. The subject of Ovariotomy has long and painfully engaged the public attention; its results, when successful, being calculated to excite wonder and admiration; whilst the fatal issue must, in many instances, have filled the mind of those who have been admitted behind the scenes, with such feelings of disgust and horror, as the recital of details of barbarity almost unparalleled in the history of science could not but excite. The question, however, is an urgent one, which admits of no delay in its settlement. Any one of us may be called upon to-morrow to determine for some unhappy female, whether she shall pass the short remainder of her life in

a condition of great and permanent discomfort, if not of actual pain; or whether the improvement of our art holds out to her a reasonable prospect of relief by the performance of an operation. It is impossible to think, without feelings of the deepest responsibility and compassion, on the number of families, in this country alone, to whom the subject is of the most vital importance, and who are, at this moment, looking to us for some solution to their hopes and fears. A disease which attacks females of all ranks, just at the time when their lives are most valuable, and which, in too many cases, rapidly proceeds to a fatal termination quite unchecked by medicine, is one that naturally claims the most favourable consideration for any plan of treatment which promises certain and complete relief. In such a case, it becomes us to cast aside every vestige of prejudice, or party spirit; to weigh deliberately every fact; to judge charitably, yet not hastily or imperfectly; and to decide only after the most careful deliberation.

It is not, therefore, without anxious thought, that we pronounce an opinion that the Table of Dr. Lee does not, in its present state, furnish us with the means of fully and fairly settling the question of the merits and demerits of ovariotomy.

We admit that the evidence upon which the question is to be decided —whether in Great Britain any more women should be submitted to this operation—is to be found in the detail of cases that have already occurred in this country. The exclusion of continental experience doubtless militates against the positive certainty that we may hereafter attain; but considering the extreme difficulty there has been in getting together a body of facts relating to this country alone, it is not reasonable at present to expect any great addition to our information from foreign sources.

But it has been objected that while we have accounts of all the successful cases, many fatal ones have been kept in the background; and there is much reason to suppose that this humiliating statement is founded in truth. Would that all our brethren counted themselves "debtors to their profession," and that operators especially were impressed with this great truth. Can there be a more flagrant violation of a solemn duty, than the practice of keeping in the background what experience has taught, that may be unfavourable to any peculiar plan of treatment or unwonted operation? What is it when a man knowingly lets his fellows carry away a false impression on such subjects, but to violate every law of truth—to indorse a lie? What moans, what bitter anguish, what unavailing regrets, have not these men to answer for in a world already so full of tears! Will nothing touch their callous mercenary minds—no consideration compel them to a tardy honesty? If insensible to the contempt of their more honourable brethren, do they never think of a yet higher responsibility? Seldom has our profession exhibited a more degrading scene, than on that memorable night at the Medico-Chirurgical Society, when appeal after appeal was made, and made in vain, to those who had experience of ovariotomy, to come forward and boldly speak the truth. The appeal, which was then scarcely responded to, has been but very imperfectly answered since.

We must be cautious, however, not to attribute too much weight to this circumstance. We have 162—or, as will be seen by and by, 167—facts to judge from, upon which we can rely—a much greater body of

evidence than has been considered sufficient to determine many similar questions. If the operation is to be judged of statistically, figures alone are admissible into our calculations. It will not do to trust to statistics, so long as they support any views to which we happen to have a leaning, and then to throw in a make-weight of *impressions* and *hear-say reports*. Such phrases as "I suspect," "I understand," and even "I have reason to know," are very apt to pass for more than they are worth, and to mislead the judgment to an extent that has always been very destructive to the exactitude of medical science. We make these observations preparatory to entering into a minute analysis of Dr. Lee's Table; but before doing so we think it advisable to remind our readers of one or two circumstances important to be had in remembrance, as aids to our judgment.

In the first place it is not uninstructive to observe the vast difference that exists between the earlier and the later operation for the removal of a diseased ovarium.

Contrast the third case in which this operation was performed in this country, by Mr. Lizars, with many of the cases in which the minor operation, as it is called, has since been resorted to.

We quote the account given in Dr. Lee's Table.

"Case 3.—March 22, 1825. Patient's age 25. Ovarian tumour, seven pounds, adhering to the brim of the pelvis, colon, and abdominal walls, which were partly overcome by dissection, tension, and the handle of the scalpel; incision from sternum to symphisis pubis. Gangrene of the intestine followed." (p. 14.)

For an illustration of the operation by the small incision, we cannot do better than refer to the account we have already given in Mr. Duffin's case.

Now we quote these two cases with no other object than that of inducing those who have very naturally formed strong opinions from what they heard of the earlier examples of ovariotomy, to suspend their judgment until they examine the improvements which experience has effected in its performance. We cannot, indeed, disguise from ourselves the fact that the improved operation is, to a certain extent, only less hazardous than the larger incision. It still retains, of course, that greatest of all its dangers—the wounding the peritoneal cavity; and is, at best, one of the most serious operations in surgery. It is fair, however, to judge of an operation by a reference to the character of the disease for which it is performed. A disease that is sure to end fatally, will justify our incurring a greater hazard for its removal, than one that is an inconvenience only; and it is thus necessary, *in limine*, to correct the notion that an ovarian tumour belongs to the latter class of cases. It is not correct to assert that in many instances a person may live for years with a dropsy of the ovarium, and may be kept in comparative comfort by being occasionally tapped. In truth, such cases are exceedingly rare. Mr. Safford Lee, in his essay on 'Tumours of the Uterus and its Appendages,' states that more than half the cases (63 out of 123) terminated by death in two years, and that 90 out of 123 died within four years. (Op. cit. p. 175.) This agrees very much with the general impression that ovarian disease, like other organic affections, tends to run its course in something like a space of three years—years which emphatically require in this case patience, resignation, and sweet temper, to render them even tolerable to the sufferer.

Tapping—the best means of alleviation in our hands—is also more fatal than is imagined by some, who make too much of the fact that persons are occasionally tapped for the thirtieth or fortieth time. Mr. Southam has collected 20 cases, 14 of which died within nine months of the first tapping —4 surviving it only a few days Of the remaining 6, 2 died in eighteen months, while 4 lived for periods varying from four to nearly nine years. Mr. Lee also has recorded the fate of 37 patients, 21 of whom died within four months; 18 of these were only tapped once, and of these again 15 died within the month. (Op. cit. p. 179.) Mr. Lee observes that it is possible that these cases are published *because* they were fatal, and are considered peculiar, and that they may therefore hardly represent the real mortality of tapping. But on applying to eminent men in London, he found that the general experience of the four whom he consulted very much bore out this result. It is but fair, however, to state, on the other hand, that Mr. Lee's assertions on this point are controverted by Dr. Atlee (Amer. Journ. of Med. Sci., N. S., No. 38); who asserts that the numerous inquiries he has made of surgeons in large practice, convince him that death, or even serious symptoms, *never* result from tapping; that life is usually prolonged, not curtailed, by resorting to it; while in several cases even permanent recovery has resulted.

This seems the most appropriate place in which to allude to certain cases that were communicated to the Quarterly Meeting of the Bath and Bristol Branch of the Provincial Medical and Surgical Association in December, 1850; in which a modification of the "minor operation" had been resorted to by Mr. Wilson, formerly senior surgeon to the General Hospital at Bristol. The principles upon which Mr. Wilson proceeded in his cases are thus stated by him:

"1st. To make as small an incision as possible.

"2nd. To draw out only as much of the cyst as could be extracted readily and without displacement of the other contents of the abdomen.

"3rd. To endeavour to keep the wound always filled with the cyst, so that neither *air* nor *fluid* should be admitted into the cavity of the abdomen.

"4th. To cut off the cyst, *not* close to the wound, but from one to two inches beyond it, so that when the portion of cyst has been removed, the cut margins could be carefully examined, and the vessels secured.

"5th. To secure each vessel on the cut margin of the cyst, separately with fine silk, and to cut off both ends close, so as to have no ligature hanging from the wound.

"6th. To close the external wound as quickly as possible."

Our limits will not permit us, nor indeed is it our object at this time, to make any comparison between the various methods that have been resorted to for the cure of ovarian disease. We only mention Mr, Wilson's cases as illustrations of our observation, that the operation of ovariotomy has undergone a vital change since it was first proposed and resorted to. Suffice it here to say, that, including two unpublished cases, with the details of which Mr. Wilson has favoured us, he has now attempted this plan in five cases. In three of them he removed large portions of the cyst; two of these patients recovered, and are now in perfect health; the third, a person addicted to habitual but not excessive spirit-drinking, died from peritonitis. In the two other cases to which we have referred, the cyst had contracted too many adhesions to the neighbouring parts, to admit of the

removal of any portion of it; and Mr. Wilson very properly desisted from the operation. Both these patients rapidly recovered from the exploratory incisions that had been made into their abdomens. The second of these afterwards died from the original disease, which was found to consist of a multilocular cyst with solid malignant deposit.

As all these cases will, doubtless, be published in the 'Transactions of the Provincial Association,' we take leave strongly to recommend them to the notice of our readers.

We would now submit that we are in a position to establish the following propositions, and to determine exactly the questions as to which we should endeavour to obtain information.

1st. There are a few cases of ovarian disease which remain stationary for years, not requiring tapping for the removal of fluid, and consequently not calling for any more serious operation. What is the real proportion of these, and are there any means of forming an accurate prognosis?

2nd. The large majority of ovarian tumours are uncontrollable by medicine, proceed from bad to worse, and are fatal within a period of between three and four years, passed in great discomfort, often in misery. Can the cases in which this event may be looked for, be certainly distinguished?

3rd. Cases are recorded, in which an operation has been performed for the complete removal of the diseased mass. Does the success that has attended such operations justify us in further resorting to them? In answering this question it must be premised (A) that in some of the cases the ovarian disease has consisted of a cyst or cysts, with or without solid deposit, quite unconnected with any of the surrounding parts, and attached only by its base—in other words, *non-adherent;* whilst (B) in another class of cases there existed *adhesions* more or less extensive. In some instances, these adhesions were torn up, and the tumour cut out; in others, after various attempts, the operation had to be abandoned.

4th. Mistakes in diagnosis have occurred; sometimes no tumour being present at all, sometimes disease, which had been mistaken for an ovarian cyst, having existed in the uterus or elsewhere.

5th. On account of the extreme difficulty or impossibility of determining the existence of adhesions, an incision has, in divers instances, been made into the abdominal cavity, through which the nature of the tumour and the extent of its connexions were examined. Does this, which we propose to call "The Exploratory Diagnostic Incision," afford us a legitimate means of verifying our diagnosis? To class such cases with those in which a large opening has been made into the abdomen, and unsuccessful efforts made together to remove the tumour, is not just; nor can they all be included under one head—"cases in which the operation had to be abandoned."

Finally, it must be remembered that in all the fatal cases of every kind, care must be taken to study the *cause* of death. This, in some instances, has proceeded from circumstances foreign to the operation; in others, from accidents which can easily be avoided in future, or from such as are common to every operation, the most trifling as well as the most serious. How long patients have survived and remained well after the operation of ovariotomy, is another question, upon which the evidence is defective; hardly more so, however, than it is in the case of other operations.

In almost all the cases of which we have a report, after the lapse of a considerable period, that report is satisfactory; for instance, case 8, in Dr. Lee's Table, in which the operation was performed in the year 1835, and the patient is still alive, having since given birth to five healthy children. Case 27: Operation, 1843; still living; has had one dead and one living child.—Case 24: Operation, 1844; died two years after, of stricture of the rectum.—Case 26: Operation, 1844; now living.—Case 50: Operation, 1846; now perfectly well. And so likewise in cases 11, 12, 46, 52, 59, 62, 63, 64, 66, 69, 70, 71, 72, 81, 97, and so on, where the result is stated after the lapse of several years. For a considerable number of cases operated on by Dr. F. Bird, we must refer our readers to the second volume of the 'Lancet' for 1850; but the details given are lamentably meagre.

It does not, therefore, seem to us a valid objection to the operation, that we do not know the final issue of the cases. On the contrary, there is reason to believe that the cure is entire and complete, where the patients have survived the immediate effects of the removal of the tumour.

Of the whole number of operations undertaken in this country for the removal of an ovarian tumour, taking into account the cases in which there has been a mistaken diagnosis, and also those cases which have been subjected to the exploratory diagnostic incision, more than a third have died —truly a startling and frightful result.

According to Dr. Lee, the exact number of operations has been 162, of which 61 were fatal; but correcting this table by the addition of Mr. Wilson's 5 cases, the numbers stand thus: operations 167; deaths 62.

In 8 of these patients, the disease was not ovarian; and two of them died from the effects of what had been done, the third surviving for only a few months. In 3 cases, the disease was a fibrous tumour of the uterus; in one instance, an enlarged mesenteric gland had misled the operator; and in the 4 remaining cases there was no tumour at all.

The results of these 8 cases, numbered as they occur in Dr. Lee's Table, are as follows:

RECOVERIES.	DEATHS.
No. 1. Fat.	No. 7. Fibrous tumour of uterus—removed.
4. Fibrous tumour of uterus.	9. Enlarged gland in mesentery—not removed—died in a few months.
5. Fat and wind.	90. Fibrous tumour of uterus—removed.
10. Tumour not found.	
106. No tumour.	

In 5 of the 8 cases, therefore, where the wound was closed on the discovery of the mistake, the patients recovered.

Non-adherent ovarian tumours were removed in 35 instances, including among them Mr. Wilson's 3 cases, in which he intended to excise only such a portion of the cyst as could easily be drawn out of the wound.

Of these cases, unquestionably the most favourable for the operation of ovariotomy, 10 died, or 1 in $3\frac{1}{2}$ cases.

The numbers referring to these cases are:

Recoveries —8, 11, 13, 16, 23, 26, 28 (health good, but motion of hip-joint interfered with), 44, 52, 54, 82, 93, 97, 98, 104, 109, 110, 111, 115, 116, 160, 161. Add to these, two cases by Mr. Wilson.

Deaths.—19, 20, 49, 85, 94, 95, 113 (tumour 14 lbs.; erroneously stated by Dr. Lee to be 24 lbs.), 124, 156; and one case by Mr. Wilson.

Two of the fatal cases—viz., Nos. 19 and 94—may, however, fairly be excepted from our calculations. In No. 19, a favourable one for the operation, the patient died (Mr. Phillips states) from a choleraic form of inflammation of the large intestine—having been purged twenty times the day before the operation, unknown, of course, to him. (Vide 'Medical Gazette,' vol. xxvii. p. 83.) In case 94, operation by Mr. Solly, the ligature slipped, and the patient died from secondary hæmorrhage. (Vide 'Medical Gazette,' vol. xxxviii.) Such a result can manifestly be avoided in future.

Our mortality is therefore reduced to 8 cases out of 35. Now, surprising as the statement may be, this cannot be considered as an excessive mortality, when compared with that of other capital operations. Many more patients die after a serious operation, than most persons are aware of. We are all disposed to picture the hazards of an operation from the results of individual experience, than which nothing can be more fallacious. If we estimated the perils of lithotomy from its results in the hands of Frère Jacques, or Mr. Martineau, we should involve ourselves in great errors; and as an illustration of the variation of individual experience at different times, we may mention that a distinguished surgeon of our acquaintance operated successfully for hernia forty consecutive times in private; but coming to operate in an hospital, he lost a great proportion of his cases. In Paris, it is said that every second hernia patient dies; and this statement would not greatly exaggerate the unfavourable experience of English hospitals. Malgaigne's statistics of amputations are well known. Calculating the results in 852 cases, he showed that in the whole number 2 out of every 5 died; while in the case of the removal of the lower extremities alone, the mortality was even higher, amounting to upwards of a half. It would be easy to prove the same thing from other statistics.

The next class of cases we shall notice includes those in which the tumour was removed in spite of the existence of adhesions; and here we meet with facts that make us blush for our profession. We are cognizant of 35 such cases, and of these, 15, *or more nearly a half than a third*, perished from the direct effects of the operation. The details of some of these cases are distressing in the extreme. According to Dr. Lee's Table they stand thus:

Recoveries.—Cases 24, 35, 36, 38, 42, 45, 46, 53, 55, 57, 81, 89, 96, 99, 117, 120, 121, 122, 126, 157.

Deaths.—Cases 3, 7 (already referred to as a fibrous tumour of uterus), 21, 22, 39, 47 (tumour free from peritoneal attachments, but adherent to Fallopian tubes and uterus), 48, 56, 83, 90 (already quoted as fibrous tumour of uterus), 100, 107, 108, 159, 162.

As specimens of some of these operations take the following:

Case 48.—Both ovaries and uterus were removed, converting the vagina into a *cul-de-sac*.

Case 108.—Long incision; extensive and strong adhesions, which were violently torn up. A personal friend, who was present at the operation, communicated all the details to Dr. Lee. Death speedily followed. (Unpublished.)

It is worthy of remark, that in one of the fatal cases—viz. No. 83, as well as in No. 158, included in a subsequent table—the adhesions were of such a character as not to be discovered until the operation had proceeded a considerable length; in this, therefore, the exploratory incision would not have answered its purpose. The operations in this class speak for themselves, and no comments can make them appear more outrageous than do, in some instances, the simple details. It will be sufficient to refer to a single instance which has lately been brought prominently before the public eye — the case operated on by Mr. Baker, at the Birmingham General Hospital, and standing last in Dr. Lee's Table.

The rule may be laid down, that *whenever we discover adhesions, except such as are of the most trifling character, the operation must be forthwith abandoned.*

Our next table will include the cases in which, on the discovery of adhesions, the operators proceeded no farther in their attempts to remove the tumour; those, also, in which they were obliged to content themselves with the partial removal of the tumour, together with all the known cases of the simple preliminary diagnostic incision.

From Dr. Lee's data we gather, that under this head there were 17 deaths and 43 recoveries; but adding the two cases communicated to us by Mr. Wilson, and another case of the late Mr. Aston Key, mentioned in the report of Mr. Arnott's fatal case in the 'Pathological Transactions' for 1848, the number of recoveries is increased to 46, thus making one death in rather more than two and a half cases. The particulars are as follow:

Recoveries.—Cases 1 (already mentioned as no ovarian tumour), 2, 4 and 5 (no ovarian disease), 6, 10 (no ovarian disease), 14, 25, 29, 30, 31 (recovered from the operation, but died five weeks after, probably from fatty disease of the heart), 32, 33, 41, 43, 51, 75, 76, 77, 78, 79, 92, 103, 106 (already quoted as no ovarian disease), 112, 114, 119, 129, 130, 131, 133, 134, 135, 136, 137, 138, 139, 140 (died from a subsequent tapping), 142, 143, 144, 145, 155.

We have not hesitated to include under the head recoveries, in this class, cases 137, 138, and 139; although, according to Dr. Lee, the result of the first is not stated, and of the two others unknown; for the operator (Dr. Bird) mentions that all these patients were tapped many times afterwards, the inference from which is obvious.

Deaths.—Cases 9 (already referred to as no tumour), 17, 18, 27, 34, 37, 58, 80, 84, 88, 101, 102, 105, 128, 132 (lived six weeks), 141, 158.

An important distinction is to be drawn between those cases in which the exploratory incision was employed with the object of discovering adhesions, and those in which every effort was made to remove the tumour, the operation being desisted from only when this was found to be impossible. Surely these two cases are not to be classed together.

"Mrs. P——. Married in 1841; never pregnant. January, 1844, first perceived enlargement of the abdomen. In 1846 had strong pressure applied to the abdomen, after being tapped by Mr. J. Brown. A second tapping; the operation of ovariotomy undertaken by Dr. F. Bird, 6th January, 1848; present, Dr. Rigby, Mr. Holt, two assistants, and Dr. Hogg, who has communicated these details to Dr. Lee. 'An incision of two inches was made in the linea alba, midway between the umbilicus and pubes, and twenty pints of liquid were drawn off by a large canula; the opening was then fairly made into the cavity of the abdomen, and the

solid tumour seized by forceps; the size, however, of the tumour was such, that the opening was of necessity extended to ten inches before it could be drawn forth; tumour then found adhering strongly behind the small intestines, and, in fact, to all the abdominal viscera. Dr. F. Bird attempted to separate it from them, by conveying his hand behind it, and, to a certain extent, succeeded in so doing; but on the tumour advancing through the opening, it brought the colon with it, rather before it, which was so firmly adhering that it defied all attempts at separation, even with the handle of the scalpel; the removal being impossible, it was replaced in the abdomen, and the external opening sewed up. The operation occupied an hour and ten minutes, during the whole of which time she was kept under the influence of chloroform. The unfortunate patient, on recovering herself from the effects of the chloroform, stated that she had heard all that was said, but suffered very little. She expired twenty-eight hours after.' (Unpublished.)" (p. 30.)

And the following:

" An incision first made of one inch, the finger introduced, and no adhesion felt; opening extended to three inches; the sac tapped; a second cyst detected and tapped, both cysts firmly adhering. Operation could not be completed. (Middlesex Hospital—Pathological Transactions.)" (p. 25.)

With reference to the causes of death also, it should be remarked, that case 9, where there was no tumour, recovered from the operation, and died a few months after, from the original disease. It is therefore now fairly included in the list of recoveries from the exploratory incision. So case 141 died next day, from the bursting of a large hepatic abscess.

Considering it, therefore, of great moment to ascertain the results of the exploratory incision, beyond which the operation did not proceed, we arrive at the following:

It was employed in cases 14, 30, 34, 76, 77, 78, 79, 80, 103, 129, 130, 132, 133, 134, 135, 136, 137, 138, 139, 140, 141 (already referred to as hepatic abscess), 142, 143, 144, 145, and in two cases by Mr. Wilson and one by Mr. Key. Out of these 28 cases 4 died, or one in $6\frac{1}{2}$; but it is to be remembered, that, of the fatal cases, No. 132 lived six weeks, and No. 141 died from the bursting of an hepatic abscess; so that *in only two* cases, or 1 in 14, could the death be fairly attributed to the operation. We wish we had the means of making this table more complete; at present it speaks favourably of the result of the " exploratory diagnostic incision." Dr. Bird, indeed, stated, at the celebrated discussion in the Medico-Chirurgical Society, that he himself had employed it in between thirty and forty instances. We need hardly say this assertion is not borne out by the tables he has since published.

There yet remain 35 of Dr. Lee's cases, which, with all our efforts to obtain their details, we have been unable to classify. In these there were 15 deaths.

The task we proposed to ourselves is now nearly accomplished; it remains to draw such conclusions as will guide us in our practice, and may, we would fain hope, prove useful to others also. We do not by any means suppose that our statements are entirely free from error; they must be taken only as approximations to the truth, but they are as near correctness as care and honest intentions can make them. In every instance in which we could do so, we have referred to the original sources from which Dr. Lee's Table has been constructed; but as a considerable number of his

cases are unpublished and communicated privately to him, many of them rest upon his own authority. In the course of our investigation we have been more inconvenienced than we dare say Dr. Lee will believe, by a little carelessness on his part in giving his references.

Thus, case 82 is not in the 'Transactions of the Provincial Medical and Surgical Association,' but in the 'Provincial Medical and Surgical Journal for 1845,' and 20 is in the '*New* Series of Guy's Hospital Reports.' But these and other such defects may well be excused, in our gratitude for the substantial good which Dr. Lee has conferred upon the profession by the publication of his paper.

The impression, then, which the attentive consideration of the subject has left upon our minds is to the following effect:

1st. That in any case in which it is considered advisable to remove an ovarian tumour, it is justifiable to make a small preliminary incision into the abdomen, for the purpose of determining whether the tumour be adherent or not.

2nd. If the tumour be adherent, the incision is to be immediately closed entirely, or to such an extent as merely to leave an aperture the size of that made by an ordinary trocar, and we may then expect that this operation will not, on the average, be followed by much more fatal results than common tapping.

3rd. That where the tumour consists of a simple cyst or cysts, with but small solid deposit, it may be extirpated with as good a chance of success as attends the performance of the more serious surgical operations, and with the further prospect of the cure remaining permanent.

4th. The existence of much solid deposit, or of extensive adhesions, absolutely forbids the operation, which should be brought to a termination immediately upon the discovery of either.

Finally, we may add our belief, that the plan proposed by Mr. Wilson, of tying each bleeding vessel separately, so as to dispense with the ligature round the pedicle, is an important improvement; and that, if experience should show that it is sufficient for the cure of the disease to remove only a part of the cyst, the operation will be rendered considerably more hopeful.

We cannot bring our notice of this subject to a conclusion, without again expressing our deep regret that it should have been discussed with so much of acrimony and party-spirit, and that the acknowledged heads of the surgical and obstetric departments of our profession should have left the operative treatment of ovarian disease in the hands of a class of men, who have taken it up chiefly with the view of gaining for themselves notoriety with the profession and the public,—and who, by their unscrupulousness in the performance of ovariotomy at all hazards, and by their want of openness in the publication of their results, have greatly contributed to prevent the real merits of this plan of treatment from being duly appreciated. We trust that the operation will not long remain under the stigma which at present attaches to it; but that experienced and intelligent surgeons, like Mr. Wilson of Bristol, will at any rate take it upon trial, and endeavour to devise means for the diminution of its fatality, and for that improvement of the preliminary diagnosis, which is one of the most important conditions to its legitimate success.

PART SECOND.

Bibliographical Notices.

ART. I.—*On the Nature and Treatment of Softening of the Brain.* By RICHARD ROWLAND, M.D., Assistant Physician, and Lecturer on the Principles and Practice of Medicine, at the Charing-Cross Hospital.—*London*, 1851. 8vo, pp. 137.

THIS work is chiefly a compilation, with some additions from the writer's own researches. The facts contained in the works of Andral, Rostan, Abercrombie, Durand-Fardel, and others, are submitted to analysis. It would have been advisable if Dr. Rowland had more carefully stated, in every instance, from whom he obtained his cases; as we are sometimes not certain whether he is referring to observations of his own, or to those of other observers. Thus the diagnosis of softening is attempted to be determined by an examination of 20 cases of softening, and 20 of cerebral hæmorrhage: and we are led to infer that the cases have all been collected by himself. But if, as may be the case, they are taken from Abercrombie, or from Durand-Fardel, or from some one else, any one going over the subject at a future time, and consulting these authors and Dr. Rowland, may perhaps reckon these cases twice over. We may extract the passage relating to the diagnosis between softening and cerebral hæmorrhage, as an instance of Dr. Rowland's manner.

"In 17 out of 20 cases where a clot was found after death, the attack was entirely without warning. In the remaining 3 cases, the precursory signs of the fit were headache, vomiting, vertigo, loss of recollection, drowsiness, and, in one instance, convulsions.

"In the 20 cases of softening, the comatose seizure was without precursory signs in 2 only. In 16 of the remaining 18, headache was a prominent symptom before the attack. It generally came on severely either a few hours, or sometimes several days, before the appearance of more decided symptoms. The motor functions were impaired prior to the comatose attack in 12. In 5 the palsy was ingravescent, gradually increasing from a slight feebleness of the limb to paralysis more or less complete. In 13 the intellectual functions were disordered in some degree very early in the disease. In 7 some symptoms connected with speech or the articulation were observed before the apoplectic seizure.

"The above details justify the opinion that in the majority of instances the diagnosis might be made with some confidence. When the attacks are quite sudden or without evident warning, the probability is great, that the case is sanguineous apoplexy. Headache and giddiness are common to both affections at their commencement; but in softening, the subsequent course is more characteristic. The dulness of comprehension, vacancy of expression, forgetfulness, especially in regard to language, hemiplegic threatenings leading to an apoplectic seizure, sufficiently indicate the nature of the cerebral lesion." (p. 122.)

If the general rule be, that in softening there are prodromata, which we believe to be correct, we cannot believe that these are present in so many cases as 18 in 20, nor do we think that the attack in cerebral hæmorrhage is without prodromata in 17 cases out of 20. Durand-Fardel's numerous observations lead to a very different inference. This accurate observer, although he does not conclude that prodromata are more common in hæmorrhage than in softening, because, says he, "this is just one of the points which observation determines with the greatest difficulty," yet remarks that he is not far from believing that the established rule may be reversed, and that prodromata are even less common in softening.* On the other hand, there is no doubt that in a great number of cases, perhaps even in a moiety, the symptoms of softening are suddenly developed.

We do not think Dr. Rowland's proportions will hold good, nor do we think that he has entered with sufficient minuteness into this difficult question of diagnosis.

We do not observe anything new in the account of the symptoms, *post-mortem* appearances, or treatment, which need detain us. We can, however, recommend the work as a useful and creditable production.

ART. II.—*On Diseases of the Mucous Membrane of the Throat, and their treatment by Topical Medication.* By WILLIAM R. WAGSTAFF, M.A., M.D. 1851, pp. 183.

DR. WAGSTAFF is an enthusiastic advocate of the practice of applying strong solutions of nitrate of silver to the tonsils and to the palatal, pharyngeal, and laryngeal mucous membranes. As in most other examples, this mode of treatment becomes, in his hands, a panacea for the most varied evils, and few affections of the throat appear to withstand a determined application of the nitrate. Dr. Wagstaff's method does not differ from those already so often brought before us by Green and others. He uses a solution of from 20 to 90 grains to the ounce of water, according to circumstances; and he has employed this solution in all inflammatory affections of the throat.—There are not, however, any new facts in the work; and the description of the several diseases of the pharynx and tonsils is not only a compilation, but is incomplete. The greater portion of the anatomical description is taken from Hasse; and in several instances the description given by Hasse of changes in the bronchial mucous membrane, is applied, apparently by analogy, to the pharyngeal membrane. The merit of Dr. Wagstaff's little work must be held to lie in the zeal with which he has advocated the employment of a method of treatment which is doubtless extremely useful.

Valentiner of Kiel has lately described very carefully catarrh of the pharynx, and has used the solution of the nitrate stronger than that recommended by Dr. Wagstaff; in fact, as strong as that used by Dr. Horace Green. We think, however, that the weaker solutions are equally efficacious.

* Du Ramollissement du Cerveau. Paris, 1843, p. 188.

ART. III.—1. *A Manual of Physiology, including Physiological Anatomy.* By WILLIAM B. CARPENTER, M.D., F.R.S., F.G.S., Examiner in Physiology and Comparative Anatomy in the University of London, &c. &c.—*London,* 1851. Fcap. 8vo, pp. 616. With Two Plates and 165 Wood Engravings.

2. *Hand-book of Physiology.* By WILLIAM SENHOUSE KIRKES, M.D., Registrar and Demonstrator of Morbid Anatomy at St. Bartholomew's Hospital. Assisted by JAMES PAGET, F.R.S., Lecturer on General Anatomy and Physiology at St. Bartholomew's Hospital.—*London.* Post 8vo, pp. 718. With Four Plates and 97 Wood Engravings.

3. *Manual of Human Physiology for Students; being a Condensation of the Subject, a Conservation of the matter, and a Record of facts and principles up to the present day. To each subject are appended, in notes, Summaries in Rhyme of the Composition of the Fluids and Solids,* &c. By JOHN MORFORD COTTLE, L.R.C.P., M.R.C.S., formerly Surgeon to the Leamington Hospital, &c.—*London,* 1851. Post 8vo, pp. 303.

THE first two of these treatises are sufficiently well known to the public, to render it unnecessary for us to do much more than announce the fact of their republication, at the same time stating, in general terms, what alterations have been made upon the previous editions. It may serve in some degree to guide our readers, however, as to their respective characters, which are much more diverse than their titles alone would indicate, if we allow their authors to express in their own words the objects at which they have severally aimed. After attributing the origin of his "Manual" to its publisher's desire to add such an elementary treatise on other departments of medical science, to those which he had already placed in the hands of the student, Dr. Carpenter continues:

"In carrying this desire into execution, the author has endeavoured to avoid inflicting upon the class for whose use the treatise is especially intended, the injury of placing in their hands such a superficial and imperfect sketch of the science, as, whilst affording them but a limited amount of knowledge of its facts, should leave them very ill-informed as to its general doctrines. His object has rather been, to convey to the student as clear an idea as possible of those principles of physiology which are based on the broadest and most satisfactory foundation, and to point out the mode in which these principles are applied to the explanation of the phenomena presented by the living actions of the human body. In this manner has the author desired to prepare him for that more detailed study of the latter, which becomes necessary when physiology is pursued (as it ought to be) in connexion with the changes produced in the living body by morbific and remedial agents, and is thus taken as a guide in the study of the causes, prevention, and treatment of disease—which should be the primary object of attention with every one who undertakes the practice of his profession." (Preface.)

Of the changes in the present edition he says:

"The whole treatise has been subjected to a most careful revision; many statements which the advance of science has shown to be doubtful or erroneous, have been omitted or corrected; and a considerable amount of new matter has been introduced. Of the First, Eleventh, and Twelfth Chapters more especially [embracing the subjects of Vital Action in general, Generation and Development, and the Functions of the Nervous System], a considerable proportion has been entirely re-written; and the author ventures to believe that the doctrines which

they contain will enable such as may master them to obtain a clearer comprehension of the facts of physiological science than they could previously have acquired." (Preface.)

It has not been thought expedient by the author to introduce many references to authorities, it being apparently his opinion that in a treatise which professes to be purely elementary, such are not called for.

We cannot but think that the title of Messrs. Kirkes' and Paget's 'Hand-book' would have been more appropriate, if it had specified *Human* physiology as its subject; for not only do we find Man the prominent consideration throughout, but the physiology of other animals is only incidentally referred to, and this in such a manner as to illustrate the vital actions of man by *similarity* or *parallelism*, rather than by *contrast*. There are many points in human physiology which can only be determined on the presumption, that in the animals nearest to him the phenomena in question are essentially the same; and thus, from what is observed in them, it is inferred that *similar* actions go on in himself. Thus, for example, the capillary circulation cannot be microscopically studied in Man, as it may be in the Bat or Frog, for want of a membrane sufficiently transparent for the purpose; but we presume, from what we there observe, taken in connexion with other evidence, that, *if* we could but see it, we should witness *the same kind* of movement of the blood in the capillary vessels of Man. On the other hand, the physiology of the lower animals, or even of plants, is appealed to *by contrast*, when they are considered (to use the language of Cuvier) as "so many experiments ready prepared for us by Nature;" the very *difference* in the conditions under which their vital actions take place, giving most important assistance in the determination of what is *essential* to the performance of the corresponding actions in Man. It appears to have been Dr. Kirkes' plan to limit himself rigorously to the former of these sources of assistance, as we nowhere find the latter appealed to. In writing the work before us, he says,—

"The primary object has been to give such an account of the facts and generally admitted principles of physiology, as may be conveniently consulted by any engaged in the study of the science; and, more especially, such a one as the student may most advantageously use during his attendance upon lectures, and in preparing for examinations. The brevity essential to this plan required that only so much of Anatomy, Chemistry, and the other sciences allied to physiology, should be introduced, as might serve to remind the reader of knowledge already acquired, or to be obtained, by the study of works devoted to these subjects. For the same end it was necessary to omit all discussions of unsettled questions and expressions of personal opinion; but ample references are given, not only to works in which these may be read, but to those by which the study of physiology may be, in its widest extent, pursued." (Preface.)

The rapid sale of the first edition of this work has shown that it has satisfactorily met an existing want; and we heartily congratulate its authors upon its success. Its peculiar feature is, as we stated in our notice of the first edition,* that it is adapted to the use of those "who

* See British and Foreign Medico-Chirurgical Review, vol. iii. p. 217. This notice, there cannot now be any impropriety in stating, was written by the late Dr. John Reid, during the short interval of comfort that elapsed between the first operation and the return of the disease to which he fell a victim. No one who knew his character can have the slightest doubt of the impartiality with which he penned it.

desire to possess a concise digest of the *facts* of human physiology, without troubling themselves too much about its *principles*, or seeking for their elucidation in the variations of plan on which the functions are performed in other animals;" and thus it is rather to be looked upon as a companion to the Manual already noticed—each serving (so to speak) as the *complement* of the other—than as a rival treatise on the same subject. One most valuable feature in it, is the multitude of references it contains to recent monographs and essays, by which we have ourselves frequently profited.—The changes in the present edition are by no means considerable; and we cannot help thinking that a little more time and labour might have been bestowed upon it with advantage. The total amount of new matter scarcely exceeds twelve pages, and we do not observe much change in the old, even on points where we should have thought it required. Thus, in pp. 480-1, we still find the contraction of the skin and dartos attributed to contractile "fibro-cellular tissue," and the non-striated form of muscular tissue described as something altogether distinct from this; no notice being taken of Professor Kölliker's researches (of which we gave an account some time since, vol. vi. p. 244) on the "smooth muscular fibre" which is, in fact, the contractile tissue alike of the skin, the dartos, the alimentary canal, &c. So we should have thought that, as the constituents of the bile are separately described, some notice should have been taken of Strecker's recent analyses, which have thrown more light upon them than any others have done. In the mention of M. Bernard's researches upon the functions of the Liver, moreover, we are somewhat perplexed by the statement that "M. Bernard believes that the sugar is formed out of the fatty matter which is known to exist in considerable quantity in the blood of the portal vein." We have carefully looked for any hint of such a transformation, which would be a complete reversal of the ordinary course of metamorphosis; and we have not been able to find any. M. Bernard points out, that as sugar is formed in the liver, in animals fed upon a purely carnivorous diet, it must be derived from some constituents of the blood *not* of a saccharine character; but we cannot find that he names fat as their probable source; on the contrary, he states over and over again that the blood of the portal vein contains little or no fat. It is true, he does say that when the production of sugar is largely augmented, that of fat is diminished; but he is here speaking of the fat *generated* in the liver, not of that in the portal blood; and we consider the fact to indicate merely that sugar and fat may both be generated in the liver out of *the same* components of the blood, their generation being in some degree vicarious. In fact, the discovery of *inosite*, or muscle-sugar, in the "juice of flesh," by Scherer; and the accumulation of facts tending to prove the actual production of fat by degeneration of muscle, leave little doubt in our minds that the liver may generate both sugar and fat from the albuminous elements of food, or from the waste of the tissues.—These, however, are but trifling drawbacks upon the general excellence of the work; and we cannot doubt but that the present edition will meet with the same success as the preceding.

A new candidate for the student's favour appears, in the third work upon our list; and we shall here again let the author speak for himself as to his design :—

"The publication of a work on the principles of physiology may at first sight appear unnecessary, when there are such works extant as Dr. Carpenter's and Dr. Kirkes's on the same subject. It was, however, undertaken under the conviction that the foregoing books are not all that is required by the student who is pursuing the study of physiology as a qualification for medical examinations. It is necessary for those examinations that he should possess a knowledge of the facts and principles of physiology, and the obvious grounds on which such facts are based. More than this is not required, and more than this cannot be obtained from a medical student without detriment to the other sciences with which he must be familiar. A recitation of such principles, with their necessary contingent matter, carefully sifted from less important minutiæ, it has been the object of the author to furnish,—necessary alike for the medical student, and as a stepping-stone to him who is purely a student of physiology. Among the sciences that form handmaidens to the practice of medicine, there are none more rich in facts than physiology, or that more require, and can so easily bear, that the same should be brought before the student in a condensed form." (Preface.)

Now, it might be supposed, from the opening sentences of the above extract, that Dr. Cottle intended to supply some deficiency left by the works to which he refers; but we soon discover that he purposes to abridge a "Manual" and a "Handbook" that are already reduced to the extreme of compression, by extracting from them just so much physiological information as may be required by the student—not, be it observed, for enabling him the better to study and comprehend the phenomena of disease,—but simply for qualifying him to pass his examinations. The object thus openly avowed is one which we cannot but condemn; and we feel sure of the concurrence of our readers in our reprobation of it. We would not judge of the book, however, by its Preface, but shall test it by its own merits. Turning to the title-page, we find the subjects arranged (or rather disarranged) in the following strange order:—

"Constitution [chemical] of Human Body—Blood—Structural Composition of Body — Respiration — Digestion — Nutrition — Secretion — Vascular Glands—Kidneys and their Secretions — Skin and its Secretions — Nervous System — Motion — Absorption — Special Senses — Animal Heat — Circulation—Voice and Speech—Functions of Reproduction."

What reason the author can possibly assign for this collocation, it surpasses our sagacity to discover. Had he put the titles of his chapters in a bag, shaken them up together, and then taken them out one by one, he would probably have got a better sequence; he could scarcely have had a worse. "Absorption" isolated from Digestion, and brought in between "Motion" and "Special Senses;" and "Circulation" detached from "Blood" and "Respiration," and introduced almost at the end of the treatise—just as if the author had forgotten it, as a matter of very trivial importance, in the earlier part of his book! It is but fair to say, however, that Dr. Cottle's book improves upon acquaintance; and that, taking each subject separately, it is on the whole very creditably put together; the information being generally derived from good and recent sources, and being much better digested and arranged than the table of contents had prepared us to expect. We must, however, except one portion—that relating to the anatomy of the liver (p. 82), which contains some most extraordinary blunders. The *acini* of Malpighi and Ruysch, which are identical with the *lobules* of Kiernan, are said, by Dr. Cottle, to be the nucleated cells, of which, according to most modern anatomists the substance of the lobules

is composed. And then, notwithstanding the weight of testimony (three lines of authors' names) which he cites in behalf of the true cellular nature of these bodies, Dr. Cottle goes on to conclude with MM. Rochoux and Guillot, that "they are not of the nature of cells, but are made up of molecules held together without capsules, and sometimes having a nucleus." It is pretty evident that Dr. Cottle has never observed these bodies for himself.

Of the rhymed summaries which our author has introduced at the foot of many of his pages, for the purpose of "fixing their facts upon the memory of the student," it will be sufficient to cite one or two examples, from which our readers may form their own judgment as to their poetical merits, and the value of the assistance they will afford.

"*Physiology of Bile.*

"Seven objects the liver effects: to be brief,
To liberate carbon, we'll say, is the chief;
By the bile, it excites peristalsis we'd urge,
Which acts in the chyme like a natural purge;
Its choleine, while staying each change of decay,
Is in chyme antiseptic the whole of the way;
The fourth, by a chemical action, is that
It effects the conversion of sugar to fat;
The blood, while it courses two orders of veins,
A notable portion of sugar obtains;
By the sixth all its parts undergo a renewal,
Again to return to the tissues for fuel;
And lastly, the oily base of the chyle,
According to Cottle, comes out of the bile." (p. 93.)

"*Human Milk.*

"The milk's composition is stated with ease;
Three organic compounds—fat, sugar, and cheese;
Two ternaries first, the latter quatern:
The salts are the same as in blood you discern;
With this special difference—a truth, though in rhyme—
It holds in solution more phosphate of lime." (p. 218.)

If any student finds that he really profits by this doggerel, he is very welcome to it; it would cost the generality, we feel assured, far more trouble to learn and to retain such stuff, than to bear in mind the few very imperfectly-stated facts which they embody.

Art. IV.—*Thoughts for the Medical Student. An Introductory Address, delivered at King's College, London, October* 1, 1851. By William Bowman, F.R.S., F.R.C.S., Professor of Physiology in the College, &c. &c.—*London,* 1851. 8vo, pp. 32.

It is always with pleasure that we select among the many Introductory Addresses which reach us at the beginning of each session, from America, as well as from our own schools, some one or two which appear to us pre-eminently calculated to sustain the dignity of our profession, and, by setting before those to whom they are addressed, the *highest* class of motives, to raise them above those of a more selfish nature, which are too

often those chiefly appealed to on such occasions. "Success in life" and "how to gain it," constitute the staple of many an "Introductory;" and it is to teachers and examples of this kind, that we owe much of that which is derogatory in the after-conduct of those to whom it is addressed, of that which tends to lower the self-respect of the profession itself, of that which helps to degrade it in the public estimation to the trading level, and to destroy whatever *prestige* it has acquired from the noble names which are its boast and honour. Such is far from being the style of Mr. Bowman's Address. His "Thoughts" are those of the Christian gentleman, and he utters them as one who feels that solemn responsibility attaching to his duties as a teacher, which the great Fichte so impressively enforced. We would gladly transfer a large portion of this admirable lecture to our pages; but we must content ourselves with a few extracts.

The first of these relates to the spirit in which we should carry on our search for truth:

"In pursuing the course thus rapidly sketched out, rational medicine adopts all the true results of observation, and seeks to reduce them into order—searching, amid the heterogeneous assemblage of reputed facts, for those leading, guiding lights which serve to dispel the gloom in which mere empirical knowledge is shrouded. Yet it does not reject or overlook what is fairly proved, because it can find no place for it as yet in its system. It is content in its present state of imperfection to mingle the empirical with the scientific, though ever striving to bring empirical facts under the control of law. In reducing the body of experience to rules, it does not make itself the slave of rules or systems, remembering the complexity of the problems before it, the uncertainty and necessary occultness of many of the conditions involved, the multitude of modifying influences. It stores up experience with simplicity of mind, is ever forward to receive new facts, and to pursue fresh avenues of research. With patient industry and singleness of purpose it adopts knowledge from every quarter, and casts aside nothing as worthless that comes to it on reasonable testimony. It is not conceited, but candid and open. It may fall into the errors that are but incident to human thought, and the usual accompaniments of periods of great mental progress, but it is always willing to be guided to the truth, being animated above all things by a love of truth. It is ready to exclaim with Locke, 'It is *truth* alone I seek, and *that* will always be welcome to me, when or from whence soever it comes.' It knows, indeed, that it is ignorant of the whole truth; that in all, even the most perfect and advanced of human systems of science, there is much to mark the poverty and weakness of our faculties, and therefore it vaunteth not its powers, or its conquests, but is tranquil, patient, humble, modest. If it discovers a new and useful truth, fraught with advantage to mankind, mindful of its great object, and true to that object, it promptly throws it into the common store, 'glad to distribute, willing to communicate' it. (p. 12.)

The second places physiological study in its true light:

"In studying physiology, you will endeavour to understand the intimate nature of the phenomena of health, the interdependence of the several organs and functions, the relation of the parts to the whole; so that, in the application of your knowledge to the explanation of diseased states, you may never lose sight of the general and leading principles, which demand attention in all instances, and can never be overlooked with safety. Your minds should be so imbued with physiological principles, that when disease is presented to you, you may at once regard it in its relation to the standard of health, and not as some separate entity, to be combated and overcome without reference to the natural powers and requirements of the bodily organs.

"So grounded in a knowledge of the healthy state, and accustomed to address

all your remedies and means to its restoration, you will be induced and prepared to cultivate, on all occasions, a *habit of thinking*—of meditating on, and of striving to understand, the morbid aberrations that you are called upon to rectify. You will gain, by degrees, a kind and extent of insight—of *scientific insight*—into the cases you have to treat, which will make you intelligent and sagacious practitioners, while it will prove a constant source of intellectual delight, and attach you to the laborious pursuits of our profession, and diffuse over even its more disagreeable provinces an ever-growing interest and charm." (p. 21.)

It has been too much the custom to isolate the phenomena of disease from those of health, as if they belonged to quite a distinct category, and were dependent upon a set of causes altogether dissimilar. It has been assumed by some pathologists, that physiology, if possessing any real value, ought to furnish a set of direct rules for the treatment of disease; and, as it cannot rightly profess to furnish these, it has been set down by others as having no value whatever; whereas the real medical philosopher rather looks to physiology as affording guidance in the pursuit of those rules, by furnishing a clue to the interpretation of symptoms, by pointing out the direction in which he may look for remedies, and by letting in, here and there, a beam of light that shall guide him through the intricacies of his search. In short, its greatest value is in the aid which it affords to the attainment of that *scientific insight* so well characterized by Mr. Bowman.

Our last extract must be the admirable conclusion.

"In conclusion, gentlemen, think nobly of your profession, and by your own conduct make it worthy in your own persons of the standard at which you estimate it. Remember, that its end is beneficent, its studies ennobling and elevating, its ministrations an exercise of our best faculties, and in harmony with the whole constitution of our nature. To excel in it is an aim worthy of all your aspirations, of all your energies; but requiring mental and moral discipline, patient and sustained labour. Go forward in this path with diligence. Make of your difficulties a school, in which strength of character may be tried and formed; and convert them from adversaries into your best friends. There is no sweeter recollection than the sense of difficulties overcome. Strive rather to shine in yourselves, than to outshine others. Seek less to derive honour from your profession, than to honour your profession by your virtues. Cultivate a love of knowledge for the sake of the benefits it will enable you to dispense, as well as for the gratification of your own higher tastes and capacities; and then, whatever worldly fortune betide, you will win the most valuable of the blessings which the occupation of a life can confer, the satisfaction, in the retrospect, of having improved your opportunities, of having acted on right principles, of having been the honoured means of benefiting your fellow-creatures, while humbly, yet earnestly endeavouring, under the Divine blessing, to accomplish the will of your Maker, and to live to His glory." (p. 32.)

ART. V.—*Elementary Physics; an Introduction to the Study of Natural Philosophy. With* 217 *Wood Engravings.* By ROBERT HUNT, Professor of Mechanical Science in the Government School of Mines, &c. &c.—*London,* 1851. Fcap. 8vo, pp. 486.

IF we were to judge of this little treatise by its Preface, we certainly should not be led to form a favourable estimate of Mr. Hunt's capacity for authorship, notwithstanding the high opinion we entertain of his scientific ability; for he strangely confounds himself and his book. In the first place, he speaks of this elementary treatise as "impressed with the idea,"

and presently afterwards of its being "convinced," that a simple treatise on Natural Philosophy is a want of the time, and that therefore it has been written—or wrote itself;—the author not alluding to any *personality*, first, second, or third, as having aught to do with its production. He of course means that *he*, being "impressed with the idea," "convinced," &c., has written the book with the view of meeting the want; and his design is, as he states it, "to give accurate information on every important fact connected with physics; to explain the experimental evidence by which each law has been developed; and, by avoiding mathematical details, while accepting the proofs they afford, to place clearly the deductions from physical investigations before those to whom the higher-class treatises are a sealed book."

We are able to recommend this little work with great satisfaction, both to the seniors and juniors among our readers; for its execution is far better than its opening promised, and is indeed so creditable to Mr. Hunt, that we fancy he must have written his Preface whilst his thoughts were distracted by other objects than that of expressing himself correctly. As a really elementary treatise on the whole circle of physical science, we know of none to compare with it, and it is therefore admirably adapted for the wants of the student: whilst, on the other hand, it may be read or looked through with profit and interest by those who have long mastered the general truths it embodies, for the sake of the many novel illustrations and applications of these which it contains. When we add that the wood-cuts are of the first quality, and the whole getting-up of the book most elegant, we leave ourselves nothing more to say in its commendation.

ART. VI.—*Familiar Letters on the Physics of the Earth; treating of the Chief Movements of the Land, the Waters, and the Air, and the Forces that give rise to them.* By HENRY BUFF, Professor of Physics in the University of Giessen. Edited by A. W. HOFMANN, Ph.D., F.R.S., Professor in the Royal College of Chemistry, London.—*London*, 1851. Fcap. 8vo, pp. 273.

WE cannot better state the objects of this little work, than in the language of its accomplished editor:

"The rapid progress which science has made during late years under the united exertions of the naturalists of all countries, has revealed to us many new and remarkable facts relating to the formation and temperature of the globe, and to the movements of the ocean, and of the atmosphere, as well as to the influence of both on climate, and on the adaptation of the earth for the dwelling of man. The deep interest taken in such inquiries has created a great and general demand for information on these subjects, especially with persons who—unable, from want of time or of the necessary preparation, to enter into the study of the actual phenomena—must necessarily be satisfied with receiving the results obtained by others. To the desire to supply this want, which is strikingly illustrated by the almost simultaneous publication of several treatises on similar subjects, these 'Letters on the Physics of the Earth' owe their origin. In their publication, it was not so much the author's object to offer new facts to the man of science, as to render to a large circle of readers some assistance towards obtaining clearer views and more precise notions of the processes which are at work, on the largest scale, on this our earth, of the causes by which they are governed, and of their influences on the condition

and general features of the surface of the globe. His Letters are addressed to a person not supposed to have any greater amount of scientific knowledge than is possessed by every man of education:—in language intelligible to such a reader, he endeavours to explain the great phenomena of nature." (Preface.)

We cannot but believe that this little book will be peculiarly acceptable to a large number of our readers, who may be considered as precisely belonging to the class which the author addresses; for the intelligent part of our profession cannot but eagerly desire all the information which scientific inquiry can afford, in regard to those continual changes in the condition of the earth, its atmosphere, and its waters, which must be looked on as having a causative relation to disease; whilst they are the very parties who have no time to study more than the *results* of the labours of physical philosophers, and are, at the same time, well prepared by general culture to comprehend and appreciate them. Our examination of this little volume enables us to say that it admirably meets this want; and we anticipate for it a wide field of usefulness. The table of contents will afford a sufficient indication of the subjects of which it treats.

> "Gravity and its Effects—The Tides—The Heat within the Earth—Warm Springs—Hot Jets, and Jets of Steam—Jets of Gas, and Mud-Volcanoes—Volcanoes and Earthquakes—The Temperature of the Outermost Crust of the Earth—The Temperature of the Lowest Stratum of the Atmosphere—The Temperature of the Highest Layers of the Air—The Temperature of the Waters, and their Influence on Climate—The Currents of the Sea—The Winds; their Influence on Climate—The Moisture of the Air, and the fall of Wet—The Electricity of the Air, and Lightning."

ART. VII.—*Petrifactions and their Teachings; or, a Hand-book to the Gallery of Organic Remains of the British Museum.* By GIDEON A. MANTELL, LL.D., F.R.S., Honorary Fellow of the Royal College of Surgeons of England, &c. &c.—*London*, 1851. Post 8vo, pp. 496. With numerous illustrations.

OUR veteran friend, Dr. Mantell, again presents us with a popular treatise upon the subject which owes so much of its elucidation to his long-continued and persevering labours, and in a form which imparts to it a peculiar degree of practical utility. His prime object has been, to render the magnificent collection of organic remains in our National Museum, no small portion of which was brought together by himself, as useful as possible to those who desire to find in it the actual objects of which they read in geological treatises; and at the same time to inspire an interest in these specimens, for their own sake, with those who merely study them as "curiosities." In both purposes we think that he has admirably succeeded; and he has further produced a book which may be strongly recommended even to those, who, by reason of their distance from the metropolis, may rarely have an opportunity of visiting the gallery which it describes, as containing a large amount of recent and most valuable palæontographical information, in a compendious form, and at a very low cost. The volume forms one of Mr. Bohn's cheap series; and we hope that its success will be such, as to encourage him in the production of similar hand-books to other departments of the British Museum.

ART. VIII.—*A Practical Treatise on the Microscope; including the different Methods of Preparing and Examining Animal, Vegetable, and Mineral Structures.* By JOHN QUEKETT, Assistant-Conservator of the Museum, and Demonstrator of Minute Anatomy at the Royal College of Surgeons of England. Second Edition. Illustrated with 12 Plates and 270 Wood Engravings.—*London,* 1852. 8vo, pp. 515.

WE are very glad to be able to announce the publication of a new edition of this valuable work, which has been, we believe, for some time out of print. Our opinion of the original was recorded immediately upon its first appearance (vol. iii. p. 188); and we have only now to say, that we consider the volume before us to be a great improvement upon its predecessor. It contains above fifty pages of additional matter, and numerous additional illustrations; and these may be said, in brief, to include a notice of every important novelty in microscopic apparatus which has presented itself during the three years' interval, whilst they also supply certain deficiencies which were pointed out by ourselves and others, especially in regard to the absence of all mention of foreign microscopes. We still desiderate a little more information from Mr. Quekett as to *his own* appreciation of the value of the different instruments and methods which he describes. There is no one who is more capable of imparting it, from the large experience which his position has afforded him, and no one who would more conscientiously give his opinion without fear or favour; and we presume that he is only withheld from offering the guidance which is desired, by that modesty which is well known to form so prominent a trait of his character.

ART. IX.—*A Popular History of Mollusca; comprising a Familiar Account of their Classification, Instincts, and Habits, and of the Growth and distinguishing characters of their Shells.* By MARY ROBERTS.—*London,* 1851. Square 16mo, pp. 376. With 18 Coloured Plates.

THIS little volume forms another of the excellent series of illustrated works on various departments of Natural History, for which the public is indebted to Mr. Reeve. Two of these,—Mr. Landsborough's 'British Sea-Weeds,' and Mr. Adam White's 'Mammalia'—we have noticed with approbation on former occasions; and it gives us great pleasure to be able to recommend the treatise now before us, as an excellent introduction to the study of the Molluscous division of the animal kingdom, alike fitted for those who desire to gain a scientific acquaintance with it, and for those who limit their ambition to the collection and arrangement of a cabinet of shells. For the authoress has right wisely and boldly given to the *animals* their due prominence, and has shown the secondary character of the beautiful tegumentary skeletons, which, with many, are the sole objects of attraction; so that a reader belonging to the first of these classes will find in her treatise a fund of information regarding the former, which will lay a most advantageous foundation for more profound studies; whilst the mere shell-collector can scarcely but be interested in the account that is given of the mode in which the beautiful objects of his attention are constructed, and of the adaptations they present to the habits of the animals to which

they appertain. When we add that the plates contain no fewer than 90 figures of shells, with their animal inhabitants, all of them *well* and several *admirably* executed, and that the text is written throughout in a readable and even elegant style, with such digressions in poetry and prose as serve to relieve its scientific details, we think that we have said enough to justify the favourable opinion we have expressed.

Our duty as critics, however, obliges us to make a few remarks, which we trust the authoress and publisher will accept as an indication of the care with which we have examined the book, and of the desire we feel that so well-planned a treatise should be equally well executed. It seems to us that, in a work professing to be a popular treatise on the Mollusca generally, a very disproportionate amount of illustration is bestowed upon the class of Gasteropods; for these, out of the *eighteen* plates, monopolize no fewer than *sixteen*, leaving only one for the Cephalopods, and one for the Bivalve Conchifera. Moreover, the account of the Gasteropods is carried to such minuteness, as to notice, not only all the orders of the class, but all the most important genera; but even here there is considerable inequality in the illustrations; for though in some instances two, three, four, or even five species of one genus have been figured, of other very important genera (*e. g.*, Haliotis) there is not a single representation. The account of the Cephalopods might have been somewhat extended with advantage; and a few figures of the strange varieties of form which this class presents, would have been, we think, at least as interesting as the multiplied figures of similar forms presented in the foregoing. Moreover, the authoress has not only given no indication of the fundamental distinction established by Professor Owen between the *Dibranchiate* and the *Tetrabranchiate* orders of Cephalopods; but has brought the "pearly" or *true* Nautilus and the "paper" Nautilus, or *Argonaut*, into a degree of proximity that will tend to keep up the misapprehension respecting their affinity, which their popular names have occasioned. Indeed, we are not sure whether the authoress herself is not under some misapprehension of this kind; for she applies to the true Nautilus those fictions about sails and oars, which poets have helped to propagate, from the times of Callimachus downwards, with respect to the Argonaut. We cannot but think, too, that it would have added to the interest of the account of this class, if some notice had been taken of those multiplied varieties of chambered shells of which the Nautilus is the only existing type, that present themselves through the whole period of geological time, from the palæozoic down to the tertiary formations. The *Lamellibranchiate* Bivalves, although (as we have just remarked) very slightly illustrated, are fully and pleasingly treated of in the text; and we the more regret the deficiency of figures, since this class presents so many interesting varieties of conformation which at first sight depart very widely from the common type, and affords, especially in the structure of the hinge, numerous instances of that design on which the authoress delights to dwell. The *Brachiopods*, though described, are not illustrated by a single figure; which is the more to be regretted, since the remarkable "carriage-spring" framework of the Terebratula is a feature that can be only understood by delineation. We should have liked, moreover, to see some notice of the remarkable predominance of this group of bivalves in the earlier periods of the earth's history. Finally, of the *Tunicated*

Mollusks no account whatever is given; and although we admit, that neither in their conformation nor their habits do they present much that is of popular interest, yet there are some points which might have been dwelt on with advantage;—for instance, the remarkable contrast which they present in the almost entire absence of properly animal powers, and the relatively-high development of the apparatus of organic life; the production of composite clusters by gemmation; the alternation of forms presented by the solitary and aggregate Salpæ; and the luminosity of many of the freely-floating species of this curious group. Besides, the *Bryozoa* are now, by the common consent of naturalists, raised to the dignity of Molluscous animals; and there is enough in their structure and economy to have furnished a most interesting chapter. For these additions, room might easily be made by a little curtailment in other quarters.

We shall not, we trust, be thought hypercritical, if we further remark that we desiderate a little more physiological accuracy in some parts of the treatise. Thus, the "mantle" is spoken of (p. 3) as a *filmy* organ; whereas it is usually thick and spongy, differing from the skin of other animals in the large quantity of muscular and glandular substance which it includes. So again, the authoress remarks (p. 66), that "doubtless the creatures take much delight in varying and adorning the fairy-like domes in which they dwell. Why else does the magilus, that singular tenant of the coral-rock, fill up the deserted portion of his shell with testaceous matter; or the cowry adorn his dome-shaped dwelling with the most exquisite embellishment?" One would think that she was describing a lady painting her face, or building up an elaborate head-dress. The physiologist well knows that the mollusk has no more control over the marking on its shell, than a lady has over the freckles which she dignifies with the name of "beauty-spots;" and that its form is as much determined by its *growth*, as is the branching of a stag's horn or the shape of a crab's claw.

The errors of this kind are, however, so few, and the excellencies of the book are so many, and render it so vastly superior to any popular treatise of the kind within our knowledge, that we can conscientiously repeat our hearty recommendation of it; and shall hope to see the defiencies we have pointed out supplied in another edition.

Art. X.—*On the Existing State of our Knowledge of Vaccination and Re-vaccination, as Preventive of Smallpox.* By Alexander Knox, M.D., Physician to the Strangford Dispensary.—*London*, 1850, pp. 57.

This little book is a reprint from the pages of the 'London Journal of Medicine,' and presents us with a very fair analysis of the important subject of vaccination. Within the limits which the author prescribed to himself, it has not been possible to go *in extenso* into the subject; but the most important and practical part of it is treated with sufficient fulness and with considerable care. Dr. Knox has not himself furnished much original information, but has brought together from various sources the best authenticated facts. We shall lay before our readers a short analysis of the work.

After alluding to the first known records of smallpox, Dr. Knox at once enters upon the question of the amount of preservative power which an attack of casual smallpox has against second attacks. He shows that the liability to a second after a first attack has been variously stated, the extremes varying as wide as one in fifty thousand cases, and one in six; but remarks that the most trustworthy evidence goes to prove that such a recurrence is not unfrequent. He then passes to the subject of inoculation, and after giving a brief history of it, decides that the advantages of inoculation were considerable; that it abridged by several days the incubative stage, lessened the amount of the eruption, and, as proved by the records of the Inoculation Hospital, diminished the mortality from three in ten, (the average of the casual disease) to three in one thousand. The disuse into which inoculation fell before the discovery of vaccination, he attributes to the dislike which medical men entertained to originating a disease which was occasionally severe or fatal, and which also sometimes gave rise to various formidable sequelæ. And as many persons escaped smallpox altogether, the propriety of producing the disease was very questionable. The history of vaccination is next given, and the author then enters on the proper subject of his treatise—viz. the evidence for, and the amount of, the protective influence of vaccination. The arguments are arranged under the following heads:

1. Popular experience as to the immunity enjoyed by those who have casually contracted cow-pox from the horse, cow, or camel. Dr. Knox does not, however, enter into any statement of the amount of this popular experience and belief.

2. The identity of cow-pox and smallpox. Dr. Knox adopts the opinion that cow-pox is merely smallpox occurring in and modified by the system of a brute; and adduces in support of his view the experiments of Sonderland, Gassner, Fomin, Thiele, and Ceely, who have all succeeded in inoculating cows, and in thus producing a disease which, when re-transferred to the human subject, was found to be cow-pox.

3. Testimony in favour of the protective influence of vaccination. This is the grand argument, and is the one which is particularly adapted for the popular discussion of the subject. It is arranged under three heads:

(*a.*) It is shown that vaccination confers a power of resistance to the contagion of casual smallpox. The evidences in support of it are—the original statement of Jenner, that 6000 vaccinated persons were exposed in every way to smallpox, but did not take it; the history of smallpox in Denmark, where the disease was excluded by vaccination; the observations of Kennis in Ceylon, and of Wendt in Copenhagen. It is therefore concluded that, without further evidence, it is incontestable that "the immunity from casual smallpox is infinitely greater among the vaccinated than the unvaccinated portion of the community."

(*b.*) Vaccination diminishes or destroys the liability to inoculated smallpox. It is assumed that for three years after vaccination this protective influence is undoubted; after that time Dr. Knox thinks the effect of inoculation not sufficiently known.

(*c.*) Vaccinated persons are to a great extent insusceptible of effect from re-vaccination. This argument assumes, that if the system be susceptible of cow-pox, it will be susceptible of smallpox, and the

reverse. Dr. Knox concludes that one half of those vaccinated are at all subsequent periods incapable of having the disease re-communicated, and therefore, it is presumed, are protected against smallpox also.

Dr. Knox then discusses the causes of failure of vaccination, as it is necessary to admit that many persons (perhaps one half) after primary, are susceptible, at some after date, of secondary, vaccination. The causes of failure are, imperfect performance of the first vaccination, the employment of unsuitable virus, and imperfect action of a suitable virus in a peculiar condition of system, and finally, exhaustion of the influence by time.

The "remedies which have been suggested to obviate partial failure of vaccination" are then considered. These measures are—1. Variolous inoculation; 2. The employment of lymph taken afresh from the cow, instead of the stock in general use; 3. Increased number of the vesicles; 4. Universal adoption and more careful performance of vaccination and re-vaccination.

The first proposition is condemned; the utility of the second is considered questionable; the third recommendation is considered "cruel, inconvenient, and superfluous;" while the fourth is considered to be the only true means of carrying out the preventive measures against smallpox. The time at which re-vaccination should be performed is alluded to, and after mentioning the opinions of Spooner, Retzius, and Wendt, Dr. Knox concludes that the exact period cannot be determined, probably varies in different persons, and must be settled according to circumstances, such as the imminence of contagion or the wishes of friends.

Dr. Knox finally sums up his argument with the statement, that "the efficient and general practice of vaccination, aided by re-vaccination under suitable circumstances, will be found an efficient safeguard against the ravages of smallpox in all constitutions not absolutely beyond the influence of any protecting agent whatever." (p. 54.)

ART. XI.—*The Pocket Formulary, and Synopsis of the British and Foreign Pharmacopœias; comprising standard and approved Formulæ for the Preparations employed in Medical Practice.* By HENRY BEASLEY. Fifth Edition. Corrected, enlarged, and adapted to the last editions of the Pharmacopœias.—*London,* 1851. 24mo, pp. 546.

IT is sufficient for us to mention the republication of this little work, which has gained for itself a high character for utility, accuracy, and comprehensiveness of information. Besides making the alterations rendered necessary by the changes in the new London and Dublin Pharmacopœias, the author has introduced a large number of additional formulæ from foreign pharmacopœias and formularies, from the pharmacopœias of the principal hospitals of this and other countries, from systematic treatises on the various departments of medical science, and from monographs and essays on particular remedies. It is of course impossible for us to speak confidently of the accuracy of a compilation from such a variety of sources; and we can only point to the success of former editions, as the best testimony in favour of the author's care and discrimination.

PART THIRD.

Periscope.

ANATOMY, PHYSIOLOGY, AND ORGANIC CHEMISTRY.

On the Impregnation of the Ovum in the Amphibia (Second Series), and on the Nature of the Impregnating Influence. By GEORGE NEWPORT, Esq., F.R.S., F.L.S.

THE author commences his paper by stating, that having given direct proof, in his former paper (see British and Foreign Medico-Chirurgical Review, vol. viii. p. 253,) that the spermatozoon is the impregnating agent, and also that the *liquor seminis* does not effect impregnation, he now proposes to detail some new experiments which bear on the views he then advanced; and especially with respect to the nature of the impregnating influence.

He first details some additional experiments with *solution of carmine*, with the object to show, that the result of one experiment mentioned in his former paper, in which he detected a small granule of carmine within the vitellary membrane, was attributable to the cause he then assigned—accidental injury to the egg; and he states that the results of his present investigations confirm him in the view then held,—that no natural perforation or fissure exists in the envelopes of the egg, either of the Frog or of the Newt, before or at the time of impregnation; and that the spermatozoon does not penetrate into, but only lies in contact with, the envelopes.

He next gives the results of some experiments with solution of potass, in confirmation of his former observations; and further shows the effect produced on the egg by immersion in solutions of potass and soda, with different proportions of the salts; and afterwards details the results of other experiments made to test some of the more remarkable ones by Spallanzani with regard to the effect of very minute quantities of the impregnating fluid. In these trials the author has proceeded by the mode of direct application of the fluid, and not by immersion of the eggs in large quantities of water with small proportions of seminal fluid, the mode followed by Spallanzani. The result of the direct application through contact, *once only* with each egg, with the point of a pin wetted with the fluid, was, that this was sometimes sufficient to effect the commencement of segmentation, and consequent *partial impregnation;* while, if the fluid was allowed to drain off the pin, by continuing the contact for a few seconds, then complete segmentation and full impregnation followed, and, other circumstances being favourable, an embryo was produced; and when the head of a pin was employed to apply the fluid, then the usual result was full impregnation; so that these results confirm those by Spallanzani. The author further states that it appeared to be of no consequence as to which surface of the egg was touched, the dark surface, light surface, or the side,—the result was the same.

He next proceeds to show, that when the egg is immersed in *pure* seminal fluid a *directly opposite result ensues.* Segmentation then seldom occurs, and the embryo is but rarely produced; and further, that the effect then produced on the egg is very similar in appearance to that of the *chemical action* of solution of caustic potass; the yolk becomes shrivelled and decays. These results he thinks are not

explained by the views at present entertained respecting the nature of impregnation. The author then refers to the observations made by himself, and also by M. Quatrefages, which tend to show that no impregnation is effected, even by the contact of the spermatozoon with the egg, when all motion in the spermatozoon has entirely ceased; and he conceives that this fact, when considered with the results now obtained, leads to a new view of the subject.

The author then applies the view of the 'Correlation of Forces' to the investigation of the function of impregnation. He thinks that impregnation is commenced if not entirely completed, by what may possibly prove to be a new condition of force, *in*, and *peculiar to*, the impregnating agent, the spermatozoon, which he designates *sperm-force*, and distinguishes from the force of growth and development in cells, through which the spermatozoon is produced. He further distinguishes it from the force of *muscular contractility*, and from that of the *nervous system*, and states that he regards the whole only as modifications of one common force, and as having correlations with the physical forces. In support of this view the author enters into details, and refers to some late analyses by Dr. Frerichs, to show that the spermatozoa, like muscle and nerve, not only have a definite structure, but also a definite chemical composition, and that this composition appears to be the same in different classes of animals. He thinks that the spermatozoa may thence be regarded as organs of a special modification of force, and that motion is the visible exponent of this form of force, since the spermatozoa are quite inefficient to impregnate when their motion has entirely ceased. The author further thinks that it is only by the adoption of views of this kind that the apparently contradictory results obtained are likely to be explained.

In the course of his observations the author states a remarkable fact, which he has repeatedly verified, and which he thinks is of importance—namely, that the *first cleavage or divison of the yolk, in the egg of the Frog and Toad, corresponds in its line of direction to the longitudinal axis of the body of the embryo* of those animals; and this he proposes to show more particularly hereafter.—*Proceedings of the Royal Society*, June 19, 1851.

On the Reproduction of the Ascaris Mystax. By HENRY NELSON, M.D.

THE author commences with a brief anatomical description of the *Ascaris Mystax*, found in the intestinal canal of the Domestic Cat; with more especial reference to the organs of generation in the two sexes. He traces the gradual formation of the semen; originally thrown off as seminal particles by the cæcal extremity of the tubular testicle, the exterior of each solid particle enlarges to constitute a cell, while the interior retains its consistency and forms a nucleus. The cell then acquires a granular protecting envelope, and in this state is introduced into the female. The solution of the protective envelope and the great enlargement of the seminal cell follow, and its nucleus is now seen to present a granular structure. The external granules of the nucleus coalesce to form a membrane, at first exactly resembling a watch-glass in shape, but by the contraction of its margin ultimately forming a curved cæcal tube. This is the true spermatic particle or spermatozoon, and is set free by the rupture of the seminal cell.

The generative apparatus of the female, commencing also in cæcal extremities, is next treated of, and the author draws particular attention to a transparent, narrow contractile portion, the oviduct intervening between the ovary and uterus, as the part in which the ovule encounters the spermatic particles, and is by them fecundated. The cæcal end of the ovary likewise throws off a solid particle, which enlarging forms a germinal vesicle and spot. As the germinal vesicle travels slowly down the tubular ovary, it acquires a thick granular investment or yolk, secreted by the ovarian walls. The ovules now present a flattened triangular shape, are placed side by side, and form one solid mass. At the commencement of the oviduct, however, they become detached, separated from each other, and pro-

pelled singly along its interior. Here the gelatinous ovule meets the tubular spermatic particles, and is surrounded on all sides by them. They are at first seen to be merely applied against the ovule; but by degrees the margin of the latter presents a rupture, some of the vitelline granules are displaced, and the spermatic particles become imbedded in the substance of the yolk itself.

While the penetration of the spermatic particles is going on, a chorion, secreted by the oviduct, surrounds the ovule, forming a spherical envelope, within which the germinal vesicle, the granular yolk, and the imbedded spermatozoa, are enclosed. The spermatic particles after penetration are seen to swell, become transparent, and ultimately to dissolve. The vitelline granules likewise either disappear altogether, or are transformed into others of a different colour; and lastly, the germinal vesicle is destroyed.

By tracing the changes of the ovule in unfecundated females of the same species, the author finds the disappearance of the vitelline granules to be dependent upon, while the formation of the chorion is wholly independent of, the influence exerted by the spermatic particles on the ovule.

As soon as the vitelline granules and germinal vesicle have disappeared, the whole interior of the chorion is filled with a clear fluid, in which a few granules and the germinal spot are seen to remain. By swelling up, this constitutes the embryonic vesicle and spot. A membrane separates from the interior of the chorion, and contracting on the granules forms a spherical yolk, in the centre of which is the embryonic vesicle. This is the perfect ovum. The subsequent divisions of the embryonic spot, vesicle, and yolk, are described; the author particularly pointing out the gyrations of the embryonic vesicle immediately after division. As soon as the whole interior of the egg has been filled by the subdivisions of the yolk, the external granules coalesce, and form a continuous membrane internal to the chorion, which by gradual depression on one of its sides forms first a fleshy cup, and then, as the membrane of its concavity touches that of its convex surface, acquires the form of a ring. The ring divides at some point of its circumference, the extremities become pointed, and thus the young Ascaris receives its characteristic shape. The author has frequently repeated his observations with a view to their verification, and has employed the camera lucida to render the illustrative figures as accurate as possible.—*Proceedings of the Royal Society, June* 19, 1851.

[This paper is of peculiar interest, especially when taken in connexion with the preceding. In the first place, the very early stage of development at which the spermatic cells are transferred from the body of the male to that of the female, is a fact which proves, with regard to the Entozoa, as did the similar observations of Mr. H. Goodsir on the Decapod Crustacea, how completely independent is the vitality of these cells, provided that they are supplied with the conditions necessary for their development. The point of greatest novelty, however, is *the entrance of the spermatic particles into the interior of the ova*, of which the author speaks with the greatest confidence; while Mr. Newport asserts with equal confidence that no such entrance occurs in the case of the Frog. These two facts—for we are much disposed to place confidence in the accuracy of both observers—do not seem to us so discrepant as might at first appear. Our readers will recollect that a similar difference presents itself in the mode in which the contents of the 'sperm-cell' and the 'germ-cell' come into relation in plants; for a complete intermixture takes place by the rupture and emptying of both these cells in the lowest cellular Cryptogamia, and a direct passage of the contents of the sperm-cell *into the cavity* of the germ-cell, in those a little higher; whilst in all the superior Cryptogamia, and in the Phanerogamia generally, the spermatic influence (communicated by the *phytozoaire* in the former, and by the pollen-tube in the latter) appears to be applied merely to the *exterior* of the germ-cell. (See 'Brit. and For. Med. Chir. Rev.' vol. iv. p. 346.)]

On the Influence of the Sympathetic and Intra-cranial Nerves on the Motion of the Pupil. By Professor JULIUS BUDGE and Dr. AUGUSTUS WALLER.

THESE authors have discovered the very interesting fact, that irritation of the cervical trunk of the sympathetic nerve, by means of the magneto-electric machine, produces an extraordinary dilatation of the pupil; and this alike in the rabbit, in which this trunk is isolated from that of the pneumogastric, and in the cat and dog, in which the two trunks are united. The phenomenon, they remark, is as constant as the contraction of the leg when the sciatic nerve is galvanized. Thus is explained the permanent contraction of the pupil after section of the sympathetic nerve in the neck, which was first observed by Petit in 1712, but which, though subsequently verified by other observers, has never led to more than a surmise that the cervical portion of the sympathetic might afford the dilating power. By galvanizing the third pair and the cervical sympathetic alternately, alternate contraction and dilatation of the pupil are produced; but with this difference, that the *contraction* is immediate, as is also the return of the iris to its previous state after the irritation is withdrawn, whilst, on the other hand, the *dilatation* requires some seconds to attain its maximum, and the subsequent return of the iris to its previous condition is slow. Moreover, the third pair loses its power of conducting irritation after being several times excited in this manner, and also very soon after death; whilst the sympathetic retains it longer.

The authors next applied themselves to determine the precise centre of this power, and they ascertained, in the first place, by galvanizing the undivided trunk in the rabbit, that irritation of any part of the cervical portion of the sympathetic, from the first to the last ganglion inclusive, would produce this result; but that below the last cervical ganglion, no such effect could be produced by irritation. On exposing the spinal cord, they found that irritation of any part of it between the first cervical and sixth dorsal vertebræ (which they hence designate the *cilio-spinal* region) would produce dilatation of the pupil; but that the effect was most decided in the centre of this region, the maximum being at the junction of the second and third dorsal vertebræ, and decreasing in intensity on passing towards either of its limits. When both of the sympathetic trunks are entire, irritation of this region produces contraction in both pupils equally; but when one of the sympathetic trunks is divided, the dilatation does not take place on that side; and if both be divided, no irritation of the spinal cord produces any effect on the pupil. It is sufficient to irritate one side of the spinal cord only, to produce dilatation of both pupils; but if the spinal cord be divided longitudinally, and the two halves be kept asunder by a slip of glass, irritation of one side then acts only on the pupil of that side. When the spinal cord is divided transversely at different points, it is found that irritation of any part that is isolated from the centre of the *cilio-spinal* region is unproductive of effect. When the spinal cord was removed, and the galvanism was applied to the dura mater lining its canal, this limitation was still more precise; for it was then found that in no other point was any effect produced, than at the intervertebral space between the second and third dorsal vertebræ. Thus it seems obvious that the motor fibres on which this action depends, issue from the spinal cord with the second dorsal nerve; and this corresponds with the results of dissection, as compared with the lowest limit of the influence of the sympathetic trunk over the movement of the pupil. All the causes which diminish muscular irritability after death, such as imperfect nutrition of the animal, lesions of the medulla oblongata, &c., diminish or destroy the power of the cilio-spinal region over the pupils; and in all cases it is to be noted that after death the power gradually recedes from the extremities of the cilio-spinal region towards its centre.

The authors then proceed to study the action of the intra-cranial nerves; and, in the first place, that of the fifth pair, respecting which there has been much contrariety of opinion. In order to remove all sources of fallacy so far as possible, they have divided all the other nerves connected with the eye (including the cervical sympathetic); and they have then found that sections of the ophthalmic

branch of the fifth pair, or of the trunk from which it is given off, causes a slow and gradual constriction, which does not commence for a minute or two after the section, and does not attain its maximum for as much more. This constriction is very decided, the pupil being reduced from three lines in diameter to one. If, instead of division of the nerve, mechanical or galvanic irritation be applied to it, the same effect is produced, but in a less degree. The constriction is in no case permanent, but departs gradually. After section of the optic nerve, section of the third pair does not produce any change in the diameter of the pupil. The fourth and sixth pairs, moreover, have not appeared to the authors to possess any influence over the movements of the iris. If the cervical sympathetic be galvanized without waiting for the gradual departure of the constriction produced by the section of the fifth pair, it is found to be powerless to overcome this constriction. If, moreover, the fifth pair be exposed, and its trunk be divided progressively from the centre towards the eye, it is found that so soon as the section is carried in front of the anterior portion of the Gasserian ganglion, the power of the sympathetic over the pupil is entirely lost. Hence it seems evident that the sympathetic fibres which have this function, pass through the Gasserian ganglion; and by other experiments it may be shown that they form part of the ophthalmic division of the fifth pair.—*Gazette Médicale*, Nos. 41 and 44.

On the Temporal Ridges of the African Cranium. By Dr. NEILL.

DR. NEILL exhibited several skulls to illustrate a point not hitherto noted by ethnologists and anatomists. On viewing the superior surface or *norma verticalis* of the African cranium, it will be found that the distance between the temporal ridges is so small, as compared with other races, as to constitute a characteristic peculiarity. Admeasurement of thirty-three African crania showed that the distance averaged four inches and a half, while the average of twenty-nine Caucasian skulls gave five inches and two thirds. The temporal fossa is larger in every respect in the African skull.—*Amer. Journ. of Med. Sci.*, vol. xxii.

On the Fibrin of Muscle. By Professor LIEBIG.

THE substance of muscular fibre has been commonly designated as *fibrin*, and has been considered identical with the fibrin of the blood. This, however, is an error, as Professor Liebig has proved by the following experiments.

When the fibrin of blood is treated with water acidulated with a tenth part of hydrochloric acid, it soon swells and becomes changed into a gelatiniform mass, which is soluble in boiling water; if more concentrated acid be added, however, it returns to its primitive volume, again swelling when water is again added. This experiment may be repeated several times, without any notable proportion of the fibrin being dissolved. On the other hand, the substance of muscle very speedily dissolves in water containing hydrochloric acid in the above-named proportion, and this at the ordinary temperature; the solution is somewhat viscid, and is rendered slightly opaque by the presence of fat. When neutralized, it coagulates; and the coagulum dissolves in an excess of alkali, or in lime-water—but not in the latter if it have been first boiled.

The proportion of this fibrin readily soluble in dilute acid, is very different in the several kinds of animal flesh; thus the muscular substance of the common fowl and of the ox dissolves almost entirely; that of the sheep leaves a considerable undissolved residue; whilst that of the calf does not yield more than half its weight to the dilute acid. This insoluble residue is white and elastic, but more gelatiniform and less white than the blood-fibrin swollen up by dilute acid.

The so called fibrin of muscle is stated by Strecker to have rather the composition of albumen than that of blood-fibrin, containing less azote than the latter; but we must own that we distrust all minute differences in these organic analyses.

The differences between two of Strecker's analyses of this very substance, for example, are as great as between one of them and the composition ordinarily given for albumen, which is itself but an average derived from a number of analyses differing considerably from each other.

When blood-fibrin is kept in water in a closed vessel, and at a moderate temperature, decomposition soon commences in it; at the end of three weeks it is completely dissolved, and the liquor, which includes some flocculi of sulphuret of iron, possesses all the characters of an albuminous solution, and forms a coagulum by heat, which has the precise composition of albumen.—*Annalen der Chemie und Pharmacie*, band. lxxiii.

[These researches are extremely suggestive, and open up the question whether the substance of muscle has that close relationship to the fibrin of the blood which has been commonly assigned to it. For ourselves, we are now disposed to believe that the fibrin of the blood is a stage of transition towards gelatin, and that its purpose is the nutrition of the simple fibrous tissues alone.]

On the Methods of Analysing the Blood. By M. GORUP BESANEZ.

IT is well known that the analyses of healthy blood given by different chemists present a very marked discrepancy. This is no doubt due in part to the diversity in the specimens of blood examined; but a large allowance is to be made, it appears, for the difference in the methods employed, at least as regards the proportions of albumen and globules. This has been recently shown by M. Gorup Besanez, who gives the following as the results of the analyses of the same samples, according to the methods of Scherer, Becquerel and Rodier, Höfle, and his own, respectively, the last being a combination of that of Höfle with that of Dumas. The first specimen was the blood of a vigorous man of fifty years old, and the results of the several analyses were as follow:

	Scherer.	Becquerel and Rodier.	Höfle.	Gorup Besanez.
Water	796·93	796·93	796·93	796·93
Solid matters	203·07	203·07	203·07	203·07
Fibrin	1·95	1·95	1·95	1·95
Globules	115·16	117·82	103·23	103·23
Albumen	58·82	63·87	50·84	70·75
Extractive matters and salts	27·14	19·43	47·05	27·14

The second specimen was from a robust man of twenty years old:

	Scherer.	Becquerel and Rodier.	Höfle.	Gorup Besanez.
Water	783·63	783·63	783·63	783·63
Solid matters	216·37	216·37	216·37	216·37
Fibrin	1·56	1·56	1·56	1·56
Globules	113·54	131·52	115·12	115·12
Albumen	64·32	65·91	51·76	62·74
Extractive matters and salts	36·95	17·38	47·93	36·95

Journ. für prakt. Chem., t. l.

[We cannot feel surprised that considerable discrepancy should exist in the results of these analyses, when we learn that in the method of M. Höfle, which is so far adopted by Gorup Besanez, a strong solution of sulphate of soda is added to the defibrinized blood, preparatory to the separation of the globules from the albuminous constituents. By this treatment, a considerable part of the contents of the blood-corpuscles will be drawn out by exosmosis, and blended with the extractive.]

On the Urine produced by Asparagus. By Stanislaus Martin.

Many substances impart to the urine a peculiar odour, and asparagus gives a very strong one. Numerous experiments lead to the belief that this excrementitial humour does not reach the bladder in its perfumed condition; that it only attains this when in contact with the atmosphere, under the influence of agents it has dissolved, and that the formation of the aroma may be regarded as a true oxidation. This is not the case with those saline and coloured substances, which, not having become completely decomposed by the vital action, are often found in the urine in their natural state, or somewhat modified. All efforts, both with and without the aid of urine, to obtain a liquid analogous to that which is elaborated after eating asparagus, have been unavailing; which is easily understood, since the organic operation is conducted simultaneously upon the component parts of the urine and the ingested bodies, while in chemical experiments we act only on the body to be decomposed and urine ready formed.—*Bull. de Thér.* t. xxxix. p. 453.

PATHOLOGY AND PRACTICE OF MEDICINE.

On the Catarrhal Pneumonia and Lobar Pneumonia of Children. By MM. Trousseau and Lasegue.

Catarrhal (or lobular) pneumonia is a disease as distinct from simple (lobar), as variola is from erythema. This is seen in their respective mortality. Of twenty children who have been admitted to the hospital clinique, suffering from *simple pneumonia*, in six months all have recovered; of nearly thirty who were attacked with *catarrhal pneumonia*, not one survived. Most of the first class of cases exhibited an excessive degree of acuteness which burnt out like a fire of straw; while several of the second, notwithstanding their fatal termination, commenced with very mild symptoms.

Simple pneumonia hardly ever affects a child below two years of age, and rarely those of two or three, but becomes of more and more frequent occurrence as the child approaches adolescence. Its cause and symptoms resemble those of the adult, with some modifications. After twenty-four or thirty-six hours, the souffle and bronchophony can alone be heard; the crepitant râle, which is often observed in the adult when the patient coughs, even when much souffle is present, is hardly ever heard in the child. So afterwards, from day to day, without the crepitation of resolution, the souffle disappears, leaving only a feeble respiration. The progress of the disease is also more rapid than in the adult. In the mild form of the disease, recovery takes place rapidly, and in large proportion; but in its grave form, many cases are lost by any mode of treatment. M. Trousseau generally bleeds the child, gives it an emetic of sulphate of copper, and then a mixture, containing Kermes mineral and extract of digitalis.

Catarrhal pneumonia commences with a catarrh, which rapidly extends to the small bronchi, and then we hear numerous and small sub-crepitant *râles* disseminated over both lungs, and especially posteriorly. These *râles* may persist for four, six, eight, or fifteen days, without any *souffle* becoming manifest; but sooner or later we hear a *souffle*, the resonance of the cries or the voice, or at least a prolonged respiratory murmur. While these latter sounds, common to simple and catarrhal pneumonia, are thus manifesting themselves, we find, by the subcrepitant *râles*, that the capillary catarrh is still persisting in the rest of the lung. The disease has extended from the mucous membrane to the parenchyma of the organ. Febrile action is less than in ordinary pneumonia, being predominant at some portions of the day, and entirely ceasing at others; and these alternations of better and worse may continue for fifteen, twenty, or thirty days; the disease being originally a pulmonary catarrh, and partaking of the obstinacy and uncertainty of catarrhal complaints. As more and more of the parenchyma becomes implicated, the fever

becomes more continuous and intense, and the respiration more difficult, until the children die exhausted. In other cases, in which the bronchial phlegmasia was very intense from the first, and the lung became rapidly invaded over a great extent, death takes place with rapidity. The progress of the disease has usually been more rapidly fatal, when it has succeeded to measles, chronic disease of the skin, or laryngitis. All means of treatment that have been tried have proved impotent.

These two affections may be compared, *exceptis excipiendis*, with erysipelas and phlegmon. Erysipelas traverses the surface, like the catarrh; and when it persists too long, it induces ulcerations of the skin, furuncles, and circumscribed subcutaneous abscess, just as the capillary catarrh induces suppuration of the lobules, little abscesses of the lungs, and circumscribed pneumonias. Simple pneumonia, on the other hand, progresses like simple phlegmon, violent in its febrile reaction, but terminating abruptly and rapidly.

It must not be supposed, from what has been said, that catarrhal pneumonia is almost invariably fatal. Although this is the case amidst the miasmata of an hospital, which exert effects at once so terrible and so difficult to avert, it is not so in private practice. In this, one-half the patients may be cured, by repeated vomiting, flying blisters, antimonials, and digitalis; but how terrible are the ravages of a disease, which, under the most favourable circumstances, kills one-half its subjects!—*L'Union Médicale*, 1851, No. 115.

On the Occasional Ill-Effects of Tartar Emetic in the Treatment of Pneumonia.

By Dr. BOLING.

IT is not to the local irritation of the gastro-intestinal mucous membrane, as usually described by authors, that Dr. Boling alludes in the present paper, but to symptoms of a far more violent and sudden character, depending apparently on a metastasis of morbid action to the abdominal viscera. Other substances, freely administered, may probably give rise to the same effects; and the combination of calomel and antimony seems more prone to do so than antimony alone.

The patient may seem to be doing very well under the antimony, the dulness on percussion and rapidity of pulse diminishing, the skin moistening, and the respiration improving; when, suddenly in some cases, more gradually in others, he becomes restless, thirsty, and somewhat purged, the belly becoming tympanitic and sometimes tender. He vomits, or tries to do so; the tongue is dry and pointed; jactitation and anxiety of countenance appear, together with delirium, and perhaps shortly before death, stupor. Occasionally jaundice supervenes; and in a few cases the matter ejected closely resembles that of yellow fever. During these occurrences, the pulse becomes frequent, hard, small, and thready. Death may take place within six hours after the first appearance of these unfavourable symptoms; more frequently it is delayed for ten or twelve hours, and, in some cases, yet longer.

Simultaneously with the advent of the above symptoms, or just preceding them, there is a more or less rapid disappearance of the symptoms of the original disease. A lung which seemed almost completely solidified, in four or five hours becomes permeable, and yields a healthy respiratory murmur—all the symptoms of the pneumonia undergoing a similar improvement. The violence and rapidity of the abdominal disease are in direct ratio to the suddenness of the improvement in the disease of the lung. In any case of pneumonia treated by antimony, the supervention of the least tympanitis, thirst, and diarrhœa, must be looked upon with suspicion, as the probable precursor of this serious condition; and Dr. Boling regards the patient's doom as almost decided, when, in addition to these symptoms, there is a *rapid* in place of a *gradual* diminution of dulness on percussion, *unattended with the crepitant râle of resolution*. The observance of this peculiarity in the physical signs has enabled him to announce portending mischief in patients apparently convalescent.

This cannot be a rare occurrence in the Southern States of America, (Dr. Boling practises in Alabama,) as the author has seen almost as many die of the induced as of the primary disease. He suspects that some of the cases described as loss of tolerance of antimony by the Italian practitioners, are of this nature; although, if so, their delineation is very incomplete. Gölis gives a graphic account of a similar train of phenomena produced by the use of large doses of calomel in hydrocephalus and cramp.

Dr. Boling is disposed to attribute this effect of antimony to its direct action on the intestinal canal, by reason of a portion of the quantity administered not becoming absorbed; and believes that our object, in attempting its prevention, should be, to administer the drug in such divided doses as to secure its complete absorption. Although never in the habit of giving very large doses, he has even diminished these; and finds the remedy just as efficacious now that he gives only from three to six grains in the twenty-four hours, as when he gave double the quantity; while the mischievous effects have been of much less frequent occurrence. He dissolves the above quantity in six ounces of water, and gives a teaspoonful every half hour in the day, and two teaspoonfuls every hour at night. He prefers water as a vehicle, as mucilaginous fluids delay the absorption of the medicine. More recently, he has administered the medicine, in somewhat larger doses, as an enema with good effect.—*Amer. Jour. Med. Sci.*, No. 44, p. 30.

[Several French practitioners have recently spoken highly of the advantage of substituting large doses of *ipecacuanha* for antimony in the treatment of pneumonia. *Professor Broussonet*, of Montpellier, prescribes a mixture containing from twenty-two to forty-five grains, to be taken within the twenty-four hours, in divided doses, at very short intervals. He does not usually begin with it until the second or third day, when the vascular turgor has been abated by local or general bleeding; while, when resolution is slow, blisters are also employed. The medicine proves especially useful in the pneumonia of the aged, and in that of lymphatic temperaments disposed to scrofula, in whom the disease shows a disposition to become chronic—a class of cases often seen at Montpellier. *M. Ressiguier* also speaks in warm terms of the success following the employment of the ipecacuanha, declaring it to be the heroic remedy in the pneumonia of the aged when the effects of bleeding are feared. In the suffocative catarrh of the aged, too, it is highly valuable, many persons recovering from a very dangerous condition by its agency. *M. Delioux*, in a recent communication to the Academy, corroborates the above view; and considers that the utility of the medicine is by no means confined to the aged. Under its use the pulse becomes slower, the skin moist, and the expectoration easy; while the resolution of pulmonary engorgements and the resorption of pleural effusion are facilitated. He considers it to be more efficacious and better borne, the earlier it is commenced with; but he employs venesection also in almost all cases. Employed externally (one part to three of lard) it acts as a useful counter-irritant in laryngitis and bronchitis.—*Rev. Méd.* 1851, vol. i. p. 541; *Rev. Méd. Chir.*, tom. 8, p. 354; *Bull. de Thérap.*, tom. xli. p. 151.]

On Amaurosis in connexion with Bright's Disease. By Dr. Hays.

M. Landouzy and other French practitioners have of late directed attention to the frequent coexistence of amaurosis with albuminuria—a connexion, indeed, long since noticed by Dr. Bright himself. In a recent discussion at the Philadelphia College of Physicians, Dr. Hays observed, that he had seen several cases of amaurosis, with striking protrusion of the eyeball, all being accompanied by albuminous urine, and exhibiting the granular kidney after death. In all these cases, too, there was a peculiar brownish-yellow tint of the skin, especially that of the face, which Dr. Hays is disposed to regard as characteristic of Bright's disease. He felt very desirous of drawing attention to the fact of the frequent dependence of amaurosis upon granular disease of the kidney, and to the importance of early

diagnosis—the early stage being that alone in which treatment can prove of any avail. In every case of amaurosis, attention should be paid to the condition of the urine.

Dr. Pepper observed that he, too, had met with the amaurotic condition of the eyes in connexion with granular disease of the kidney; but he had not observed prominence of the eyeball.—*Amer. Jour. Med. Sci.*, vol. xxii. p. 274.

On the Pathology and Treatment of Delirium Tremens. By Dr. MOREHEAD.

DR. MOREHEAD believes that the delirium and sopor of *intoxication* are due to the immediate influence exerted upon the brain by the large quantity of alcohol contained in the blood, and that these effects are recovered from only as the alcohol is eliminated. So in *delirium tremens*, the symptoms are due to the same poison more slowly introduced, and, so to say, incorporated with the cerebral substance, and are recovered from after a slower process of elimination. This view he thinks is confirmed by the fact, that the treatment which keeps in view the moderating of the symptoms, rather than their extinction by opium, (many cases indeed doing well without any treatment at all,) is the safest and best. It is also illustrated by what is observed in cases of *poisoning by datura*, which gives rise to a form of delirium very like alcoholic delirium, and yet no one would think of treating it by opium. Temporizing measures are pursued, and the patient's strength husbanded, while the poison is being eliminated. Experience has amply shown the danger of the *free* use of opiates in the treatment of delirium tremens, at all events as it is met with at Bombay; while the *cautious* use of this drug, with cold affusion and tartar emetic, has been very successful. "The opium in moderate doses calms the deranged action of the brain resulting from the direct action of the alcohol; the tartar emetic and cold affusion control and moderate the excessive distribution of blood in the capillaries of the organ." Large and frequent doses of opium only add to the congestion, and aim at the fulfilment of an indication sought to be obtained in no other form of poisoning—viz., the dissipation of the effects of a foreign agent before time has been allowed for its elimination.—*Trans. Med. and Phys. Soc., Bombay*, No. 9, p. 128.

On the Simultaneous Occurrence of Hyperæsthesia and Anæsthesia of the Skin in Neuralgia. By Dr. TURK.

DURING or after an attack of severe neuralgia, there is sometimes a more or less considerable amount of hyperæsthesia of the superficial layers of skin seated over the affected parts, so that not the least contact can be borne. Much oftener, however, the contrary is the case—viz., anæsthesia of the superficial layers over the points of deeper-seated pain. The degree of this superficial anæsthesia is proportionate to the amount of pain in the deeper layers of the skin; and it is sometimes so considerable that nipping the skin with the nails, or blistering it by the application of heat, is unfelt. These opposite conditions of the same part may be observed for some time after the attack of spontaneous pain has disappeared. The two conditions do not always observe the same limits. Sometimes the most superficial layer of the skin only is anæsthetic, and the formation of a moderately thick fold of skin causes pain; while at other times the chorion is anæsthetic throughout, and thick folds of the skin may be pinched up, pain being only felt at the subcutaneous layers.

The intensity of the anæsthesia diminishes in proportion to the distance from the site of the spontaneous pain. An entire side of the body has been observed to be anæsthetic; but the insensibility has usually become so diminished towards the boundary, as to be incapable of being tested by irritation by the finger, though readily so by the application of hot or cold bodies. This hemiasthænia sometimes

extends to the wall of the mouth, and side of the tongue. Oftentimes it has been only observed over the greater part of half the body, certain portions of the trunk or head being exempted. In some cases the functions of the senses have also been found disturbed.

The hyperæsthesia of the deeper layer of the skin rarely assumes such an extension as the anæsthesia of the superficial ones; though cases have been observed in which it, too, has occupied one entire half of the body. In double-sided neuralgia, the anæsthesia and hyperæsthesia are also double, being more intense on one side than the other, if that is the case also with the neuralgia. In some cases, both sides of the body are affected symmetrically, having their limits then drawn horizontally.

Besides the occurrence of these conditions in neuralgia, it not unfrequently happens, according to the author's experience, that in typhus there is a more or less intense hyperæsthesia of the deeper layers of the skin in various parts of the body, especially the calves of the legs. It is sometimes so considerable, that the half-soporose person utters expressions of pain under the influence of even but moderate pressure. Examined after the fever had run its course, no abnormal conditions of sensibility were observed in some of the patients; but in others spontaneous pains, accompanied by hyperæsthesia and anæsthesia of different parts (especially the leg and foot), and in different degrees, remained for weeks.—*Froriep's Tagsberichte*, 1851, No. 273.

On Nausea and Vomiting as signs of Cardiac Polypus. By Dr. Baskin.

By the term cardiac polypus, the author alludes to those fibrinous concretions which become organized some time prior to death, and by their presence in the heart occasion decided disturbance of its functions, and ultimately endanger life, or cause death. He relates two cases, in order to show that the sudden occurrence and continuance of nausea and vomiting, concurrently with other symptoms of heart-disease, may be considered as a pathognomonic symptom of the formation of these polypi. In these two cases there occurred a sudden aggravation of the symptoms some weeks prior to death; and simultaneously therewith, the patients were seized with nausea and vomiting, which continued until death. Aran and Hope refer to this symptom.—*Amer. Journ. Med. Sc.*, No. 44, p. 404.

On the Appearance of the Tongue in Malarious Diseases. By Dr. Osborne.

Dr. Osborne states that his attention has been directed, for several years, to a peculiar appearance of the margin of the tongue, which, from its invariable presence in diseases and predispositions generated by malaria, he terms the *malarial margin*. The changed condition of the edge of the organ occupies a greater or less space, according to the extent to which the infection of the system has occurred. The colour is usually a faint blue, which becomes lost or merged among the various colours imparted to the tongue in different diseases. The most fixed condition is an appearance of indentation or transverse crimping, which is apparently confined to the sub-mucous tissue, the superficial integument continuing smooth, moist, and transparent. It appears like a continuation or encroachment of the inferior surface of the tongue upon the superior and lateral edges, being greater the nearer the root of the organ is approached. Dr. Osborne has had great opportunities of observing this appearance in individuals of the white, black, and mixed races on the banks of the Warrior river (Erie, Ala.); and in no case has its presence failed to indicate the agency which the remote cause of fever was exerting on the system. In a considerable number of cases of neuralgia, dropsy, and inflammation, its presence has alone led to the true diagnosis of the malarial nature of the disease, when all other signs of this have been absent. In many instances, it has caused life to be saved by indicating the necessity of administering quinine.—*Amer. Jour. Med. Sc.*, No. 44, p. 555.

SURGERY.

On the Treatment of Fracture of the Clavicle without Bandages. By M. ROBERT.

M. ROBERT, on presenting to the notice of the Society of Surgery a case of fracture of the clavicle, in which the displacement, that had been considerable, left no traces of its existence, commented on the inconveniences attendant upon the employment of the bandages usually employed in this accident. The pad placed in the axilla sometimes induces ill effects, which may continue long after the cure is accomplished; such as painful swelling, with consequent stiffness of the arm and hand, and compression of the nerves, even to paralysis—inconveniences which are nowise compensated for by the advantages of the bandage. M. Robert dispenses with bandages, and makes the patient lie in bed, entirely supported on the sound side. A pillow is, to this end, placed under the back; and the injured shoulder, abandoned to its own weight, falls backward. The cases in which he has tried this plan, without encumbering the patient with bandages, have constantly done well, consolidation taking place in from twenty to twenty-five days.—*L'Union Medicale*, 1851, No. 112.

On Lithotrity. By Professor PANCOAST.

PROFESSOR PANCOAST states that, in private practice, he "crushes" at least four or five times, for once that he cuts for the stone; and mentions some of the circumstances which guide him in the choice of the mode of operating.

1. *No female should be cut, except in a case of urgent necessity.*—Calculi of even the hardest description may be broken up without difficulty, as the female urethra will admit very powerful instruments. Large calculi are thus more easily treated in women, as in them there is no deep *bas-fond*, and no prostate to become enlarged, fungous, or irritable. "The exceptional cases, in which cutting is allowable, are those in which the stone is of moderate size, and jammed-in at the inner end of the urethra, so as not to be readily pushed back, or when a rough stone of considerable dimensions has excited so much irritation and thickness of the coats of the bladder as to make that organ utterly intolerant of such a degree of distention by the injection of fluids, as to enable us to open and shut the lithontriptic instrument without injury to its coats. In two cases of this sort I have, however, succeeded in getting a sufficient degree of toleration for the use of the instrument, by injecting two or three ounces of sweet oil instead of water or mucilage."

2. *In male children, lithotomy should almost always be preferred.*

3. *In respect to the condition of the urethra*, most writers state that stricture, undilatability, or irritability of this part contraindicate lithotrity; but Professor Pancoast has always been able to overcome obstacles arising from these conditions. If strictures do not otherwise yield, he divides them with Dr. Dodd's instrument, and then dilates them. When the irritable or undilatable state of the canal is not relieved by the gentle use of dilating instruments, the frequent use of the warm bath, and the correction of the state of the urine by regimen, he gently cauterizes the canal, and after an interval reverts to these means again.

4. *The condition of the urinary apparatus.*—The choice of an operation at puberty and upwards, must depend upon the state of the prostate, bladder, and kidney. If the *kidney* be seriously involved, and especially if the symptoms indicate suppuration, the system is already too much undermined to withstand the shock of lithotomy and the protracted after-treatment. If any operation at all be admissible, it is lithotrity. If the *bladder* "be diseased in its mucous coat only, although freely throwing out mucus, muco-purulent matter, or pus, or acting upon the urine, so as to cause a deposit of white gravel on its lining membrane, with the

prostate and neck in a tolerably healthy condition, and capable of bearing for a few minutes, without much suffering to the patient, the injection of half a pint or more of warm water or mucilage, and the stone be not excessively hard, it is, I believe, your bounden duty in all cases to operate by the crushing process." When the organ will not easily bear such dilatation, is hypertrophied, columnar, or sacculated, and is easily irritated by instruments, it is the condition of the *prostate* that must determine our choice. "On this subject, which has given rise to much conflict of opinion among writers, I entertain somewhat peculiar opinions, the result of a good deal of experience and careful observation. If the prostate be healthy, firm, and but little or not at all enlarged, it is, I think, the duty of the operator to crush; but if it is enlarged generally, and especially in its middle lobe, and, as is commonly the case under these circumstances, bleeding after the use of the sound, and with a deep *bas-fond* behind it, it is, I believe, better to cut. It might seem to you that the large size of the prostate should offer a contraindication to lithotomy, and render lithontripsy the more eligible operation. But you must recollect that the larger prostate gives a deep *bas-fond*, and the fragments of stone will therefore not so readily escape, requiring to be minutely broken, which would necessitate the frequent use of the lithontriptor. The irritation arising from this, and the motion of the fragments in the bladder, will tend to produce so much vesical irritation as to endanger and perhaps destroy life. . . . . The enlargement of the prostate is, *per se*, a strong reason with me in favour of lithotomy. It is in itself a disease, over which our medicines have but little influence. It is said by Lallemand, that the most effectual method of reducing its size, and relieving its distressing symptoms, is to divide it freely; and for this object alone he has made the bilateral cut, as in the operation for stone. Of this I am well assured, that I have seen it greatly reduced in size, and cease to cause much inconvenience, after lithotomy."—*Phil. Med. Exam.*, vol. vii. p. 166.

On Opening Bubos by Multiple Punctures. By M. VIDAL.

M. VIDAL strongly recommends that venereal bubos should not be allowed to open of themselves; for when they are left to nature, the skin becomes detached, and thinned, a great loss of substance ensues, a tedious recovery takes place, and an unsightly deformity is left. Opening by caustic, too, leaves disfiguring scars; and the same inconvenience results from large incisions, and cutting away, by the bistoury, portions of skin that are too much changed to unite. Still the bistoury is much more easily managed than is the caustic, and M. Vidal much prefers removing by it a portion of half dead, detached skin, or a gland which is an obstacle to reparation, to attacking such parts by caustic. By its aid the cicatrix may be rendered more regular, and cause less deformity. But cases requiring large incisions and excision are rare, especially if the bubo be early treated, and punctures are made as soon as matter is formed.

The bubo should be shaved, and any remains of plaster &c. removed. A straight, narrow bistoury, or even a lancet, may be employed. If the abscess is recent, and suppuration not extensive, *one* puncture at the fluctuating point may be employed, as a single gland may be then inflamed, and we thus discharge the pus from it. The other glands are only engorged; but if they afterwards suppurate, they must be treated in like manner—so that in this way we may have to open three or four of them in succession, especially in scrofulous subjects. When the abscess is intraglandular, its deeper seat renders it more difficult of recognition; and it is far better opened by a straight bistoury, which also acts as an exploratory instrument, than by caustic. If the collection be both extensive and superficial, several simultaneous punctures are required. These must not be practised in the thin skin of the fluctuating centre, but at the circumference. The bistoury is passed in obliquely, and guided subcutaneously towards the centre, arriving thus at the pus by a circuitous route. In this way, we divide the skin only where it is adherent,

intact, and possessed of its vitality; while if we penetrated the denuded, half-dead, and thinned skin of the centre, there would be danger of the apertures enlarging by mortification, and approximating to each other so as to constitute a large breach of surface, giving admission to the air, and being followed by all the disadvantages of large openings made by the knife or caustic. The bubo must not be pressed for two days after making the punctures, and it will then gradually discharge itself, the space being filled up in proportion as the pus is evacuated, and the walls of the abscess are retracted. The cure is rapid, and after its completion not a trace of the disease is left—the cicatrices which remain not showing more than leech-bites, and like them eventually disappearing. Sometimes the little oblique tracts made by these punctures become obliterated before the pus is completely evacuated; but as it is rare for them all to do so, we may, by gently pressing the tumour once a day, discharge the pus by such of them as remain open—this compression being uncalled for during the first day or two, when the tracts are free, and the pus abundant. Occasionally all the tracts show a great tendency to close up, and it is preferable to allow them to do so, rather than apply tents to keep them open—making one or two new punctures if required, which is not always the case, as the remaining pus is sometimes resorbed.—*Bull. de Thérap.*, vol. xli. pp. 204—212.

On Anchylosis of the Lower Jaw. By Dr. WERNHER.

S. R——, æt. twenty-three, when three years of age underwent severe salivation, after which the jaw remained in a fixed state. Notwithstanding the absence of masticatory power, he was well nourished. The jaw was quite immovable, firm pressure or traction exerting no effect upon the position of the teeth. The incisors and molars were indeed, for the most part, wanting, the roots of such as did exist projecting beyond the alveoli of the diminutive jawbone. The jaws were so far separated, that with some trouble the little finger could be introduced in front; but from the anterior molars on each side backwards, bony arches connected the upper and lower jaws. The buccal mucous membrane was attached to the gums at the anterior edges of these arches, but the temporal and masseter muscles remained free. Speech much resembled that which takes place with the mouth closed; and food which did not require mastication was introduced between the defective teeth. To remedy this state of things, the gums were separated by an incision from the cheeks and lips, and a broad portion of the connecting arch on either side removed by a small saw. The jaws could now be expanded, by aid of a mouth-speculum, to the extent of half an inch, some painful stretching of the muscles being induced. The patient was enabled, too, to voluntarily close the mouth again, proving that twenty years of inactivity had not destroyed the functions of the joints and muscles. After several weeks' perseverance in gradual dilatation, a still wider expansion was obtained, enabling the patient to chew food that was not too hard, which, indeed, the loose state of his teeth also prevented him biting.

Anchylosis of the lower jaw may occur in three localities. 1st. The head of the condyle may become fixed in its glenoid cavity. This is the most frequent form, examples of which are recorded by Sandifort, Blandin, Cruveilhier, Howship, Holscher, Hyrtl, &c. 2nd. The coronoid process may become attached to the zygomatic arch. Of this but two observations are recorded, one by Sandifort ('Museum Anat.' vol. iv.), and by Sebastian in his 'Essay on Anchylosis,' published at Groningen, 1826. 3rd. The alveolar processes may become conjoined. Of this there are three examples on record, besides the one now narrated, which are to be found related in Walther's 'Museum Anat.,' in 'Rust's Magazin,' Band 1, and in Bonnet's (cited from Kunholz) 'Maladies des Articulations.'

Of the fifteen cases of anchylosis of which Dr. Wernher has thus been able to collect accounts, both sides of the jaw were affected in seven, and one side in eight. In the three examples of osseous connexion of the alveoli, the incisors

were somewhat separated. It has been supposed by Cruveilhier and others, that anchylosis of the one side entails that of the other by the complete immobility it induces; but in seven of the fifteen cases quoted, the joint on one side remained quite free. How little prolonged immovability is a cause of anchylosis, may be judged of by the case now recorded, in which it had continued for twenty years, and by others of a much longer continuance recorded by Walther, Cruveilhier, Howship, and Kunholz—facts applicable to the question of anchylosis in general, and opposing the theoretical views of several eminent surgeons.—*Oppenheim's Zeitschrift*, Band 44, pp. 375—386.

On the Treatment of Sprain by Hot Water. By Dr. JACKSON.

WE had recently (vol. vi. p. 542) occasion to refer to M. Baudens' mode of treating sprain by the prolonged use of *cold* water; and now Dr. Jackson informs us that, as the result of multiplied experience, he can earnestly recommend placing the sprained limb immediately in very *hot* water. He does not remember having had recourse to it at a later period than two hours after the accident; but he believes it should be employed at any time before the setting-in of inflammation, the tumefaction immediately consequent on the accident being no obstacle. As soon as the pain is completely relieved by the hot water, the patient should be put to bed with his limb raised; and after a few hours, cold is to be applied, whether there is pain or not. Low diet, purging, and in the plethoric, venesection, are required. If inflammation has already set in, free leeching should be resorted to, dispersing it after its activity is allayed by a large blister, caused to rise rapidly, and afterwards dressed by plantain or cabbage-leaves. Although hot applications are so useful prior to the occurrence of inflammation, warm poultices, after this has become developed, are mischievous, augmenting the *engorgement*. In protracted sprain, salivation may be required.—*Amer. Journ. of Med. Sc.*, vol. xxi. p. 345.

[While M. Baudens declares that the application of cold water alone suffices for the cure of the sprain, Dr. Jackson, it would seem, from the above account, only resorts to hot water as a means of procuring some alleviation of pain, as after its application he follows the ordinary means of treatment. We have great confidence in the early and continuous application of a strong lotion of muriate of ammonia.]

On Aneurisms of the Extremities. By M. CHASSAIGNAC.

DURING the relation of a case of popliteal aneurism, M. Chassaignac makes several general observations of interest.

1. *Inequality of the thickness of the coagulum of an aneurismal sac.*—The coagulum is sometimes traversed by an accidental longitudinal canal, maintaining the circulation through the aneurism. An examination of this canal, first in an aneurism of the lumbar aorta, and subsequently in various popliteal aneurisms, shows that it never traverses the coagulum in its centre, but that the thickest portion of the coagulum always occurs exactly opposite the portions of the walls of the vessel that have undergone greatest dilatation.

2. *Non-obliteration of collateral arteries arising from the walls of an aneurism.*—It might be supposed, *à priori*, that the distension of an aneurismal pouch by a coagulum, and the diseased condition of the arterial walls, must inevitably lead to the obliteration of vessels springing from it. In many aneurisms that he has examined, M. Chassaignac has found, that while in all other parts of the aneurismal sac the internal and middle tunics have been replaced by atheromatous substance, at the orifices of the collateral arteries which opened into the tumour, the internal tunic completely preserved all its properties. The orifice of each artery opened into the centre of a disc, as clean and as completely defined at its circumference as if punched out of a sound artery. The cause of the preservation of these sound parts of the tunic, when all around is so diseased, is a

curious subject of inquiry; but upon such preservation seems to depend the maintenance of the circulation in those arteries at the orifice of which it exists. In several preparations of aneurism examined, M. Chassaignac has found that the sole collateral arteries which were obliterated were just those unprovided with the protective disc.

3. *Diagnosis.*—It is remarkable, in the case in question (produced by the kick of a horse), how long the diagnosis continued obscure. In fact, only three weeks prior to the operation, so experienced an observer as M. Cruveilhier could detect no pulsations. So rapid, however, at last, did the progress of the aneurism become, that it doubled its size within the forty-eight hours prior to the operation.

The *auscultation* of this tumour furnished M. Chassaignac with a new occasion of pointing out the difference which exists between the simple *souffle* heard in aneurism of the limbs, and that which results from varicose aneurism, dependent upon a communication between the artery and vein. This last is a confused, tumultuous, boiling sound, which once heard can never be forgotten. The *longitudinal propagation* of this boiling sound is a sign first indicated by M. Chassaignac, the sound being heard with great intensity when the ear is applied in the course of any of the veins communicating with the artero-venous aneurism, whether situated above or below the point of communication. In ordinary aneurism, there is a *bruit de soufflet* or *de râpe*, distinctly intermittent, and audible neither above nor below the tumour; while in varicose aneurism the sound resembles a continuous subterraneous boiling noise, propagated longitudinally.

4. *Pain during the application of the ligature.*—Notwithstanding the certainty that no nerve was included, the patient complained of severe pain when the ligature was tightened; and M. Chassaignac's experience has taught him, that however isolated the nerves may be, there is always severe pain at the moment of constriction—a pain differing, however, from that consequent on the inclusion of a nerve. When a nerve is included, the pain is immediately referred to the part where it is distributed; while, when this accident has not taken place, the patient does not indicate any especial part as suffering.—*Archives Générales,* tom. xxv. pp. 26—51.

On Amputations in Children. By M. GUERSANT.

THE amputations at the *Hôpital des Enfants* are of frequent occurrence, not less than from eighteen to twenty taking place annually; being usually performed for white-swelling or other chronic disease. M. Guersant is, however, no advocate for hasty operations in such cases, as the lymphatic habit upon which the disease of the joint depends may often be ameliorated, and a valuable though an imperfect limb be preserved. Much depends upon the social position of the parents. The working-man has not at his command those resources which may be required for years during an endeavour to preserve the limb of the child; and after the operation the latter may be apprenticed to many trades, even though he has a wooden leg. The child placed in easy circumstances can command prolonged medical attendance, sea-air, change of climate, or whatever may be deemed beneficial, and amputation need not be performed until all other means have been exhausted. After a long period, however, all the chronic disease in a scrofulous child suffering from arthritis seems to concentrate itself in the diseased joint; and upon the removal of this, his health may become re-established. Amputation frequently succeeds better in debilitated than in very strong and vigorous children.

Whenever possible, M. Guersant prefers the months of May, June, and July, for the operation, as unfavourable complications are of more common occurrence in the cold and changeable seasons of winter and spring. The child requires but little preparation; the means which have already been employed for the improvement of its general health, as iodine, bitters, cod-liver oil, &c., all placing it in the best condition for undergoing the operation. If a large eater, the food should be somewhat

diminished two or three days before; and any existing diarrhœa must be arrested by anodyne injections and bismuth.

M. Guersant sometimes employs the oval operation, but hardly ever the circular. In most cases he prefers the flap, which renders the co-operation of the assistants easier, occasions little inflammation or suppuration in children, frequently allowing of union by the first intention, and affords a better covering for the bone. Chloroform is employed, and the principal artery of the limb carefully compressed, so as to avoid hæmorrhage. In very hot weather, the edges of the wound are united by some points of suture, and the stump left exposed to the air. When bandages are employed, the stump is dressed daily. On the evening of the operation a little broth is allowed, next day a stronger soup, and the day after that sometimes a little roast-fowl.

By observing these rules, M. Guersant finds, as a general rule, that eight or nine cases in ten recover. If erysipelas occur, leeches are applied to the nearest lymphatics; and if these do not suffice, a circular blister is placed around the stump; emetics and purgatives, but especially the former, being given. In cases of purulent resorption, he has obtained some benefit from aconite. If the surface of the wound takes on a greyish colour, and becomes covered with false membranes, chlorined water or lemon-juice is the best application. When union by the first intention does not take place, the inner lip of the wound should be stimulated, and then strapping applied; and when fistulæ occur, they will usually be found dependent upon small portions of bone tending to necrosis.—*Gaz. des Hop.*, 1851, No. 54.

[A writer in the *Bull. de Thérap.* (tom. xl. p. 81) observes, that M. Guersant did not lose a single case of amputation during 1850, though the thigh, arm, foot, and shoulder, were among the parts removed. The greater success of operations on the young has long been known, and is usually attributed to the greater vitality of childhood and the absence of mental disquietude. However this may be, M. Guersant's especial success is probably, in a great measure, due to his habit of ordering good, nutritious diet as soon after the operation as possible. Under the influence of this, the children rapidly recover strength and flesh, the wound assumes a healthy aspect, and the colliquative diarrhœa, so common prior to the operation, ceases. Abstinence is ill borne at this tender age, and most of these children have become exhausted by suppuration prior to the operation.]

MIDWIFERY, &c.

On Delay in Dividing the Funis. By Dr. Storer.

Dr. Storer observes that it has been for many years a rule with him to wait until all pulsations have ceased to be felt, before applying a ligature to the funis. Early in his career he met with two cases in which profuse bleeding occurred from the funis after it was divided, which could not have happened had not the blood continued flowing in the cord. As in the vast majority of cases the pulsation ceases in a few minutes (in one only has he known it continue for twenty-five), it seems best to listen to the dictates of nature, as considerable danger of injuring some of the important viscera may arise from too suddenly diverting the circulation upon them. It is not unusual to find the funis pulsating with great force at birth; and is it not more rational to allow this to subside gradually, than to check it all at once? May not some of the cases of hæmorrhage from the mouth and nose, which have been published, be due to the pulmonary congestion thus caused? Dr. Channing recently met with a case in which the child, born apparently quite healthy, died while being dressed, from violent hæmorrhage from the mouth, none proceeding from the funis, which had been tied directly after birth.—*Amer. Jour. Med. Sci.*, vol. xxii. p. 82.

Wine and Honey in Infantile Marasmus.

Dr. Baun states, that in the marasmus of infants he has derived truly remarkable benefit from the employment of a mixture consisting of one part wine and two or three of honey, giving several tea-spoonfuls daily. Not only Madeira but good Burgundy may be so employed, or when diarrhœa is not present, the Rhenish wines. Refreshing sleep, and an increase of animal temperature, are the first effects, and an improved digestion a later one.—*Journ. für Kinderkrank.*, xvi. p. 412.

Treatment of Asphyxia Infantum.

Dr. Tott states, that he has often succeeded in restoring life in the *asphyxia asthenica infantum* after the failure of the usual means, by causing a person to stand on a table, and pour cold water from a tea-kettle on to the pit of the stomach. In this way Professor Hasselberg saved many lives.—*Jour. für Kinderkrank.*, xvii. p. 15.

Discussion on Congenital Syphilis.

An interesting discussion on congenital syphilis has occupied the Academie during several sittings, the more prominent points only of which our space enables us to notice. The immediate occasion of it was the presentation of a report by M. Cazeaux on a memoir, recently submitted to the Academie by M. Depaul. Our readers are aware that M. P. Dubois some time since expressed his belief, that abscesses met with in the *thymus* of the fœtus are of syphilitic origin; and M. Depaul makes the same declaration in respect to multiple abscesses, which are occasionally met with in the *lungs* of infants born dead. He has himself met with fifteen of such cases, most of them, also, exhibiting pemphigus and other evidences of syphilis; and he considers that the fact of a fœtus manifesting such abscesses would warrant the treating the parents for syphilitic infection, even though they exhibited in their own persons no signs of its existence. To these conclusions the reporter entirely demurs, objecting to the insufficiency of the proof of the syphilitic character of these abscesses; and denying, even when pemphigus is present, upon which M. P. Dubois is accustomed to lay so much stress, that such proof is strengthened, inasmuch as the best observers are agreed that syphilis does not exhibit itself until several days after birth, and that pemphigus is a very rare form of manifestation—depending probably, in the cases in question, upon some hygienic errors. He strongly disapproves of treating the parents for syphilis on such insufficient grounds. M. Gibert also believes this would not be justifiable, save in the case of the production of successive dead children, and at the desire of the patient. As a general rule, when the product of conception is infected, marked symptoms of constitutional syphilis may be discovered in one of the parents. M. Huguier observed, that among several hundred children who have been born of syphilitic mothers at the Lourcine, he has never seen one presenting pemphigus, or other syphilitic symptom, at birth—the eruptions usually appearing from the tenth to the thirtieth day.

On the other side, MM. Ricord and Lagneau believe, that the opinion entertained so generally at the end of last century, and beginning of this, of the existence of a syphilitic cachexia, capable of generating a variety of diseased conditions of organs, is not destitute of foundation; and that the production of cachectic, or dead children, and the repeated occurrence of abortion, amply justify the putting one or both parents under treatment. M. Paul Dubois combated the objections of the reporter with great vigour. He stated, that as regards suppuration of the thymus, he had not advanced it as a conclusive sign of syphilis, but as a highly probable one, seeing its frequent coexistence with other symptoms of the disease, requiring, however, additional investigation. With respect to syphilitic *pemphigus*, however, he expresses himself most positively; and his assurance

of its existence at birth, founded upon his vast experience, is highly important, opposed as it is to the statements of Trousseau and other able writers on the diseases of infants, who agree in stating that syphilitic eruptions never appear until several days after birth. The pemphigus which M. Dubois regards as syphilitic is characterized by very large and closely approximated vesicles, containing a very yellow pus, and especially occurring on the palms and soles, where they lie on a blue or violet-coloured skin, strongly contrasting with the rose-colour of other parts. Some have become burst and emptied prior to birth, others are only just appearing, while others again are quite evolved. In all the cases in which the pemphigus has put on these characters, the children have died in a few days, whatever care may have been lavished on them. The pemphigus, occurring as a few scattered vesicles a few days after birth in feeble children, is not syphilitic. He does not regard the treatment of the parents for syphilis, when properly conducted, as a very serious matter; and it is not to be compared in its ill-effects with a succession of dead children.

M. Cazeaux terminated the discussion with the following conclusions:—1. In the immense majority of cases, the symptoms of constitutional syphilis are not manifested until some weeks after birth.—2. Such excessive rarity should give rise to great caution in the nosological classification of the lesions of new-born children.—3. The coincidence of old syphilis in the parents, and of doubtful or ill-characterized symptoms in the infant, is insufficient to establish the relation of cause and effect between the two facts.—4. If pemphigus, and abscess of the lungs and thymus, may rigorously be referred to a syphilitic origin, there is no proof that they may not be due to other causes.—5. Pemphigus has long been observed, and referred by authors sometimes to syphilitic cachexy, and sometimes to other causes.—6. In the actual condition of science it is impossible to distinguish alterations which may be syphilitic from those which result from other morbid conditions of the parents or fœtus; and this uncertainty should render a practitioner very circumspect in pronouncing an opinion upon the nature of such lesions.—7. It is not allowable to put parents under treatment, when the child exhibits only doubtful signs of constitutional syphilis; but even were such signs certain in the child, we must seek for their exact explanation in the history of the parents.—8. The question is still open for study, and requires numerous researches for its elucidation.

Dr. Behrend, the able editor of the *Journ. für Kinderkrankheiten*, referring to the above discussion, lays down the following positions, as to the results of his own experience. 1. Syphilis may be imparted to the fœtus either by the father or mother, or by both together. It is most frequently communicated by the father.—2. The general effect of this syphilitic poisoning on the fœtus is the production of a diminution of its vital energy—a diminution varying in intensity, according to the degree of virulence of the disease in the parents, and the congenital vital capabilities of the fœtus.—3. Under the influence of a very considerable diminution of this vital energy, the fœtus dies between the third and sixth month, and is expelled. Most of the cases of so-called habitual abortion depend upon the syphilitic condition of one or both parents.—4. If the fœtus remain in utero until the eighth month, it may die either during the act of labour, or soon after, mostly exhibiting very remarkable appearances that can only be ascribed to syphilis.—5. These are chiefly excoriations on various parts of the body, especially the feet and hands, as if these had been scalded; rhagades at the angles of the mouth and alæ nasi; superficial ulceration of the nares; condylomata within the mouth; pemphigus; rupia; ecthyma; cutaneous ulcerations; tophi and caries.—6. To the doubtful, but very probable, signs of syphilis appertain suppurations of internal organs, but especially of the lungs and thymus; for the due estimation of which, however, additional observations are required.—7. Roseola syphilitica, appearing in a fortnight or later, on a healthy child, may be a consequence of congenital syphilis; but it may also have arisen from a fresh infection acquired during or immediately after birth.—8. The birth of a child suffering from congenital

syphilis fully justifies the antisyphilitic treatment of the parents, and especially the mother, as soon as she becomes pregnant again, even if she has not just before, or at an earlier period, herself manifested symptoms. It is through the mother we must influence the fœtus, and endeavour to save its life.—9. If the mother or father manifest signs of the syphilitic dyscrasis, we must at all times employ antisyphilitic treatment, in order to enable them to propagate sound children.—10. A child born with symptoms of syphilis must be treated as early as possible for these, in order to give it the only chance of living.

As it has frequently been stated, that women are incapable of producing children while actually labouring under syphilis, Dr. Beitler relates a very remarkable case, in which a woman during 14 years exhibited very marked secondary and tertiary syphilis, and bore during that period four living children, her symptoms undergoing exacerbation at each pregnancy. The children exhibited no external signs of syphilis, and died of other diseases several years after. The woman never aborted, and her husband remained unaffected. Dr. Spiro of Moscow also states, that among 64 pregnant women, suffering from syphilis, who came under his own care, 6 became pregnant while undoubtedly suffering from secondary syphilis. In 12 this was doubtful; and in 46 the syphilis was acquired during pregnancy. He believes that the conceptive power of the female and generative energy of the male are considerably diminished by syphilis.—*Bulletin de l'Acad.*, xvi. pp. 920, 970, 1009, 1226; *Revue Médicale*, 1851, vol. ii. p. 147; *Journal für Kinderkrankheiten*, xvii. 38; *Casper's Wochenschrift*, 1851, No. 39.

MATERIA MEDICA AND THERAPEUTICS.

On the Topical Use of Chloroform. By Dr. RAUCH.

To obviate the volatile character of chloroform when employed topically, Dr. Rauch combines it with olive oil and some liquor ammoniæ, forming an emulsive liniment. This is less expensive, relieves sooner, and is not so volatile as chloroform. The ingredients were at first employed in equal parts; but were afterwards used in other proportions, according as to whether a counter-irritant effect (when more ammonia and chloroform must be added) were desired or not. It is applied on a woollen cloth, so folded that the inner layer is saturated by the liniment, and the outer kept dry, so as to prevent evaporation. When first applied, it feels cool, then smarts and burns so for ten minutes as hardly to be borne; and then an agreeable coolness, with relief of pain, succeeds. When it causes too much irritation or vesication, it should be removed, or applied to another locality. The skin is made red by it, and often vesicated; and if a mere rubefacient is required, it should be applied by friction, or the cloth should remain on only for a short time. When a speedy vesicant effect is required, it is more useful than a sinapism or blister, and is easier of application, especially in children, who often fall asleep during its application. Dr. Rauch found it of great use, combined with other means, in cholera; and in relieving the painful affections of the abdomen in children, it is preferable to any anodyne. In the case of superficial burns, a compound of equal parts of chloroform, olive oil, and lime water, has been found highly useful.—*Amer. Jour. Med. Sci.*, vol. xxii. p. 112.

On Ung. Picis in Ophthalmia Tarsi. By Dr. PARRISH.

DR. PARRISH, believing the obstinate disease *ophthalmia tarsi* to be analogous in its nature to *tinea capitis*, in which *ung. picis* proves of such great service, has resorted to it in the former affection with almost undeviating success. It is carefully rubbed into the roots of the *tarsi* at bed-time, and washed away with Castile soap and tepid water in the morning.—*Amer. Jour. Med. Sci.*, vol. xxii. p. 274.

On Chloric Ether as a Disinfecting Agent. By Mr. HILDRETH.

THE gas which results from the combustion of chloric ether in a common lamp (consisting chiefly of hydrochloric acid with a little chlorine) may, Mr. Hildreth states, be advantageously substituted for chlorine as a fumigating agent. As such it has been employed in the Massachussetts General Hospital without inconvenience. Dr. Warren states that he has given it a full trial, and considers it the best and most convenient gaseous deodorizer. He burns it in a lamp like camphine.—*Amer. Jour. Med. Sci.*, vol. xxii. p. 86.

On Sumbul in Delirium Tremens. By Dr. MEINHARD.

NUMEROUS hospital observations at St. Petersburgh show that the root of Sumbul proves a rapid and desirable means of cure of the delirium tremens of drunkards, being in very many cases to be preferred to opium. It is especially in the erethitic form of the disease that it is useful, the excitement being quickly calmed, and a critical sleep with general diaphoresis ensuing. The constipation usually supervening requires an aperient. It is administered as an infusion, or an infuso-decoction (half-oz. to six oz.), a spoonful being given at first every hour, and then every second hour, continuing it for some days after sleep has been procured.—*Schm. Jahrb.*, vol. lxx. p. 172.

On Medicinal Powders. By Dr. BECKER.

TINCTURES undoubtedly form some of the most valuable of our pharmaceutical preparations, holding in solution, as they do, the most active principles of vegetable substances. Still the administration of the alcohol constituting so large a share in their composition is often objectionable; and on these grounds Dr. Becker proposes the substitution of medicated powders. He mixes equal parts of the tinctures of hellebore, euphrasia, cinchona, or whatever the substance may be, with sugar, and then evaporates, so as to drive off the alcohol, designating the residue as *helleborus saccharatus, cinchona saccharata*, &c., and so on.—*Büchner's Repert.*, B. vii. p. 365.

[Supposing the unimpaired medicinal properties of the tinctures can be thus fixed in these powders (which is problematical), this mode of administration would prove a great boon to physician and patient. Not only is alcohol obviously mischievous in many cases wherein the active principles of which it is the vehicle are indicated; but in others, in which such contraindication is not so apparent, it has often proved a means of inducing a habit of dram-drinking, which prevails, even among respectable females, to a far greater extent than is usually supposed.]

Quinine in Urticaria. By Dr. WICKHAM.

DR. WICKHAM has found in the wards of M. Legroux several cases of urticaria, complicated with severe pain in the joints, yield readily to quinine—a remedy, he observes, also useful in simple urticaria, which exhibits the same fugacious characters as rheumatism. It is from its analogy to neuralgia that M. Cazenave has recommended arsenic in urticaria.—*Rev. Med. Chir.*, viii. p. 260.

On the Administration of a Single Dose of Quinine in Intermittent Fever. By Dr. MEYER.

IN a former number of this journal (vol. v. p. 524) we quoted what seemed a somewhat extraordinary statement by Dr. Pfeufer, that he had been very successful in treating ague by the administration of a single ten-grain dose of quinine, allowing at the same time a generous diet. In the present paper, Dr. Meyer states the

results of a trial he made of this plan upon some of the military at Berlin. From five to ten hours prior to the expected paroxysm, ten grains of quinine were given with ten grains of sugar, a nutritious diet of meat and beer being afterwards ordered, and ferruginous preparations administered. The cases amounted to twenty-eight, three tertians and twenty-five quartans, the disease having continued from two weeks to eighteen months, and for the most part having been treated without success, or at all events without durable success, by quinine, arsenic, &c. A tabular view of the results is given, the cases being ranged in three categories. In the first, consisting of ten patients, the single dose, followed by good diet and martial preparations, though it suspended the attack for a period varying from eight to twenty-eight days, did not cure the disease. These patients exhibited œdematous countenances, cachectic habit, and considerable enlargement of the liver and spleen. The second category of sixteen patients (three tertians and thirteen quartans, the majority of three or four months' duration) were cured by the single dose; and the two cases of the other category were cured by a repetition of the ten-grain dose after a fortnight. The conclusion Dr. Meyer arrives at is, that not only will the majority of simple intermittents yield to the ten-grain dose, followed by good diet and *ammon. mur. ferrugin.*, but that this treatment suffices in most cases of obstinate quartan, complicated with enlarged spleen and liver, when endemic influences do not maintain the disease, or have not induced such changes in the blood as to require more time to overcome.—*Casper's Wochenschrift*, 1851, No. 27.

On Iodined Oil. By M. GUIBOURT.

M. GUIBOURT recently read a report of a committee, consisting of himself and MM. Souberain, Gibert, and Ricord, appointed by the Academy, to consider how far a definite combination of iodine with oil might be employed with advantage therapeutically. M. Marchal first proposed the employment of this substance in 1848, under the idea that iodine was the active element in cod-liver oil, and that a preparation containing a large proportion of this substance would prove of advantage. Since then, MM. Personne and Deschamps have each proposed formulæ for its preparation. M. Personne believes, that small as is the proportion of iodine in cod-liver oil, it is a very important ingredient; and without expecting in all cases to replace cod-liver oil, he believes that a larger proportion of iodine, combined with an assimilable substance, may prove a highly valuable medicinal agent—the oily vehicle enabling it to penetrate into the economy, and abandoning it there gradually as it itself is burnt off during respiration. According to his plan of procedure, five parts of iodine are dissolved in 1000 of sweet almond oil, and a jet of aqueous vapour is passed through the mixture until quite decolorized. Five other parts of iodine are now added, and the decoloration similarly performed. No traces of vapour of iodine are perceived; but the vapour of the water which is condensed exhibits strong traces of the presence of hydriodic acid. The oil is washed with a weak alkaline solution, as long as any acid reaction appears, and is then filtered. By adding successive quantities of iodine, double the quantity may be combined, but it is then difficult to avoid obtaining a high-coloured liquid.

In this way an oil may be produced, differing little in taste or appearance from almond oil, so that it can be easily administered alone or in emulsion. When forty grammes of it are swallowed, an hour and a half elapses before it shows its presence in the saliva. It becomes more and more manifest during twelve hours, and then diminishes, being still very sensible after eighteen or even twenty-four hours, but quite disappearing after thirty. M. Deschamps produces an iodined oil by the agency of alcohol; but the reporter regards it as nowise superior to that prepared more easily by M. Personne's method.

M. Gibert has experimented with both these oils. In several cases of chronic impetigo, in which it has been employed internally and externally, a rapid resolution of the eruption has resulted, more rapid than under the agency of cod-liver

oil, which is so much more difficult of administration. It has failed in several chronic scrofulous *engorgements*, which had also resisted cod-liver oil. M. Gibert's experiments with the oil have not yet been sufficiently numerous to enable him to estimate its exact value; but he is even now enabled to state that it possesses considerable resolvent power in certain of the chronic eruptions and glandular enlargements. M. Ricord has employed it during a year in a great variety of scrofulous affections, most of which have been mistaken for syphilitic disease. He has derived excellent effects from it in strumous bubo, tubercular epididymitis, and in some cases of scrofulous engorgements of the joints. In these cases, satisfactory effects have much more promptly followed the use of the iodined than of the cod-liver oil. M. Puche, M. Ricord's colleague, has come to a similar conclusion. The dose given has been usually 60 grammes per diem; but this has frequently been increased to 100. It is usually well borne, only exciting purgation exceptionally, when large doses are given.—*Bull. de l'Acad.*, vol. xvi. pp. 1141—57.

On the Suspension of Gum-Resins. By M. POULENC.

IN this paper M. Poulenc describes the mode he successfully adopts of suspending gum-resins, as assafœtida, ammoniacum, myrrh, &c. in mixtures or enemata. If the division of the body be accomplished by means of yolk of egg, it is a very tedious process; but if to every gramme of the gum-resin we add six or eight drops of sweet almond oil, it easily becomes broken up. When the oil has become well incorporated with the mass, and a homogeneous paste is produced, the vehicle is to be added, at first gradually, and then entirely. A complete emulsion is thus produced in a very short time. Another advantage of this mode of preparation is, that it allows the mixture to be warmed, if required, before employing it, without causing any coagulation. M. Poulenc employs the same means of subdividing the gum-resins which enter into the composition of emplastra.—*Bull. de Thérap.*, vol. xli. p. 118.

Mode of administering Balsam of Copaiva. By M. CHERVET.

THIRTY parts of the balsam are stirred round in a glass mortar with four of sulphuric acid. The mass quickly solidifies, and may be made into pills, which may be afterwards covered with a coating of gum and sugar. If the copaiva be adulterated with castor oil, the solidification does not take place; while, if adulterated with turpentine, although solidification does take place, the mass, when placed in water, becomes covered with a white, bitter, resinous substance. The medicinal virtues of the copaiva are by no means impaired by the above proceeding, while its disagreeable flavour is destroyed.—*Jour. de Chimie Médicale*, 1851, p. 437.

On the Tartrate of Soda as a Purgative. By M. DELIOUX.

M. DELIOUX recommends the crystallized tartrate of soda as a most agreeable and certain purgative, being quite equal in power to the sulphate of soda or magnesia, and not repugnant to the taste. The medium dose, for active purgation, is ten drachms, little or no colic attending its action. The sulphate, phosphate, and tartrate of soda, and the tartrate of soda and potassa, may indeed be substituted for each other as regards their purgative action; but the tartrate of soda surpasses them all in pleasantness of taste. M. Delioux is no believer in the doctrine which connects the purgative action of a body with its sapidity, the purgation resulting from the indigestibility of the body swallowed, and the exosmosis it gives rise to.—*Bull. de Thérap.*, vol. xli. p. 20.

The Solution of Iodine facilitated by Tannin.

AFTER numerous attempts to procure the solution of small quantities of iodine in water, without the intervention of iodide of potassium, M. Debauque discovered that the addition of an ounce of the syrup of orange-peel to a six-ounce mixture, enabled this to completely dissolve from twenty-five to thirty centigrammes of iodine. Seeking for the principle contained in the syrup which produced this effect, he concluded it could only be the tannic acid contained in the bark of the curacao; and direct experiments with small quantities of tannic acid proved this to be the case.—*Bull. de Thérap.*, vol. xli. p. 75.

FORENSIC MEDICINE AND MEDICAL STATISTICS.

Rarity of Repetition of Attempt at Suicide by Fire-arms. By M. H. LARREY.

M. H. LARREY, in a recent discussion, observed, that according to his experience suicidal maniacs may make repeated attempts at terminating their existence by poison, drowning, or other means of inducing asphyxia, and even by the sword or dagger; but that individuals who have once attempted to kill themselves by *fire-arms* scarcely ever renew their suicidal endeavour, but resort eagerly to all surgical means capable of correcting or effacing the effects of their mutilations. Among numerous others he might allude to, he referred to two young soldiers, now at the Val de Grace, who having in vain endeavoured to blow their brains out, have never since shown the slightest attempt to repeat the act. A case occurred to Dupuytren in the person of a soldier, who after having in vain attempted his life several times, at last endeavoured to blow out his brains, but only succeeded in mutilating his face. Cured, however, of the effects of this serious accident, he became also for ever cured of his suicidal mania. M. Larrey inquires, whether the cerebral commotion produced in these cases effects a salutary perturbation in the mental condition?

M. Brierre confirmed M. Larrey's statements; and observed, that it may be advanced, if not as an absolute, at least as a very general rule, that individuals who have once endeavoured to shoot themselves never repeat the attempt. Frequently, at the end of several years, they make new attempts at suicide by other means. Persons, on the other hand, who have failed in accomplishing their death by the various other means, frequently recur to those among them which they have already uselessly employed.—*L'Union Medicale*, 1851, No. 89.

On Poisoning by Datura in Bombay. By Dr. GIRAUD.

THE subject of poisoning by Datura excites great interest in Bombay, in consequence of the frequency of its occurrence, and the secrecy with which the crime is perpetrated. During 1848, there were forty-nine males and two females treated for it in the Native Hospital. Of this number, four only exhibited alarming symptoms, and no case proved fatal. The Datura, indigenous to India, is probably less fatal than the D. *Stramonium*, several examples of fatal poisoning by which are on record. The powdered seeds are employed, concealed in rice or other grain; but the quantities given are unknown, so secret has the administration (usually to facilitate robbery) been kept. In many cases, three stages of symptoms are observed; primary delirium; sopor, or even coma; and secondary delirium—a single stage, that of delirium only, occurring in mild cases. The primary delirium may be vociferous or merely garrulous, the patient usually manifesting excessive timidity. In both this and the soporific stage, he is constantly engaged in picking at real or imaginary objects, and sometimes in performing such antics as to render laughter on the part even of friends unavoidable. Several of these

movements seem to depend upon perverted vision, which destroys the power of judging of the distance of objects, and which may be due to the widely-dilated pupil, a persistent symptom. The pulse and temperature, usually normal, undergo in some cases extremes of exaltation or depression. In the majority of cases the soporose stage is absent, the delirium subsisting for from six to ten hours. On recovery, the person usually recollects nothing since the meal at which he was poisoned, so rapid are its effects. Emetics are given if the patient is seen early enough. Afterwards leeches are applied to the temples, and cold affusion to the head, while small doses of opium with tartar emetic are administered internally.—*Trans. Med. and Phys. Soc., Bombay,* No. 9, p. 128.

On the Poisonous Substance of the Agaricus Muscarius. By M. APOIGER.

As the endeavours of Le Tellier, Vauquelin, and Braconnot, to isolate the poisonous substance of the *A. muscarius,* led to no result, M. Apoiger resolved to institute a series of new ones, by treating the juice and watery extract of the fungus by a neutral acetate of lead. The result is, that while Le Tellier's researches led him to regard the poisonous principle as a base, those of M. Apoiger lead to the conclusion that it is an acid; the basis observed by Le Tellier not being found poisonous on experiment. The acid precipitated by the acetate is soluble in ether, crystallizable, and actively poisonous when given to animals. It is to be distinguished from the boletic or fungic acid, which, when saturated with potash, does not prove poisonous. The acid nature of the poison may explain the observation made by Mirabelli, that ammonia is the best antidote. Besides this acid, a very pleasant mushroom-smelling volatile oil is separable from the fungus.—*Buchner's Repert.,* B. vii. p. 365.

On Congestion of the Lungs in Poisoning by Opium. By Mr. CARTER.

MR. CARTER relates six cases of death from opium, in all of which very marked congestion of the lungs was observed; and he has added references to the only three other cases he can find in which the condition of these organs is mentioned, similar appearances having been also observed in these. In several of Orfila's experiments such congestion also existed; and Mr. Carter believes that in the narration of cases of poisoning by opium, this, one of the most ordinary *post-mortem* appearances, has been unaccountably overlooked.—*Trans. Med. and Phys. Soc., Bombay,* No. 9, p. 35.

On Mud found in the Bronchi in Drowning. By Mr. CARTER.

MR. CARTER relates the following case in consequence of its rarity; Orfila stating that in fifty dissections, sand, gravel, &c., has been found in the trachea but once, and Devergie declaring that mud is never found there except after prolonged submersion. A Hindoo lad was discovered with his face downwards in a drain, twelve hours after he had been thrown into it—the drain containing two feet of water and one of mud. On examination, sixteen hours after death, a bloody fluid, mixed with mud, flowed from the mouth and nares. The trachea contained mud in large quantities; and this extended into the minute bronchi, so as to impart a dark colour to the substance of the lung. A little was found in the œsophagus; but none in the stomach.—*Trans. Med. and Phys. Soc., Bombay,* No. 9, p. 43.

Statistics of the Lying-in Institution at Mayence. By Dr. F. KILIAN.

THESE statistics embrace 42 years (1806—48), during which period 7739 women have been delivered at the Institution. Some of the particulars of their cases are but imperfectly recorded; but those which have been noted are interesting, as

exhibiting the results of a very uniform system of practice followed by the successive directors of the establishment. This practice is founded upon the maxims inculcated by Böer, of leaving the cases as much as possible to nature, and avoiding all meddlesome interference.

The 7739 mothers produced 7833 children (91 twin births, and 1 triplet), 7369 being born alive, 464 still-born, and 147 dying in a few days—that is, 611 still-born or dying (nearly 1 in 19). The presentations were as follow:

Head	7559
Face	38
Arm	31
Breech	115
Feet	81
Knee	5
	7829

Of the 7559 children offering *cranial* presentations, 333 were born dead—several, however, having died prior to the commencement of labour, as attested by their putridity—leaving 236 (1 in 32), who died during labour, including premature births. In 92 of the 7559 cases, aid was required.—Of the 115 *breech* presentations, there were 16 twin cases; and in 20 the child was premature. Assistance was required in 7 cases.—Of the 81 *foot* presentations, 19 were twin cases, and 23 premature children. In 2, aid was required for the delivery of the head.—Of the 38 *face* presentations, in 3 only were the forceps employed. All were born alive except 2, in whom traces of putrefaction were present.—Among the 31 cases of *arm* presentation, 5 were twins, and 2 premature births. All were turned: 12 being born alive, 12 still born, and 7 having died prior to labour.

Premature Births.—These amounted to 250 (1 in 31), the proportion varying much in different years. A far larger proportion occurs in the latter half of pregnancy than in private practice, exceeding those of the first half by 11 to 1; the greatest absolute numbers occurring in the seventh and eighth, the least in the fourth month. There were 13 twin births; 114 children being born alive, and 149 dead. There were 215 cranial, 20 breech, and 23 foot presentations; 5 being unrecorded.

Prolapsus of the Funis.—Of this, 32 instances are recorded, being, Dr. Kilian believes, far fewer than really occurred. Of the 32 children, 11 were born alive, 17 still-born, and 4 putrid or non-viable. Interference was resorted to in 7 cases; in 4 of which (2 living, 2 still-born) turning, and in 3 (all living) the forceps were employed. Of the 25 cases left to nature, 7 were born living, 4 were putrid or non-viable, and 14 were still-born. In 4 of these cases the cord had remained too long pulseless to justify interference, while in 2 born *living*, it did not pulsate during labour, nor until some moments after its completion. Dr. Kilian considers that no general rule can be laid down for the treatment of these cases; but, without declaring so absolutely in favour of this view as Hoffman of Würzburgh does,* he believes that the majority of cases should be left to nature; the possible ill-consequences of our interference, to the mother as well as to the child, inculcating the necessity of restraining it within the narrowest possible limits.

Operations.—The *forceps* were employed 79 times (1 in 99), 61 of the children being born living, and 18 dead. Dr. Kilian attributes the rare use made of this instrument (compared to what is usual in continental practice) to the *little intermeddling which takes place in the early stage of labour*, whether for the purpose of regularizing abnormal activity of pain by depletion and other means, or of exciting it, when defective, by ergot or stimulating drinks.—*Turning* is recorded as having been performed in 34 cases—a number the author believes to be below the real one. In three of these the mother died; in one instance, suddenly, while the hips were passing; *post-mortem* expulsive action completing the delivery. Of

* Neue Zeitschr. für Geburts. 1847.

the 34 children, 15 were born living, and 19 dead.—*Perforation* was resorted to in 3 cases.

Maternal Mortality.—Of the 7739 women delivered, 41 (1 in 188) died; 11 of these deaths occurring in women who had undergone operations for delivery, and 30 among those whose cases were left to nature. In 22 cases there was something remarkable in the progress of the labour, or in the delivery of the placenta. In 14 cases death occurred from peritonitis, and in 17 the cause is not stated. In explanation of so small a mortality, the practice of non-interference, so strictly observed, is to be borne in mind; as is the remarkable fact, that during the 42 years of the existence of the establishment, it has never been visited by an epidemic of *puerperal fever.* Among the circumstances which may be supposed to favour the exemption from this scourge, is the fact that no clinique for medical students exists, and there is, consequently, far less mental and physical disturbance of the women during labour than in establishments where there is a clinique. Deaths, too, are known by the patients to be rare; and when they do occur they are carefully concealed from the cognizance of the other inmates; and indeed all sources of mental disquietude are sought to be avoided. Attention to cleanliness and ventilation also prevails. It is not, however, meant to be asserted that by these and other precautions puerperal fever can always be prevented; and indeed the town of Mayence seems to share the immunity of the hospital, as the oldest practitioners cannot call to mind the prevalence of an epidemic of this disease, even when large portions of the continent had been ravaged by it. The same immunity prevailed with respect to the cholera of 1832 and 1849.—***Neue Zeitschrift für Geburtskunde,*** vol. xxx. pp. 116—152.

Revaccination in the Prussian Army, during 1850.

THERE were 44,539 individuals revaccinated (33,466 of whom presented distinct marks of the former vaccination.) As the result of the revaccination—

25,030 presented regular vesicles.
7,509 presented irregular vesicles.
12,000 manifested no effect.

As a result of vaccinating this 12,000 again—

2355 furnished vesicles.
8766 remained unaffected.

Among those successfully revaccinated, in this or former years, there occurred 10 cases of varicella, 22 of varioloid, but no example of variola.

The 176 cases of the different forms of variola, observed in the entire army, during 1850, are thus distributed—

76 cases (18 varicella, 52 varioloid, 6 variola) occurred in soldiers not revaccinated.
68 cases (20 varicella, 47 varioloid, 1 variola) in soldiers in whom revaccination failed.
32 cases (10 varicella, 22 varioloid) in soldiers successfully revaccinated.

176—*Med. Zeitung,* 1851.

INTELLIGENCE.

PRESENT ASPECT OF THE EXTRA-MURAL SEPULTURE QUESTION.

THE majority of our readers are doubtless aware that, after all the real difficulties connected with the suppression of the practice of interment within the metropolis had been overcome—after the important and complicated claims of the clergy and others, possessing what were held to be vested interests, had been satisfactorily arranged—and when a legislative measure for securing to the inhabitants of London the vast boon of extra-mural sepulture had been prepared, defended, and carried successfully through Parliament, by the direct authority of the Government,—at the eleventh hour, when the time had arrived for putting into practical operation the provisions of this long-desired and salutary enactment, unexpected obstacles arose in certain departments of the public service, which have hitherto put an entire stop to all progress, and threaten to perpetuate indefinitely many of the most objectionable evils of the present system. As we hold the suppression of intra-mural sepulture and its repulsive concomitants to be one of the most urgent ameliorations required for the improvement of the public health, we are anxious, in the present crisis, to lay before our readers a concise statement of the facts of the case, in order that the true source of all the impediments to the practical application of the Metropolitan Interment Act may be properly comprehended.

In February, 1850, the General Board of Health presented a "Report on a General Scheme for Extra-Mural Sepulture," in which they thoroughly examined the three schemes that might be proposed to effect the requisite changes:—(1.) That of granting the necessary powers to the several parishes: (2.) That of encouraging the formation of commercial cemeteries, like those of Kensal Green, Norwood, &c.: (3.) That of appointing a special board or commission. The Board of Health, for the reasons assigned in their Report, advised the selection of the last of these plans; and the legislature, on the motion of the Government, adopted and embodied that recommendation in the Metropolitan Interment Act of 1850.

The General Board of Health was appointed to carry into effect this enactment; and it appears from their official minutes, which were called for and printed by the order of the House of Lords, that the Board zealously and efficiently set to work to realize what the public was so anxiously awaiting. On November 23rd, 1850, the Board made a distinct proposal to the Lords of the Treasury, whose sanction was requisite, that all the existing metropolitan cemeteries should be purchased according to the provisions of the Act. Two entire months elapsed before any reply was received from the Treasury; and it was not till after the meeting of Parliament that authority was given for the purchase of all the metropolitan cemeteries—a permission accompanied, however, by a recommendation that one or two of these cemeteries only should in the first instance be obtained. The Board of Health hereupon proceeded, and consulted with some capitalists for the advance of the money required. One Assurance Company was willing to make the advance, when a doubt arose as to the sufficiency of the security provided in the Act; and so the negotiation failed.

In consequence of this unforeseen difficulty, the Board, with the sanction of the Government, drew up such clauses for amending the Act as would have removed all further impediment as to raising money. Instead of these clauses, providing a remedy for what was merely a technical difficulty, the Chancellor of the Exchequer, without any communication with the Board, introduced a Bill of his own, authorizing the advance from the public money of a sum sufficient for the purchase of the two cemeteries then under arbitration. The Treasury, at the same time, sent peremptory orders to the Board of Health to stop instantly all negotiations for the purchase of the Abbey Wood estate at Erith; the treaty having in view the formation of a National Cemetery for the eastern part of the metropolis, being then all but completed. The Board of Health earnestly protested against the purchase of merely two out

of the eight metropolitan cemeteries, and pointed out the serious obstacles that must of necessity arise if that procedure were adopted. It is understood that the arbitrator to whom the question of the purchase of these two cemeteries was, according to the provisions of the Act, referred, has made his award; but it does not appear that the money has been paid, nor is it known whether it will be, or, if so, what use will be made of the cemeteries. In the meantime, we observe by the newspapers that the Chancellor of the Exchequer is in communication with a Company proposing to form a general cemetery on the commercial principle.

Such is, briefly, the past and present state of the Extra-Mural Interment question, after the lapse of nearly a year and a half since the Act was passed. Nothing has been done to put an end to the enormous evils connected with the burial of a thousand corpses weekly in grave-yards already crowded to excess; and, so far as is yet known, the Government is unprovided with any plans for the future.

On carefully considering the whole of the somewhat complicated discussion carried on between the Treasury and the Board of Health, it is evident to us that the obstruction has arisen entirely from the former body. The official minutes afford unmistakable indications that the Board of Health, from first to last, zealously devoted itself to give full and early efficiency to the Act of Parliament; whilst the Treasury, commencing with a needless delay of two months, instead of affording the co-operation and assistance which were indispensably required for the realization of a measure of this magnitude, interposed difficulties and checks at every stage of the procedure; and ended by putting a final stop to the provision of a National Cemetery, by which half the metropolis, and that the most densely populated, would, ere this, have been relieved, once and for ever, from a system as revolting to the feelings as it is dangerous to health. The sole cause of all the difficulty was a technical defect in the Act relative to raising the necessary funds; and the Government by two or three clauses could, as the Board of Health proposes, have removed the impediment. But instead of this obvious procedure, the Chancellor of the Exchequer, by his Act, and by his subsequent steps, has stopped the execution of a most salutary law, passed with the public approval, supported by the Bishop of London and his clergy, and hailed by all interested in Sanitary Reform as one of the most important measures hitherto adopted. It is essential, in considering this question, to bear in mind that by the Interment Act, not only was sepulture within the whole metropolis to cease, but other and scarcely less important benefits were secured; the burial of all, even of the poorest classes, would have been conducted with propriety and solemnity; houses of reception, or mortuary chapels, constructed in accordance with ecclesiastical architecture, would have been provided in the several districts of the metropolis, where, with the assent of the friends, the corpse would have been deposited awaiting interment—thus relieving the densely-populated rooms of the poor from one of the most distressing accompaniments of death in this vast metropolis, whilst the public health would have been protected by the adoption of precautionary measures supervised by competent medical officers.

It is not difficult to perceive, in the whole of this transaction, the usual apathy and indifference of the "red-tapists" who administer the government of this country, to all that concerns the public health; and so long as the principle is persisted in of weighing great sanitary ameliorations by their cost in pounds, shillings, and pence, will the poor be decimated by preventible disease. Something surely might be learnt from the enlightened example of the citizens of New York, who, with a population not exceeding a fifth of that of London, did not think three millions sterling too great a price for transporting the waters of the Croton river forty miles for the supply of their city. We can, for ourselves, only express the hope that in the ensuing session of Parliament, when this question must again be discussed, wiser counsels may prevail; and that the efforts that have been made to re-invest the burial of the dead with its due solemnity, and to rescue the living from the dangerous and revolting pollutions to which they are daily subject, may no longer be nullified by ministerial apathy or official parsimony.

BOOKS RECEIVED FOR REVIEW.

The Life of the Hon. Henry Cavendish, including Abstracts of his more important Scientific Papers, and a Critical Inquiry into the Claims of all the Alleged Discoverers of the Composition of Water. By George Wilson, M.D., F.R.S.E. (Printed for the Cavendish Society.) London, 1851. 8vo, pp. 478.

Physiological Chemistry. By Professor C. G. Lehmann. Vol. I. Translated from the Second Edition, by George E. Day, M.D., F.R.S., Professor of Medicine in the University of St. Andrews. (Printed for the Cavendish Society.) London, 1851. 8vo, pp. 455.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Second Series, Vol. xvi. London, 1851 8vo, pp. 361. With plates.

Neuralgia: its Various Forms, Pathology, and Treatment. Being the Jacksonian Prize Essay of the Royal College of Surgeons for 1850. By C. Toogood Downing. London, 1851. 8vo, pp. 375.

The Pathological Anatomy of Bronchitis, and the Diseases of the Lung connected with Bronchial Obstruction. By W. T. Gairdner, M.D., F.R.C.P.E. Edinburgh, 1851. 8vo, pp. 82.

On Nervous Affections connected with Dyspepsia. By William Bayes, M.D. London, 1851. Post 8vo, pp. 88.

On the Morbid Conditions of the Bile and Gall-bladder. By Edwards Crisp, M.D. London, 1851. 8vo, pp. 18.

Lectures on the Physical Diagnosis of the Lungs and Heart. By Herbert Davies, M.D. London, 1851. 8vo, pp. 288.

Outlines of General Pathology. By M. J. Linton, M.D. St. Louis (U. S.), 1851. 8vo, pp. 205.

On the Nature and Treatment of Softening of the Brain. By Richard Rowland, M.D. London, 1851 8vo, pp. 137.

Elementary Physics, an Introduction to the Study of Natural Philosophy. By Robert Hunt. London, 1851. pp. 486. With a Coloured Plate, and 217 Wood-engravings.

Remarks on the Plea of Insanity, and on the Management of Criminal Lunatics. By William Wood, M.D., Medical Officer of Bethlem Hospital. London, 1851. 8vo, pp. 70.

Inaugural Address delivered at the Opening of Sydenham College, Birmingham. By John Boon Hayes, Lecturer on Anatomy, Physiology, and Pathology. Birmingham, 1851. 8vo, pp. 24.

A Manual of Human Physiology for Students, &c. &c. By John Morford Cottle, L.R.C.P., &c. London, 1851. 12mo, pp. 303.

The Pocket Formulary, and Synopsis of the British and Foreign Pharmacopœias. By Henry Beasley. Fifth Edition, corrected and enlarged. London, 1851. 24mo, pp. 546.

Report of the Proceedings of the Pathological Society of London, Fifth Session, 1850, 1851. London, 1851. 8vo, pp. 196.

On Improving the Condition of the Insane. By Henry Monro, M.B. London, 1851. 8vo, pp. 23.

Guy's Hospital Reports. Second Series, vol. vii. Part II. London, 1851. 8vo, p. 200. With Four Plates.

Familiar Letters on the Physics of the Earth; treating of the Chief Movements of the Land, the Waters, and the Air; and the Forces that give rise to them. By Henry Buff, Professor of Physics in the University of Giessen. Translated by A. W. Hoffmann, Ph.D., F.R.S. London, 1851. Fcap. 8vo, pp. 273.

The Principles and Practice of Obstetric Medicine and Surgery, in reference to the Process of Parturition. By Francis H. Ramsbotham, M.D., F.R.C.P., &c. &c. Third Edition enlarged. London, 1851. 8vo, pp. 726. With 120 Engravings on Steel and Wood.

Physiological Researches. By Sir Benjamin C. Brodie, Bart., D.C.L., F.R.S. Collected and Republished from the Philosophical Transactions. London, 1851. 8vo, pp. 146.

Elemens de Morphologie Humaine. Par J. E. Cornay (de Rochefort). Paris, 1850. 12mo, pp. 244.

Minutes of the Board of Health, relating to the Metropolitan Interments Act, since August, 1850. Parliamentary Paper.

Hand-book of Physiology. By William Kirkes, M.D., assisted by James Paget, F.R.S. With Illustrations on Steel and Wood. Second Edition. London, 1851. Post 8vo, pp. 718.

Lectures on the Principles and Practice of Surgery. By Bransby B. Cooper, F.R.S., Senior Surgeon to Guy's Hospital. London, 1851. 8vo, pp. 964.

On the Progress of Public Hygiène and Sanitary Legislation in England, and the Advantages to be derived from their further extension. By J. Ingram Ikin, F.R.C.S. Worcester, 1851. Royal 8vo, pp. 23.

A Manual of Physiology, including Physiological Anatomy. By William B. Carpenter, M.D., F.R.S., F.G.S., &c. &c. Second Edition. London, 1851. Fcap. 8vo, pp. 616.

A Practical Treatise on the Microscope. By John Quekett, Assistant Conservator of the Hunterian Museum, &c. Second Edition. London, 1851. 8vo, pp. 515.

A Popular History of the Mollusca; comprising a Familiar Account of their Classification, Instincts, and Habits, and of the Growth and Distinguishing Characters of their Shells. By Mary Roberts. London, 1851. Square 8vo, pp. 326. With 18 Coloured Plates.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1852.

PART FIRST.

Analytical and Critical Reviews.

ART. I.

1. *Die Bright'sche Nierenkrankheit und deren Behandlung.* Eine Monographie von Dr. FRIEDR. THEOD. FRERICHS, ordentlich Professor der Medicin und Vorstand der medicinischen Klinik in Kiel.—*Braunschweig.* 1851. 8vo, pp. 385.

Bright's Disease of the Kidney and its Treatment. A Monograph. By Dr. FRIED. THEOD. FRIERICHS, &c. &c.

2. *On the Nature and Treatment of Diseases of the Kidney connected with Albuminous Urine* (*Morbus Brightii*). By G. OWEN REES, M.D., F.R.S., F.R.C.P., Assistant Physician to, and Lecturer on Materia Medica at, Guy's Hospital; Principal Medical Officer to the Pentonville Prison.—*London,* 1850. 8vo, pp. 134.

3. *Contributions to the Pathology of the Kidney.* By WILLIAM T. GAIRDNER, M.D.—*Edinburgh,* 1848. 8vo, pp. 54.

4. *The Pathology of the Kidney in Scarlatina.* Illustrated by Cases. By JAMES MILLER, M.D., L.R.C.P., Physician to the Western General Dispensary, &c.—*London,* 1850. 8vo, pp. 177.

5. *The Cyclopædia of Anatomy and Physiology.* Edited by R. B. TODD, M.D., F.R.S., &c. Art. *Ren.* By G. JOHNSON, M.D., F.R.C.P., Assistant Physician to King's College Hospital, &c. 1848.

DURING the last twenty-five years, diseases of the Kidney have received a large share of attention from the most distinguished pathologists of Europe; yet even now only are we beginning to arrive at a definite conclusion as to their nature. Still, when we consider the position at length attained, we see no reason to regard the labour and ability spent on these researches as having been expended in vain. The master mind of him whose name is associated with one of the most, if not *the* most important of the diseases of the kidneys, at once detected the close relation which exists

between certain lesions of those organs, albuminous urine, and anasarca; and thus laid the foundation for all our subsequent knowledge: he traced, also, as far as the means at his disposal, and the imperfect acquaintance of anatomists with the structure of the kidney permitted, the various stages of the local lesion. The microscope has afforded to modern pathologists a mode of distinguishing from each other diseased products necessarily confounded as one by the most accurate of earlier observers; while Mr. Bowman's admirable account of the anatomy of the kidney has enabled pathological anatomists to determine definitely the seat of those morbid products. In kidneys grouped together, but a short time since, as examples of the same disease anatomically considered, recent observers have demonstrated the existence of morbid substances dissimilar, both physically and chemically, from each other; and have shown that each of these new products may occupy, in different cases, different anatomical elements of these organs. The relation of these morbid products to each other, it is the duty of the pathologist to determine; that of the clinical physician, to prove the relation between the local lesions and the symptoms; to ascertain, that is, if diseases anatomically and pathologically distinct be symptomatically different. The pathological anatomist has demonstrated in certain diseased kidneys the presence of fat, and in certain other diseased kidneys the presence of protein-matter; the pathologist has shown, that, in some cases at least, the latter, by a metamorphosis of its elements, is converted into the former, so that protein and fatty matters may unquestionably have their origin in the same pathological action; while clinical researches have proved that the same symptoms ultimately ensue from retention of the elements of the urine in the blood, however different the pathological process by which the local lesion leading to that retention is induced. In the present article, we propose to give a full analysis of the work of Frerichs; and this, because it is the most recent of the works before us, and the most complete treatise on Bright's disease yet published; and also because its author, by his previous researches, has proved himself so careful an observer, and so able a reasoner, that his statements deserve the most serious consideration.

While for the most part following him, in the order he has adopted, and endeavouring to give our readers, as fully as the space at our disposal permits, a correct idea of his facts and his opinions, we shall point out the principal particulars in which he differs from, and in which he is supported by, the other observers whose contributions to the pathology of the kidney we have above enumerated.

There is no organ of whose normal structure it is more essential that a clear understanding should be gained, before a correct opinion respecting its pathological condition can be formed, than the kidney; accordingly, Drs. Frerichs, Gairdner, Rees, and Johnson, each preface their treatise with an account of the anatomical constitution of the healthy kidney. In regard to certain points, some difference exists in the descriptions of these observers, on which a discrepancy of opinion with respect to the signification of certain anatomico-pathological phenomena rests.

Frerichs' account of the structure of the kidney agrees in the main with that of Mr. Bowman. The Malpighian capsules are formed, he says, by sacciform dilatations of the extremities of the tubules, and not by lateral

enlargements. The cells coating the basement-membrane of the tubules of the cortical substance, are roundish or irregularly polyhedral in form; each cell contains a nucleus; in the interior of the cells is ordinarily a little finely granular matter; and fat-globules are for the most part found aggregated about the nucleus. The cell-wall, remarkable for its delicacy, is one of the first structures to undergo decomposition; and after its destruction, free nuclei, interspersed among amorphous granules, alone remain in the interior of the tubules. The epithelial cells are arranged more or less regularly on the interior of the basement membrane, in such a manner that a free channel is left in the centre of each tubule. Sometimes the cells are imperfectly developed, and then their place is supplied by nuclei and granular matter; the nuclei themselves are occasionally wanting. The tubules of the cortical substance lie in apposition with each other, held together by a finely-granular uniting medium. The fluid which escapes on breaking down the tissues of the kidney, contains large numbers of bodies resembling the muscular fibres of Kölliker. The exact position occupied by these fibre-cells and their arrangement, Frerichs says he has been unable to determine. Mr. Bowman's description of the arrangement and distribution of the vascular system of the organ is adopted as correct by Frerichs, Gairdner, Johnson, and Rees. Frerichs confirms the statement respecting the structure of the Malpighian bodies; the walls of the little vessels constituting the glomeruli are formed by a simple membrane studded with nuclei; there is no appearance, he says, on these vessels, of such cells as are described by Gerlach.

Frerichs' description of the structure of the kidney differs from that of Goodsir, Bowman, and Johnson, inasmuch as these observers affirm that the tubules are imbedded in a fibro-cellular matrix, which constitutes, so to say, a delicate fibrous skeleton for the organ. Mr. Goodsir regards this fibro-cellular tissue as analogous to the capsule of Glisson in the liver. "Of a uniting structure able to be divided into fibres, of a fibro-cellular matrix, in which, according to Goodsir and Johnson, the tubuli uriniferi and the vessels are imbedded, I," writes Frerichs, "have been unable to convince myself." Dr. Gairdner has figured this fibrous network, but is inclined to regard it as made up, at least in great part, of the walls of the capillary plexus of vessels. The presence of a distinct channel in the centre of the tubules, as described by Frerichs, is denied by Dr. Gairdner. "The cavity of the tubules," he says, "appears to be entirely filled with these cells, and by the secretion which distends them, and which, when freed, filters away between." Our own observations lead us to agree with those who maintain, that unless the epithelial cells be detached from the basement membrane in consequence of incipient decomposition, a free channel does invariably exist in the tubules of the cortical substance of the healthy human kidney. A point of considerable interest, concerning which, Frerichs, as we have seen, speaks positively, is with reference to the presence of a small portion of fat in the epithelial cells of the healthy kidney. Dr. Johnson anticipated Frerichs in this statement. Dr. Gairdner, however, maintains, that the least trace of fat in the cells of the kidney is an evidence of disease. The opaque granular aspect of the cells mentioned by Frerichs and Johnson, Dr. Gairdner says, is due to a deposit of lithate of ammonia in their interior.

The chief differences, then, between these observers, are with reference to the existence of the fibro-cellular matrix of Goodsir, the arrangement of the epithelium in the interior of the tubules, and the presence of fat in the cells of the healthy kidney.

After this account of the anatomy of the organ, Frerichs gives a brief general sketch of the pathological changes which must result from an increase in the hydrostatic pressure, or in the elasticity or porosity of the vessels, and then proceeds to the anatomical characters of Bright's disease. He divides the lesions of the kidneys into three stages.

1st. The stage of hyperæmia, and of commencing exudation.
2nd. The stage of exudation, and of commencing metamorphosis of the exudation matter.
3rd. The stage of degeneration (*rückbildung*), of atrophy.

First stage.—The kidney is enlarged; it is often double its normal size, weighing as much as ten or twelve ounces. Its surface is smooth; its capsule, which is more vascular than natural, is readily detached. The venous network on the surface of the organ is abnormally loaded with blood. The increase in size depends chiefly on an increase in the thickness of the cortical substance. The cortical substance is dusky red, soft, and infiltrated with a bloody fluid. The substance, as well as the surface, is frequently studded with roundish dusky-red points, which give it a dotted aspect. The pyramidal portion is simply loaded with blood. The calices and pelvis contain a turbid and generally bloody fluid; the mucous membrane lining them is hyperæmic, and thickened. The venous plexus of the cortical substance, the portal systems of Mr. Bowman, and the vascular coil of the Malpighian bodies, are distended with dark blood. The blood may escape from the vessels of the Malpighian bodies, from the venous plexus surrounding the tubules, or from the veins on the surface of the cortical substance. The small round spots seen on the outer surface of the organ, are due to blood extravasated from the vessels of the Malpighian bodies passing into the tubules, as shown by Mr. Bowman; when blood escapes from the plexus of veins between the tubules, it forms clots of some size, which are found imbedded in the tissue of the organ; when rupture of a superficial vein ensues, the blood is situated immediately beneath the proper tunic or capsule.

The epithelium in this stage is little changed. In many cases the canal of the tubules of the cortical substance is occupied by coagulated fibrin, in which epithelial cells and blood-discs are sometimes entangled; if many of the coagula have been detached, then the basement-membrane is found at intervals deprived of its epithelium. The lesions indicative of the first stage of Bright's disease, our author has only found after death from scarlet fever or from exposure to cold—i. e., as the result of very acute disease. It is rare for patients to die in this stage. Out of 292 fatal cases of Bright's disease, 100 of which were recorded by Bright, 14 by Christison, 37 by Gregory, 8 by Martin Solon, 48 by Rayer, 45 by Becquerel, 10 by Bright and Barlow, 9 by Malmsten, and 21 by Frerichs himself, in 20 only were the kidneys in the first stage of the disease.

The diagnosis of Bright's disease at this stage, from simple congestion, can only be made, in the majority of cases, when casts of tubes are found

in the interior of the tubules, or in the urine. Dr. Bright described this as the first stage of the disease known by his name, and figured it in his fifth plate.

Second stage.—In this stage the exudation-matter increases in quantity, and the hyperæmia of the kidney gradually disappears. The seat of the exudation-matter may be limited to the interior of the tubules, or it may infiltrate the whole parenchyma of the organ. At a more advanced period, the exudation-matter begins to experience metamorphosis. The epithelial scales, and the fibrinous matter in the interior of the tubules, are resolved into fatty molecules; occasionally the exudation-matter seated between the tubules and around the Malpighian capsules, is more or less completely converted into fibrous tissue (*bindegewebe*).

The vascularity of the kidney is lost by degrees, the red of the cortical substance being replaced by a yellow colour. At first a few vessels may be seen ramifying on, and a few red points studding, the yellow tissue; subsequently these disappear, and the whole cortical substance is left of an uniformly dull yellow colour. The kidney is large, its weight being from 6 to 15 ounces. It is soft and brittle; its cut surface gives exit on pressure to a turbid, milky-looking fluid. The surface of the kidney is sometimes smooth and even, sometimes beset with small granulations. These granulations are produced by the distension with exudation-matter, of loops of tubules seated at the surface. The capsule is opaque and thickened; it is readily detached from the surface of the organ. On section, the exudation-matter is seen to be seated chiefly in the cortical substance; frequently, however, it dips into the bases of the pyramids, separating the tubules from each other, and producing an appearance resembling a plume of feathers. The cortical substance is sometimes as much as an inch in thickness. The pyramids are of a dusky red colour, contrasting strongly with the pale cortex. In this stage of the affection, injections enter very imperfectly into the vessels of the substance of the kidney.

Histological examination shows that in parts the Malpighian capsules retain their normal size, but the vascular coil in their interior is smaller than natural, and often bloodless, and covered with a finely granular substance; at other places, the Malpighian bodies are considerably enlarged, and between the vascular glomerulus and the capsule is a thick layer of finely-granular fibrinous matter, mixed with numerous fat-globules, and occasionally with crystals of cholesterine; sometimes the vascular coil is observed in a shrunken state at the bottom of the capsule. Healthy Malpighian bodies are generally to be found in the vicinity of those just described.—It would seem, that so long as the fluid excreted by the vascular coil finds a ready exit, so long the Malpighian body itself retains its normal appearance; but if the stream be no longer sufficiently strong to wash out the fibrinous coagula from the tubules, then the capsules are distended, and the vascular coil is surrounded by a fibrinous coagulum, which subsequently undergoes the fatty metamorphosis. If the fluid retained within the capsule, however, should not coagulate, then the vascular coil is simply compressed into a small space, and lies, as Mr. Simon first pointed out, at the bottom of the capsule. The tubules undergo a marked change in this stage; the epithelial cells are enlarged, and filled with a finely-granular matter abounding in fatty molecules; they lose

their normal form, and their walls grow indistinct; ultimately they break up into a granular detritus. Such of the cells as escape infiltration shrivel up, and are finally resolved into a detritus like the former. The relative proportions of exudation-matter, of fat, of epithelial scales, and of the detritus of the same, will vary in different cases. Occasionally, Frerichs continues, fat-globules, more or less closely aggregated together, alone remain, and not unfrequently blood-discs are entangled in the coagula. The tubules are at places enlarged, being dilated in some cases to thrice their normal size, and at the same time they have a varicose appearance, and their walls are thickened. If a kidney thus affected be hardened by alcohol, or by pyroligneous or nitric acid, and a fine transverse section be then made, the preparation appears made up of an aggregation of roundish or oval cysts from $\frac{1}{50}$ to $\frac{1}{20}$ of a line in diameter. It was this appearance, Frerichs thinks, which led Mr. Simon to regard cyst-formations as an essential part of the structural changes in Bright's disease. Greatly enlarged kidneys are found chiefly in the acute and dyscrasic forms of the disease; in the more chronic and latent cases, and especially in those due to mechanical impediment to the venous current, the increase of size is trifling.

As the amount and character of the exudation varies in different tubules, and the degree of the metamorphosis of the latter varies, so will the hue of particular parts vary. As a rule, the more completely the exudation-matter has experienced the fatty metamorphosis, the yellower is the tint; and the greater the amount of exudation-matter accumulated in detached tubules, the more marked and prominent are the granulations.

The second stage of Bright's disease is observed in the dead subject much more frequently than the first; thus, in 139 of the 292 cases before referred to, the kidneys were in this stage. Frerichs says, that to his second stage are to be referred Bright's first and second; Rayer's second, third, and fourth; Christison's second, third, fourth, and seventh; Martin Solon's second and third; and Rokitansky's second, third, and fourth forms.

Third stage.—In the third stage, the weight of the kidney is often reduced to three, two, and even to one and a half ounces. The capsule, white, opaque, and thickened at places, adheres firmly to the cortical substance. The superficial surface of the organ is uneven, rough, and furrowed. The granulations are usually about the size of poppy-seeds, or pin-heads, occasionally they attain the size of small peas. The outer surface of the organ is generally of a dirty yellow colour, the depressed puckered portions being, as a rule, pale. In consistence it has almost the toughness of leather; the toughness being the more marked, the greater the atrophy. On section, the breadth of the cortical substance is found to be considerably less than natural, and in it are granulations similar to those seen on the outer surface, separated from each other by a pale puckered tissue. The pyramids also are diminished in size, although much less so than the cortex. The pelvis of the kidney is sometimes dilated; its mucous membrane is thickened and abnormally vascular, or of a uniform grey colour. The adipose tissue in which the kidney is imbedded, is increased in quantity.

Although vessels are seen ramifying on the outer surface, distended with blood, yet the greater part of the capillary system is impervious to injection.

In this stage, the histological examination of the kidney shows that metamorphosis of the exudation is gradually progressing, and that the tubules and Malpighian capsules are becoming atrophied. The tubuli uriniferi are distended with the substances found in their interior in the second stage, and projecting above the level of the surrounding parts from the granulations. Johnson terms these granulations "sets of gorged tubes."

The degeneration of the contents of the tubules has advanced; a uniformly granular mass, consisting of protein and fatty molecules, scarcely affected by acetic acid, fills the majority. Between the granulations, and especially in the deeper furrows, tubules may be detected in all stages of atrophy. The canals collapse, after the expulsion of their contents; their contour becomes irregular; and at the same time the basement-membrane falls into folds, and their diameter diminishes. Subsequently their outline becomes undistinguishable, and by the close approximation of their walls an amorphous fibrous mass is formed, capable of being broken-up by careful manipulation into separate fibres, among which are often seen fibre-cells in nowise distinguishable from those found in the healthy kidney.

Only a small number of the Malpighian capsules retain their natural appearance. The majority shrink, in some instances, to half their normal size, forming roundish or oval bodies, more or less completely filled with fat-globules. The remains of the glomerulus lie buried in the fatty mass, and cannot always be brought into view.

When a part of the fibrin is exuded into the interstitial tissue of the kidney, Frerichs says, it is developed into uniting tissue (*bindegewebe*), which by its contraction compresses the neighbouring parts, and thereby favours atrophy. This uniting tissue consists chiefly of elongated fibre-cells; but fully developed fibres are also to be detected, separable from the remains of the basement-membrane with considerable difficulty. The Malpighian capsule is greatly thickened by the layers of fibres around it, and not unfrequently a part of the exudation-matter inside the capsule is organized into fibre-cells. Fat-globules are occasionally imbedded among the fibres. Kidneys are found in the third stage of Bright's disease, about as frequently as in the second. Thus, in the 292 subjects before referred to, the kidneys were in the first stage in 20, in the second stage in 139, and in the third stage in 133 cases.

Frerichs' third stage includes Bright's third, Rayer's fifth and sixth, Martin Solon's fourth, and Rokitansky's fifth and seventh forms.

In addition to the essential anatomical changes in the kidney just described, other lesions of that organ are frequently found after death from Bright's disease. These are—

1. Old apoplectic clots; these are formed during the first stage of the disease; or during exacerbations in its progress.

2. Suppuration of the kidney. This is rare, but it occurs now and then. Pus was found in the kidney in 6 of the 292 cases before referred to. Frerichs considers Rayer's theory of intercurrent inflammation unnecessary for the explanation of the presence of pus in the organ.

3. Cysts. In the contents of the cysts so frequently found on the surface and in the substance of the kidneys, Frerichs has never been able to detect the salts of the urine, albumen and the salts of the blood only having

been present in the cases he has analyzed. Cysts are formed in Bright's disease, according to Frerichs, in consequence of obstruction of some of the tubules, and distension of the portion of the tubules above the point of obstruction. The impediment to the exit of the fluid may be either a fibrinous coagulum, or construction of the tubule by the contraction of newly-formed tissue, situated externally to the tubule. This explanation agrees with that offered by Johnson, and supported by Gairdner.

4. Crystalline deposits in the kidneys.

Crystals of uric acid and its salts are occasionally found within the tubules of kidneys affected with Bright's disease. Frerichs, in one case, found crystals of uric acid collected into groups the size of pin-heads. The cortical substance felt as if strewed over with particles of sand. Gluge, Johnson, and Gairdner have all described somewhat similar cases.

The presence of tubercles appears in some cases to be a cause of irritation of the kidney, and so of hyperæmia and Bright's disease. Local impediments to the return of blood from the substance of the organ, are occasionally found—e.g., old clots in the renal veins, and enlarged glands near the hilus of the kidney pressing on the same vessel.

Chemical changes of the kidneys in Bright's disease.

Healthy kidneys contain, according to Frerich's own researches, from 16·30 to 18·0 per cent. of fixed principles, of which 0·63 to 1·0 per cent. is fat. One hundred parts of dried renal substance, taken from the cortex, contained 4·4 to 5·05 parts of a buttery fat.

In the first stage of Bright's disease, as well as at the commencement of the second, the fatty element of the kidney is not increased in quantity, nay, in some cases, its per centage appears to be slightly diminished, in consequence of the loss of epithelium, and the presence of the fibrinous coagula. As the disease progresses, and the metamorphosis of the epithelium and fibrinous clots goes on, the relative proportion of the fat increases. Thus, in one of Frerichs' cases, which proved fatal in the second stage, one hundred parts of dried kidney yielded 9·4 parts of fat, and in another case he obtained as much as 13·9 per cent. of fat from a portion of dried cortical substance.

In the third stage, in consequence of a large portion of the contents of the tubules having been expelled, the quantity of fat is less than in the second stage; thus, in one case, fatal in this stage, one hundred parts of dried kidney yielded only 4·40 parts of fat. As a rule, the quantity of fat proved to be present by chemical analysis is less than would have been supposed to exist, judging by the microscopic characters; consequently, no opinion as to the amount of fat present in the kidney, should be given from a microscopic examination alone. Dr. Rees's observations agree on this point with those of Frerichs: thus he found a healthy kidney yield 1·86 per cent. of fat; an enlarged, greyish, oily-looking kidney, 2·902 per cent. of fat; and an enlarged kidney, considered by the gentleman who gave it him to be fatty, only 0·50 per cent. of fat.

It is rare to open a subject which during life suffered from Bright's disease, without finding lesions of other organs than the kidneys. These lesions may have been the cause or the consequence of the Bright's disease. Frerichs has analyzed the appearances found after death in the 292 fatal cases previously mentioned. The following are the results he obtained.

Circulatory Organs.—In these 292 cases, the heart was hypertrophied 99 times; 41 of these 99 cases were complicated with disease of the valves; in 42 there was simple hypertrophy; in 16, atheroma of the large arteries.

Lungs.—In 175 of the 292 subjects, the lungs were the seat of disease. The most common lesion was œdema; this occurred in 75 cases, and in 4 of the 75 was conjoined with *œdema glottidis;* in a majority of these cases there was disease of the mitral valve. Pneumonia was present in 27, gangrene of the lung in 2, and pulmonary apoplexy in 8 cases; these last 8 cases were complicated with valvular disease of the heart. Vesicular emphysema was found in 22 cases; and in 37 there were tubercles in the lungs.

Liver.—Of the 292 cases referred to, the liver was the seat of cirrhosis in 26, of fatty degeneration in 19, and of carcinoma in 1 case.

Spleen.—In 26 of the 292 cases there was chronic, and in 4, acute enlargement of the spleen. In these latter cases, Bright's disease commenced during the course of typhus fever.

Stomach.—This organ was diseased in 31 cases. In 24 cases, there was chronic catarrh of the mucous membrane; the majority of these 24 individuals had drank to excess. In 3 cases there were simple chronic ulcers in this organ, and in 4 *carcinoma pylori;* in 1 case there was dark "typhous" softening of the stomach.

Central Organs of the Nervous System.—In 11 of the cases there was *apoplexia cerebri;* 8 of these 11, however, were the subjects of hypertrophy and valvular disease of the heart, and in 2 there was atheromatous disease of the arteries.

In 40 cases, the quantity of fluid under the arachnoid and in the lateral ventricles was increased, but in 10 only was it considerable in degree; in 2 cases there was meningitis; in 1 a tumour, supposed to be of a carcinomatous nature.

The *Serous Membranes* were the seat of inflammation and exudation of lymph in 81 cases—viz., the pleura in 35, the peritoneum in 33, and the pericardium in 13 cases.

Bones.—In 7 of the 292 cases, white swelling and caries existed; in 2, necrosis.

In 5 cases there was sloughing of the *skin;* in 3, enlargement of the *lumbar glands,* sufficient to impede the return of blood by the renal vein; in 1, a stone in the bladder, and in 1, *cystitis exsudativa.*

It will be observed, that Frerichs gives no description of that disease of the kidney, to which Dr. Johnson once proposed to limit the term Bright's disease; and in an historical notice appended to one of the sections above analyzed, he maintains that fatty degeneration of the kidney, as a pathological lesion *sui generis,* has no existence; that whenever fat is found in the kidney, whether its seat be the interior of the tubuli uriniferi, the epithelial scales, the Malpighian bodies, or the interstitial tissue, it has but one origin, and that is the metamorphosis of pre-existing protein-matter. This is a question of great pathological interest.

At the time Dr. Johnson published his first paper on the subject, the origin of free fat, as a pathological product, was very imperfectly understood; it was, in fact, pretty generally supposed to be invariably formed directly by secretion; to be, in every case, the result of malnutrition.

But we now know that fat is constantly formed in the substance of various tissues and organs, by a new arrangement of their ultimate elements, by a metamorphosis or degeneration of the normal protein constituents of the part, and that it may also be formed by the same process from protein matter, the product of diseased action. Now, there can be no question that Dr. Johnson included in the term fatty kidney, those cases in which the fat was due to metamorphosis of fibrinous coagula,—cases which bear a closer resemblance to a lung the seat of advanced pneumonic consolidation, than to the fatty liver to which he considered them pathologically allied. Still the question remains, whether there be not a disease of the kidneys, in which the only anatomical lesion is the presence of large quantities of fat in the interior of the epithelial cells, and in which the fat is deposited as fat from the first—a disease in which the fat is produced by a pathological process analogous to, if not identical with, that which leads to an abnormal deposit of fat in the liver. To those who hold that, in their healthy condition, the epithelial cells of the kidney contain a small quantity of fat, the pathological relation of fatty liver and one form of fatty kidney would seem, *à priori*, highly probable; in both diseases there would be merely, to use Dr. Johnson's term, an "exaggeration" of one of their normal constituents. But this problem can be solved only by direct observation; and our own experience certainly leads us to incline to the opinion, that there does exist a fatty kidney, such as that which Dr. Johnson first described—a disease in which the only lesion able to be discovered after death, is an abnormal amount of fat within the epithelial cells of the kidney—a disease which has no relation to inflammation of that organ, even using the word inflammation in its widest sense. The following is Dr. Johnson's description of such a kidney;—"it is large, smooth, soft, and mottled, and scattered over with hæmorrhagic spots. On a microscopic examination no inflammatory product can be detected, and no increase in the number of the epithelial scales; there is simply a great accumulation of oil-globules in the epithelial cells." Mr. Simon agrees with Dr. Johnson in separating the disease here briefly described, from those diseases of the organ in which inflammation or its products plays a part, and from all diseases attended with desquamation of the epithelium of the kidney, or with the exudation of fibrinous or protein matter. At the same time Mr. Simon adds,—"In its progress this disease has invariably appeared to me to have the subacute inflammation grafted on it,"—i. e., we suppose, he has never met with a kidney in which there was not, in addition to fat within the epithelial cells, protein-matter of new formation, or more or less desquamation of the epithelium.

Dr. Gairdner nowhere in his treatise adverts to the possibility of the fat found in the kidney being the result of a metamorphosis of protein-matters. He admits that in some cases of Bright's disease, he has found an accumulation of fat in the interior of the cells constituting the only lesion. His Fourth observation refers to the case of a man, in whose lungs tubercles were found in large number after death. In this case the liver-cells contained an excess of fat. The surface of the kidneys was smooth, the organs themselves of normal consistence; the cortex was of uniform density, and mottled throughout. On microscopic examination, the secreting cells were seen to be well formed, and the presence of

an oily deposit in them was the only abnormal appearance. Dr. Gairdner proved that this substance was fat, by the use of ether. Dr. G. O. Rees considers that a large number of the kidneys described as fatty, are really the seat of strumous deposit. Whether by the term strumous deposit, Dr. Rees means a deposit of a specific nature, or merely fibrinous exudation-matter occurring in a strumous subject, we do not very clearly understand. Dr. Johnson's second form of fatty degeneration, in which, he says, the changes characteristic of desquamative nephritis are combined with those proper to his first form of fatty degeneration, is clearly referable to Frerichs' second and third stages of Bright's disease, the fat having its origin in the metamorphosis of protein exudation-matter; and to these two stages of the disease chiefly it is, that the majority of the cases detailed by Dr. Gairdner as examples of oleo-albuminous exudation are to be referred. Dr. Gairdner says distinctly, that in the far larger proportion of the subjects that fell under his observation in the dead-house, which, during life, had suffered from albuminous urine and dropsy, the kidneys were the seat, not of a deposit of oil only, nor of a deposit of albuminous matter only, but of the two conjoined; and that in comparatively few cases was the oily matter limited to the secreting cells. In these latter cases, it seems probable that the fat is deposited as such in the interior of the secreting cells; and having no power to pass into other parts of the organ by endosmosis, accumulates at the spots where it is first formed; being only found free in the interior of the tubules, after it has burst, by over-distension, the secreting cells.

Dr. Johnson had probably his first form—i. e., the primarily fatty kidney, in view, when he dwelt on the fact that the Malpighian corpuscles were ordinarily free from fat, when the tubuli generally were the seat of extensive and pretty uniform fatty degeneration; and Drs. Frerichs and Gairdner had probably Dr. Johnson's second form of fatty kidney before them, when they described the Malpighian bodies as frequently the seat of oleo-albuminous matter. In the latter case, the protein-matter primarily exuded, coagulating around the vascular coil of the Malpighian body, or surrounding the capsule, undergoes fatty degeneration; while in the former case, the fat, being limited to the cells, can accumulate to a very trifling extent only in the interior of the Malpighian capsule.

If future research confirm what past investigations have rendered highly probable—viz., that there is a disease of the kidney (comparatively rare, however, in which the only anatomical lesion is the presence of fat in its secreting cells, and that the fat in these cases is formed directly, and is not the result of a metamorphosis of protein exudation-matter, then the merit of having pointed out that affection will belong to Dr. Johnson; for although Canstatt, Gluge, and several other German observers, had previously demonstrated the existence of free fat within the tubules of the kidney, yet he was the first who described a case in which the fat was limited to the interior of the secreting cells; and it is this form of fatty kidney only, which can be considered as a disease differing *in toto* from congestion or nephritis. Dr. Johnson has also described certain other pathological states of the kidney, in which, he says, the epithelial cells are thrown off from the interior of the tubules in large quantities. This diseased condition he likens to desquamation of the cuticle, and to the shedding of the epithelium

in some forms of bronchitis. He terms the disease itself desquamative nephritis. The disease of the kidney which follows scarlet fever is, according to Dr. Johnson, acute desquamative nephritis; but he admits that fibrinous casts of tubes and blood-discs are passed with the urine in large numbers; and we have seen that it is to the separation of these clots from the tubules, that Frerichs ascribes the detachment of the epithelium. With reference to the existence of a disease in which the quantity of epithelium thrown off from the tubes is out of all proportion to the number of the casts of tubes in the urine, Dr. Gairdner's observations confirm those of Dr. Johnson.

From the account of the anatomical characters of Bright's disease given by Frerichs and others, it seems probable that either of the subjoined morbid products may be found in the kidneys of those who during life suffered from the symptoms of that affection, and that each of these products presents the following peculiarities with reference to its seat.

1st. Fat limited to the interior of the cells lining the tubules. Pathologically this disease is allied to the fatty liver; and as the cells lining the Malpighian capsules are exceedingly flat, but little fat can accumulate in their interior; hence, in this form of Bright's disease, the Malpighian bodies are nearly or quite unaffected. Johnson and Todd had probably this disease before them, when they affirmed that Malpighian bodies were anatomically normal in Bright's disease.

2nd. Lymph capable of fibrillating, and subsequently of contracting.

3rd. Lymph susceptible of development into fibre-cells, fusiform fibres, and ultimately true fibres.

These forms of exudation-matter have for their especial seat the intertubular tissue. Pathologically, the disease characterized by their presence is allied to cirrhosis of the liver. Johnson, Simon, and Gairdner, refer the fibrous tissue found in atrophied and other kidneys to the remains of the basement-membrane of the tubules. Henle and Frerichs, whose statements in this particular our own observations fully support, have detailed cases in which they think there could be no doubt that new fibrous tissue had been formed. These observers were fully aware of the close resemblance borne by the collapsed tubules to fibrous tissue. Dr. Gairdner has described a diseased state of the kidney, which, he says, consists primarily in obliteration of the capillary system of vessels throughout the organ. Kidneys thus affected are said by Dr. Gairdner to be pale, heavy, and voluminous, abnormally firm and elastic, their surface furrowed or botryoidal, and their capsule more firmly adherent than in health. When examined microscopically, the tubules are seen to be free from any deposit; the fibrous tissue between the tubules always appears somewhat exaggerated. Very little injection can be passed into the capillaries of the cortical substance. The increased size and weight of these kidneys seem to us to speak strongly against the opinion expressed by Dr. Gairdner with respect to their pathological condition. Obliteration of their capillaries could not account for these changes in their physical characters. These kidneys, which he names waxy, we believe to be identical with those described by Henle and by Frerichs as the seat of the development of fibrous tissue between the tubules; and the failure of injection to have been due to the compression of the vessels by the new tissue.

4th. Exudation-matter capable only of granulating, and peculiarly prone in the kidney, as elsewhere, to undergo fatty degeneration or metamorphosis. This constitutes the most common form of Bright's disease. The morbid product frequently occupies all parts of the gland—the interior of the secreting cells, the tubules, the Malpighian capsules, and the tissue between the tubules. It was probably kidneys of this description which Canstatt and Th. Siebold,* Toynbee, Hessling, &c., had under observation, when they stated that the Malpighian bodies were filled with amorphous or granular fibrinous matter in Bright's disease.

But we return to our analysis of Frerichs' work. Having concluded the description of the anatomical lesions found after death from Bright's disease, he gives in the third chapter a general sketch of the affection in its acute and chronic forms; the type of the acute form being those cases of general anasarca with albumen in the urine, which occur after scarlet fever, or exposure to cold and wet; of the chronic, those cases which commence insidiously, or in which the general febrile disturbance that accompanies the disease at the outset subsides, while the anasarca and the albuminous urine continue.

The *Fourth* chapter is devoted to the consideration of the special symptoms, in the following order:

1st. *The Symptoms of Disordered Uropoësis.*—Pain is sometimes experienced in the region of the kidneys, on pressure or movement only; at other times it arises spontaneously. The character of the pain is ordinarily dull. Almost constantly present in the acute, it is observed only at the outset of the chronic form. The left kidney is usually more sensitive to pressure than the right, because it is more readily reached by the hand.

Passing by a short section on the symptoms elicited by percussion and palpation, and one on frequent micturition, we come to an able section on the changes in the urine itself. These changes affect both its quality and quantity. In the acute disease, the urine is, as a rule, bloody, and of a dirty brown colour; on standing, it lets fall a thick sediment, consisting chiefly of distended blood-discs; in addition, however, it contains mucous corpuscles, epithelium from the bladder, ureters, and pelvis of the kidney, as well as cylindrical bodies measuring $\frac{1}{60}$ to $\frac{1}{40}$ of a line in breadth, and $\frac{1}{4}$ to $\frac{1}{2}$ of a line in length. These cylinders—the casts of tubes described by Nasse,† Henle,‡ and Simon§—are formed of amorphous fibrin. Whether they have their origin in exudation complicated with hæmorrhage, or are clots of blood simply, may be determined by their colour, and the number of blood-discs present. Crystals of the salts of uric acid, uric acid, and oxalate of lime, are sometimes found imbedded in the fibrin. At the outset a copious lateritious sediment, composed chiefly of the salts of uric acid, is often present. In some cases the fibrin does not coagulate, Frerichs says, until after it reaches the bladder, or even until after the expulsion of the urine.

The urine rarely continues bloody until the termination of the disease. The fibrinous cylinders or casts of tubes are at first studded with epithelium scales or their nuclei; if, however, the exudation go on more rapidly than the new epithelium can be produced, then they are homogeneous;

* De Morbo Brightii, 1844. † Correspond. rh. u. Westph. Aerzte, 1843, § 121.
‡ Zeitschr. f. rat. Med. Bd. I. § 60, u. 61. § Müller's Arch., 1843, § 26.

these latter casts, on account of their pale contour, easily escape observation.

The quantity of the urine in the acute form is diminished. Its re-action is acid; its specific gravity usually raised; Rees says it is rarely lower than 1017; Frerichs once found it 1032; Christison, 1046; and Heller, 1047. Albumen is present in considerable quantity during the whole course of the disease. At the outset, it is usually smaller in quantity than at a later period. According to Frerichs' own observations, the quantity of albumen varies from 8·2 to 12·5; 17·5 to 24·8 per 1000; Becquerel found 11·9; Simon, 15·0, 18·0, and 33·61; Christison, 27·0; Heller, 15·0; Schmidt, 7·86—21·87; Gorup, 20·17 per 1000. The quantity of albumen passed in the 24 hours varies from 77·5 to 387·5 grains; usually it amounts to from 124 to 232·5 grains.

The quantity of the fibrinous cylinders cannot be readily appreciated, in consequence of the presence of epithelium, &c. In one case in which the cylinders constituted the chief part of the sediment, Frerichs found the latter amount to 6·0 per 1000. The other constituents of the urine, in the acute form of Bright's disease, are considerably diminished in quantity; the actual diminution will depend on the number of the urinary tubules blocked up by the fibrinous coagula. Frerichs found the urea vary in four observations from 7·9 to 14·2 per 1000. In 24 hours the quantity of urea passed ranged between 115 and 192 grains. The uric acid, although it is frequently diminished in quantity, is by no means constantly so. The inorganic constituents of the urine are, also, almost always less abundant than in health, especially the common salt; of the latter, Heller could frequently only find a trace. As soon as the febrile symptoms cease, and the diet is changed, this abnormity in a great part disappears, even although the local affection continues to progress.

In the chronic form of Bright's disease, the urine is ordinarily of a pale yellow colour, and slightly turbid. The peculiar aromatic smell of healthy urine is replaced by an insipid odour, resembling that of weak broth; Dr. Walshe has compared this odour to that of whey. On standing, the urine lets fall some of the matters on which its turbidity depended; the deposit consists of pavement-epithelium, the secreting cells of the urinary tubules, and fibrinous cylinders. The latter are sometimes studded with epithelial scales, and sometimes contain fat-globules. The transparency of the casts varies with the time they have remained in the tubules. If an acute attack be engrafted on the chronic disease, then blood-discs and clots entangling the same again appear in the urine. Should pus be formed in the kidney, which is rare, pus-corpuscles will be found enclosed in the fibrinous coagula.

Frerichs considers that the amount of the fibrinous coagula, and of the albumen, afford an approximative criterion of the intensity of the exudative process. The fibrinous coagula in the urine are, he says, true indicators of the process going on in the kidney. Lehmann describes* very transparent, hollow, cylindrical bodies found in the urine in Bright's disease, which he considers to be the *membrana propria* of the urinary tubules. Frerichs, with justice, says these are merely hollow fibrinous coagula; they are rendered more transparent by acetic acid, while the basement-membrane is unaffected by that re-agent. Frerichs never saw a case, such as Rayer

* Physiologischen Chemie, B. II. § 391.

describes, in which the turbidity of the urine was removed by ether. The re-action of the urine is usually faintly acid, sometimes neutral, rarely alkaline. In 1 only of 41 cases observed by our author, was the urine alkaline. The specific gravity of the urine in the advanced stages of the chronic form varies from 1004 to 1012. If, however, a local inflammation be set up, then the specific gravity is raised. The quantity of the urine is generally large, and in the later stages of the disease often exceeds greatly the amount drank; at times, however, it is considerably diminished, in consequence of an increase in the dropsical effusion, or in company with the phenomena of uræmic intoxication. The largest quantity that Frerichs has found on record, as passed in 24 hours, is 192 ounces; the case was one of Pfeufers,* and the patient was taking diuretics: the smallest quantity is less than an ounce (Christison). The urine is always poor in fixed constituents, particularly in urea and uric acid; the salts, too, are considerably below the standard of health. The quantity of the albumen varies from 2·5 to 15·0 per 1000, and from 46 to 310 grains in 24 hours, ordinarily ranging between 93 and 186 grains in 24 hours. Dr. Bence Jones gives 45 grains and 5 drachms as the extremes; and 1½ to 3 drachms as the average quantity passed in 24 hours.† Although the albumen is usually most abundant, as shown by Christison, at the outset of the disease, yet the reverse is true in some cases. It is, according to Frerichs, not the duration of the disease which determines the quantity of albumen passed with the urine, but the activity of the local process. Careful watching of the amount of albumen passed in the urine is, then, he adds, indispensable for the prognosis and the treatment. Cases sometimes occur in which the albumen disappears, at the same time that the detachment of the cylindrical coagula (casts of tubes) continues; the latter are then of the greatest weight in forming a diagnosis.

The quantity of urea is always below the standard of health, not only as to the per-centage, but also as to the amount excreted in 24 hours. Frerichs found in 37 analyses, that the quantity of urea in 1000 parts of urine varied from 1·57 to 16·72 parts, the usual range being between 3 and 7 parts. From 15 to 263 grains of urea were excreted in 24 hours, the amount generally varying between 46·5 and 124 grains. Ordinarily the quantity diminishes as the disease advances. Yet to this, also, there are exceptions. The diminution in the uric acid is less considerable, and less constant than in the urea. The quantity varied in Frerichs' analyses from 0·2 to 1·4 per 1000; in the majority of cases between 0·2 and 0·6 per 1000; the amount of uric acid excreted in 24 hours ranged from 4·2 to 19·7 grains, the average being 7 grains.

The extractive matters and the salts also experience, in the course of chronic Bright's disease, more or less diminution in quantity. This, however, is by no means constant. The alkaline and earthy salts appear about equally diminished. Frerichs found the quantity of these salts to vary from 1·3 to 19·35 per 1000; the quantity passed in twenty-four hours being between 62 and 399 grains.—Thus, these salts were usually under the physiological mean, but were also many times above it. It must be borne in mind that the various complications which occur in the course of Bright's disease, modify considerably the constitution of the urine.

* Zeitsch. f. rat. Med. Band I. § 58. † Medical Times, Jan. 3rd, 1852.

Alterations in the Composition of the Blood.—In the first stage of the acute form, the blood drawn has usually a buffy coat; a fact long known. The serum is frequently more or less milky. Turbidity of the serum generally may be due—1st, to colourless corpuscles being suspended in the serum; 2ndly, to the presence of finely divided fat; and, 3rdly, to the diffusion through the serum of an albuminous substance in the form of minute molecules, Zimmerman's molecular fibrin. The last-mentioned cause of turbidity was first pointed out by Simon and Scherer. By which of these causes is the turbidity of the serum in Bright's disease produced? In one of three cases examined by Frerichs, fat was the cause of the turbidity; the milkiness was removed by ether; in the two remaining cases, the turbidity was due to the presence of protein-molecules; ether had no effect on it; carbonate of potash, nitre, &c., rendered the serum completely clear. It follows, from these and similar observations previously made by Rayer, Christison, Simon, and Scherer, that the turbidity of the serum in Bright's disease is sometimes due to suspended fat, and sometimes to the presence of albuminous molecules. The ultimate cause, Frerichs observes, is the same apparently in both cases—viz., diminished alkalescence of the blood, by which, in the one case, the albuminate of soda is decomposed, and finely granular albumen separated; in the other case, saponified fat is set free. The faintly alkaline reaction of the turbid serum, Frerichs considers, supports this explanation.

The specific gravity of the serum, and the amount of its solid constituents, are, at the outset of the disease, nearly normal; but both quickly fall, as albumen passes away with the urine. If, as frequently happens, copious dropsical effusions occur at the same time, the thinning of the serum follows still more rapidly. It is the albumen especially, which is diminished in quantity.

In the case of a woman, æt. 21, suffering from anasarca of eight days' duration—the anasarca being the consequence of scarlet fever—Frerichs found the specific gravity of the serum 1·019, and from 1000 parts of the same serum he obtained only 51·7 parts of albumen. The fat, salts, and extractive matters remain nearly normal. The fat appears even, occasionally, somewhat increased. A greater amount of urea is found in the serum at an early period of the disease, than in healthy blood. Christison detected an excess of urea on the ninth day of the disease; and the occurrence of uræmic symptoms before that date proves that the urea may accumulate in the blood in a yet shorter time. There is no diminution in the number of the red corpuscles at the outset of the disease, or no greater than there is in all febrile affections as a consequence of altered diet.

The fibrin in this stage is often greatly increased; yet its quantity is sometimes normal.

In the farther progress of the disease, the clot becomes smaller; the buffy coat disappears, and only recurs if intercurrent inflammation of parenchymatous organs or serous membranes is set up. The serum is more abundant, and at times appears milky. The density of the serum and the amount of its solid constituents are, as a rule, diminished. In some cases, when, in the course of the disease, the amount of albumen passed by the kidneys diminishes, and the dropsical effusions disappear, the serum again becomes heavier, and its albumen attains to nearly, or

even completely its normal amount. Frerichs says, that the results obtained by analyses of the blood, are in opposition to Christison's opinion, that the serum during the middle period of the disease resumes its normal constitution. The fibrin, after the first stage, is normal in quantity; any subsequent increase in this constituent, Frerichs considers due to an exacerbation of the local disease, or to a fresh pathological action being set up.

The blood-corpuscles diminish as the disease progresses, frequently sinking to a third of their normal mean. This diminution in the blood-discs advances gradually, as Christison pointed out, with the advancing disease. There is no relation between the number of the blood-discs and the quantity of the solid constituents of the serum.

With reference to the quantity of inorganic salts, extractive matter, and fat in the blood, in chronic Bright's disease, comparatively few observations have been made. Scherer found the extractive matters increased; a mean of six analyses gave about 2·4 parts for a thousand parts of blood. Frerichs obtained in one case 1·86, and in a second, 1·42 parts of fat, from 1000 parts of blood. Becquerel and Rodier found the fat increased, in one case being 2·67 per 1000, while in another it was in nearly normal proportion, 1·56 per 1000. Scherer observed no increase in the fat.

The quantity of the salts of the blood continues normal, or experiences a slight increase. In Scherer's analyses they varied from 8·8 to 10·28 per 1000; the mean gave an increase of 1·2 per 1000. Schmidt's analyses likewise showed a slight increase in the salts; he found in Bright's disease 10·93 and 10·44; the normal amount, according to the same observer, being from 9·99 to 10·45. Rees obtained only 7·5, 7·0, and 6·5, alkaline salts from 1000 parts of blood.

The amount of the excrementitious constituents of the urine retained in the blood, is regulated by the state of the urinary secretion. Heller obtained from one case 1·74, and from another, 1·85 p. m. of urea; Bright and Babington, from 1000 parts of serum, which contained only 50 parts of albumen, 15 parts of urea. Rees found as a maximum 0·5, as a minimum 0·209 parts of urea in 1000 parts of serum. Christison states, that during the middle period of the disease, the urea disappears, to recur in large quantities in the last stage. Frerichs considers that the quantity of urea in the blood depends, not on the stage of the disease, but on the more or less free secretion of the urine. Garrod has recently pointed out* the presence of uric acid in the blood in Bright's disease; in one case he found 0·037 parts in 1000 of serum, and in two other cases, 0·012 and 0·027 respectively.

The blood in the chronic form of Bright's disease is, then, generally characterized—

1st. By a normal, under some circumstances increased, quantity of fibrin.
2nd. By a diminution of the albumen in the serum, varying in amount according to the state of the urinary excretion.
3rd. By a progressively advancing diminution of the red discs.
4th. By a slight increase in the soluble salts.
5th. By more or less accumulation of the excrementitious constituents of the urine.

* Medico-Chirurgical Transactions, vol. xxxi.

In the acute form, shortly after the commencement of the disease—

1st. By an increase in the fibrin.
2nd. By a diminution in the quantity of the albumen.
3rd. By the presence of urea.

Analyses of the blood have thrown no light on the genesis of the local disease. The deviation in the composition of the blood is the consequence of the abnormal urinary excretion. We can readily understand why the albumen of the serum is diminished in quantity, and why the excrementitious elements of the urine are retained in the blood. It is more difficult to point out the cause of the progressive diminution of the blood-corpuscles; Rees' well-known views on this subject Frerichs considers purely hypothetical. Although incapable of explaining the pathogenesis of Bright's disease, the results of the analyses of the blood offer a reasonable explanation of many of its symptoms.

Habitus of the Patient.—The profound alteration in the composition of the blood in Bright's disease gives a peculiar aspect to the patient, from which the experienced physician may, with some degree of certainty, arrive at the diagnosis. The skin has a greyish-white hue, sometimes bordering on yellow; the pallid face is puffed, particularly under the eyes; there is an expression of indifference and apathy; and the mobility of the carriage is gone. These peculiarities are not present unless the composition of the blood deviates considerably from that of health, and effusion of serosity into the cellular tissue has taken place.

Dropsy.—As Bright himself pointed out, the disease may be, during its whole course, unattended by effusion of the least trace of serosity into the cellular tissue, or the serous cavities. Frerichs remarks that it is impossible to state in what proportion of cases serous effusion is absent, because its occurrence is so often overlooked, those cases which present it being frequently set down as typhus, apoplexy, &c.; and even in the subject, the lesions of the kidney are often of a kind readily passed by unheeded. In all statistical tables, therefore, this class of cases has too low a figure affixed. Of 430 cases of Bright's disease collected by Frerichs from various sources, 54, or nearly one-eighth, never had dropsy. The seat of the dropsy often changes; at one time the serous infiltration is seated in the head and upper extremities; then, disappearing from those parts, it may occupy the abdomen, thorax, &c., or the reverse. Sudden increase of the effusion is sometimes observed, particularly after exposure to cold, or diminution of the urinary secretion. A current of air falling on a part, even though for a short time only, suffices to render it, after a few hours, œdematous.

Erysipelatous redness, excoriation, and, ultimately, moist gangrene, frequently occur on parts of the skin greatly stretched by subcutaneous effusion, or exposed to pressure. In the further progress of the case, effusion of serosity takes place into the serous cavities, the parenchyma of the lung, or the meshes of the submucous tissue of the velum pendulum palati, pharynx, lig. ary-epiglottica, and vocal cords (œdema glottidis). Anasarca is the most frequent form of dropsy in Bright's disease. It is never absent when lesion of the kidney is the primary affection. Ascites, the next most common form, rarely attains to a great amount, unless the

disease of the kidney be accompanied by cirrhosis of the liver, or valvular disease of the heart. Next to ascites, hydrothorax is the most common variety of dropsy in Bright's disease; the effusion usually occupies both sides of the thorax, though there is generally, Frerichs thinks, a larger amount of fluid in the left than in the right pleura. A frequent accompaniment of hydrothorax is œdema of the lung. This lesion is chiefly found in cases of Bright's disease complicated with insufficiency or stenosis of the mitral valve. Considerable effusion of serosity into the pericardium was present in 3 of 69 cases of Bright's disease examined by Malmsten, and in 11 of 100 cases observed by Bright himself. Œdema glottidis is yet more rare. It is extremely uncommon to find effusion into the cranial cavity, or œdema of the brain-substance, sufficient to explain the suppression of the cerebral functions.

As to the chemical composition of the fluid effused in Bright's disease, it differs in some essential points from that found in other cases of dropsy. The poverty of the serum of the blood in albumen, its relative richness in salts, as well as the retention of urea in it, must be exhibited also in the dropsical fluid; in fact, these abnormities are more marked in that, than in the blood itself: our knowledge of the laws of diffusion would lead us to expect this. The specific gravity of the effused fluid varies between 1005 and 1012; the amount of its solid constituents is small; the salts are nearly as abundant as in the blood itself; the albuminous constituent of the effused fluids in Bright's disease is, as a rule, smaller than in other cases of dropsy.

The capillaries of some parts, under the same circumstances, constantly pour out a fluid richer in albumen than those of other parts. This law was first closely fixed by Schmidt. Frerichs gives the following table in support of it (p. 81):

1000 *parts of fluid contained—*

	PLEURA.				PERITONEUM.		
Albumen	28·50	26·12	25·64	24·9	23·8	11·32	3·92
Salts .	7·55	7·64	8·30	7·82	10·8	9·77	9·03
	Schmidt	Schmidt	Frerichs	Frerichs	Marchand	Schmidt	Schmidt

	CRANIAL CAVITY.		SUBCUTANEOUS CELLULAR TISSUE.		
Albumen . . .	10·03	7·98	7·0	5·42	3·60
Salts	9·69	8·46	9·1	15·62	7·70
	Schmidt	Schmidt	Simon	Heller	Schmidt

As Lehmann showed, this rule only holds true so long as the conditions of effusion are the same. If there co-exist with the disease of the kidneys,

an impediment to the circulation or inflammatory stasis, then the albumen increases in quantity. Thus, the abdomen being punctured in ascites from Bright's disease, Lehmann found the specific gravity of the fluid removed was 1005, two days after it rose to 1011·5; the examination of the body after death demonstrated the existence of peritonitis. One peculiarity of the dropsical fluid in Bright's disease arises from the admixture of urea. Urea was first detected in the fluid removed from the peritoneum by Nysten, in 1810; it is now rarely sought for without being discovered; at the same time, Frerichs remarks that he has frequently found it replaced, in dropsical fluids removed from the dead body, by carbonate of ammonia. Frerichs' own observations lead him to the conclusion, that the quantity of urea is always greater in the dropsical fluid than in the blood.

The largest quantity of urea which has been found in the fluid effused in Bright's disease is 4·2 per 1000 (Marchand); Frerichs himself obtained 1·62, 1·05, 1·45; Rees only 0·415 and 0·15. Frerichs has repeatedly sought for uric acid in the effused fluids, but hitherto without success.

The following are the conclusions with reference to the genesis of the dropsy in Bright's disease, arrived at by our author:

1st. Acute dropsy—e. g., post scarlatinam, &c.—has the same cause as the kidney disease; it is the consequence of paralysis of the capillaries of the skin and subcutaneous tissue excited by exposure to cold.

2nd. In the course of Bright's disease, the thinning of the blood causes great predisposition to dropsy. Trifling circumstances are then sufficient to determine the occurrence of serous effusion.

3rd. Organic disease of the heart, liver, &c., by offering an impediment to the onward current of the blood, is a frequent cause of the dropsy, and, at the same time, of the kidney-disease. Hydræmia favours the occurrence of dropsy, but the latter may ensue independently of the former.

Changes in the Function of the Skin.—When the disease commences suddenly, the skin is hot and dry; when gradually, the skin from the outset is pale and cool. General perspiration can rarely be excited by diaphoretics; partial sweats are common. The perspiration contains urea, or the products of its decomposition. This fact explains the cause of the urinous odour of the perspiration in suppression of urine. Sebastiani states that the perspiration is either neutral, or it contains much carbonate of ammonia. In four of forty cases observed by Frerichs, the patients complained of a troublesome itching of the skin, increased at night; Siebert made a similar observation.

Uræmia, or Uræmic Intoxication.—This is one of the most original and important sections in Frerichs' work. A train of symptoms frequently arises in Bright's disease, due to the contamination of the blood with the excrementitious constituents of the urine. There are two forms of uræmia, acute and chronic. Early in Bright's disease, patients often complain of headache or of a confused sensation in the head; their eyes grow dull and expressionless; they are forgetful and indifferent; and slow and inactive in their movements. If the urinary secretion becomes more abundant, these symptoms diminish, or they may disappear entirely. In other cases they increase in intensity, the drowsiness passes into stupor: at first, the patient may be aroused by loud calling and speaking, and then he gives

rational answers; subsequently, the coma is complete, and the respiration becomes stertorous. Delirium is an infrequent symptom; when it does occur, the patient will repeat over and over again the same word or sentence. Convulsions frequently precede death.

The acute form of uræmia commences suddenly, and manifests itself in the three following ways: by depression of the function of the brain, by irritation of the spinal cord, and by both sets of symptoms conjoined.—In the first form, the patients sink suddenly into a state of deep stupor, out of which they are very soon unable to be aroused. The face is mostly pale, and the pupils immovable; in other cases there is circumscribed redness of the cheeks, the conjunctivæ are then injected, and the pupils small. The pulse ranges between 60 and 90; on the occurrence of coma, it usually increases in size and hardness. The respiration is sometimes stertorous, the character of the stertor differing, as Dr. Addison pointed out,* from that in cerebral hæmorrhage.—In the second form, convulsions occur suddenly, similar in character to those seen in eclampsia and epilepsy. The whole muscular system is usually affected. Consciousness is undestroyed.—In the third form, coma and convulsions are conjoined.

These acute forms of uræmia are usually the result of sudden suppression of urine, particularly in Bright's disease, from scarlet and typhus fevers.

Acute uræmia may be readily confounded with cerebral hæmorrhage, hysterical convulsions, reflex spasms of various kinds, narcotic poisoning, typhus fever, &c.

Closely allied to the foregoing disorders of the nervous system, as consequences of uræmia, are certain affections of the senses. The most striking of these is loss of vision; *Amaurosis uræmica.*—Like coma and convulsions, this local nervous affection may be slowly developed, or it may manifest itself in a few days, or even hours. The patient complains of a sensation, as though a mist lay before his eyes, which from time to time becomes denser. The only change perceptible to the physician is some sluggishness of the pupil. Landouzy affirms† that amaurosis is one of the most constant symptoms of Bright's disease. Bright and Barlow noted its occurrence in 4 out of 37 cases.

The sense of hearing is affected in Bright's disease about as frequently as that of vision. There was singing in the ears and difficulty of hearing in 6 of Bright's and Barlow's 37 cases, and in 4 of 41 cases observed by Frerichs; thus the sense of hearing was affected in 10 out of 78 cases.

During the convulsions, the pulse, on account of the disturbance of the respiratory movements, is accelerated, and at the same time it is often irregular; in the intervals between the attacks of convulsions, it resumes its normal rate and regularity.

A febrile disturbance (*febris urinosa*) closely resembling typhus fever in its general characters, is sometimes produced by uræmia.

The cessation of the symptoms of uræmia is usually accompanied by a profuse secretion of urine.

Vomiting is one of the most constant and early symptoms of uræmia. The vomited fluid is generally alkaline, and contains carbonate of ammonia; when acid, the presence of ammonia in the egesta is proved

* Guy's Hospital Reports, 1839. † Archiv. Génér., Nov. 1849.

by the addition of liquor potassæ. Frerichs says, "I have very frequently sought for undestroyed urea in the vomited matters, but always in vain." Further, in uræmia produced by extirpation of the kidneys and injection of urea into the blood, Frerichs always found a large quantity of carbonate of ammonia in the vomited matters, but not a trace of urea. Stannius and Cl. Bernard also failed under similar circumstances to detect urea. Lehmann and C. Schmidt, however, obtained urea in the matters vomited in cholera; yet when the phenomena of uræmia, the so-called cholera-typhus, had commenced, Lehmann found carbonate and other compounds of ammonia. The resolution of urea into carbonate of ammonia in uræmic vomiting is not effected, Frerichs maintains, as Bernard and Barreswil supposed, in the stomach, but takes place in the blood, and within the vascular system.

Uræmic vomiting, by affording an exit from the blood for the products of the destruction of the urea, appears in some cases to prevent the development of severe nervous symptoms. A case of this kind was long since described by Christison. In the experiments of A. Bernard and Barreswil,* after extirpation of the kidneys, large quantities of an ammoniacal fluid were poured out by the mucous membrane of the stomach and intestinal canal. The animal continued awake, so long as this secretion continued; directly it ceased, the symptoms of uræmic intoxication set in.

It is doubtful in what relation the diarrhœa, which occurs in Bright's disease, stands to the uræmia.

The older physicians frequently asserted, that in cases of suppression of urine, the breath and perspiration had a fetid urinous odour. Many modern observers have denied this. Frerichs says, that whatever difference of opinion there may be as to the existence of this odour, it is a fact, that when the symptoms of uræmic intoxication, coma, convulsions, &c., commence, carbonate of ammonia is mixed in considerable quantity with the expired breath, and that the quantity of the ammonia is in proportion to the intensity of the uræmic phenomena.

"I have," he writes, "repeatedly demonstrated the ammonia contained in the expired air of sick men, and of animals into whose veins urea was injected after extirpation of the kidneys; reddened litmus paper quickly turned blue in the air issuing from the mouth and nostrils; a rod moistened with hydrochloric acid produced, when held in the same air, a more or less thick cloud. Animals, into the veins of which urea was injected, continued quiet and awake so long as the expired air was free from ammonia, but as soon as a rod dipped in hydrochloric acid produced a white cloud when held in the expired air, the disorders of the nervous system characteristic of uræmic poisoning manifested themselves."

Frerichs' own observations have not enabled him to say anything definite as to the state of the sweat in uræmic intoxication.

After death from uræmia, no lesion of structure of the central organs of the nervous system can be detected. The membranes of the brain and spinal cord are normal; the quantity of fluid in the ventricles rarely exceeds an ounce—i. e., is within the range of health. In four cases of amaurosis uræmica mentioned by Landouzy,† in one recorded by Bright, and in one observed by Frerichs, the optic nerves and the visual apparatus appeared normal. The stomach, also, even when during life it has been the

* Archiv. Génér. de Méd., April, 1847.

† Compt. Rendus de l'Acad. des Sc., xxxix.

seat of severe symptoms, is usually found after death unchanged in texture. So the intestinal mucous membrane may be normal in appearance, when during life there has been profuse diarrhœa. The kidneys exhibit the lesions characteristic of one of the three stages of Bright's disease. The blood is sometimes firmly, at others imperfectly coagulated; Frerichs thinks that in all the cases of uræmia he has seen occurring spontaneously or produced artificially, it has exhibited a peculiar shade of violet. Christison, Jaksch, and Hamernjk, have observed cases in which the blood had an ammoniacal odour similar to that of decaying urine. Chemical analysis proves, Frerichs affirms, that the blood, in every case in which the symptoms of uræmia are present, contains carbonate of ammonia, and also, usually, traces of undestroyed urea. The quantity of carbonate of ammonia varies greatly.

The above are, according to Frerichs' researches, the most important facts known concerning uræmia.

The cause of the symptoms of uræmia has been generally sought in the retention of the constituents of the urine in the blood. Osborne considered arachnitis to be the cause of the coma and convulsions; Prevost, Dumas, and Henle explained the occurrence of these symptoms by a supposed collection of watery fluid in the ventricles. Examinations after death refute these two hypotheses. Dr. Rees rejects the urea theory, because the symptoms and the quantity of urea in the blood bear no relation to each other—i. e., there may be much urea in the blood and no symptoms, and severe symptoms and little or no urea in the blood; and supposes that a certain thinness of the blood is the essential determining cause of the phenomena. But, Frerichs says, there is no striking hydræmia in the acute form of uræmia. The experiments of numerous observers have proved that extirpation of the kidneys is followed by death in from three to five days. Vauquelin and Segalas injected urea into the veins of animals, and the only result was an increase in the secretion of urine; they injected urine itself, and death ensued. Frerichs, as Bichat, Courten, and Gaspard had previously done, repeatedly injected from 20 to 40 grammes of filtered human urine, sometimes even with the addition of urea, without any ill effects resulting.

It is clear, from these experiments, that neither urea, uric acid, nor urine itself with its extractive matters and salts, occasions the symptoms observed after suppression of urine.

In support of the statement that an accumulation of urea in the blood does not necessarily induce the symptoms of uræmia, Frerichs refers to cases reported by Drs. Bright, Christison, Rees, in which that substance was obtained in considerable quantity from the circulating fluid, the patient being at the time free from all nervous symptoms: and adds,

"My clinical and experimental experience are in complete unison with the observations of the physicians above referred to. Blood was drawn from a man, suffering from advanced granular degeneration of the kidneys, in consequence of an attack of pericarditis; it contained more urea than I ever saw in that fluid. The patient died of the pericarditis without having manifested any symptoms of uræmia."

Dr. Bence Jones says, that urea is "probably not much more poisonous than nitrate of potash." He thinks it possible, that oxalic acid may accu-

mulate in the blood, and thus may cause the symptoms of the so-called uræmic poisoning.*

The following is Frerichs' own theory of uræmia. The symptoms of uræmic intoxication, he says, arise in consequence of the urea accumulated in the blood being converted by the agency of a suitable ferment, into carbonate of ammonia, while yet within the vessels. For the supervention, then, he adds, of uræmic intoxication, two agents are necessary—1st, an accumulation of urea in the blood; 2ndly, the presence of a ferment by the agency of which the decomposition of the urea may be effected.

If the urea, after collecting in quantity in the blood, be suddenly decomposed, then the symptoms are those of apoplexy; if its decomposition is effected more gradually, then the symptoms resemble those of typhus terminating in coma or convulsions.

With the causes which occasion the development of the ferment, we are, Frerichs says, but imperfectly acquainted. In the acute blood-disease—e. g., typhus, scarlet fever, and cholera—this agent is rarely absent. Slight febrile disturbance, as from exposure to cold, or trifling local inflammation, seems in some cases to give the impulse necessary for the destruction of the urea. In cases of Bright's disease which arise during pregnancy, the ferment is usually developed. Diet appears to exercise some influence on the decomposition of the urea. On this perhaps, Frerichs adds, depends the fact that the uræmic symptoms are more common from Bright's disease in England and Scotland, than in France and Germany.

The presence of the ferment is manifested only by its effects. Frerichs offers no other proof of its existence.

Frerichs likens individuals whose blood is impregnated with urea, to animals into the veins of which amygdalin has been injected; from the presence of this agent alone they suffered little inconvenience, but a single sweet almond taken into the stomach suffices to deluge the blood with prussic acid, and to cause death in an instant. Such is the novel theory of uræmic intoxication propounded by our author. In order to demonstrate its truth, it must, he says, be proved—

1st. That in every case of uræmic intoxication, a resolution of urea into carbonate of ammonia takes place.

2nd. That the symptoms characteristic of uræmia can be produced by the introduction of carbonate of ammonia into the blood.

Two series of experiments are described by Frerichs, as offering the required proof.

"In the first series of experiments, a solution of from thirty to forty-six grains of urea was injected into the veins of animals, the kidneys of which had been previously removed. They remained for some hours perfectly free from convulsions. . . . . In from 1¼ to 8 hours they became restless, vomited acid chyme, or a slimy yellow alkaline mass, according to the state of fulness of the stomach at the commencement of the experiment. At the same time that ammonia was perceptible in the expired air, convulsions supervened, which occasionally ceased and returned again, and gradually passed into stupor with stertorous breathing. In some cases, convulsions were absent, and then sopor and coma were the first symptoms. After death, which took place from 2½ to 10 hours from the time of the injection of the urea, ammonia in large quantity was found in the blood; the contents of the stomach emitted, in most cases, a strongly ammoniacal (urinous) odour, and con-

* Lectures on Animal Chemistry, Medical Times, Jan. 3rd, 1852.

tained much carbonate of ammonia; in one case only was it somewhat acid, and even then it contained ammonia. This basis was detected in the bile and other secretions. The stomach was usually injected, and of a dusky-red colour. The brain and its membranes were normal in appearance; and the quantity of fluid in the ventricles was not increased.

"In the second series of experiments, a solution of carbonate of ammonia was injected into the veins of animals. Convulsions, often very violent in character, instantly ensued, and stupor quickly supervened. The respiration was difficult, the expired breath was loaded with ammonia, and vomiting of bilious matters occurred. The stupor lasted for some hours, and ammonia was expired during the whole time. Gradually, however, the latter disappeared, and then, by degrees, the animals recovered their senses. When more carbonate of ammonia was injected, while the animal lay in a state of stupor, the convulsions and vomiting recurred, and the urine and the stools passed away involuntarily; after the lapse of five or six hours the ammonia again disappeared from the blood, and the animal again became lively."

Although death by uræmia is the natural termination, so to say, of Bright's disease, yet the fatal result is sometimes caused by other lesions—e. g., by inflammation of serous or parenchymatous structures, by sinking from vomiting, diarrhœa, dropsy, tubercular suppuration, asphyxia, &c.

The diagnosis of uræmia from *apoplexia cerebri*, typhus, gastritis, convulsions of various kinds, and narcotic poisoning, is to be made by a careful examination of the quantity and quality of the urinary secretion, the presence of ammonia in the expired air, and the symptoms of disease derived from the organ the functions of which are disordered: thus uræmic coma is distinguished from that dependent upon hæmorrhage into the brain, by the absence of paralysis of the voluntary muscles, the more frequent and softer pulse, and the more rapid breathing. The character of the stertor, too, differs in the two. The early occurrence of delirium and coma, and the absence of the eruption and of enlargement of the spleen, aid in diagnosing uræmia from typhus fever. At the same time it must be borne in mind, that an eruption closely resembling the mulberry rash of typhus is sometimes present in the uræmia which follows cholera.

Disturbance of the Functions of the Primæ Viæ, Dyspepsia, Vomiting, and Diarrhœa.—When vomiting occurs during the first stage of Bright's disease, Frerichs says it is due to irritation of the nerves of the kidney, being produced in the same manner that it is when calculus is contained in that organ. In the second stage, disorders of the stomach have their origin chiefly in congestion of the mucous membrane, the hyperæmia being, in a large proportion of cases, due either to the abuse of alcoholic liquors, or to a mechanical impediment to the circulation. Towards the termination of the disease, vomiting occurs as a consequence of uræmic intoxication; and it is in this class of cases only, that ammoniacal compounds are found in the egesta. This last form of vomiting is especially difficult to control by the use of remedial agents. Sometimes it continues so long, and so constantly, that the patient sinks. Obstinate vomiting was observed by Frerichs in 11 out of 42 cases of Bright's disease; it occurred in one-sixth of Rayer's cases; in 8 of 20 cases recorded by Bright and Barlow; and in 8 of Malmsten's 68 cases. Diarrhœa, on the whole, seems to be less frequent than vomiting; 5 cases only have been observed by Frerichs in which it was a prominent symptom. The diarrhœa itself is always

injurious; it rapidly endangers life from collapse; and is restrained by drugs with difficulty. The dropsy usually increases, in spite of the large quantity of watery fluid poured out by the intestine. After death in these cases, the intestinal mucous membrane sometimes appears healthy; in other cases it is thickened, red, and ulcerated.

Pseudo-Rheumatic Pains.—In the course of Bright's disease, and especially in its latter stages when the changes in the blood are the most marked, pains referred to the muscles and fasciæ, rather than to the joints, are frequently complained of by the patient. These pains belong to the same class as those observed in persons the subject of cancerous cachexia. Frerichs mentions the case of an individual who died from hydronephrosis, in whom their severity was so great that the physician mistook the case for rheumatism. These neuralgic pains are most severe when anasarca is absent or trifling.

The Complications of Bright's disease are considered in the *fifth* chapter. They are divided by our author into three classes. The first class embraces those processes which precede or contribute to the production of the kidney affection. To this class are referred all those conditions by which mechanical congestion of the kidney is produced—e. g., diseases of the heart and great vessels, and pregnancy; and also those diseases which lead by their long continuance to great changes in the composition of the blood —e. g., scrofulous suppuration, caries, and necrosis of bones, perhaps inveterate syphilis, and mercurial cachexia, &c. The second class includes those diseases which have the same exciting cause as the kidney affection. To this class belong diseases of the stomach and air-passages, arising from intemperance, or heart-disease, hæmorrhage into the substance of the brain or lungs from valvular disease of the heart, &c. The third class comprehends those complications which arise from the morbid condition of the blood which is the consequence of the kidney-disease; hither are to be referred inflammations of serous membranes, pneumonia, and the phenomena of uræmic intoxication. As the same lesion when complicating Bright's disease, is not always due to the same cause, it will have, in different cases, very different pathological significations. Frerichs devotes seventeen pages to the consideration of these various complications. As the most important of these are given in other parts of the book, we shall not here refer further to them.

In the *sixth* and *seventh* chapters, the Frequency, Duration, and Course of the disease are very briefly described.

The Terminations of the disease are treated of in the eighth chapter.

About two-thirds of the acute, and not more than one-ninth of the chronic cases, Frerichs reckons, terminate favourably. As to the local lesion, complete recovery is only possible when the coagula can be ejected from the tubules, and the basement-membrane retains the power of regenerating the epithelium detached with the fibrinous clots. Subsequently, fatty degeneration, or atrophy, more or less general, according to the extent of the organ occupied by the fibrinous coagula, must be the result, even though the urine resume its normal characters, and the patient be restored to apparent health. These cases frequently relapse. After atrophy of a large part of the gland and obliteration of its vessels, the remaining portion is constantly, Frerichs remarks, in a state of hyperæmia, in con-

sequence of the supply of blood by the renal artery being the same after as before the diminution in size.

Frerichs has analyzed 241 cases collected from the writings of Bright, Barlow, Gregory, Christison, Martin Solon, Rayer, Malmsten, and from his own notes, for the purpose of ascertaining the frequency with which various lesions produce death. The following are the results obtained :

Death ensued in 86 cases from uræmic intoxication.

„	20	„	pneumonia.
„	11	„	peritonitis.
„	9	„	pericarditis.
„	7	„	pleuritis.
„	20	„	sinking, the result of vomiting and diarrhœa.
„	10	„	„ profuse dropsical effusion.
„	27	„	„ tubercular disease of the lungs.
„	6	„	„ caries of the joints.
„	5	„	„ gangrenous erysipelas of the skin.
„	26	„	effusion of serosity into the lungs and pleuræ.
„	2	„	œdema glottidis.
„	6	„	cerebral hæmorrhage.
„	1	„	meningitis.
„	1	„	cerebral softening.
„	1	„	carcinoma ventriculi.
„	2	„	phlebitis, the result of venesection.

In the *ninth* chapter, Frerichs considers the Etiology and Pathogenesis of Bright's disease. The predisposing causes are: age, sex, constitution, position in life and occupation, and climate.

The exciting causes are referred by Frerichs to three heads—

1st. Those which act on the nerves of the vessels of the kidney directly, as cantharides, turpentine, and other diuretics; or indirectly, by reflex action, as cold.

2nd. Those which act by inducing a morbid condition of the blood, which latter leads to exudation into the kidney.

3rd. Those which act mechanically by impeding the return of blood from the kidney.

They are—1. The abuse of powerful diuretics. 2. Mechanical injury to the region of the kidneys. 3. Exposure to cold and damp. 4. The abuse of alcoholic liquors. 5. Long-continued suppurations. 6. Blood-diseases. 7. Mechanical impediments to the passage of the blood from the veins of the kidney.

This chapter contains but little novelty. Frerichs criticizes the oft-quoted experiments of Johnson and Simon on the production of fat in the kidneys of cats. Gluge had demonstrated* long before the experiments referred to were made, that fat exists in considerable quantity in the kidneys of healthy cats. Frerichs extracted from 27·70 to 32·50 per cent. of fat from kidneys removed from healthy dogs. It is probable, therefore, that Gluge was in error when he stated that in the kidney of the dog there is only a trifling quantity of fat. Gluge thought he had induced fatty kidneys in dogs by feeding them on oil and water, and also by the administration of mercury. With reference to the influence of climate on the production of Bright's disease, Dr. Morehead, of Bombay,

* Atlas der Patholog. Anat.

has recently shown,* that this affection is exceedingly common among certain classes of the varied tribes and nations of the continent of Asia. The cases recorded by Dr. Morehead refer to individuals of all castes; among them were some "who must have arrived in Bombay already affected with the disease from Cabool, Khorassen, Scinde, Cutch, Kattyawah, Bengal, and the coast of Africa."

An inquiry into the Nature of the disease occupies the *tenth* chapter. The passage of albumen and fibrin into the *tubuli uriniferi* is, Frerichs maintains, the starting-point of all the other phenomena which accompany Bright's disease; not only the visible local changes of the parenchyma of the kidney, but also almost all the alterations in the composition of the blood, and the disorders of the various functions arising from the latter, being strictly referable to it.

As to the anatomical changes of the kidney, they are clearly explained by the passage of the plasma of the blood into the urinary tubules. The albumen passes away with the urine; the greater part of the fibrin coagulates in the tubules of the cortical substance, and remains in them till washed away by the fluid poured out from the vessels of the Malpighian bodies. The expulsion of these clots is attended by detachment of the epithelium coating the tubules; while the latter, stripped of their secreting cells, ultimately collapse. The Malpighian capsules experience similar changes; the fluid issuing from the glomeruli, when the tubules are closed by coagula, collects and distends the capsules. The fibrinous part of this fluid coagulates, forming a coating to the vessels which are subsequently obliterated; ultimately the fibrin breaks up into granular protein molecules and fat-globules.

The effusion of the blood-plasma is produced in Bright's disease, as in every other case, by over-distension of the vessels. Thus, hyperæmia of the kidney is the first stage; this congestion is quickly accompanied by exudation of fibrin into the urinary tubules. The larger the number of tubules filled with fibrin, the greater the impediment to the influx of the blood, and the larger and paler the organ; some of the distended tubules project above the level of the surface of the cortical substance in the form of fine granulations. These conditions are included in the second stage. The fibrinous exudation matter and the epithelium subsequently experience fatty metamorphosis. Thus, Frerichs says, are formed the fatty kidneys which have been figured as a peculiar form of disease, *steatosis renum*. In other cases, the epithelium-scales imbedded in the fibrin shrink and break up, the tubules being filled with their fragments. Thus arises, says our author, one form of Bright's disease which has been named *Nephritis desquamativa* by Todd and Johnson.

In time, the tubules, from which the coagula have been expelled, collapse, and those which are still distended project as granulations on the superficial and cut surfaces of the cortical substance. The greater the number of coagula detached, and the greater the number of the collapsed tubules, the smaller the kidney. The gland is atrophied. This is the third stage. The change in the composition of the blood, in which we have the key to the various functional disturbances, is thus explained. The continued loss of albumen and fibrin—from 77 to 387 grains daily—

* Transactions of the Medical and Physical Society of Bombay, No. X.

necessarily leads to hydræmia; on this depends the changes in the *habitus* of the patient, the tendency to dropsy, the disturbance of the secretion of the mucous membranes, the inflammatory exudative processes, &c. The occlusion and collapse of the tubules occasions the collection of the excrementitious constituents of the urine in the blood, in which we have an explanation of the coma, convulsions, &c.

The exciting causes, according to Frerichs, of the effusion of albumen and fibrin into the tubules are—1st. Impediments to the onward flow of blood in the veins. 2ndly. Paralytic dilatation of the capillaries, which may be occasioned either by specific irritants, as turpentine, or by alterations in the composition of the blood—e. g., in scarlet fever, cholera, or by reflex action from the skin.

After stating his own views, Frerichs gives a critical survey of the current theories of Bright's disease.

Bright's disease is said to consist essentially in fatty degeneration of the kidneys. Canstatt, Gluge, Johnson, and Todd, support this view. The occurrence of albumen in the urine is explained by supposing that the distended secreting cells press on the capillaries between the tubules, and so cause congestion of the vessels of the Malpighian bodies. This theory, Frerichs considers untenable, because, among other reasons, fat exists in the kidneys of some animals without albuminuria being the consequence.

Dr. Johnson has lately* advanced another theory to account for the presence of albumen in the urine in these and other cases, and also for general dropsy, and hypertrophy of the heart in Bright's disease. He supposes that blood, charged with urea, passes with difficulty through the capillaries generally, and so congestion, effusion of albumen and fibrin, and even rupture and escape of blood ensue. When the cells of the kidney are filled with fat, or are thrown off from the basement-membrane in large quantities, urea must necessarily accumulate in the blood. Hypertrophy of the walls of the small arteries and of the heart is the result of the impediment to the passage of the blood through the capillaries. Dr. Bright long since suggested the possibility of hypertrophy of the heart, which he found frequently conjoined with renal disease, arising in consequence of the altered quality of the blood, "so affecting the minute and capillary circulation, as to render greater action necessary to force the blood through distant subdivisions of the vascular system."† At present this can be regarded as nothing more than an hypothesis.

Bright's disease following acute exanthemata, it has been maintained, depends on desquamation of the epithelium of the kidney. Drs. Johnson and Todd are the supporters of this theory. That exudation of fibrin is the starting-point in these as well as other cases of Bright's disease, Frerichs thinks, is proved by the urine containing from the outset of the disease fibrinous casts of tubes, in which the epithelium is imbedded.

Frerichs considers that Mr. Simon, when he affirmed Bright's disease to be subacute nephritis with cyst-formations, mistook dilated tubules cut across for newly-formed cysts.

Drs. Gairdner and Johnson had anticipated Frerichs' explanation of the foundation of Mr. Simon's theory.

* Medico-Chirurgical Transactions, vol. xxxiii.

† Guy's Hospital Reports, vol. i. p. 397.

A lesion of the kidney analogous to cirrhosis of the liver, Frerichs considers rare, although he admits its occasional occurrence.

Frerichs states that Bright's disease cannot be held to be blennorrhœa of the kidney, as some have asserted, because the urinary secretion is diminished at the commencement of the disease.

We may add to these theories two others—namely, that supported by Dr. Graves, that Bright's disease is primarily a functional disease of the kidneys. Dr. Bright himself leant to this opinion. "I consider the disease in its commencement," he writes,* "entirely functional." And that of Finger, of Prague,† and Dr. Walshe‡—namely, that the disease is primarily a blood-disease, of which the lesions of the kidney are the mere manifestation.

"The true explanation seems to be," says Dr. Walshe, "that the state of the blood prevents the kidney from acting properly on the elements it is accustomed to excrete, not that its own functional aptitude is at the outset seriously impaired; in other words, that in the commencement the renal cells are still quite able to separate urea, if healthily constituted blood were offered to them by the vessels."

Finger expresses his views thus:

"As in other blood-diseases, so also in Bright's, the blood is first diseased; and like other dyscrasiæ, so also that in question may terminate fatally before it has occasioned the diseased product—i. e., the degeneration of the kidneys."

That is to say, that a patient may die from the morbid state of the blood which constitutes Bright's disease before the kidney itself has become the seat of any lesion.—Dr. Walshe appears not to have been aware of Finger's opinion.

With reference to diagnosis: the only unequivocal sign of the existence of Bright's disease, Frerichs maintains, is the presence of the so-called "casts of tubes" in the urine. As to the value of the presence of albumen in the urine as a diagnostic sign of Bright's disease, any disease which occasions mechanical hyperæmia of the kidney may give rise to simple albuminuria. After rich food and great excitement, some healthy individuals pass urine containing albumen. Becquerel, Simon, Schmidt, Gregory, Christison, and others, detail cases of this kind. Frerichs says, "I myself know two vigorous young men who exhibit the same phenomenon." Between these cases of simple albuminuria, and Bright's disease, there is a difference in degree only; the former pass into the latter when fibrin escapes with the albumen. The case of the Edinburgh graduate, who always passed albumen with his urine when he took certain articles of diet, and who subsequently died of Bright's disease, is well known.

Mercury exhibited freely has been supposed to be a cause of albumen in the urine. Mr. D. Francis, under the direction of Dr. Rees, tested the urine of fifteen patients, all of whom were under the influence of mercury; no trace of albumen was detected in any of the cases. These results agree with those obtained by Martin Solon and Rayer.

Dr. Rees, among other writers, states that urine containing semen is albuminous. Frerichs remarks, with reference to this point, that pure

* Guy's Hospital Reports, vol. v. p. 101.

† Einige Worte über die Albuminurie und die Brightische Krankheit. (Prag. Vierteljahrssch. III.)

‡ Lancet, July, 1849.

semen contains no trace of albumen, and therefore cannot give rise to albumen in the urine. The flocculent deposit let fall by the urine occasionally in dyspepsia consists, according to Frerichs, of salts of lime, and of albumen. Dr. Rees had previously arrived at the same conclusion.

The urine is the most certain guide in giving a prognosis. A sudden increase in the quantity of albumen passed shows an increase in the local disease. Diminution of the albumen is favourable, if at the same time the specific gravity of the urine rises; but if the latter falls, and the urine at the same time is diminished in quantity, the degeneration has extended. The prognosis is also very unfavourable if the albumen disappears altogether, and the urea excreted in twenty-four hours falls much below a fourth of its normal quantity.

The *thirteenth* chapter contains a description of certain forms of Bright's disease. These varieties are named by Frerichs according to their exciting cause. Passing by the short descriptions of Morbus Brightii simplex (rheumaticus, traumaticus, and toxicus), M. B. cacheticorum, and M. B. potatorum, we come to an exceedingly full section on Bright's disease from morbid conditions of the blood—e. g., cholera, typhus, and the acute exanthemata. F. Simon, in 1832, showed that the urine during the stage of re-action of cholera often contained albumen; more recently, fibrinous casts of tubes have been found in the same urine. A very good description of the symptoms of cholera-typhus precede a section on its nature.

Frerichs regards the symptoms of *Cholera-Typhus*, with Oppolzer and Hamernjk, as the result of uræmic intoxication. His reasons for adopting this opinion are founded,—1st. On the phenomena of the disease; 2nd. On its anatomical character; 3rd. On chemical analysis.

The symptoms are identical with those of uræmia. Urea has been repeatedly found in the blood. According to Frerichs' own observations, carbonate of ammonia may always be detected in that fluid, while the breath and perspiration are frequently loaded with the same volatile alkali. After death, although the kidneys may appear to the unaided eye unchanged in structure, yet, if more carefully examined, the greater part of the *tubuli uriniferi* will be found filled with fibrinous coagula. During re-action, hæmorrhage into the kidney frequently takes place; it is indicated during life by intense pain in the region of the kidneys.

Bright's Disease from Scarlet Fever.—Albuminous urine is usually excreted, Frerichs says, between the 18th and 21st days. The symptoms of the disease and the complications are the same as those previously described.

Bright's disease of the kidneys sometimes occurs without dropsy, during the first stage of scarlet fever. The symptoms are those of uræmia—vomiting, convulsions, and coma, terminating in death. These cases are, Frerichs says, often incorrectly interpreted. Sometimes the symptoms are referred to the intensity of the fever, at others, to the imperfect development or to the retrocession of the rash, to congestion or to acute inflammation of the brain, or to effusion of serosity within the cranial cavity. Frerichs asserts, that albumen may be found in the urine, urea and carbonate of ammonia in the blood, and fibrinous exudation in the tubuli uriniferi.

Anasarca is occasionally seen during the process of desquamation after scarlet fever, when no albumen can be detected in the urine. The serous effusion in these cases is ascribed by Frerichs to paralysis of the nerves

supplying the vessels of the skin and subcutaneous tissue—an explanation purely hypothetical.

The kidneys after death from scarlatinoid albuminuria exhibit the lesions characteristic of the first and second stages (Frerichs) of Bright's disease. In the fluid which exudes from the cut surface on pressure, numerous fibrinous coagula can be detected. These casts of tubes were found by Frerichs in the fluid expressed from the kidneys of a man who died from bronchitis on the seventh day of scarlet fever; and in that obtained from the same organs removed from a man who died of uræmia on the ninth day of scarlet fever. Frerichs rejects *in toto* the opinions entertained by Drs. Johnson, Todd, and others, with reference to the occurrence of a disease subsequently to scarlet fever, to which the term desquamative nephritis can be applied.

Dr. Miller thus describes the microscopic characters of the kidneys which he examined after death from scarlatinoid albuminuria:

"The interior of the tubules is for the most part very frequently (*sic*) encumbered with disarranged gland-cells, in aggregated masses, giving an obscure and confused appearance to the contents of the tubule; frequently distending it, and rendering the cell-nuclei imperceptible. A yellowish tinge of their contents and blood-discs is sometimes conspicuous. The tubules are found, too, in some specimens, of very unequal calibre, shrunk, and completely altered in appearance. They are divested of their epithelial lining; and were it not for the nuclei persistent in the wall of the tubule, it would be difficult to recognise them."

This latter appearance, Dr. Miller says, he has observed in kidneys that had performed their functions healthily. "The last appearance worthy of notice is the frequent occurrence of oleo-albuminous particles, in abnormal proportion, within the kidney-cells."

The origin of the kidney-disease, like that of the throat-affection, is seated primarily, Frerichs thinks, in that morbid state of the blood which constitutes the basis of the exanthematic process.

A few cases of anasarca with albuminous urine, occurring after measles and small-pox, contained in the writings of Gregory, Christison, Huss, Rilliet and Barthez, and Rayer, are referred to by Frerichs. No case of the kind appears to have fallen under his own observation.

With reference to the occurrence of Bright's disease in the course of, or subsequently to, continued fever, little is known. In 38 cases of typhoid fever, examined by Becquerel, the urine was albuminous in 8. The albumen generally appeared when the disease was at its acme; less frequently at the time of incipient convalescence. It disappeared with the fever. The quantity of albumen in his cases was usually small. Oppolzer and Finger* frequently found albuminous urine in exanthematic typhus—i. e., typhus with mulberry rash. In one of their cases the quantity of albumen passed amounted to a third, and in another to half, the volume of the urine. In a case of this description, observed by Frerichs himself, every drop of urine contained numerous fibrinous coagula. Christison mentions a case of typhus with roseolous rash, which proved fatal on the tenth day. The patient died in convulsions. Urea was detected in the blood. The kidneys were greatly congested. Frerichs saw a similar case, but the uræmia did not manifest itself till the thirteenth day of the fever; the patient died on the fifteenth day.

* Prag. Vierteljahrsschr. III.

Various diseases of the heart are frequently found to coexist with Bright's disease. The relation of the former to the latter has been repeatedly questioned. Frerichs agrees with Christison that the disease of the kidney is secondary to that of the heart. Cirrhosis renum, or that form of Bright's disease in which the principal lesion is the development of fibrous tissue between the tubules, is especially liable to occur when congestion of the kidneys results from disease of the central organ of the circulation. Frerichs doubts if simple hypertrophy of the heart can lead to Bright's disease. We have already alluded to Drs. Bright and Johnson's views on this subject.

Rayer first pointed out the connexion between pregnancy and albuminous urine; Lever between the albuminuria of pregnant women and convulsions—*eclampsia parturientium*. Lever suspected from the symptoms, that urea must have accumulated in the blood. Subsequent observers confirmed the accuracy of Lever's opinions. The albumen usually disappears from the urine by the tenth or twelfth day after delivery. In a case of Dr. Lever's, no trace was to be found so early as thirty-six hours after parturition. The kidney-disease, however, may progress, and then all the stages of Bright's disease are passed through. Frerichs has found fibrinous casts in the albuminous urine of pregnant women. *Eclampsia parturientium* is, then, merely a variety of uræmia. Of 20 cases observed by Devilliers and Regnauld, in which albuminous urine was passed by pregnant women, eclampsia followed in 9. The older writers were aware of the tendency of pregnant women, the subjects of anasarcous swelling of the upper extremities and face, to suffer from convulsions. When abortion has followed the retention of the constituents of the urine in the blood of the mother, Frerichs has never seen the child anasarcous. Pregnant women suffering from Bright's disease are especially liable to local inflammation, peritonitis, pleurisy, &c. These affections are commonly established after delivery.

Eleven of Devillier's and Regnauld's 20 cases, 2 of Lever's 12 cases, and 3 of 7 observed by Frerichs, terminated fatally. After death, Frerichs has invariably found fibrinous clots in the *tubuli uriniferi*. Two causes conspire, he remarks, to produce Bright's disease in pregnant women—viz., altered composition of the blood, and mechanical disturbance of the circulation in the abdomen.

In the Treatment of Bright's disease, a consideration of which occupies the *fourteenth* chapter, Frerichs says, we must keep in view; 1st. The cause which produced and keeps up the disease; 2ndly. The local action in the kidney itself; 3rdly. The secondary symptoms.

Over certain of the known causes of Bright's disease we can exert some control—e. g., exposure to cold and moisture, and errors in diet. Again, the evil effects of profuse suppuration &c. on the general health, and so on the kidney, may be obviated; and the hyperæmia of the kidneys induced by disease of the heart and liver, may be diminished by aloetic and other purgatives. Over the morbid condition of the blood, which first occasions the exudation into the kidneys in the exanthemata, we can exert no influence.

In the first stage of the disease, the object in the treatment of the local process is to lessen the hyperæmia of the kidneys. This may be effected by general or local bloodletting, or by derivatives.

General bloodletting is only admissible in very acute cases, when there

is great pain in the region of the kidneys, scanty or bloody urine, and a full and hard pulse. In other cases, cupping-glasses or revulsives are, if possible, to be substituted.

The tendency to hydræmia is always to be kept in mind. Frerichs' experience leads him to refrain from the employment of counter-irritants, issues, and setons to the lumbar region; he has never seen any favourable influence exerted by them on the local disease, while troublesome ulcers sometimes follow their use. The secretions of the skin and mucous membrane are to be increased. In the acute form, Frerichs recommends antimony as a diaphoretic; in chronic cases, camphor, guaiacum, and analogous drugs, and at the same time vapour-baths, plain or medicated. Saline aperients are to be avoided, on account of their action on the kidneys. In the acute form, single doses of calomel may be given, combined or not with jalap, rhubarb, &c. Subsequently castor-oil, senna, colocynth, and other drastic purgatives, are to be selected.

If the flow of urine is not sufficiently abundant to detach and wash out the fibrinous coagula, then diuretics are to be administered. The seltzer and Vichy waters may also be taken with benefit. In the advanced stage of the disease, Frerichs recommends strongly tannic acid, for the purpose of restoring the tone of the capillary vessels of the kidney, and diminishing the amount of albumen poured out with the urine.

As it is to the alterations in the composition of the blood that the secondary diseases are due, so it is from the state of that fluid that the indications for the treatment of the consecutive symptoms are to be drawn.

The indications for the relief of anæmia and hydræmia, on which, directly or indirectly, so many of the secondary symptoms depend, are, to favour the formation of blood-discs, and to free the plasma from its excess of water. The former is to be effected by good diet, vegetable bitters, and the employment of iron; the lactate is the preparation of iron recommended by Frerichs; the latter indication is to be fulfilled by increasing the secretion of the skin and intestinal mucous membrane.

The acute anasarca, which commences at the same time as the kidney-disease, if the patient be strong and the febrile symptoms urgent, is to be treated by general bloodletting; under other circumstances, it is to be combated by diaphoretics and aperients. Hydragogue purgatives are sometimes useful. The value of elaterium, Frerichs considers, has been abundantly proved by Bright and Barlow, Christison, and Malmsten. Purgatives should never be administered when spontaneous diarrhœa is present. The mildest diuretics only are to be employed. Christison found digitalis and cream of tartar of service; Bright used uva ursi and pyrola umbellata; Rayer, inf. rad. armorac. The contra-indications for the employment of diuretics are, great pain in the region of the kidneys, bloody urine, and far advanced degeneration. If these means fail in relieving the anasarca, then acupuncture or simple incisions* are to be made; if ascites be present, then paracentesis abdominis may have to be performed. The dangers to be feared from these measures, however, are considerable—namely, gangrene of the skin, and peritonitis. One of the cases reported by Frerichs proved fatal from peritonitis, the consequence of the introduction of a trocar into the abdominal cavity.

* See Mr. Hilton, Guy's Hospital Reports, 1851.

The *secondary inflammations* are difficult to treat. When they occur early in the disease, and are detected at once, general bloodletting may be cautiously employed; usually, however, exudation to a considerable amount has taken place before the disease is discovered. In pneumonia large doses of tartar-emetic and acetate of lead, with opium or digitalis, are the remedies from which the most benefit is to be anticipated. In pleuritis, pericarditis, and peritonitis, Frerichs recommends, if the anasarca does not prevent, local bloodletting and derivatives, and internally calomel and small doses of tartar-emetic.

The highly philosophical researches of Dr. J. Taylor render it in the highest degree probable that mercury exerts no beneficial influence over this class of inflammation.*

In the treatment of *uræmic intoxication*, the first object to be attained is the restoration of the urinary secretion. Mild diuretics are the best remedies for the accomplishment of the desired end. Should these fail, then hydragogues are to be employed. Little hope can be entertained of diuretics acting, in the advanced stage of degeneration of the kidney. The second object is to prevent the injurious influence of the carbonate of ammonia developed in the blood on the nervous centres. When convulsions have commenced, this indication requires our first attention. Hydrochloric and the vegetable acids, Frerichs says, are the remedies which naturally suggest themselves; they pass into the blood, and are excreted again, either in their primitive or an altered form, with the urine. At the same time the patient may be washed with vinegar, and enemata containing acetic acid administered. If marked symptoms of cerebral congestion are present, purgatives and bloodletting may be required.

Vomiting, consequent on irritation of the kidneys, is to be relieved only by treating the local affection; that from chronic dyspepsia, consequent on the abuse of spirituous liquors, or disease of the heart, by bitters, narcotics, and antacids. Uræmic vomiting is most obstinate. Christison recommends creosote. Narcotics, Frerichs says, are of no service.

Diarrhœa, when it occurs during the latter stages of the disease, is very obstinate. Frerichs has generally found relief follow the use of liq. ferri muriatis.

In *bronchial catarrh*, expectoration is to be favoured if deficient by senega, ammoniacum, &c.; if in excess, it is to be restrained by tannic acid, acetate of lead, muriate of iron, and other astringents. Alum is sometimes extremely useful.

When the disease is the consequence of pregnancy, it is a question in some cases whether premature labour should not be induced.

Frerichs' work concludes with an Appendix containing records of seventeen cases of Bright's disease; some details with reference to the conditions of the urine in four cases of the same affection; an account of a series of experiments similar to those performed by Mr. Robinson† and with the same results, only that a microscopic examination showed in many cases the presence in the urine of casts of tubes; and the particulars of the sets of experiments performed for the purpose of proving the truth of his theory of uræmia. From the analysis we have given of its contents, our readers will have formed an idea of its general excellence and originality. Much

* Medical Times, vol. xxi. p. 33. † Medico-Chirurgical Transactions, vol. xxvi.

that it contains requires confirmation; much may require modification; but whatever changes our views on the subject may undergo, we believe that this treatise must always be regarded a most important contribution to our knowledge of the diseases of the kidney. The merits of Dr. Gairdner's treatise cannot fail to be highly appreciated; while Dr. Johnson's original views and careful observations have already earned for him an honourable place among British pathologists.

Art. II.

On the Transmission, from Parent to Offspring, of some Forms of Disease, and of Morbid Taints and Tendencies. By James Whitehead, M.D., F.R.C.S., on the Medical Staff of the Lying-in Hospital, and Lecturer on Obstetric Medicine, at Manchester, &c. &c. — *London*, 1851. 8vo, pp. 351.

It is now four years since, that we reviewed, at some length, a work by Mr. Whitehead, on Abortion and Sterility; and the favourable opinion we then entertained and expressed of his discernment and sound judgment, make us welcome his present contribution on a subject eminently requiring both. In the work alluded to, Mr. Whitehead introduced some observations on syphilis as a cause of abortion and sterility, and he also distinctly noticed the transmission of secondary syphilis from the husband to the wife, and thence to the offspring, and detailed what he considered as the general signs of constitutional syphilis in the female, and some supposed pathognomonic evidences of its existence on the cervix uteri. The remarks he then made were, as he says, only passing and imperfect, and he reserved for the present publication a more full exposition of his views on this subject. The title of "Hereditary Disease," which he has adopted, is certainly too comprehensive, and needs the more explanatory reference on the title-page. The main object, however, of his inquiries, is with the transmission of *syphilis*, to which the notice of other well-known hereditary diseases is subordinate and incidental. It is not, however, in the more beaten path of inquiry, that Mr. Whitehead has followed; his object has not been to discuss the more palpable forms of syphilis and syphilitic infection; but he has endeavoured—and, we think, with considerable success—by a well-observed set of cases, to track out its baneful influence in its more concealed taints, when the symptoms by which it is specifically recognised have often been so changed as to divert the attention of the practitioner from their original alliance with it. There is not, we fully believe, a single subject in practical medicine which has been so unaccountably overlooked as this, nor one in which the valuable observations of bygone authors have been so lazily noticed, or so indifferently pursued. Mr. Whitehead devotes several pages to what he calls the mediæval doctrine with reference to syphilis; in which he shows that the description of a chancre by De Hery, in the sixteenth century, is, in every respect, as correct as John Hunter's; and that the opinions of the former respecting secondary syphilis, which he considered to be more important than the primary, appear to be more in conformity with modern investigation than those of the latter. "Syphilis," says Mr. Whitehead, "has had its inter-

regnum of neglect;" and his quotations from the ancient French of De Hery prove this in an interesting manner.

The first chapter of Mr. Whitehead's work is devoted to the doctrine of Hereditary Transmission; the second and third, to Illustrations of Transmitted Syphilis; the fourth, to the External Characters of Constitutional Syphilis; and the fifth, to the Treatment.

Doctrine of Hereditary Transmission. — In this chapter, Mr. Whitehead cursorily notices the origin, varieties, and intermixture of races, the transmission of congenital peculiarities, and atavism; and passing from the consideration of the continuation of healthy structure, as thus seen on a great scale, he enters on the investigation of the transmission of certain pathological conditions. The peculiarities of a well-marked temperament, leading to a particular diathesis, afford a familiar example of morbid transmission. Mr. Whitehead believes in the occasional origin of deformities in the fœtus from vivid external impressions acting through the imagination of the mother, and he relates some cases in confirmation of this view. In one case which happened to him, a lady was in great dread, in five successive pregnancies, of her children being born blind, on account of her sister's eldest child having had this congenital defect. The result of these pregnancies is thus summarily stated: "Out of five children born at the full term of utero-gestation, each as remarkable for plumpness and vigour as the mother is for a well-developed frame and robust health, the first, third, and fifth of her children had defective development of the left eye, amounting in one to deformity; and the second and fourth had complete loss of vision of the same side." Mr. Whitehead also thinks that connate deficiencies may be occasionally continued hereditarily—a doctrine which, like the preceding one, has been, and continues to be, disputed.

Hereditary diseases are divided by our author into those which are induced by faulty habit, the influences of climate, ill-assorted marriages, &c., and those which result from the introduction of a morbid poison. It is with the latter particularly that he has to do in this publication; but he passes in review a series of diseases which belong to the former, such as scrofula, malignant disease, erysipelas, mental derangement, the purulent diathesis, gout, rheumatism, &c., most of which are aptly illustrated by cases which he has observed. We do not think it necessary, however, to follow his observations on these subjects by any detailed account or criticism.

Illustrations of Transmitted Syphilis. — Thirty-three cases, occupying in their detail a large portion of the entire work, are related by the author, which are supposed to sanction by their analysis the following important propositions:

"1st. That the venereal poison, once introduced into the human body, into the circulation, is liable to remain in the system for an indefinite period; and although there be no outward sign to indicate its presence, that it is nevertheless liable, under favouring circumstances, to reappear and develope itself in various forms of secondary phenomena, known as Lues Venerea.

"2nd. That lues venerea, whether latent or manifest, is capable of being communicated from person to person; and that the poison thus received by one at 'second hand' may be thence conveyed to a third, from the third to a fourth, and probably much farther.

"3rd. That the mode of contamination is commonly through the medium of the

genital organs; but, as a disease of the blood, it may be conveyed through any other channel by which certain of the secretions, or perhaps the blood, of an infected person, are admitted into the circulating current of another; as by the mouth, the nipple, an abraded surface, by vaccination, or through the current of maternal blood destined to nourish the child in the womb.

"4th. That a woman who has been thus infected, although no visible sign of her actual condition be manifest on the outer surface of the body, yet does she, in perhaps nine cases out of ten, bear specific evidence of its presence in her own person; and this evidence is to be found precisely in the part where a knowledge of the laws of organic function would lead one to look for it. The establishment of this fact leads inferentially to the suspicion, that the genital secretions in man may continue to be charged with poisonous matter for a long time after the disease itself appears to have been cured. The extreme susceptibility to excoriation of the glans and præputium penis which is generally noticed in tainted persons, and which in itself constitutes a suspicious symptom, materially strengthens this supposition." (pp. 202, 203.)

The propositions which we have just quoted in detail are systematically examined by the author.

1. *Duration of Lues Venerea.*—The opinions, and still more the practice, of medical men are greatly at variance on this cardinal point. With some, the treatment of a chancre is made a light thing of, advantageously contrasted with the management of gonorrhœa, and when it has healed, treatment is suspended; and should the person remain for two, three, or six months free from any further manifestation of the disease, he is pronounced sound, and permitted to marry. With others, on the contrary, the treatment of syphilis is carried on for six or eight months, without which an individual is not considered secure from secondary symptoms. M. Cazenave, a man of great experience, is amongst the latter number. Of late years, the necessity for prolonging the treatment of syphilis, and exacting a much longer time for the proof of a complete elimination of the poison from the system, has been gaining ground amongst the most discriminating practitioners. Even under the most careful treatment, and after the longest period which has been named, a feeling of insecurity lest a taint is still reserved in the blood, ready to manifest itself in unmistakable signs when the health is from any cause low or weak, or, as we have ourselves seen it, in the feebleness of old age, or still further in its transmission to the wife or the germ, is strongly entertained by a comparatively few, but still an important body of practitioners. This view, which was entertained by De Hery, and well described by Astruc, was opposed by Hunter, whose great and revered name has tended to perpetuate what, with our author, we believe to be a serious error; and it is only quite lately that a more careful and candid inquiry has ended in its revival. Mr. Whitehead's cases, and those enumerated by Cazenave, go to prove that the poison may remain latent for an indefinite period; and our author draws an analogy between it and the incubation of the hydrophobic poison, which has been known to last for ten or twelve years, and even for longer periods. Scrofula, too, which Mr. Whitehead regards as "nearly allied to syphilis," does not, in many instances, develope the congenital taint until puberty, middle age, or even the late periods of life. In one of Mr. Whitehead's cases (No. 14), a female had a primary sore at seventeen years of age, soon after her marriage, which was considered cured, although she

occasionally had for two or three years afterwards sore throat, cutaneous eruptions, purulent leucorrhœa, and a tender state of the vulva. Her husband died twelve years after, and during his life she had been sterile. She married again, two years after the death of her first husband, a healthy man, perfectly free from syphilitic taint, and by him she became pregnant. The child, when born, was healthy, but in a month's time it became "wan and emaciated; its face, breast, breech, and limbs were covered with scaly blotches of a dark-red colour, circular in form, and of various dimensions. The throat was swollen, the mouth and lips sore, the voice husky, the breathing noisy." These signs of syphilis were removed by treatment: they occurred fifteen years after the first reception of the poison. Other cases are recorded, on evidence which we think conclusive, showing the poison to have remained in a latent state, with more active accessions at different times, for periods varying between six and fourteen years. Some of the cases are thus referred to by Mr. Whitehead, in language which we cannot do better than transcribe:

"The taint in case 3 endured fourteen years, and terminated in malignant disease of the womb, which destroyed life at the age of forty-two. The offspring of eleven pregnancies were sacrificed to the malady in this one instance. I treated case 4 eight years after the invasion of primary symptoms; case 12 thirteen years after the first infliction, which appears to have been derived from one having the disease in a secondary form; case 13 about eight years; case 15 seven years from the date of the primary inoculation; and case 21 was of eight years' duration. In none of these had a fresh inoculation been received, but the poison seemed to remain as actively noxious at the respective epochs named, as it had been at any time during its existence. In the preceding six cases, not fewer than forty-six children were sacrificed to this devastating malady.

"Case 22 is that of a midwife who contracted disease while in pursuance of her ordinary calling, through a wound in the finger. Her patient, at the full term of her first pregnancy, had suffered from primary syphilis during the first part of the period, but was said to have been cured. The vagina was, however, studded with warts at the time of delivery. The midwife, who was for several years attended by a practitioner of high respectability, had secondary syphilis in a most severe form, which, though actively treated, relapsed yearly, and reappeared in the spring of 1850, nine years after the inoculation, in characteristic form." (pp. 207, 208.)

Infectious Properties of Lues Venerea.—It is well known that John Hunter, after much and careful observation, entertained the opinion that syphilis, in its secondary form, had lost its infectious character, and was incapable of being transplanted by inoculation from one person to another. He considered that the matter from a secondary venereal sore and the blood-taint had ceased to be venereal; and that as a secondary or constitutional poison, it could not be transmitted from the mother to the fœtus in utero. His authority has continued to lead the mass of medical men to the present time, although it failed to convince the late Mr. Hey, of Leeds, who, in a paper read before the Medico-Chirurgical Society, in 1816, narrated some cases which were directly opposed to John Hunter's doctrines, and perfectly in keeping with Mr. Whitehead's views. It is a subject full of practical importance. Can a man who has been cured of primary syphilis, apparently in good health, with, perhaps, an obscure, ill-defined remnant of secondary syphilis, transmit it to a female through the semen, and, by infecting her blood, graft the poison in the germinal

vesicles of her ovary, and thus infect the fœtus from its source? Mr. Whitehead's cases, as also those of Hey, Colles, and Cazenave, appear to us to be quite conclusive in affirming this view, and our own experience abundantly corroborates it.

In case 1, a man had a primary sore six months before marriage, which was cured; but a small wart, which was supposed to be harmless, sprang up on the site of the chancre, and gradually enlarged. The wife contracted a warty state of the vagina, and she had sore throat and feeble health. The child manifested syphilitic symptoms eight days after birth. In this case, many months had elapsed since the cure of the primary sore. In case 2, the husband had primary syphilis six months before marriage, from which he recovered, and appeared in perfect health at the time of marriage. Three months after marriage he first showed symptoms of secondary syphilis, consisting of sore throat and a free eruption of syphilitic roseola. It was, however, before this external development of the syphilitic poison—when he was, to all appearance, perfectly well—that the indications of the poison were visible in the wife, who first came under the care of the author, two months after marriage, with a syphilitic eruption, and patches of excoriation about the vulva, with a copious purulent discharge. In a few months after treatment, she became pregnant, and was confined with a seven-months' child, which was born dead, with desquamation of the cuticle. The particulars of this case are followed out with regard to both the husband and wife, the latter of whom was again confined of a still-born child, and subsequently of a healthy-looking female child. This child, on the thirty-fifth day, was affected with syphilis, and died on the forty-third day after birth.

In case 5 the indications were much like the preceding case. Mr. Whitehead treated the husband for primary syphilis, and felt certain that he was cured long before he incurred the risk of infecting his wife. Moreover, the wife's symptoms were at no time of a primary character. The husband suffered from secondary disease twelve months afterwards, without any intermediate infection. Other cases of a like import are related (cases 21, 27), which leave no doubt on our minds as to the truthfulness of the conclusions at which Mr. Whitehead has arrived. The author speaks, however, of two cases of gonorrhœa (cases 9, 19) communicating a venereal poison to the wife and offspring; the taint being carried on for a length of time, and exhibiting in its development the characters of constitutional syphilis. It does not appear to us, however, that these cases are sufficient to establish the fact that gonorrhœa can produce secondary syphilitic sores in its transmission to the wife and offspring. Mr. Whitehead has, in our mind, too readily adopted the term gonorrhœa from the patients, on whose testimony it depended, without allowing for the great probability that the urethral discharge was the product of a urethral chancre. It is, of course, well known to practical surgeons, that occasionally a chancre will be formed and pass through its stages in the urethra; that it may then easily be mistaken for gonorrhœa—furnishing, however, even to its last remnant, the source of true syphilitic inoculation. Surely, if gonorrhœa were capable of producing the ordinary sequelæ of primary syphilis, we should constantly meet with it in practice; but we are confident in saying that this is not the case. We should demur, therefore, to the opinion of Mr.

Whitehead, as being deficient in positive proof, which is thus expressed: that "even old strictures of the urethra, or chronic irritability of the canal near the neck of the bladder, or in any part of its extent, the residue of gonorrhœa of a remote date, eliminates, if not always, yet under certain disordered states of the system, a noxious secretion capable of communicating a disorder very similar to, if not identical with, secondary syphilis." We cannot acquit Mr. Whitehead, in this particular instance, of a hasty and, we believe, an erroneous judgment; but it forms a single exception to his wariness in yielding too readily to the testimony of patients.

Modes of Contamination.—We have already referred to the transmission of lues venerea by means of sexual congress, from a husband who has at the time, and has had for a length of time before, no recognisable trace of the disorder. The semen is, under these circumstances, the ready vehicle of inoculation to the wife. But while we readily allow the proof of this important fact in the cases furnished by Mr. Whitehead, and others whom he has quoted, we are hardly prepared for such an extension of the sources of contamination as is allowed by our author. "I believe," says he, "that all the secretions, from mucous surfaces at least, in one thus labouring under constitutional syphilis, are in a measure contaminated; and, if so, it is not unreasonable to infer that the infection may be conveyed through any of these media." Mr. Whitehead, like the mediæval authorities, regards the semen as a means of eliminating the poison; and sexual congress, therefore, as directly purifying the blood. He also, too,—as will be seen by the following quotation, which embodies both opinions,—is disposed to receive favourably the idea that the infection of syphilis may be communicated through the atmosphere.

"A young man twenty-five or twenty-six years of age, of active habits, strong and robust, had a virulent gonorrhœa five years ago, of which he was cured at least twelve months before marriage. He has never had chancre, nor any other attack of gonorrhœa since that now named. In 1850, while in pursuit of his occupation, a heavy weight fell on his foot, fracturing one of the metatarsal bones and otherwise contusing the limb. Ten or twelve weeks afterwards, having been confined to bed in the interim, I was called to see him—(he was under the care of another practitioner for his injury)—having a number of erythematous nodes on the skin of the arms, thighs, legs, face, and elsewhere, with several periosteal swellings of like appearance and extremely painful; he had also at the same time, venereal ulceration of the throat, sclerotitis of the left eye, enlarged nucheal glands, and a glandular swelling in the groin of the same side as the injured limb. His general health had been remarkably good up to the date of his injury, and on making very careful inquiry, I was not able to attribute these symptoms to any antecedent cause except the gonorrhœal affection mentioned, and their development at this time to suspension and irregularity of the sexual function, consequent upon the local injury. His wife, who was barren, and who had been for some time in delicate health, also experienced an aggravation of symptoms under which she had long suffered, attributed by her to anxiety and watching, but which were doubtless determined by other causes. It was probably owing to the appearance of venereal symptoms in the singular manner now specified, that De Hery and his contemporaries were induced to believe in the infectious properties of the atmosphere surrounding an individual thus diseased, a mode of contamination in which, not having witnessed a satisfactory instance of the kind, I do not at present believe, although I would not deny the possibility of it under certain conditions. We must acknowledge at least that an impure atmosphere may be injurious to those constantly inhaling it; that certain kinds of noxious effluvia are pro-

ductive of certain forms of disease; and that in only a few diseases is the effluvium emanating from the suffering individual more sickly and offensive than that which is emitted from one labouring under syphilis in some of its forms." (pp. 218—20.)

Another source of infection, besides that through the sexual organs, is from *lactation;* and our author enters into full details of the experience of different individuals on this subject. He quotes from De Hery, Wiseman, Astruc, Portal, Hey, Colles, Cazenave, and some others, in confirmation of the opinion that nurses labouring under constitutional syphilis may infect the infant; and an infant, so circumstanced, may infect the nurse by inoculating the nipple. The latter of these opinions is abundantly proved by the cases he has quoted. From these we learn, not only that a *child* born with the syphilitic taint can affect the maternal nipple, but that nurses accustomed to drain the breasts of women may get the mouth affected with syphilitic ulceration, and communicate the infection to different persons. An instructive case of this kind is quoted from Mr. Hey, in which a poor blind woman had drawn the breasts of a female supposed to be labouring under venereal disease, after which she became affected with ulcers at the angles of the mouth. Unconscious of this disaster, she went on with her avocation of sucking the breasts of other women, to whom she communicated the infection. The symptoms which resulted are thus described by Mr. Hey:

"Mrs. B. had her breasts drawn twice by this poor woman, upon the death of her second child, who died of the small-pox; and within three or four weeks afterwards she perceived a swelling of the axillary glands, and complained of soreness in her throat. She consulted the late Mr. Billam, a judicious surgeon, who assured her that the disease affecting her throat was venereal, and treated it agreeably to that opinion. During this treatment she became pregnant; but continued the use of the medicines prescribed, till she arrived at the fifth month of her pregnancy. At the end of the seventh month she miscarried of a dead child. I attended her during labour, and perceived nothing amiss in the vagina or contiguous parts. She assured me that these parts had never been affected with disease, and that previously to this confinement she had borne three healthy children.

"She became pregnant again in 1772; continued to enjoy good health, and was delivered February 26th, 1773, of a child apparently healthy, whom she herself suckled. When the child was about six weeks old, an eruption, which I judged to be syphilitic, appeared on its legs and arms. I immediately put both the mother and child upon a mercurial course, giving the former small doses of hydr. submur., and the latter hydr. cū crætâ. By this treatment the child was in a short time freed from the eruptions; but continued to take the medicine till the beginning of August.

"In October following, two or three small ulcers appeared on the outside of the labia pudendi of the child, and on this account the mercurial course was resumed, with the addition of an occasional dose of hydr. subm. The ulcers were soon healed; but in May, 1774, the nostrils became sore, and the nose was also tender. At the same time the voice of the child grew hoarse; the mercurial course was repeated, and continued two months. The child took the medicines also during the months of September and October, after which there was no recurrence of disease.

"In June, 1775, Mrs. B. bore another child who was apparently healthy at birth, and continued to be so for a few weeks. Blotches of a copper-colour then appeared on the skin, but soon went away on having recourse to the mercurial medicines. After some time the blotches appeared again, and were accompanied

with a small ulcer on the labium pudendi, as in the former case. The child was, however, completely cured by a repetition of the treatment, and remained well." (pp. 225—227.)

The danger of a child thus transmitting a syphilitic taint to the nipple, and so poisoning the nurse's blood, is illustrated by other cases, and also by the experience of the author himself; and we hold this opinion to be of infinite moment, and one which no experiments by inoculating with the lancet should overturn. We have ourselves witnessed this deplorable event quite recently in our own practice. A gentleman had a chancre six months before marriage, for which he took mercury and was cured; and his marriage was permitted by a surgeon of high reputation. The wife bore two children, both of whom pined away and died. She had recently been confined with the third child; but, as it was supposed that her milk was not good, a healthy young woman had been engaged as a wet-nurse. In a short time her left nipple became very sore, and deeply ulcerated, and resisted various efforts to cure it, so that at last she was obliged to abandon her charge. Another nurse was sent for in her place, who became affected in a similar way; and it was at this time that we first saw the case. On examining the child, which was beginning to emaciate, we found marks of syphilitic lepra about the nates and legs, which had not been suspected; and on inquiry it was ascertained that the former children had had cutaneous eruptions. On cross-questioning the husband, we found that three months after marriage he had had sore throat and a dark-red eruption, principally at the edge of the hair at the back part of the head, which had never entirely left him. The palm of the right hand had, at the time we saw him, a mottled aspect, and the skin was desquamating; and some small circular copper-coloured spots were visible on the thighs. The wife's health had been ailing, but she had not manifested any notable symptom of syphilis. The sores on the nipple of the nurse we saw had an angry look, with indurated margins. The surface of the mammæ for a considerable distance around them was inflamed, and an abscess eventually formed in the gland itself. The whole of this family—husband, wife, child, and nurse—were treated for *syphilis.*

Cases, however, where the taint has been transmitted, during lactation, to the child from the nurse, are certainly more rare; and Dr. Colles was of opinion that a diseased nurse could not infect the child unless she had ulceration of the nipple. Mr. Whitehead's cases, however, satisfy us that the taint may pass from the mother to the child without any such lesion, the milk being the means of its conveyance. Two out of five of these cases are thus shortly referred to :—

"In case 12, the mother contracted primary syphilis from her husband six weeks after her sixth delivery. All her previous children were healthy. The infant, also in health up to the date of its mother's accident, was covered with secondary syphilitic eruptions and other symptoms at three months old, several weeks before any secondary indications manifested themselves in its mother.

"Case 15 is, in many respects, similar to the preceding; the mother having contracted a primary affection from her husband one month after her second delivery, the child and she being at the time in good health. Secondary symptoms of characteristic form and of considerable severity came out in the infant a length of time before the complaint had assumed the secondary type in its mother." (pp. 229, 230.)

Mr. Whitehead details a very striking case which occurred in his own practice, in which a child was inoculated with syphilis concurrently with vaccination; and we confess that we see no grounds for scepticism with regard to it. A healthy-born child was thriving well at the third month when he was vaccinated. The vesicles, on the fourteenth day, had been converted into two "deep, broad ulcers with extensive induration," which occasioned absorbent inflammation and an axillary abscess. "A number of copper-coloured blotches were at this period present on the skin; the mouth was inflamed and excoriated, and appeared to have communicated a similar lesion to the mother's nipples." The child died at four months and a half. It was ascertained that the child who had furnished the virus had had purulent ophthalmia, with blotches on the skin of a venereal character. The mother in this case became fearfully infected from the child. Two practical observations, or rather salutary cautions, are thus expressed:

"1st. That in the performance of vaccination, it is impossible to be too scrupulous, not only as to the health of the child by whom the virus is furnished, but also as to that of its parents: even an unhealthy condition of specific character known to have existed in those of the third generation anteriorly, should constitute a sufficient ground for refusing to make use of the matter produced by the grandchild, however healthful this may appear to be. 2ndly. That instruments employed in operating upon any individual, should not be used upon another patient before they have been carefully cleansed, and immersed in boiling water, with a view to destroy the potency of any matter which may have adhered to them from a previous operation." (p. 234.)

Other Modes of Infection.—Mr. Whitehead considers that the infection of lues venerea may be communicated by contact with a raw surface on another individual, also to any sound mucous surface, or even to the healthy integument. He illustrates the first by the case of a midwife who caught syphilis when attending a female during her labour affected with secondary symptoms—the point inoculated being her right forefinger, on which was a scratch. A similar case in the person of another midwife is related, who at the time of infection was seven months gone in pregnancy. The child borne by her had in five months a chancrous ulcer under the tip of the tongue, sore throat and mouth, a pemphigous vesicle on the second toe of the right foot, and other specific symptoms, which were cured by anti-venereal treatment. The transmission of a secondary poison by contact with a mucous membrane, is illustrated by cases in which the labial mucous membrane was affected by kissing tainted children, and a severe form of constitutional syphilis resulted. "One undoubted instance," says Mr. Whitehead, "has fallen under my notice, where secondary chancre arose in a part of the body previously sound, by the application of the matter of a secondary sore." The opinion of the author on the more doubtful, not to say fanciful, modes of infection, is thus summarily expressed:—

"Although we may discredit the possibility of receiving the infection by drinking out of the same glass, or smoking the same pipe, or sleeping in the same bed that has been previously occupied by one diseased, inhaling the atmosphere which surrounds him, using the same bath, or steadfastly regarding a gonorrhœal ophthalmia; yet I, for my part, would submit to many inconveniences rather than knowingly incur the risk of contamination by inattention to even such trifling and apparently harmless circumstances as these. Indeed, I am strongly inclined to coincide with Biett's conviction, that 'there exist certain forms of syphilis with

which every species of contact may prove dangerous.'* The truth of this remark will probably be acknowledged by many experienced practitioners." (p. 239.)

Mr. Whitehead combats the following opinions of M. Ricord; (1) that tertiary syphilis is not communicable; (2) that there is no such thing as an infection of the child by the mother, she having been contaminated by the father; (3) that for a fœtus in utero to be infected, the parents being healthy-looking, it is necessary for the mother, by direct inoculation, to have an indurated chancre and all its consequences. A reference to Mr. Whitehead's cases appears to us to establish the opposite conclusions; and our own experience on these points coincides with his.

With regard to the age at which lues venerea shows itself in the offspring of a tainted parent, there is no uniformity. In many cases the syphilitic poison kills the fœtus at an early period of intra-uterine life; in others the cutaneous disease has been the most prominent, the fœtus being expelled with extensive desquamation of the cuticle, which Mr. Whitehead is not disposed to regard as the result of death and decomposition, but rather as the result of intra-uterine pemphigus. The fœtus may have syphilitic eruptions on its skin at the time of birth; but more frequently they appear from the fourth to the twentieth day after birth. Sometimes, the symptoms are delayed as long as one year, and in one case in Mr. Whitehead's experience, after five years. He does not notice the condition of the placenta, and especially of the maternal part of it, which, in our experience, is frequently the seat of syphilitic disease.

Of Certain Abnormal States of the Uterus which Indicate the Presence of Lues Venerea.—Before describing the peculiar uterine marks of lues venerea, Mr. Whitehead notices the fact, which is borne out by his cases, that in whatever part of the body the syphilitic poison may have been first implanted, whether the nipple, the lips, or skin, the sexual organs are almost sure to suffer. A puriform vaginal discharge is the most invariable symptom, which is said by Mr. Whitehead to differ from the product of simple inflammatory or ulcerative action, by having "a greenish tint, which is often very difficult to remove from the linen by washing." But surely this is not peculiar to syphilis. We have seen the greenish discharge quite irrespective of any syphilitic taint; and when the taint has been most certain, the discharge has not had the greenish tint. We doubt much whether this observation will hold good; and certainly it would, in our judgment, be reprehensible for any practitioner, seeing the green stain, to anticipate the presence of lues venerea.

Amongst the constitutional phenomena, Mr. Whitehead particularly notices, that the skin of the face has a *leaden* or *ashy pallor*, specially marked in the lower eyelid, which he thinks is never seen in any other form of disease. The uterine indications of the presence of lues venerea are thus noticed.

"They may be reduced to the following forms: 1st, hypertrophy, implicating the lower section merely, or extending upwards to the body, or even involving the whole organ; 2nd, induration, existing circularly or partially, or extending in some instances as far as the touch can ascertain the condition; 3rd, erythema, presenting an even surface of a dark red, glistening aspect, or being interspersed with a number of white elevations usually denominated follicular enlargements;

* Cazenave, op. cit. p. 115.

4th, excoriation, the cuticle, when the parts happen to be viewed at an early period, being broken in such manner as to present an appearance as though the subjacent structure had increased to a dimension beyond the capacity of its cuticular envelope, which seems as if it had burst from over-distention; 5th, aphthous ulceration; 6th, endo-metritis, inflammation and ulceration of the inner surface of the uterus, terminating externally, where it is sometimes seen to surround the orifice, at other times implicating but one labium, and limited outwardly by a defined margin; 7th, warty excrescences." (pp. 252, 253.)

We have examined the more extended account which the author gives of these local symptoms, with great interest, with the hope of gathering some one or more indications which might be considered pathognomonic of the existence of the syphilitic poison. We are bound to say, however, that however accurate Mr. Whitehead may be in his direct ocular observations of the uterus when females have had the venereal poison, yet we do not recognise, in his account of them, such indubitable characters as would allow us to pronounce of any one of them, that *it* reveals the syphilitic mark and cannot be anything else. We remember well Mr. Whitehead's former description of some of these signs; and they were of far too important a character, not to have been subjected by every person who has to deal with these diseases on a large scale, to careful and impartial observation. To be able to detect syphilis, when not manifested on the surface of the body, by inspecting the uterus, and there to find its presence unequivocally marked, would have been a boon to practical medicine. We quite concur with the author, that hypertrophy and induration of the body and neck of the womb are the unquestionable results of syphilitic inoculation; but we should utterly fail to distinguish them from the hypertrophy and induration which may occur independently of syphilis. Of the *third* indication which has been quoted, Mr. Whitehead says that, when present, "it may pretty confidently be asserted, that the patient has the syphilitic taint, and that she will be liable to transmit the evil to her offspring." We know this state very well, but we demur to its exclusive syphilitic origin. The *fourth* mark—namely, that of patchy excoriation of the cervix, without ulceration, and the appearance which is well described by the author, as if the cuticle had broken from over-distention, does certainly occur as a sequel of gonorrhœa; but so it does from simple inflammation of the cervix, especially when attended with ovarian excitation. The fifth mark of uterine syphilis, denominated, generally, aphthous ulceration, is thus carefully described:—

"Sometimes the lower section of the uterus, morbidly enlarged, presents a number of aphthous ulcerations, very similar in aspect to those so denominated in cases of thrush in children. But, as seen on the labia uteri, they are surrounded by, or rather are situated upon, a dark red, inflamed surface. They are slightly raised, in shape irregularly circular, and appear at first view as grey isolated incrustations. If a piece of dry lint be brushed over them, the crust is detached, leaving a red surface of corresponding dimensions, which is minutely granular, and bounded by a well defined, slightly elevated margin. They are about a quarter of an inch in diameter, larger or smaller. The presence of this eruption, so far as I know at present, is peculiar to the syphilitic diathesis, which, in the cases I have investigated, resulted in some from primary syphilis long passed, in others from the infection derived during the existence of what Cazenave denominates the primary stage of a severe secondary affection. A woman so disordered is capable of transmitting the taint to her offspring, in whom it is liable, if not actively treated, to run a rapid and destructive course." (p. 255.)

We have at the present moment a lady under our care in the seventh month of her fourth pregnancy, who has this state of the cervix, with some patches of diphtheritic disease of the vagina. We attended her in a former pregnancy for the very same disorder, which came on at the fourth month; and although greatly mitigated by local and constitutional remedies, the latter being principally directed to the disturbance of the digestive organs, yet she was never quite free from it. The child born was a healthy male, who has continued well to the present time. Her present disorder appears to be a disease of pregnancy, just like the former attack, but we have not the faintest suspicion of syphilis.—We confess, too, that we can discover nothing distinctive of syphilitic disease in the *sixth* mark, although we are disposed to look with great suspicion upon the presence of warts as an indication of the syphilitic diathesis. But although we have less faith than the author in these supposed venereal symptoms, yet we candidly acknowledge our obligation to him for his careful scrutiny of the cases he has detailed. The uterine symptoms, as he has described them, when taken concurrently with the history of the cases, afford valuable confirmatory evidence, and when seen may even suggest an inquiry into any antecedent syphilitic taint. Still, we repeat, we do not think them pathognomonic of lues venerea. The author concludes this chapter by a notice of syphilis as a precursor of cancerous disease of the uterus, and he strengthens his own opinion of their morbid relation by a reference to some cases of Mauriceau's. We have ourselves long noticed in the history of cancerous disease, a pre-existing syphilitic affection, and we have regarded the syphilitic induration of the cervix as peculiarly predisposing to the development of the cancer-cell. Mr. Whitehead's observations on this important relation of syphilis to cancer we consider to be perfectly true, and we hope to see this subject more fully worked out by him at some future time.

External Characters of Constitutional Syphilis.—Having remarked on the orderly development of the syphilitic poison in its primary form, and the uncertainty as to *time* of its appearance in the secondary form in adults, the author remarks on the rapid infection of the fœtus in utero, if syphilis be contracted in the acute form during pregnancy. The following numerical statement of the injurious influence of syphilis on fœtal life is interesting:—

"Out of 256 deliveries of syphilitic women in my own practice, 110 terminated prematurely at different periods of the process. In 5 cases the event happened at two months; in 30, at three months; in 13, at four months; in 4, at five months; in 10, at six months; in 39, at seven months; in 16, at eight months. Only two of these were born alive; they were seven-months' children. One of them died on the second day, the other a few days later.

"Of the remaining cases, amounting to 146, said to have been at the full term when delivery took place, 63 died at the following ages: 12 during the first week; 2 in the second week; 1 in the third week; 5 in the fourth week; 8 during the second month; 6 during the third month; 17 during the second quarter of a year; 3 in the third quarter; 1 in the fourth quarter; 7 during the second year; and 1 in the third year of life. A few were still-born, and a considerable number of those who survive are still infants, a large proportion of whom may probably not live beyond the period of early childhood." (p. 265.)

The most frequent and characteristic external phenomena of syphilis are

seen upon the cutaneous surface; and the author relates at some length his experience on these disorders, as they occur in infancy and childhood. The following forms of skin-diseases, arranged in the order of their frequency, are described:—1st. Exanthemata; 2nd. Squamæ; 3rd. Papulæ; 4th. Tubercula; 5th. Pustulæ; 6th. Vesiculæ; 7th. Pemphigus. The particular description of these disorders is very clear and truthful, and free from the redundancy of expression, which is a prevailing error with the author. He notices three varieties of exanthemata: first, roseolous blotches coming out about the end of the first week after birth, and attacking with almost equal frequency the face and breech; a second variety consists in broad erythematous patches of a dark red hue, principally affecting the mucous orifices, as the vagina, the anus, the mouth, &c.; the third variety is described as a measly appearance, which may not improperly be called rubeola syphilitica.

The *squamous* eruptions are lepra, psoriasis, and fissuring of the rectum. The following quotation of the leprous variety is a favourable example of Mr. Whitehead's graphic delineation:—

"The leprous variety commences in form of papular patches situated on the cheeks, hands, and feet. At first, a dark red spot is noticed, which, if closely examined, will be found to consist of one or a congeries of small papulæ. Each papule is early covered by a dry scale, which shortly becomes detached at its circumference; it soon separates altogether, leaving a surface at first red and glistening, which in a short time shrinks and grows pale. But before the complete separation of the cuticle from the first spot, a circular row of papules springs up around the original one, each having a separate incrustation, which in like manner falls away, and is succeeded by another row still more external, and thus the spot increases in diameter, becoming at length confluent with others that may be situated in the vicinity. The disease thus assumes a serpiginous aspect, although very different from tubercular serpigo, properly so called. Its most common situations, in infancy, are the face, hands, and feet. In the adult, it seldom occurs on the face, but is generally confined to the hands; and it may be here noticed, that there is scarcely an outward symptom which is more certainly indicative of the presence of syphilis in the constitution than this. The lepra manuum and the lepra pedum of infants, does not, generally speaking, assume that decidedly annular form which is seen in the adult; it is usually more oval, and the patches are more numerous, especially on the sole of the foot, and they are more liable to become confluent." (p. 271.)

Without following our author's description of the intermediate forms, we notice that he concurs with M. Dubois in the syphilitic origin of fœtal and infantile pemphigus. We have already remarked, that Mr. Whitehead considers the desquamations which are seen on the fœtal skin in still-born syphilitic fœtuses, as the probable result of pemphigous vesications. He speaks, too, of the fatality which accompanies this disorder, as almost invariable, "when pemphigus exists at birth, the child living;" and says that it usually occurs "in the offspring of those mothers who have suffered severely from primary syphilis during pregnancy, or who have experienced an acute attack of the disease in secondary form."

Sloughing of the pudendum in children, as described by Mr. Kinder Wood and Dr. Percival, is regarded by Mr. Whitehead as intimately allied with syphilitic pemphigus. This form of disorder occurred in some of Mr. Whitehead's cases. Thus, in case nine, a child five months old, amongst other signs of constitutional syphilis, had most prominently—

"A deep and extensive ulceration of the pudendum, implicating the two upper thirds of the right, and about one-third of the left labium, with their superior commissure and urethral orifice. This was said to have commenced in form of blood-blisters. The surrounding textures, as far as the mons Veneris and right groin, were phlegmonous, and in the flexure of the groin was a broad patch of ulceration—not an open abscess—of smaller extent, but similar in character to that on the pudendum." (p. 100, 101.)

Patches of ulceration, of a less destructive character than that attacking the vulva, may attack other parts of the body, especially at the different flexures. The character of the ulcer is described as peculiar. Its surface is covered by a grey coating of glutinous purulent secretion, there being no appearance, during the first days—sometimes for many days—of granulations. The edges are somewhat raised and thickened, occasionally overhanging; and very often warty vegetations may be seen to spring up from the surface of the sore, especially from any deep part of any fold of skin which traverses it.

Cracks and fissures of the skin, alopecia, ozæna, stomatitis, inflammations of the conjunctiva and sclerotic, are severally described amongst the external phenomena of lues venerea. Iritis, the result of syphilis, the author believes but rarely to happen in infantile life; and periostitis, caries, and exostosis, are rather sequelæ of constitutional syphilis in the adult than in the child.

"The more common mode, however, in which this taint is liable to produce its effects upon the osseous system, is indirectly by impairment of the nutritive functions. In cases of infantile syphilis which have been badly treated, the organs of locomotion, after a time, begin to suffer in an especial manner, while the brain and nervous system become morbidly irritable. The brain is often disproportionately large, and the sensorial faculties abnormally exalted; the muscular and osseous tissues suffering a corresponding amount of deprivation, or even of actual decay. The process of ossification is manifestly delayed, as may be witnessed in the state of the cranial bones; the fontanelles, and often even the linear sutures, remaining open to a late period. The muscles are attenuated; the joints apparently, sometimes in reality, enlarged. The long muscles especially are liable to become emaciated, while the cylindrical bones are weakened, and a decided case of rickets is determined. Associated with this state we commonly observe a tumid abdomen, due to glandular hypertrophy, with wasting of the rest of the body." (pp. 288, 289.)

Erythematous stains of the skin, and a peculiar form of eczema, are included in the outward manifestations of inherited syphilis; while rheumatism is rightly regarded as more commonly a sequel of gonorrhœa than of syphilis, and has not been known by the author to have been hereditarily continued. Passing from these more common forms of syphilitic disease, Mr. Whitehead expresses his belief, that "there is scarcely an organic disease met with, from whatever cause arising, which may not also have its origin in the venereal poison;" and this opinion is but a reiteration of that so remarkably indicated by De Hery.

We have already noticed the peculiar leaden, ashy look of constitutional syphilis, as described by Mr. Whitehead. In the chapter under consideration, he again reverts to the physiognomical aspects, and in subsequent paragraphs he notices the peculiar tint of syphilitic eruptions, and their characteristic circular form. The former is thus described:—

"The colour of the cutaneous eruptions has been spoken of by writers from the earliest date of its history. It appears to have been a matter of some difficulty with many to discover an object to which it could be accurately likened. It was by some denominated a dark red, brown, or livid colour; others compared it to the colour of the flesh of cured ham; Nisbett and Swediaur first designated it a copper-colour, which term was adopted subsequently, and is still preserved. There are many genuine cases of lues venerea, however, especially in infants, where the comparison is by no means applicable. The hue varies according to complexion and temperament, and is especially influenced by the state of the nervous and circulatory forces. It is different also in different forms of eruption, those most nearly corresponding to Nisbett's designation being the tubercular and the areolæ of the pustular varieties. The roseolous blotch also has commonly a decided coppery tint, but is often a shade lighter. Erythema occupying the neighbourhood of the mucous orifices has a dull red colour, not unlike that of polished mahogany which has been some time in use, and the same remark will apply to rubeolous erythema of the abdomen and other parts of the body. The herpetic and squamous varieties present an ashy hue on the central parts, with a circle of darker aspect surrounding. It varies also according to the nature of the primary affection, being, as a general rule, of a lighter shade in the eruptions supervening upon gonorrhœa, than in those which were preceded by chancre. In short, the feature of the external aspect of lues venerea possesses characters peculiarly its own, not correctly comparable with those of any other known object. Cazenave proposes to designate it simply the syphilitic tint. According to this author, its seat is in the colouring layer of the skin, the rete mucosum, and not in the capillary plexus. This seems to be demonstrable by the fact that it does not disappear under momentary pressure of the finger, as does the blush of simple inflammation, and it becomes more deeply evident as the subacute symptoms subside into the chronic form. Its long persistence after the surface has healed is deemed to be further illustrative of the same fact." (pp. 298, 299.)

Mr. Whitehead has analyzed the blood in syphilitic disease, and he finds it altered in its constituent proportions and sensible properties. The colour of syphilitic blood has nothing of the rich claret colour of venous blood, but it has a dull and more decidedly dark red tint. The tint of the blood corresponds with the peculiar colour of the syphilitic eruptions; and it can "scarcely be doubted," says our author, "that the red tint of venereal blotches is due to localization of the red particles of the blood; hence it is not unreasonable to consider the appearance of the one as a true manifestation of the state of the other."

The proportions of the clot and serum vary, but their average proportions are stated as—serum 543, clot 457, in 1000 grains.

"The clot or red constituent separated into two different portions, divided from each other by a distinct line of demarcation. The upper portion was moderately firmly contracted, deeply cupped on the surface, covered with a coat of light yellow, semi-transparent fibrin, which was surrounded by a fringed border. The lower portion was perfectly fluid, and appeared gritty or disintegrated. The proportions of these two parts relatively to each other appeared to be about one of the fluid portion to sixteen or twenty of the coagulum. The colour of the clot was darker than that of the uncoagulated part; that of the fluid sediment being of a bright red. The latter was evidently very heavy." (p. 302.)

The fibrin in proportion to the whole mass of blood, was 3·04 per 1000 grains.

"The serum was thick like mucilage, semi-opaque, of a greenish-buff colour, and had a specific density of 1031. On evaporation at a low temperature, a solid residuum was left amounting to the enormous quantity of 623 parts in 1000.

This, after incineration, left 25 per 1000 grains of ashes. Portions of blood incinerated altogether left from 29 to 31 grains of ashes per 1000." (p. 302.)

The syphilitic tint is no doubt in part due, as the author remarks, to this state of the serum; whilst the excess of albuminous matter in the serum, and the blanched and semi-transparent aspect of the fibrinous crust surmounting the clot, indicate respectively defective fibrinization and an impairment of the nutritive properties of the blood.

Treatment.—Mr. Whitehead remarks on the heap of remedies which have been tried for the cure of syphilis, and the absence of uniformity in its treatment at the present time. Sarsaparilla, in his experience, used alone, has not had any more beneficial effect than so much water; and he considers the virtue of the compound decoction of sarsaparilla to reside in the other ingredients, as the sassafras, mezereon, and guaiacum, which "are really efficacious in themselves."

The *solanum dulcamara* has fallen, in Mr. Whitehead's opinion, into unmerited neglect, and he refers to a most intractable case of syphilitic lepra, which was completely cured by it.

Speaking of the use of *copaiva* in gonorrhœa, Mr. Whitehead remarks, that when exhibited in full doses, so as to produce nausea and vomiting, it will often succeed in arresting the discharge and curing the disease, when in mild form, in a few days, or even in a few hours, without any local remedy.

The *rumex aquaticus*, or hydrolapathium, is spoken of as a remedy of great value in rupia syphilitica, and perforating ulcer after syphilis, but of doubtful value in the other forms of syphilis. The author has tried it very fully.

Nitric and nitro-hydrochloric acids, which were supposed to possess anti-syphilitic properties, are discarded as anti-venereal remedies by Mr. Whitehead, who considers them useless, except as local remedies in the form of gargle to moderate the effects of ptyalism.

But however trifling the effect of these various remedies may be in the cure of syphilis, they have some advantage over the inert practice which has been recommended by Mr. Rose; and Mr. Whitehead argues, we think very justly, that Mr. Rose's opportunities for watching the results of his non-mercurial treatment have not probably been such as to leave his apparent success unquestioned. There can be no doubt, according to the observation of Mr. Rose, that a primary sore may be cured without the employment of specific remedies; but it requires a long time to elapse before the proof is clear that the virus is eradicated. Mr. Rose's subjects were soldiers, a class of men whose disciplined habits and hygienic regulations render them, on the one hand, peculiarly fitted to cast out a poison from the system, or to stay its spread, and on the other, from their liability to removal, to prevent its being traced in after years. In Mr. Whitehead's judgment, the soldier said to be worn out by hard service, is too often the soldier worn out by neglected syphilis. In confutation of the supposed success of Mr. Rose's plan of treatment, Mr. Whitehead appeals to many of his own cases, in which the primary sore had been healed without treatment, and yet the infection had subsequently been conveyed to the wife and offspring. There is no gainsaying, in our mind, our author's conviction against the curability of syphilis by non-specific treatment, and the lamentable results which these records furnish of its deceptive influence.

Of *iodine* and its preparations as anti-syphilitic remedies, Mr. Whitehead does not appear to entertain so favourable an opinion as we believe is generally admitted in London. In the small doses in which iodide of potassium is commonly administered, from two to five grains, he has failed to obtain favourable results; but when given in doses of ten grains to a scruple or half a drachm three times a day, so as to produce iodism, in itself no small evil, "its curative effects are in some instances all that can be desired." He refers to several of his cases in support of his opinion, that iodide of potassium, as well as some others of the iodine compounds, are inadequate to effect the eradication of the venereal poison. When associated with mercury, as a proto-iodide, Mr. Whitehead's experience, like that of M. Cazenave, has been every way satisfactory.

Mercury, in the author's opinion, is the remedy for syphilis. "Judiciously administered, carefully watched, and its different preparations suitably adapted to the severity and stage of the disease, and the peculiar habit or temperament of the patient, there is no remedy which can for a moment bear comparison with it in efficacy." Mr. Whitehead opposes the notion, that salivation is all that is required to guarantee the curative influence of mercury. In cases where salivation follows on a very small dose of mercury, such as a single grain of calomel, he argues against the idea of the mineral pervading the textures of the body, so as to neutralize or eradicate the syphilitic virus. There are two modes in which the curative effect of mercury may be obtained, either "by means of small doses, so as to develop the required action slowly, or by affecting the system quickly and fully by large and often-repeated doses." The first mode ought to be adopted when the symptoms are mild or of long standing. If salivation be produced by the small doses above-mentioned, say a grain of calomel, then the dose must be diminished to a quarter, an eighth, a twentieth, or the fifteenth of a grain.

"In all cases where it is determined to administer mercury in small quantities, as a curative remedy, in adult patients, it is always desirable to repeat the doses daily, until its action upon the system is manifested by incipient ptyalism. This should take place about the end of the second or third week. An interval of a few days may then be allowed to pass as a period of rest, during which time the bowels ought to be freely acted on by aperients, and then the remedy be recommenced and continued until a like phenomenon is again brought about. In this manner the remedy should be repeated during a period varying from four to eight or twelve months, after which space of time the iodurated sarsaparilla may be given for one, two, or three months with advantage." (p. 331.)

Large and frequent doses are indicated in iritis, acute arthritis, periostitis, and caries of the cranial bones, to be succeeded, when the acute symptoms are arrested, by "small doses of a mild preparation, continued for the space of several months." Of the different preparations of mercury given internally, the *oxide* is spoken of as the mildest and most manageable; and the *unguentum hydrargyri* as "the most efficient and manageable form for producing the specific effect of the drug upon the system, whether in the adult or the infant." *Calomel* is useful as a mercurial purgative before a mercurial course. It is useful, too, to induce slow action in very small doses; but it should not be given to children, excepting to arrest acute cases of organic inflammation. The *bichloride of mercury* is highly extolled,

as combining the properties of a mercurial alterative with those of a metallic tonic; and it can be used in lues venerea, in scrofulous subjects, and also in children with a feeble, languid circulation. The *peroxide of mercury*, finely levigated, is said by the author to be a valuable endermic medicine.

The simultaneous use of iodine and mercury, introduced through different channels, is thus spoken of:—

"In many cases of lues venerea, even those of an inveterate character, as impetigo, eczema, and the serpiginous tubercle, occurring as a relapse at a remote period, the symptoms will yield under their employment, and if the remedies be continued for a length of time after the cure seems to be complete, no further troubles will arise. The way to make use of them is to introduce the mercury endermically, and the iodine, in form of iodide of potassium, by the stomach. Supposing the case to be one of impetigo in a child three years old, the diluted mercurial ointment should be spread upon a piece of flannel sufficiently large to cover the lower half of the abdomen, the sub-umbilical region, where it is to be constantly maintained by means of a broad bandage. An additional quantity of the ointment should be spread upon the same flannel once a day, and continued until the alterative action—not the mercurial crisis as commonly understood—is evident. At the same time, half a grain or more of iodide of potassium, dissolved in a dessert-spoonful of compound decoction of sarsaparilla, must be given three times a day. Their use should be continued for a length of time after all symptoms have disappeared. Even in cases where the disease has assumed the form of mesenteric hypertrophy with cachectic habit, I have seen this plan of treatment attended with complete success." (p. 336.)

Arsenic and *sulphur* are referred to by the author; but their anti-venereal powers are inconsiderable.

Mr. Whitehead enumerates the advantages which he supposes to belong to mercurialization by inunction, over the introduction of the mineral by the stomach. Amongst these, we may notice that it is less liable to interfere with digestion; it is not changed by decomposition, as it may be when meeting with acids in the stomach; and it need not salivate, or if it does, the disagreeable effects of salivation are greatly mitigated. We must express some surprise at the slight manner in which he alludes to mercurial fumigations; the use we have made of them since Mr. Langston Parker brought this plan of treatment again into notice, having strongly impressed us with their efficacy in appropriate cases.

We cannot forbear from quoting Mr. Whitehead's method of treating infantile syphilis, although our own experience is more favourable than his as to the use of the hydr. c. cretâ, to the exclusion of the endermic method of mercurializing.

"The treatment for children should be commenced with an aperient, provided the bowels are not already relaxed. The most proper medicine for this purpose is a powder composed of one or two grains of hydr. cum cretâ, two grains of rhubarb, and a few grains of magnesia or scammony, to be repeated daily to the third time. If the bowels were previously relaxed, the aperient may be dispensed with. Then during the space of three or four days more, the ung. hydr. fort., diluted with an equal quantity of cerat. cetacei, should be applied to the abdomen upon a piece of thick flannel or felt, in the manner already stated. During another week the ointment used should be twice diluted, and for the remainder of the treatment the dilution may be carried one or two degrees further. At the end of sixteen or twenty days, it should be discontinued for two or three days, and the aperient given once or oftener according to circumstances. After this, the diluted remedy should be resumed, and continued for a like period, being followed again by an

interval of rest as previously, and again practised a third, a fourth, a fifth, or even a sixth time, the quality of the application being regulated, as to strength, as necessity may require. Conditions may arise requiring the use of other remedies, or the discontinuance of the alteratives—an interruption which ought, however, to be avoided if possible. But should severe griping, diarrhœa, or restlessness come on, the ointment may be either discontinued for a few days, or be combined with a little camphor, extract of opium, morphine, extract of poppies, or aromatic confection. If the skin should be irritable, or become irritated by the remedy, the diluting material may partly consist of confection of almonds, with the addition of a few drops of hydrocyanic acid." (pp. 339, 340.)

When the offspring is affected with syphilis, we cordially concur with Mr. Whitehead in putting both patients under the influence of mercury. For this purpose he recommends mercurial inunction, to be followed for four, six, or eight weeks by the daily use of the ioduretted sarsaparilla. During the continuance of this treatment the author urges the importance of abstinent diet, the avoidance of acids and saccharine substances, and great attention to preserving the skin warm.

When iodine is being taken, the diet ought to include animal food, to the exclusion of farinaceous substances; a little bread, and certain vegetables, such as carrots, cauliflower, and some of the leguminous varieties which contain but little fecula, may be taken occasionally.

Cod-liver oil is, as our author mentions, of great value in infantile syphilis. In rickets, and chronic cutaneous affections, attended with general wasting, it is particularly useful; and although unequal to the cure of the syphilitic taint, it will improve the general health, so as to admit of the employment of a mercurial course.

Antimony and ipecacuanha are recognised as valuable auxiliaries to mercury in constitutions not readily susceptible to its action. They may be given in nauseating doses by themselves, when mercurial inunction is being employed; or combined with the mercurial in pill when administered internally. Venesection and the warm bath are similar auxiliaries.

Mr. Whitehead concludes this chapter by noticing various external applications which are useful in "limiting the extent of the sloughing process, and in diminishing the spreading tendency of some eruptive disorders, in allaying local irritation, correcting and stimulating unhealthy and defective action, and in disposing ulcerated surfaces to cicatrize."

Our readers will see, from the few remarks which we have judged right to intersperse with our review of Mr. Whitehead's opinions on a question of large practical significance, that we side with him against the views of Hunter, and against the opinions of M. Ricord. Without adopting in detail all that Mr. Whitehead believes with reference to the mode of infection of lues venerea, and some other matters, we concur with him in all the main points on which he has insisted. It lies particularly in the path of obstetric practice, to recognise the wide-spread evil which he has so ably described; and we are well assured that from the adoption of limited views on this subject, there result a frightful destruction of fœtal life, and a formidable amount of disease in adult life which might otherwise be arrested and removed.

With some trifling faults of style, useless repetitions here and there, and occasional obscurity, which a careful revision would readily have remedied, the work is eminently sound and philosophical, and calculated to be of great and lasting service to the profession.

Art. III.

Alcoholismus Chronicus eller Chronisk Alkoholssjukdom, &c. Af Dr. M. Huss. Andra Afdelningen.—*Stockholm*, 1851.

Alcoholismus Chronicus, or Chronic Alcohol-disease, &c. By Dr. Magnus Huss. Part Second. 8vo.—*Stockholm*. 1851, pp. 215.

In a former number of this Review, (Jan. 1851, p. 47,) we briefly adverted to the important researches of Professor Huss upon the subject of chronic poisoning by alcoholic drinks. The evil effects of over-indulgence in spirituous liquors have long been recognised; the effects of large doses of alcohol are well known; but the changes eventually produced in the system by prolonged and habitual dram-drinking, have never until now been classified and arranged, so as to add a new disorder to our long catalogue of disease. The former portion of the work now before us, was necessarily incomplete; it was rather a record of cases carefully and judiciously observed; and our author wisely refrained from publishing the second part, until further researches and experiments had convinced him of the correctness of his views. We complained at the time, that there was no word of the treatment, etiology, or pathology of the disease, in the part we then reviewed: and we had so often experienced the disappointment of waiting for a second and *conclusive* part of an important publication, which second part, perhaps, never made its appearance at all, that we blamed our excellent author, perhaps too hastily, for not giving the full and complete view of his researches in a single volume.

We are glad, indeed, to find that we have judged rashly, for Dr. Huss has early redeemed his promise of finishing his work; and we have now, for the first time, the full history of this singular but most interesting disorder laid before us. Dr. Huss was well aware that the correctness of many of his observations would be questioned; and he has, in this second part, anticipated most of the arguments that might be urged against the existence and causes of this new malady; while the work has now assumed a continuous shape, to which the former portion stands in the light of an appendix and record of cases for reference.

A description of the malady would scarcely be perfect without some notice of the history and bibliography of the abuse of ardent spirits, and especially of intemperance in Sweden—a country that seems to rival, if not to surpass, even the northern portion of our island, in the quantity of alcoholic drinks consumed by the labouring classes. In Sweden, brandy has been of late years distilled mostly from potatoes, and the cheapness of the material renders it easily accessible to the working population, for Swedish brandy is sold at a much lower rate than even the most inferior whisky in Scotland.

The bibliography of intemperance prefixed to Dr. Huss's essay is not a little interesting. We are surprised to find that so many authors have written on the dangers of spirit-drinking, long before the origin of temperance societies. A curious passage is cited by our author from Seneca, (Epist. 95,) from which it would appear, that the use of hot wines produced, upon the enervated Romans of the empire, symptoms very analo-

gous to those we shall presently describe. Among German writers, Ploucquet ascribed the results of dram-drinking to the acetate of copper communicated to the spirit by its being kept in untinned copper vessels. As this, however, is not now the case, and especially in Sweden, we must seek some other cause. Perhaps the very best description of the disease now designated as Alcoholismus Chronicus, is given by our countryman Lettsom, who accurately records the loss of power of the arms and legs, and the accompanying anæsthesia, nor does he forget to notice the opposite character of excessive sensibility.

From the bibliography our author passes to the Description of the disease. We alluded in our former notice to many of the symptoms of this strange disorder; but for a complete picture of its ordinary progress and termination, we shall here use the words of Dr. Huss.

"We give the name of Alcoholismus Chronicus to those groups of nervous symptoms, which, affecting alike the motor and sensorial powers, and the mental capacities of the individual affected, proceed generally in a slow and chronic course, and are not to be referred directly to any lesion of the nervous system appreciable during life, or discoverable on *post-mortem* examination. Such symptoms are to be met with in persons who have long taken ardent spirits in excess." (p. 18.)

It is true that this definition is far from being complete; but to render it more clear, Dr. Huss adds the following history of a case, premising that it is purposely so arranged as to embrace as many as possible of the symptoms:

"A man, aged about forty, has for the last ten or twelve years been addicted to dram-drinking, sometimes to such an extent as to produce intoxication, but more generally he has consumed from six to eight glasses of raw spirits daily, without being seriously affected by this quantity. For the first six or eight years, after giving way to this pernicious custom, his health was in no ways impaired, but for the last four years a change has taken place in this respect, and he has never perfectly recovered since he had an attack of delirium tremens. When the last-named disorder had subsided, he returned to his evil courses, and led a life more irregular than heretofore, for, inasmuch as he had formerly continued to take his ordinary meals, he now rarely consumed any solid food, save when he took a morsel or so along with each dram of raw spirits that he swallowed. Dyspeptic symptoms now appeared, and soon after he remarked that his hands trembled much, especially in the mornings, but subsequently these tremors continued throughout the day, and were increased by exertion, while his bodily powers were much diminished. Nothing he found so effectually overcame this nervous debility (nerfsvaghet) as an additional glass of brandy. He next complained of a peculiar sensation, as if a veil were suddenly passed before his eyes; this occurred chiefly in the morning, though he experienced it likewise during the day, if he strained his eyes by looking fixedly at any object. He now found that his speech was becoming indistinct during the early part of the day, from a peculiar tremulous motion of the tongue. His sleep then became broken by frightful dreams, and often before falling asleep, he experienced a creeping sensation beneath the skin of the feet and legs, with spasms and cramps in the gastrocnemii and other muscles. Gradually these 'formications' were felt also during the day, becoming more urgent and painful, and causing an indescribable restlessness in the patient, shooting upwards to the nates, and then extending to the hands and arms. The patient often complained that he felt as if small ants or other insects were making their way beneath the skin. The tremors now increased in the hands and arms, and he shuffled in his gait, especially when he first attempted to move forwards. The muscular power of the extremities was much diminished; the want of it was most felt when any unusual exertion was required. The knees frequently gave

way, and when the patient attempted to grasp any object, it slipped from between his fingers (slapphandhet); gradually the weakness of the limbs became more apparent, and it extended upwards to the muscles of the trunk, so that at length he could neither stand nor sit, but was obliged to remain constantly in the recumbent position. While this was taking place, the sensibility of the skin, hitherto unaltered, began likewise to diminish, first in the points of the fingers and of the toes, and subsequently over the back of the hands and feet, to the forearm and leg. Ultimately, the toes and the legs became insensible, but sensation reappeared below the knee-joint. The same occurred in regard to the fingers and forearm, but less completely, and at a later period of the malady. Along with these symptoms the patient had now more or less of vertigo, sometimes merely to the extent of a sudden and transitory darkness before the eyes, but at other times he would immediately have fallen to the ground if he had not laid hold of some object near him. Hallucinations of various kinds now came on, particularly in the evening, and before falling asleep, and these fantasies often banished sleep altogether. They varied much in character, but often the patient imagined that he saw persons or objects around him, and he occasionally heard voices and laughter. The pupils during this time were dilated and less sensible to light than usual.

"After appropriate treatment, and abstaining from spirituous liquors, his health was in great part restored, and he continued comparatively well, *as long as he observed a regular course of life, and took his ordinary meals.* After a time, however, his resolution failed, he relapsed into his bad habits, and his symptoms speedily returned. The digestive functions were now more affected than on the former occasion, there was frequent vomiting of tough acid mucus, with a sense of weight and distention at the epigastrium after taking food. He became somewhat emaciated, and his skin assumed a dirty-yellowish hue. Soon the formications and muscular debility returned, not alone, but accompanied with painful cramps and startings in the feet and calves of the legs. These startings resembled those produced by electric shocks; at times they were only momentary, but often were more prolonged. Soon they extended to other parts of the body, and became more like convulsions, under the influence of which the patient occasionally became insensible. Gradually these convulsions increased in severity till they formed complete epileptic seizures, of daily occurrence, followed often by delirium and hallucinations. Vision now became imperfect, the letters of a book, when the patient attempted to read, seemed to run together into an illegible mass. The powers of thought and of memory were likewise notably diminished.

"Some improvement again took place under medical treatment, but it was only for a short time, and a new symptom now showed itself in the shape of pains in the legs, which were most severe in the afternoon, but which at times, especially in the mornings, entirely ceased. Even when these pains were least severe, the patient still experienced a certain degree of restlessness in the legs, so that he continually moved them up and down in the bed. When the pains were at their height, he described them as if the flesh were burned or cut with knives. The strength daily declined, diarrhœa came on, the skin assumed the appearance of parchment, he was fearfully emaciated, and lay in a condition of low muttering delirium, till death at length closed the scene." (p. 21.)

We make no apology for this long extract, since it presents so complete a *résumé* of the symptoms, which a much longer description on our part might have only rendered confused. We shall not attempt to follow our author in the careful and minute analysis of the individual symptoms above described, nor shall we more than allude to the classification of the varieties of the malady, as this part of the subject had received some notice in our former review. Above fifty pages of the present volume are devoted to the description of the six varieties of alcoholismus chronicus. 1. The

preliminary. 2. The paralytic. 3. The anæsthetic. 4. The hyperæsthetic. 5. The convulsive. 6. The epileptic. Nor need we be detained with the very complete history of the *post-mortem* appearances given in the fifth chapter; for in this malady, as in most nervous disorders, we find little or no appreciable alterations of the nervous structure. It is our wish, in this second notice, to consider the arguments advanced by Dr. Huss in favour of his theory of the disease, and to see how far he has answered, or rather anticipated, the objections raised against it by ourselves and others. It is therefore to the questions of the differential diagnosis, and of the pathology and nature of the malady, that we shall now direct our attention.

In the former part of his work, Dr. Huss has given at great length the history of several cases, whose symptoms closely resembled those of alcoholismus chronicus, yet they were the effects of other poisons, often of metallic poisons, or were produced by diseases of various kinds. Among these, poisoning by the salts of lead occupies an important position, on account of the great similarity between many of its symptoms and those of alcoholismus chronicus. The distinctive differences between the two disorders are laid down at great length by our author.

"It is of importance in the first place to learn the previous history of the patient, to ascertain whether he has been habitually intemperate, or whether he has worked at the preparations of lead. In lead poisoning there is the very characteristic appearance of the blue circle upon the gums, and likewise the attacks of colic, which, with but few exceptions, precede the other symptoms, and are never met with in alcoholismus chronicus. In patients suffering under lead poisoning, the breath has a peculiar and most nauseous smell, while the skin is of a dusky-grey colour, easily distinguishable from the yellow hue of habitual spirit-drinkers. In lead poisoning we do not meet with the symptoms of chronic gastro-enteritis, or of hepatic affections, such as constantly accompany alcoholismus, while the latter is frequently preceded by one or more attacks of delirium tremens. The chemical examination of the excretions, or of the blood, will often aid us in doubtful cases, as where individuals have been simultaneously exposed to the causes of both disorders; for lead may frequently be detected in these during life, as well as in the various tissues and organs after death. I acknowledge, however, that it is not possible to draw the line of distinction between paresis and anæsthesia saturnina, and paresis and anæsthesia alcoholica, or between the epileptic seizures and convulsions, which may occur in both disorders." (p. 139.)

It is not easy to mistake a case of poisoning by arsenic in large doses for one of poisoning by alcohol; but the sequelæ of the former, if the patient should recover, bear no small resemblance to the symptoms of the latter disease. There is more difficulty in chronic cases of arsenical poisoning. The tremors observed in slow poisoning by this mineral are not relieved (?) by a dose of alcohol, nor are they more severe in the mornings than at other times, and they are generally accompanied with spinal pain and irritation. But perhaps the most marked distinction is to be found in the mental condition of the patient. In the more advanced stages of alcoholismus chronicus, the mind seldom or never remains unimpaired; vertigo, tinnitus aurium, muscæ volitantes, and dilated pupils, with hallucinations and delirium, are constantly present; and these symptoms are rarely, if ever, seen in arsenical poisoning.

"It has been stated," observes Dr. Huss, "that the retailers of spirits (*Krogsvärdar*) are in the habit of adding arsenious acid to the brandy they dispense, in

order to make the liquor stronger; that is, sharper and more intoxicating. We can only answer that it is neither probable, nor yet confirmed in any way, that such admixture is of common occurrence, nay, that it is ever attempted." (p. 143.)

The admixture in question seems to us simply absurd; for allowing for a moment that arsenious acid has any taste at all, the quantity required to impart any flavour or "biting property" to the brandy, would ensure the poisonous effects of this mineral being speedily developed.

The occurrence of various nervous symptoms among gilders, and especially among those employed in the process termed water-gilding, has been well described by Merat and by Dr. Watson. The mercurial fumes arising in the above-named process, rapidly produce their deleterious effects. Mercurial tremors, however, are generally preceded by the specific action of this metal upon the gums, the teeth are often loosened in their sockets, and salivation occurs; but the mental faculties remain intact, and vertigo, scotomata, and hallucinations are very seldom observed. In chronic alcoholismus, loss of power of the limbs, and tremors, appear before any pain is felt in the affected members: in mercurial poisoning, pain is one of the earliest symptoms. Dr. Huss tells us that a glass of spirits has no beneficial effect, even temporarily, upon mercurial trembling; but the case of Chattin, detailed by Dr. Watson in his thirty-eighth lecture, would seem to contradict this assertion. If the complaint consists in loss or diminution of nervous power, we see no reason why the energies might not be temporarily restored in the one case as well as in the other. The tremors produced by mercury are, it is stated, much more severe and obstinate than those which result from alcohol.

Ploucquet* maintained that all the symptoms observed in habitual spirit-drinkers, were to be referred to the presence of salts of copper, and especially of the acetate, in the spirit; the poisoning being derived from the untinned copper vessels in which the liquor was preserved. Such vessels are, however, rarely, if at all, now used; so that the dangers so feelingly described by Sir George Baker and others, no longer exist. Still, with the increased consumption of brandy in Sweden, the symptoms of chronic poisoning by alcohol have kept pace. The course of chronic poisoning by the salts of copper has not been very accurately described; but we do not find that those who have written on this subject, with the exception perhaps of Ploucquet, have included paralysis and tremors among their lists of symptoms. Diarrhœa and colic are early and prominent symptoms in the colics of Madrid and Poitou, as well as in the dry belly-ache of the West Indies, described by Quier; but in all these, the intellect remains almost unimpaired to the last. In chronic poisoning by the fumes of phosphorus, formications, tremors, and muscular debility occur, but cerebral symptoms are rarely observed. Workers in lucifer-match manufactories are also liable to a peculiar form of caries of the jaws.

Dr. Huss confesses that in chronic poisoning by secale cornutum, the symptoms are sometimes so closely allied to those of alcoholismus, that they cannot easily be distinguished. An example of this kind is given in Case 43 of the former part of the work before us. Our author, however, correctly shows that concomitant circumstances render the diagnosis much less difficult. In poisoning by secale, the symptoms are much more

* Warnung an das Publikum vor einem in mehreren Branntweinen verborgenen gift.

rapidly developed; they commence with severe pain of the stomach, and are accompanied with colic and jerking and convulsive movements of the limbs, followed by paralysis and anæsthesia, though the brain is generally unaffected, the skin does not assume the characteristic yellowish hue, and the digestive functions after a time, if the patient recovers, resume their former vigour. Women are more frequently affected by ergotism than men, and the poisonous effects of secale are generally manifested in many members of one family or community at one and the same time, and persons of all ages and conditions may be affected, if they have partaken of the deleterious food.

Besides the comparatively well-known effects of certain vegetable and mineral poisons upon the system, our author remarks that certain stages and conditions of various chronic disorders occasionally present a striking resemblance to the phenomena of alcoholismus chronicus. This is particularly the case in the disease, described chiefly by French physicians, and first by Calmeil, under the name of "Paralysie generâle des aliénés." When Dr. Huss first began to study the course of alcoholismus chronicus, he was some time in doubt whether the field of investigation had not been pre-occupied, under another name, by Prus, by Bayle, and by other French writers. All these authors, however, not excepting even Brierre de Boismont, agree that the disorder just referred to does not occur except in conjunction with mental disease, and usually at a far advanced period of insanity. It is, however, well known that many of those who labour under the various forms of mania, have been in early life much addicted to the use of ardent spirits, to which indeed the loss of the mental faculties may be in part attributed. For brevity's sake, we must refer our readers again to the description of an ordinary case of alcoholismus chronicus given at the commencement of this notice. They will there find that, for some time at least, and often for years, the symptoms recorded will abate and entirely disappear, if the patient can be induced to give up the habit of spirit-drinking. Such is not the case in the general progressive paralysis of the insane, and in this latter disease tremors do not appear so early, while faltering of the speech, with retraction of the corners of the mouth, and a vacant expression of the countenance, are some of the first phenomena observed. If, as sometimes happens, tremors do occur in the commencement of the disease, they are not confined to the hands and arms, but prevail over the whole body. The pupils are rarely dilated, nor is the sensibility to light diminished. Nor are the symptoms most urgent in the mornings, and the characteristic creeping sensations beneath the skin are not complained of; and, lastly, a dose of alcohol affords no relief.

General progressive paralysis shows itself first in the lower extremities, but without cramps or startings in the legs and feet; while sleep may continue undisturbed and digestion unimpaired for a long period—nay, the appetite may be morbidly increased. In alcoholismus chronicus, sensibility is first diminished in the points of the fingers and toes, and extends from thence upwards towards the trunk; but in general progressive paralysis it often shows itself over the whole body at once, as is specially remarked by Lunier and Göricke. Mental derangement, too, often commences by "monomanie ambitieuse," while in alcoholic poisoning nothing of this kind is seen; there are hallucinations certainly, and, towards the end, muttering

delirium, but there rarely is, except under the influence of delirium tremens, any approach to maniacal excitement. This our author regards as a special point in forming a correct diagnosis.

In tabes dorsalis, in spinal irritation, and in some forms of disease of the heart and great vessels, we occasionally meet with symptoms which may leave us in doubt as to their true nature, especially when patients labouring under these maladies have been for a long time of intemperate habits.

We next arrive at the important chapter devoted to the Etiology of this disorder.

Almost all the cases that our author has observed have been the result of drinking the impure brandy that is distilled in Sweden from potatoes. Beer, ale, wine, and porter, are but little used by the working-classes in that country.

The various causes are considered under the two heads of predisposing and direct.

Among the former, Dr. Huss enumerates the climate of Sweden, the modes of life, and the condition of the dwellings of the Swedish peasantry, with the age, sex, and temperament of the patients. And first, with regard to climate. The northern nations have long been notorious (we do not hesitate to employ the word) for their addiction to ardent spirits. It has been asserted that this baneful habit arises partly from the severity of the climate, and partly also from the inhabitants of these regions being less civilized than the nations of the south of Europe. The latter assertion our author, as a true patriot, indignantly repudiates, and we can, from personal observation, corroborate the justness of his opinion. The severe weather to which the Swedish labouring classes are exposed, and the indigestible food which forms their daily nourishment, are, in his opinion, the causes why the Swedish workman considers ardent spirits as a necessary of life, without which he would be unequal to his daily task. This has now unfortunately become a fixed belief among the common people, and the daily use of brandy has become so general, that it can scarcely be repressed. From the constant use of so dangerous a stimulant, springs naturally the abuse of alcoholic fluids.

As to the influence of the seasons, the majority of the observed cases occurred in the months of June, July, August, September, and October; but our author does not regard the months above named as influencing the frequency of the disorder, except that it is precisely during summer that the Swedish labourer receives the highest wages, and is consequently enabled to spend more of his earnings in brandy. After all, however, the difference of season is not very remarkable, for in the year 1848, 32 patients were treated in the Seraphimer Lazarett for chronic alcoholismus, and 16 of the 32 cases occurred during the above-named five months. Twenty-nine cases were observed during the five summer months of 1849, out of a total of 61; and in 1850, there were 46 patients in all, 22 of whom came in during the period above referred to.

The larger towns, and particularly Stockholm, present the great majority of cases. It is possible, too, that the unhealthy dwellings of the poorer classes accelerate the outbreak of the malady; for many persons live closely packed together in a single room, from which, during winter, the external

air is carefully shut out; but Dr. Huss has seen many individuals affected with the disorder, who had enjoyed the advantages of good air and of a nourishing diet, and whose dwellings were comparatively roomy and clean. Our author questions the hereditary nature of the complaint; bad example, he observes, and not hereditary predisposition, renders the son of an intemperate father a drunkard.

During the years 1848, 1849, 1850, 139 persons labouring under various forms of alcoholismus chronicus were treated at the Seraphimer Lazarett. Of this number, 14 were between 20 and 30, 44 between 30 and 40, 57 between 40 and 50, 23 between 50 and 60, and one was 65 years of age. It appears, then, that the greatest liability to the disorder exists between the thirtieth and the sixtieth years of life; but it may be observed that few habitual drunkards survive the latter period.

Of the above 139 patients, 123 were males, and 16 were females. Our author believes that women usually begin to drink at a later period of their lives than men.

The table given below, of the respective occupations of the patients, presents some interesting facts.

Mechanics (chiefly journeymen)	29
Servants and day-labourers	23
Non-commissioned officers and soldiers	16
Persons in reduced circumstances, but formerly belonging to the better classes of society	13
Inspectors and toll-keepers, particularly in employment of custom-house	12
Sailors	8
Porters of iron (*Jernbäräre*)	6
Distillers and spirit-retailers	6
Divers trades	8

Of the 16 females, 6 were widows, 5 were married, and 5 unmarried; and 6 of the whole were employed in retail spirit shops.

Mode of Life.—(Lefnadsätt.) "Habits or customs would be perhaps the more appropriate word."—The habitual use of ardent spirits, and especially of brandy distilled from potatoes and drunk unrefined, is well known to be one of the most frequent vices of the Swedish capital. We must not forget that Dr. Huss carefully distinguishes between the use and the abuse of alcohol —between spirits taken in moderation and spirits drunk to an excess. We should regard the statements here recorded, of the quantity of spirits consumed daily by a moderate dram-drinker in Stockholm, as incredible, had we ourselves not made inquiries on this subject in the large manufacturing town in which we write. A few years ago, we conversed upon this point with a man of high intellectual attainments, and of great mental powers, but who had unhappily abandoned himself to this pernicious custom. He stated that his ordinary consumption was from 20 to 25 glasses of raw spirits daily, but that he occasionally exceeded 30 glasses in the twenty-four hours, and rarely, even with the largest quantity, became intoxicated! He had persevered in this course of life for many years, but like many of those who long bear up against the effects of the poison, he had always been a very early riser, both in winter and in summer. At length, when past the age of fifty, he was carried off by an epileptic seizure, followed by symptoms of delirium tremens.

The following description by Dr. Huss of the intemperate habits of a so-called moderate dram-drinker among the working-classes of Stockholm, will not appear so difficult of belief, after the example just quoted.

"A man of this class rises at five or six in the morning, according to the season of the year, and swallows, before going out, a cup of coffee with a glass (℥ii. to ℥iii.) of brandy in it. He returns at eight to breakfast, which meal is washed down with another glass of his favourite spirit. At dinner he repeats the dose of brandy, and often adds another half-glass. About five or six P.M., when his work is finished, another glass is swallowed; and supper at eight is concluded by a similar libation. During the day, therefore, he consumes from five to six glasses of brandy, or from ten to fifteen ounces of spirit. Such a mode of life is far from being regarded as intemperate; on the contrary, many of the working-classes, and especially the iron-porters (*Jernbäräre*), regard it as a necessary accompaniment of their laborious occupation. Those who exceed the above-named quantity are by no means looked upon as drunkards; of such men their fellow-workmen would say, 'They drink freely, but not more than they can bear'! On Saturdays and Sundays more spirits are taken at once, and these men are then often intoxicated." (p. 174.)

Dr. Huss corroborates the observation we have ourselves made, that many habitual dram-drinkers will consume from 12 to 15 glasses of brandy or other spirit daily; and he has known some who drank from 16 to 20 glasses, or the enormous quantity of from 40 to 50 ounces of raw spirits, every day!

Our author next pursues his investigations into the influence of this large quantity of alcohol upon the bodily functions; he seeks to ascertain how long such a course may be persevered in with impunity, and also whether the symptoms that sooner or later appear, are to be ascribed solely to the excess of ardent spirits, or may arise from the admixture of foreign substances with the brandy distilled in Sweden. To elucidate these questions, Dr. Huss instituted a series of experiments on animals, in conjunction with Professor Dahlstrom, of the Veterinary School at Stockholm. During eight months, they administered daily to three dogs of various ages, but of nearly equal size, six ounces of Swedish brandy. To one the purified brandy was given, but to the other two the spirit was given unrefined, and consequently in the state in which it is generally consumed by the working-classes in Stockholm. This difference in the quality of the spirit produced no diversity in the symptoms exhibited by the three animals. Intoxication and intense thirst were occasioned by each dose during the first three months; but the dogs continued fat, and apparently well. In the fourth month, the bark of the animals became hoarse; they had a dry cough; the eyes were staring, and full of tears; hearing was much diminished; and the animals' sleep became restless, with frequent jerkings of the limbs. After the completion of the fourth month, the dogs trembled when they attempted to stand, their walk was shuffling, and there was evident weakness of the extremities, especially in the hind legs, so that they often remained in a sitting posture when taking food. Cramps and convulsive movements next appeared in the extremities and in the trunk, both during sleep, and when the animals were awake and lying on their sides. The sight of other dogs, however, roused them at all times from their apathetic condition, and they endeavoured, even in their weakened state, to attack and bite them. The powers of the animals diminished more and more, as the administration of the alcohol was persevered in; and

the sensibility of the skin, especially that of the ears, was remarkably lessened. The appetite now fell off rapidly; but the irritability towards other dogs continued unabated to the last. No diminution of the deposit of fat beneath the skin was observed; it had even rather increased at the period of death, which in all three occurred about the eighth month.

Although experiments on animals cannot be regarded as positive evidence of the actions of remedies and poisons on the human frame, it must be acknowledged, that the results here detailed are strongly corroborative of Dr. Huss's views. Nor should the fact be overlooked, that the impure and the refined brandy produced similar effects in the dogs, which would hardly have been the case if the disorder had been occasioned by some poisons mingled with the spirit.

"For how long, and in what quantity, must brandy be daily taken to produce the effects of chronic poisoning on the human system?"—This question will, of course, admit only of an approximative reply. In dogs the complete disorder was developed, and ran its course, in from 4 to 8 months. Many of Dr. Huss's patients informed him, that they had consumed large quantities of brandy daily, for 5, 10, or 15 years, and of these a considerable number had been only moderate drinkers, (℥ x. to ℥ xv. daily,) before they became what is thought intemperate among the labouring-classes in Sweden. Our author believes that persons may become to a certain degree hardened against the effects of alcohol, as against the operation of other poisons, and that this probably renders the malady more slow in its development. Temperament, constitution, and age, are also circumstances which modify very considerably the period of the outbreak of the disease. Many persons, observes our author, who have for years daily consumed from twelve to fifteen glasses of brandy, can safely affirm that they have never been thoroughly intoxicated (riktigt full) during the whole period; so that it would seem that the prolonged and daily use of ardent spirits produces the disease more surely than absolute intoxication. If attacks of delirium tremens come on, and succeed each other rapidly, they appear to accelerate the outbreak of alcoholismus chronicus.

Our limited space does not permit us to examine the succeeding question—viz., "Under what conditions and circumstances do the symptoms of chronic poisoning first appear?" We shall merely give the conclusions at which Dr. Huss arrives in this regard:

"It does not, therefore, appear that the habit of dram-drinking is of itself always capable of producing directly the symptoms of chronic alcoholismus, but that other circumstances often contribute thereto, for the affections of the nervous system are almost always preceded by morbid changes in the digestive functions."

That alcohol in large doses weakens the digestive organs, and predisposes to disease, we have no doubt; but still, it seems to us that the action of the poison is more or less direct upon the nervous system.

The fourth question considered in this chapter is of the greatest practical importance; as it is against this portion of our author's theory that the most serious objections have been raised:

"Are the symptoms of this disease produced by alcohol alone, or are they to be referred entirely, or in part, to various foreign matters present in the brandy consumed in Sweden?

"Many writers have asserted that the symptoms of this disorder were unknown,

or nearly so, till brandy was distilled from potatoes, and they would ascribe the disease entirely to the potato oil (Finkeloljan) contained in the liquor thus manufactured. We must not however forget, that as soon as this process became general, the price of brandy was greatly lowered, and it thus became accessible more easily to the lower classes. To determine how far the symptoms may have been produced by the potato oil, careful experiments on animals were again had recourse to, but with only negative results."

In the human subject, a dose of three grains of this oil causes irritation in the stomach and intestinal canal, with colic and diarrhœa. None of the characteristic symptoms, however, appeared; and Dr. Huss has seen several patients labouring under alcoholismus chronicus, who had abstained altogether from potato brandy, and had confined their potations to cognac, rum, and arrack. Dr. Huss, therefore, concludes that it is the alcoholic principle contained in potato brandy, and not the potato oil, which gives rise to the disease.

It has been likewise suggested that the peculiar volatile compound, vulgarly termed "*stick*," which seems to be produced in the distillation of brandy from *diseased* potatoes, may have some connexion with, if it be not the sole cause of, the malady.

In the year 1849, the cases of alcoholismus chronicus in Stockholm were doubled in number; and the brandy which was consumed that year was mostly distilled from diseased potatoes, the sale of which for food was prohibited. It was remarked by many of the retail spirit-sellers, that when certain casks of that year's brandy were first opened, a disagreeable suffocating odour, like that from newly-cut horse-radish, was sensibly perceived; but after the cask had stood open for some time this could no longer be detected. Some brandy distilled in the ordinary way, but from diseased potatoes, by Dr. Hamberg, in the chemical laboratory, exhaled exactly the same odour. In 1850, the brandy consumed in Stockholm did not present the same peculiarity, and there was, in that year, a marked decrease in the number of cases of the disease. Dr. Huss thinks it probable, that the presence of this volatile ingredient may, at all events, have augmented the injurious action of the spirit upon the nervous system, and he allows that it may even directly have occasioned many of the symptoms.

Solanine, the poisonous principle of the potato tribe, has been sought for in the Swedish brandy; but no solanine passes over in the process of distilling brandy from healthy potatoes, though we are not so certain of this when the potatoes are diseased.

The salts of *copper* have not been found in the brandy of Stockholm.

We think that the question of the influence of some unknown product produced in the distillation of brandy from *diseased* potatoes, is a subject which requires further consideration and inquiry. Still, the disease existed before the potato rot was known, it has continued since, and it has occurred in persons who have not confined their potations to potato brandy, or even in such as have not employed it at all.

We next meet with a short chapter on the immediate Causes of the malady. These, as may be expected, are not very obvious; the poison has been gradually accumulating in the system, and often requires no immediate cause to produce the outbreak. Intoxication, delirium tremens, fevers, and intense mental excitement, may be reckoned among these; but perseverance in the abuse of alcohol is undoubtedly the most constant and the most certain.

In reference to those circumstances that tend to bring on one species of the disorder in preference to another, it has been remarked by Dr. A. H. Wistrand, that the soldiers in the garrison at Stockholm are peculiarly liable to be affected by the epileptic variety.

What, then, is the nature of this disorder, and what is the operation of alcohol on the solids and fluids of the body?

Dr. Huss rightly maintains, that the symptoms of alcoholismus chronicus are closely allied to those of chronic poisoning in general. Still, however strong the points of resemblance may be, there are certain differences too manifest to be lightly passed over. The cessation of the symptoms when the patient leaves off dram-drinking, would not be altogether conclusive; but on the one hand, in chronic poisoning by mineral salts, the evil consequences persist long after the deleterious ingredients are avoided; and on the other, we see, that persons who have not drunk the Swedish brandy, in which some specific poison may be suspected to exist, have yet been attacked with the disease, and have soon recovered on leaving off their intemperate habits. In the blood of persons poisoned by arsenic, by mercury, or by lead, these metals have been discovered by analysis: in habitual dram-drinkers, alcohol has been found in the blood, but no traces of metallic poisoning could be detected.

That alcohol is the real and sole cause of the symptoms, is proved, our author thinks, by the following facts. The disorder will often, of itself, and without medical treatment, diminish and disappear, if the patient relinquish his habits of dram-drinking, especially in first attacks. It was this circumstance which first directed Dr. Huss's attention to the disease, and showed to him its probable nature and cause. That the varieties of the malady are to be referred to one common cause, is proved by many patients having been affected, at different periods of their lives, with all the six forms enumerated by Dr. Huss. At the time that our author sent this second part of his work to the press, the review of the former part in this periodical had not reached Stockholm; but by anticipation he has answered most of the objections which were there raised against his theory. On this subject, however, we shall allow Dr. Huss to speak for himself.

"While engaged in preparing the accounts of the cases which I published in the first part of this work, I had constantly before me the suspicion, that the symptoms there described might arise from other causes than from alcoholic poison, and that I was possibly confounding together several widely-different affections of the nervous system, under the delusion, that because they existed in dram-drinkers, they arose from that habit alone. But the more that I investigated the cases that came before me, the more was I convinced that the nervous affections of dram-drinkers formed of themselves a special and distinct disorder, having indeed in many points a close resemblance to other maladies, and especially to those occasioned by chronic poisoning, but yet differing so far that a distinct line of demarcation can be fairly drawn. At first I suspected that what I observed arose from the admixture of various metallic salts with the brandy of Stockholm, this taking place either during the process of distillation, or being added thereto at a subsequent period; but on analysing various specimens of brandy procured at different periods of the year, and from various retail dealers, *no traces of copper, lead, arsenic, or any other metallic poisons, were discovered.* My suspicions next fell on the potato oil (*Finkeloljan*); but here, as before stated, I found myself in error, and as a last resource I directed my attention to the volatile

compound, vulgarly called 'stick,' from its suffocating odour. And my researches here led me to the conclusion, that this compound may possibly increase the severity of the symptoms, and even render the disease more frequent, but that as it rarely occurs, it can hardly be regarded as the sole cause. I could not discover solanine or any narcotic alkaloid in the brandy that was examined. Then it struck me that the food of the lower classes in Stockholm contained the deleterious ingredient I was in search of, such as the secale cornutum, lolium tremulentum, raphanus raphanistrum, &c. But had such been the case the disorder would have been more widely distributed, and would probably have prevailed more among the temperate than the intemperate, for the drunkard is notoriously a small consumer of bread and vegetable food. Was it possible, then, that brandy distilled from corn, mixed with any of the above-named vegetable poisons, might produce these symptoms? But the brandy consumed in Stockholm is mostly brought from the southern provinces, where it is distilled almost exclusively from potatoes. Foiled in my researches, I next commenced the experiments on animals detailed above, and these confirmed my ideas of the true cause of the malady. I have no doubt that my views will be called in question by many of my continental brethren, who may not have remarked this disorder in the course of their professional experience; but I trust they will first make themselves acquainted with the symptoms here described, and then consider if some of the nervous affections they have observed in dram-drinkers, do not come under the category of this disease.* It is quite possible that these symptoms are more frequent in our northern clime than elsewhere, and that this may in part arise from the sanitary condition of our labouring-classes; but I am fully prepared to maintain, that the chronic poisoning I have described, has its origin in the abuse of alcoholic fluids." (p. 198.)

The operation of excessive and prolonged doses of alcohol upon the system has been often described, and has formed one great argument on the side of the total abstainers from all spirituous beverages. That spirits, habitually taken, produce injury to the system, few, we think, at the present day, will deny. We believe, also, that alcohol thus consumed, creates a state of chronic inflammation of the mucous membrane of the stomach and duodenum, and that it interferes with the first processes of digestion, either directly or indirectly, by its debilitating effects on the

* We are glad to corroborate, from our own observation, the correctness of Dr. Huss's opinions regarding this malady. We are convinced from what we have already seen, since we first became acquainted with the disorder when reviewing the former part of his work, that Alcoholismus Chronicus will be found to prevail in this country, not perhaps to the extent that it occurs in Sweden, but in proportion to the amount of intemperance among our labouring-classes. We do not think that dram-drinking is carried in Great Britain to the extent that prevails in Sweden. The high price of food, and of ardent spirits, prevents that excessive indulgence in this pernicious habit, which would otherwise occur; but we suspect that if due investigation were made in our large manufacturing towns, and especially in Glasgow and Edinburgh, and other towns in the northern parts of this island, the symptoms of this disorder would soon be detected there. Since our attention was directed to this disease, we have met with some cases in which several of its symptoms were more or less developed; but it was not till within the last two months that we fell in with a well-marked and decided case of alcoholismus chronicus. The patient was a female of the middle classes, and in easy circumstances, who had been for several years—but we could not ascertain how long—an habitual, but secret spirit-drinker. The medical attendant was not a little puzzled with the peculiarity of the symptoms. When we first saw this individual, the disease was far advanced. The sleep was much disturbed; there was frequent muttering delirium, but when spoken to, she answered correctly; the eyes were red and suffused; the tongue dry; pulse 100 or more; and the appetite was almost entirely gone. The patient had first noticed a difficulty in holding her needle or in making use of it (*slapphandhet*), and she gradually lost the power of both hands and of the forearm (paresis); subsequently the feet became affected in the same way, commencing at the toes, and here there was well-marked loss of sensibility (anæsthesia), as far nearly as the knee, above which joint, in both legs, there was intense sensibility of the skin and subjacent tissues (paræsthesia), so that the patient screamed out when the thighs were even lightly touched. There was a good deal of general tremor about this person, but the case was then far advanced, and she died about a fortnight after we saw her.

nervous system. The liver becomes incapable of fulfilling its office of cleansing the blood, which is brought to that organ overloaded with oily particles and other products of carbon. This fat is partly deposited in the liver itself, and partly carried forward into the general current of the circulation, to accumulate in other organs and tissues. As the irritation of the liver is prolonged, the organ becomes altered in structure, till serious organic disease perhaps is produced. To all these points, as well as to the injurious action of alcohol on the kidney, Dr. Huss directs our attention; but the subject has been so fully handled by Dr. Carpenter and by others, that we may omit this portion of our author's observations; and shall proceed at once to his researches on the operation of alcohol on the blood and its component parts.

"The blood of dram-drinkers is impregnated with a foreign substance (alcohol). Alcohol passes directly from the stomach and intestines into the blood; a part of the spirit is perhaps then decomposed, but another portion is carried directly by the portal system through the liver, and from thence to the lungs, where much is exhaled with the air that is breathed, but the remainder passes into the arterial system, and from thence over the whole body.

"As long as alcohol remains in the blood, a certain degree of poisoning of that fluid takes place, which exhibits itself either by direct intoxication, or, in more gradual doses, by certain lesions of the nervous system. But as the poison in the blood is of a volatile nature, it is decomposed in its passage through the various tissues, and its component parts are separated from each other to form new combinations, or else they are more rapidly carried off by the excretions. It therefore follows, that to produce the symptoms of poisoning, a constant supply of the pernicious fluid should be kept up, so that the blood should always contain more or less of the poison. Now, it is precisely in those individuals who pursue this course as dram-drinkers, that the disease is constantly developed, while those who occasionally take spirits to great excess, but at other times abstain altogether, are usually exempt. I have, therefore, throughout this essay, always insisted on the necessity of a constant use of ardent spirits to produce the nervous symptoms I have described. It is not possible, however, to determine the actual quantity of the poison that must be consumed, ere the disease appears; as this depends upon so many extraneous circumstances." (p. 203.)

The constituents of the blood, Dr. Huss informs us, are materially altered under these conditions, as regards the relative proportion of the component parts. In all cases of the malady, our author has observed that the blood contains a much larger proportion of oily and fatty particles than in health; and these were visible to the naked eye, both in the arterial and in the venous system. In fine, all authors are agreed, that the blood of dram-drinkers contains a much larger proportion of carbon than that of healthy individuals. Scharlau has estimated the excess of carbon in the blood of drunkards to be not less than 30 per cent. In corroboration of these statements, Dr. Huss refers to the experiments and researches of Bouchardat and Sandras, as also to the investigations of Böcker upon the relative proportion of the blood-corpuscules under such conditions; and comes to the conclusion, that both the arterial and venous blood of dram-drinkers is loaded with fat, that it is impregnated likewise with alcohol, that the solid constituents in defibrinated blood are diminished, as is likewise the proportion of the blood-globules.

"This result, however, holds good only for a certain period of the dram-drinker's life—viz., as long as the nutritive functions continue uninjured. During this time

the patient remains stout and plump, but as the digestive powers begin to fail, and the appetite diminishes while the craving for brandy is increased, the fat deposited in the cellular tissue is re-absorbed, and emaciation begins. As this progresses, the patient becomes generally cachectic, the serum is augmented, the proportion of the blood-globules is diminished, and the blood when drawn shows less and less disposition to consolidate. Still, even in such cases, I have detected the oil-globules in the blood of both sides of the heart." (p. 206.)

Much has been written regarding the influence, direct and indirect, of ardent spirits upon the brain and spinal cord. Alcohol, in large doses, is well known to produce an exciting effect at first, which is afterwards followed by depression.

"The progress of chronic poisoning, by ardent spirits, is in exact accordance with what is observed in acute cases, but is kept up at a lower degree by repeated fresh doses of the poison. As acute poisoning, by ardent spirits, is produced solely by the alcohol in the blood, the same process necessarily occurs in the chronic forms; but another alteration of the blood takes place under the latter condition, which is of great importance in the consideration of our subject. It has been shown above, that the venous blood of drunkards contains so much carbon, that it cannot be adequately arterialized in the lungs, and thus the arterial blood is loaded, more or less, with the peculiar constituents of the venous. It is these two circumstances which eminently conduce to the outbreak of the disease. The constant irritation of the brain by alcohol, is followed, sooner or later, by depression of its energies, which the imperfectly arterialized blood is unable to restore, and both thus conduce to one end—the lowering of the vital powers of the brain and nervous system. Another important point is, that ardent spirits occasion, for the time, more energetic contractions of the heart, by which the blood is driven with greater force towards the brain, and produces constant congestion of that organ, as is evidenced by the dilated condition of the capillaries we meet with after death. Lastly, the symptoms of chronic poisoning usually commence when the patient loses his desire for food, and, omitting his regular meals, lives almost entirely upon brandy. It is probable that a portion of alcohol is decomposed by the food that is taken with it; but when the latter is omitted, and the patient gives himself up to dram-drinking without eating, the morbid effects of the poison much more rapidly supervene." (p. 207.)

Excellent and comprehensive as is the whole of this essay, Dr. Huss candidly acknowledges that many points regarding the malady require further investigation.

"Much yet remains to be cleared up, as for instance, whether it is the alcohol in the blood, or the changes in its component parts, which produce the symptoms; how it happens that the loss of nervous power shows itself in the extremities, when the brain is the part primarily affected; whether it is the cerebrum, or the cerebellum, or both together, that suffer under the poison," &c. &c. (p. 208.)

The indications for the treatment of this remarkable disorder are, we think, pretty obvious. Our first care, observes Dr. Huss, must be to remove the cause, as far as possible, by cutting off the supply of ardent spirits. Secondly, attention should be directed to restoring the tone of the digestive organs; and, thirdly, to repair the lost energies of the nervous system.

It might be dangerous to withdraw the whole of the stimulus at once, but its place can be gradually supplied by bitter tinctures, which likewise act beneficially on the organs of digestion. In restoring the lost energies of the nervous system, Dr. Huss has tried with success the potato oil (*Finkeloljan*), of which so much has been said above; but he has found

this remedy to lose its power in proportion as the disease advanced. Opium has answered well in the forms of the malady characterized by increased sensibility, and also where formications and cramps have been predominant symptoms. Camphor, especially in large doses, was most useful in subduing the intense irritability and restlessness of the patient, and where hallucinations and muttering delirium have prevailed; while nux vomica, in the form of strychnia, or the Faba St. Ignatii, was the principal remedy where paralysis and anæsthesia existed. Phosphorus and the preparations of iron helped to accelerate the convalescence.

We have been thus brief in our analysis of the mode of treatment adopted, because we are sensible that the employment of remedies must depend upon the features of each individual case, and that no rules can be laid down for the management of this disease, so efficacious as a thorough acquaintance with its nature and its causes. Our object has been to describe the course of the malady, and to give, as far as lies in our power, the theories and the deductions of the author in a distinct and comprehensible form.

Whatever subsequent investigations may reveal, it is to Dr. Huss we owe the first clear and well-digested description of a disease, which we ourselves are almost convinced has been hitherto overlooked by the medical world, but which we suspect will be found to prevail in all countries and nations where dram-drinking is a habit and custom. As a scientific essay, Dr. Huss's book is far above the ordinary standard; it cannot fail to raise the author still higher in the estimation of his medical brethren; while, by pointing out the terrible consequences of indulgence in ardent spirits, he has rendered an important service to society at large.

Art. IV.

Mémoires de la Société de Chirurgie de Paris. Tome Deuxième. 4to, pp. 639.—*Paris*, 1851.

In our former analysis of the Transactions of the Parisian Society of Surgery, we noticed a portion of the contents of this volume, and now proceed to dispose of the remaining papers, some of which possess considerable interest.

I. *Case of Extirpation of a Scirrhous Parotid Gland.* By M. Monod.

A swelling, increasing in size, and accompanied by lancinating pains, had manifested itself, during a twelvemonth, in the parotid region, in the person of a clergyman, æt. 50, of robust habit, and in good health. A hard, insensible tumour, having the skin, in a very vascular condition, adhering to it, was considered, in consultation, to be scirrhous in its nature, and to call for extirpation. The operation proved one of considerable difficulty, owing to the strength of the deep-seated adhesions, which had to be separated by the fingers. M. Monod approves of the rule that has been laid down in these cases, of detaching the tumour from its adhesions anteriorly and posteriorly, and then attacking its deep-seated portion from below upwards, always directing the knife towards the tumour. The preliminary ligature of the carotid is thus rendered quite needless, and if any considerable artery is opened, the bleeding may always be arrested by

pressure on the carotid, and the operation finished before the vessel is tied. In the present case, a good deal of bleeding took place from the surfaces of the incisions both during and after the operation, but only one vessel had to be tied. Owing to the great depth and irregular form of the wound, it was deemed proper not to attempt union by the first intention. By the fortieth day it had become quite superficial, so that the patient could return to the country. He continued to suffer considerable pain in mastication, however, for about a month afterwards.

The tumour removed was about the size of an egg, hard and lobulated, and exhibited, on incision, the primary stage of scirrhus—the gland in several parts being unaltered. M. Monod observes that the question may here be asked, as in other similar cases, whether the *parotid itself has been actually extirpated?* In reply to those who maintain the impossibility of performing the operation without doing injury to the carotid, which lies embedded in the gland, he refers to the observations of MM. Nelaton and Denonvilliers, which prove that, as a general rule, the artery lies merely in contact with the gland, but not enveloped by it—the gland, in most cases, presenting on its deep surface a furrow in which the artery is lodged, this furrow being converted into a sheath by loose cellular tissue connecting its edges. It is possible, too, for the changed form of the gland to efface the furrow, and thus completely to separate the artery from it. These considerations, then, lead to the belief that, in the majority of cases, the parotid may, with suitable precautions, be extirpated, without any injury being done to the carotid. But, in the present case, there was also ample proof that the tumour removed really was the parotid itself, and not an enlarged gland which had pushed it backwards. Those who were present, and saw and felt the vast chasm exposed, leaving only a thin layer of tissue in front of the spinal column, and exhibiting all the muscular structures, as if for a demonstration, could entertain no doubt about the matter. A small portion of the gland around the meatus auditorius, was, in fact, all that was left of it. The adhesions, too, were so strong, as to require a very different amount of force for their detachment, from that necessary for the enucleation of a diseased gland. A no less strong proof was derived from the effects of the division of the facial nerve which passes through the parotid. Prior to the operation, the patient suffered from paralysis of the lower portion of the cheek; but immediately after it every part supplied by the facial became paralyzed. The branches of the nerve were found embedded in the substance of the tumour.

II. *On the Treatment of Unconsolidated Fractures by Acupuncture.* By M. Lenoir.

The case which gave rise to this paper was one of ununited fracture of the femur, which came under M. Lenoir's care four months after the accident, and in which, after ineffectually trying to manage it during two months by maintaining the ends of the bone in exact and immovable contact, he resolved to resort to acupuncture, which Wiessel had already advantageously employed in 1844. Four needles were introduced between the ends of the bones, seven months after the accident, and retained *in situ* for six days, by which time they had excited pain and other symptoms of

inflammation. They were re-introduced during other five days at another point; exact adaptation of the fragments and immovability of the limb being sedulously maintained by suitable apparatus. The limb was examined on the twenty-third day, but as the fractured part yielded somewhat under the hand, the splints were re-applied, and the limb re-examined thirty-five days later. Perfect solidity was now obtained; and six months after the operation had been undertaken, the patient was able to leave the hospital, resuming afterwards the ordinary labours of the peasant.

M. Lenoir observes that there is nothing more difficult in practice, than the exactly distinguishing between what should be called *unconsolidated fracture*, and *false joint from deficient callus*. Doubtless, the one condition is often the consequence of the other; but, it may be asked, where does the first terminate, and the other commence? An important question, since the treatment proper for mere delayed consolidation is not that which is suitable for a completely-organized abnormal articulation. Most writers state that, in fact, a false joint only exists when all hope of obtaining union is lost; as if all fractures were cured within a certain lapse of time, in all subjects, whatever the physiological or morbid conditions of these may be. But in subjects even in excellent health, and of a favourable age, six months may pass without union resulting, as every surgeon of large practice can bear witness. In such, certain modifications of the apparatus, additional docility on the part of the patient, or care on the part of the practitioner, may suffice to secure a favourable result.

It is less, in fact, in the lapse of time, than in the disposition of the fragments, that we must seek the differences between delayed union and false joint; and the teachings of pathological anatomy are here of high import. Observations on man, and experiments on animals, exhibit distinct forms of false joints as consequences of fracture. 1. In the *pseudarthrosis with continuity* of Breschet, the separated fragments are united by a band or ribbon of fibrous tissue, the laxity of which admits of the movement of the ends of the bones; these are more or less rounded, but there is ordinarily no cartilage on their surfaces. Sometimes the ligamentous substance, which is attached to, and unites them, partakes of the nature of cartilage, and in certain portions of its extent the ligamentous and cartilaginous tissues become blended. — 2. In the *pseudarthrosis with contiguity* of Breschet, we always find that the ends of the bones are more or less in proximity; their opposed surfaces being, when the fracture is of old date, covered with cartilage; and an abnormal synovial membrane, with a ligamentous substance more or less resembling a fibrous capsule, surrounding the whole. This kind of false joint has been described by many writers, and is easily produced experimentally. Of nine *pseudarthroses* thus produced by Breschet and Villermé, six were of this description.—3. The fragments may be devoid of all means of union, the extremities of the bones being mobile, and placed at some distance from each other. Earle and other writers furnish examples in which the interposition of muscles has prevented union.—4. Norris endeavours to establish the existence of another form, in which the osseous extremities are united and surrounded by cartilage, movement being possible between them. M. Lenoir believes that this is but a stage in the formation of normal callus, which has been mistaken for false joint.

In respect to the *mode of development* of the two first-named varieties, the fibrous tissue that, at a later period, unites the fragments in the *pseudarthrosis with continuity*, results from the plastic substance furnished by the ends of the bones at the early stages of every formation of callus—solid matter not becoming deposited. The ligamentous substance is thus always found broader where attached to the fragments, than at its middle part. In the *pseudarthrosis with contiguity*, either the fibro-plastic matter is not furnished by the periosteum and fragments of bone—its component parts not being found in the blood, as in several diatheses—or the matter after exudation loses its consistency, and becomes unfit to effect consolidation, as occurs in certain severe forms of fever; or, again, this matter, though sufficiently secreted, and possessed of the necessary qualities, becomes disintegrated as soon as formed by the extremities of the fragments of the bones themselves, as when the limb is subjected to violent or constant movements.

In all such cases a false joint is formed sooner or later. At an early period, however, the phenomena which characterize pseudarthrosis and simply delayed union are the same. The sole difference recognisable in these two states is, that the causes which produce the solution of the formative matter of the callus are in the first case of prolonged or constant operation, while in the other they are temporary and sudden. Thus in a patient suffering from insufficient food, or from the action of syphilis or scorbutus, the solidification of the fracture may remain suspended, or nearly so, during all the time these causes are in operation; and only resume its normal course, when the blood, modified by regimen or treatment, can furnish the due solidifying material. So, too, under the operation of local causes disintegrating the effused matter (as ill-reduction of the fracture, excessive riding of the fragments, the injudicious application of bandages, or the incessant movement of the parts), the consolidation is deferred until these errors are remedied. The practical rule is in all cases to act as if we had to do with a simple case of delayed consolidation, and to employ at first only the simplest means, as, for example, acupuncture. This would prove unavailing in a case of pseudarthrosis with continuity, in which resection of the bones would alone avail. In all cases in which the seton is indicated, acupuncture may be advantageously substituted; for while acting in the same manner as the former, the latter admits of a more graduated application, according to the indications offered and the effects produced. The absence of these advantages in the seton may in some cases explain its failure.

III. *Case of Rupture of the Vagina, with Passage of the Fœtus into the Cavity of the Abdomen.* By M. Danyau.

This occurred in the person of a little, robust, bow-legged woman, 28 years of age. She had been already pregnant three times, delivery having on the two first occasions been accomplished by perforation, owing to the great contraction of the entrance of the pelvis. On the third occasion labour was induced at the eighth month, and was followed by peritonitis, iliac abscess, and puerperal mania. On the 18th June, 1848, arrived at the end of her fourth pregnancy, she came to the hospital with commencing labour pains. The liq. amnii had been discharged nine hours; and under the influence

of strong pains it was hoped that, owing to the small size of the child's head, the narrow orifice might be passed. The severity of the pains, however, rendered the woman very restless; and while tossing about she fell off the bed. She resumed her place unaided, and declared she had received no hurt. However, the pains at once ceased, the head could no longer be felt, the abdomen became very tender, and the woman's voice, pulse, and countenance underwent such alterations as to lead to the conclusion that the child had passed into the cavity of the abdomen. M. Danyau, called to her one hour after, resolved upon attempting turning in preference to the Cæsarian section. On passing in the hand, the uterus was found thrust upwards, a little forwards, and to the right—the entire left half of the vagina being separated from it. Owing to the small size of the child, its extraction was performed with more facility than had been anticipated—a perforation at the base of the cranium with Smellie's scissors sufficing to lessen the head sufficiently. The placenta was easily removed from the abdomen; and no intestine descended through the vaginal aperture. No hæmorrhage occurred; but the patient seemed reduced to a state of hopeless exhaustion. She rallied, however, and in fifteen days, though advised to the contrary, she left the hospital. An examination, *per vaginam*, made on the ninth and fifteenth days, furnished little idea of the severe lesion that had occurred—scarcely even any irregularity remaining at the place where the rupture had occurred, and the cervix uteri appearing just as it should do at the end of a fortnight. Soon after going out, she was seized with iliac inflammation, requiring antiphlogistic treatment, from which she completely recovered.

M. Danyau refers to Goldson's work (1787), in which the author relates a similar case to the above, and collects various instances to show that many cases reported as examples of rupture of the uterus have really been examples of rupture of the vagina. This view was enforced in the treatise *De Rupturâ Vaginæ*, published by Böer at Vienna, in 1812, in which additional confirmatory facts are adduced. All these cases have been republished in the *Archives Générales* for November, 1827. M. Danyau has not been able to find many cases on record, narrated with sufficient exactitude to assure their identity with his own, as examples of rupture of the peri-uterine portion of the vagina, with passage of the child into the abdomen. We subjoin in a note the references he has supplied.* Of the 17 cases to which these accounts refer, and in none of which gastrotomy was resorted to, 4 only terminated successfully—those of Ross, Douglas, Smith, and the author. In the 13 others, death resulted, either because the nature of the case was misunderstood, its progress too far advanced for interference—such interference being too long delayed,—or from consecutive accidents, of which last, however, only one example is on record. The rarity of such consecutive accidents, and the successful issue of the 4 cases, teach the necessity of prompt decision, as well as careful examination.

* Pouteau (Œuvres Posthumes); Ross (Duncan's Annals, 1798); Andrew Douglas (Lond. 1785); Courtial (Obs. Anat. sur les Os, 1705); Moreno (Arch. Gen. xix. 301); Wittig (Obs. Vag. Disrupt., Berol, 1828); Smith (Med.-Chir. Trans.); Michaelis (Siebold's Lucina, vi. 305); Ingleby (Facts and Cases, p. 204); Siebold (Journal, xiii. 46); Bonetus (Sepulchr. § 38, lib. iii.); Journ. der Geburtsh, i. 72, 1787.

IV. *A Case of Medio-Carpan Luxation Backwards.* By M. Maisonneuve.

In this paper M. Maisonneuve relates the particulars of what he believes to be an undescribed luxation, observed in the wrist of a man who died soon after a fall from a height of forty feet. At first sight, it had the appearance of a fracture of the radius, the hand being carried backwards, so as to present the figure of 7. On dissection, the extensor tendons were found uninjured, though thrust backwards, and the seat of the lesion was discovered to be in the medio-carpan articulation. The radial portion of the scaphoid, the entire semilunar, and a portion of the pyramidal bone, remained attached to the forearm, their anterior and posterior ligaments being uninjured. The bones of the second row had undergone no alteration of their relations to the metacarpus, but they were separated from, and mounted backwards upon, those of the first row. The accident was, however, a luxation complicated with fracture, as a portion of the scaphoid remained united to the trapezium, and a portion of the pyramidal, carrying with it the pisiform, had followed the unciform bone. The flexor muscles were stretched, but not torn. The external and internal lateral ligaments of the radio-carpal articulation were completely torn, as were the ligamentous bands uniting the two rows of the carpus. The signs furnished by the accident were precisely those which Voillemier attributes to the radio-carpan luxation; but the diagnosis between these accidents would be of little consequence, as the treatment would be the same.

V. *On some Points of the Surgical Anatomy of the Mammary Region.* By M. Giraldès.

In this paper M. Giraldès draws attention to certain dispositions of the fibrous capsule of the breast, which, although adverted to by A. Cooper and other writers on the anatomy of the organ, he believes have not yet received their full practical application. The first of these is the occurrence, at the anterior portion of the fibrous capsule, of certain small cavities, filled with fine fat, communicating with the adipose structure covering the organ, and either terminating in *culs-de-sac,* or sinuously traversing the substance of the gland. These fatty interspaces do not always intercommunicate. At the posterior surface of the breast, the capsule exhibits no asperities or cavities, but consists of a very dense, resisting, fibrous lamella, which is continued into the substance of the gland. The circumference of the fibrous capsule is continuous with the fascia superficialis, one lamella of which is fixed to the edge of the capsule, while the other passes behind it; a kind of cavity of considerable size, in which fluids may accumulate, being thus formed. By means of the attachment of this fascia to the clavicle, the mammary gland is, as it were, suspended, and maintained *in situ;* and hence, whatever size it may reach, it does not undergo displacement.

Abscesses of the breast may occupy different localities, some being developed in the adipose tissue covering it, and others within the substance of the gland, or even behind the organ, between it and the fascia superficialis. The glandular abscesses are always developed within the little fatty cavities above described, or in some of the sinuosities which lead

from these and traverse the gland. As these cavities do not always intercommunicate, M. Giraldès recommends that the abscesses which form in them should be opened by means of punctures, in place of free incisions, as these last may implicate parts that are not the seat of suppuration, and may yet fail to open into the source of the pus, which may originate at some distance from the point at which its presence is apparent. When, however, the abscess is situated at the posterior part of the gland, free incisions at the circumference of the organ are required.

In the next place, it is to be observed, that the fibrous capsule of the breast completely surrounds all the lactiferous canals, giving to each lobe and lobule of the organ its special envelope. This fibrous covering is more firm and resisting in the young, and more lax in women who have borne children. The consideration of the presence, amount, and density of this fibrous sheath, especially where it surrounds the glandular vesicles, is of great importance in enabling us to properly comprehend the nature of certain tumours of the breast. The vesicles or acini terminating the lactiferous canals are liable to become abnormally dilated, and then the fibrous tissue which accompanies them becomes more or less hypertrophied. In this way are produced hard, rounded, movable, tumours, of a size varying from a pea to a pigeon's egg, more or less embedded in the substance of the gland. They are especially found in young persons, and are often supposed to originate after external irritation, as by pressure. They have been mistaken for scirrhous and fibroid tumours, and on examination after removal, are found to consist of a whitish or yellowish mass, in which the fibrous element predominates. But upon more minute inspection, this fibrous substance is found to be pierced by cavities of varying calibre, in which is found a mixture of liquid and altered epithelium. In some cases the fibrous hypertrophy may be absent, and the vesicular tumour may inflame, burst, and send forth fungous vegetations.

VI. *Observations upon the Mediate Contagion of Syphilis.* By M. Cullerier.

Every practitioner accustomed to treat syphilis must have met with cases of chancre proceeding from persons in whom the most scrupulous examination can detect no cause for the infection. A prostitute, who sees a number of persons in succession, may thus impart chancre to some of these, without herself exhibiting any sign of disease—she having, in fact, served as the mere means of transfer or vehicle of the poison, furnishing an example of mediate contagion. M. Cullerier cites the opinions of various authors, both old and recent, admitting the possibility of this occurrence; but not being aware of any positive experiments demonstrating it, he communicates in his paper the results of two which he instituted. A young girl was admitted, with open bubo in each groin, unaccompanied by any trace of ulceration in the genitals, the vagina however being red, and secreting muco-pus abundantly. Six weeks after admission, the sores in the groin had diminished in size by one-half, and the vaginitis was abated. She was again examined, to make sure that no ulcer existed, and on trial, the vaginal mucus was found uninoculable. M. Cullerier now transferred, by means of a spatula, a considerable quantity of the pus from the inguinal sore into the vagina, and then made the

patient walk about the ward for half an hour, under *surveillance.* The thigh of the patient was now inoculated by the moisture of the vagina, which was then thoroughly washed out, and injected with alum water. In forty-eight hours a most characteristic pustule resulted from the inoculation, which, for additional examination, was left till the next day, and then destroyed with the Vienna paste. No increase of inflammation, or other morbid appearance, occurred in the vagina, and in two months the patient left the hospital quite well.—In the second case, with open bubo in either groin, the genital passages were in their normal condition. Pus from the bubo was introduced high up in the vagina, and the patient walked about for an hour. Inoculation was performed with some of the fluid from the vagina, in which no pus could be detected. In this case, in which no prior vaginitis prevailed, exactly the same results followed.

Not only do these cases prove the point at issue, but they show the utility of abundant local ablution, as a prophylactic in syphilis. M. Ricord has stated that the venereal disease would be less prevalent if women were more clean; and M. Cullerier adds, that it would be still less so if men were more careful and less confiding. For, as we have seen, the poison may remain with impunity for some time in the vagina, so may it under the prepuce; but many men do not take the same precautions after certain unsuspected connexions, which they would after having had intercourse with common prostitutes. M. Ricord relates the case of a young man who had connexion with a woman suffering from chancre, and on the same day with his usual mistress. He infected the latter, without suffering himself, he having neglected ablution before visiting her, and possessing a long prepuce.

Doubts have been raised as to the possibility of effecting mediate contagion by means of inert bodies, owing to the erroneous opinion prevailing, of the necessity of vital caloric for the preservation of the virulent powers of the pus. M. Ricord has preserved chancrous matter between glasses, just like vaccine lymph, for several days; and M. Cullerier has found pus inoculable which had been exposed to the air for two days. If, for the glass in which it was kept, we substituted a cup, spoon, fork, or pipe, in the case of labial chancre, or clothes, an utensil, or a privy-seat, in chancre of the penis or vulva, the same consequences might follow. M. Cullerier is of opinion, that cases which are related as examples of the contagiousness of constitutional syphilis, really originate in some such manner; while again the mediate contagion effected through the vagina may, in other cases, give rise to an erroneous belief in the identity of chancre and blennorrhagia.

VII. *On the Autoplastic Treatment of Ranula.* By M. Forget.

In this communication, M. Forget relates two cases of ranula, in which he successfully employed a modification of M. Jobert's stomatoplastic procedure. He prefaces the narration by a defence of the opinion generally entertained, that the affection is due to an obstruction or obliteration of the salivary ducts. This view is opposed by those observers who regard ranula as always resulting from an accidental cyst, analogous to the closed cavities in other parts of the body (examples of such sublingual cysts, simulating ranula, are indeed met with); and by Stromeyer and

Fleischmann, who regard it as the abnormal increase of a natural bursa. M. Maisonneuve denies that any positive cases of salivary ranula are on record, inasmuch as obliteration or obstruction of Wharton's canal is attended with inflammatory action, which is not found in ranula; but the occurrence of this inflammatory action, in fact, much depends upon whether the obstruction has been suddenly or slowly produced. The great dilatability of Wharton's duct is amply proved by the size and number of the calculi that have been found within it. From M. Forget's view of the nature of the affection, it follows that the various means employed for the relief of the affection should have in view, not the obliteration of a cyst, but the re-establishment of the course of the saliva, by means of a permanent artificial opening; and it is to this end that M. Jobert's operation is directed.

Before adverting to its application in the two cases in question, we may just refer to a very interesting clinical lecture on ranula recently delivered by M. Jobert himself.* In this he observes that great confusion in treating the disease has resulted from confounding, under the term "ranula," all the cystiferous tumours developed at the side of the tongue. To obviate this, he terms ranula arising from obstruction of Wharton's duct "salivary ranula," while he applies the term "mucous ranula" to the tumours which by their external appearance simulate this. When the obstruction of Wharton's duct is complete, *an acute salivary ranula* may form very rapidly and assume a large size, raising the tongue and projecting the sub-maxillary gland at the side of the neck. The *mucous ranula* (in M. Jobert's opinion the most frequent form) is composed of a cyst developed in Fleischmann's bursa at the root of the tongue, behind the frænum. In the majority of cases, it projects more on one side than the other. Owing to its depth, the tumour is not transparent, unless very recent and tensely distended. When of old formation, and after being subjected to repeated operations or attacks of inflammation, the walls become thickened, so as even to acquire a fibro-cartilaginous consistency. Developing itself at first in the mouth, it eventually appears in the cervical region, giving rise to a tumour on each side of the median line. Fluctuation may then become as distinct in the neck as in the mouth; and the fluid may be pressed from the one part to the other. A third form may be termed the *follicular ranula*, resulting from an obliteration of one or more of the canals of the mucous follicles. It is superficially situated, and almost always multiple, consisting of several small tumours in immediate proximity; or if single, it is very irregular. At first it is rounded, but soon becomes pedunculated. It is quite transparent.

In M. Forget's first case the tumour had previously been once punctured, and once punctured and cauterized, two months since; and had now again reached a very inconvenient size at the right side of the mouth.

"I commenced by rendering the entire external surface of the tumour raw by means of a superficial dissection, which implicated the mucous membrane, but not the cyst itself. I extended the dissection as far as possible, and obtained a raw surface at the floor of the mouth, which reached beyond the circumference of the tumour. Next, the cyst was opened by an incision extending through its entire length, parallel to the body of the lower jaw, an abundant albuminous fluid being thus discharged. I then introduced the extremity of my finger into the cavity, and ascertained that it terminated in an *infundibulum*, which was prolonged backwards from the centre of the jaw in the direction of the conduit of the sub-maxil-

* Gazette des Hôpitaux, No. 100. 1851.

lary gland. Concluding from this that the tumour resulted from a dilatation of this duct, and not from a cyst unconnected with the salivary passages, I continued the operation. The incision of the tumour had furnished two flaps, and I divided each of these in the centre, so as to obtain four of a tolerably regular triangular shape, and which were free and floating in all their extent from base to summit. Having carefully waited until all bleeding had ceased, I next fixed these flaps in proper position, turning each from within outwards, and commencing with the two nearest the tongue. One of these was laid in the sublingual groove, and maintained there by two points of suture, the other being carried beyond the frænum, and secured in the same way. The two anterior flaps were then adapted to the buccal floor. For these last, one point of suture, applied to the summit, sufficed; while, for the other two, in addition to this, a second suture was applied at that edge which corresponded to the raw edge of the mucous membrane, with which they were brought in contact. The immediate result of the operation was to maintain the salivary cavity widely open, so that the end of the little finger easily entered it." (p. 229.)

Considerable tumefaction of the flaps and neighbouring mucous membrane ensued, requiring frequent and free application of nitrate of silver during a week. The sutures were removed two days after their application. At the end of five weeks the cure was complete; and M. Forget has since had repeated opportunities of assuring himself that it has continued permanent. The floor of the mouth is as flat on the one side as on the other, and under the tongue an elliptical opening exists, which easily admits a small probe, and from which a fluid having all the characters of saliva flows. The inflammatory swelling of the flaps would have obliterated the opening, if the aperture, during the treatment of the case, had not been frequently dilated, and the adhesions broken down by a probe.—In the *second* case the swelling had reached the size of an egg. The operation was performed in the same manner; but during the cauterization that was resorted to, to diminish the tumefaction of the flaps, stomatitis was excited, which produced a sublingual tumefaction or commencing ranula on the opposite side, which, however, disappeared with the inflammation that had caused it. For many days the opening manifested a great tendency to close, but was prevented from doing so by the daily introduction of a probe for two or three weeks.

VIII. *On Spontaneous or Senile Gangrene.* By M. Chassaignac.

M. Chassaignac, believing that cases of this affection are of sufficiently rare occurrence to call upon those having opportunities of observing them to communicate the particulars, relates two somewhat remarkable examples that have occurred to himself.

1. Denis Chevallier, æt. 57, fisherman, of good health, active habits, and temperate, was admitted into La Charité, 29th September, 1843, suffering from sphacelus of the right hand and forearm, as high up as the union of the upper with the middle third. The opposite arm exhibited no lesion, except a notable diminution of temperature, while the pulse could not be felt in either its radial or brachial artery. The two lower extremities possessed their normal temperature, but the femoral arteries were pulseless —*the pulse in fact being nowhere to be felt except in the two carotids.* The heart furnished a faint *bruit de lime*, and its pulsations were notably intermitting and irregular. The patient stated that only eight days before, his arm became benumbed without obvious cause, and the mortification soon followed. He exhibited a degree of cerebral torpor, and was unaware of

his danger. A supporting treatment was pursued, alum and bark, with chlorine, being applied to the part. No improvement took place. By the 8th of October, the line of demarcation had become deep, and the eliminatory process active. Delirium now set in, and he died on the 10th. The *post mortem* was made thirty-four hours after death. The heart presented on its exterior, large *polygonal* white patches, which M. Chassaignac regards as traces of partial pericarditis. This polygonal form differs from the traces of old organic changes on the surface of other viscera, which are usually more or less *circular*. The right cavities of the heart presented nothing remarkable, except a general increase proportionate to the hypertrophy of the organ. The left auricle was hypertrophied, and the auriculo-ventricular orifice was ossified and narrowed. The left ventricle was red and hypertrophied, the aortic orifice being narrowed, and its valve ossified. The entire arterial system was remarkable for the great development of the *vasa vasorum;* and the supra-diaphragmatic portion of it was notably hypertrophied, so that the right carotid and internal mammary arteries were three times their normal volume. The inner surface of the aorta was covered with small, very numerous, osseous plates ; and the *vasa vasorum* formed a complicated network quite down to the femorals. The right subclavian was very large, and contained a coagulum (as did the aorta at its commencement) which extended through the axillary and brachial arteries to the gangrened part. A less firm coagulum was observed on the opposite side. The profunda of the brachial on each side contained very firm coagula.

Although the co-existence of arterial lesion and this form of spontaneous gangrene has long been known, M. Chassaignac believes that there does not exist an example in which the connexion of the two phenomena has been rendered so evident, whether we consider the generality or the intensity of the changes in the arterial system. The arteries were almost everywhere more or less obturated, producing an absence of pulse in all the great arterial trunks, which yet proved compatible with the maintainance of life, without the production of marked general symptoms, during sixteen days. It may seem remarkable, in this case, that although the lesions of the vessels so exactly corresponded in either extremity, the left forearm should have escaped the gangrene which affected the right; but it is probable that the blood obtained access to the former by some narrow, unperceived channel. After ligature of a vessel, the circulation in an extremity is maintained by the anastomosing branches; but here all the branches, as well as the trunks, were obturated, and the valvular orifices much narrowed; and although life could not be prolonged without some kind of circulation, the dependence of this upon the left cavities of the heart had long ceased to be appreciable. One remarkable circumstance in the case, was the completeness with which the respiratory functions were maintained during almost its entire course.

2. Bartholomew Mocret, æt. 78, was admitted Sept. 14, 1849, having prior to admission applied several times on account of severe pains in the great toe, and an ulceration opposite the left metatarso-phalangeal articulation, to which nitrate of silver had been applied. On admission, the entire toe was black and dry, the skin of the foot and ankle being as hard as marble, and insensible to pressure or pricking. No pulsation could be

felt in the dorsal, posterior tibial, and popliteal arteries; and even on the other side this could only be perceived with difficulty lower down than the femoral. Cupping glasses were applied along the course of the femoral artery, and stimulating applications made to the gangrened parts; but as these means proved of no avail, and the general health began to give way, amputation was performed, on the 5th of October, at some fingers' breadth above the ankle, where the tissues appeared quite healthy, and sensation continued complete. Three arteries were tied, the wound was dressed by *occlusion*,* and the tincture of aconite, which M. Chassaignac employs as a preventive of purulent infection after all operations, was administered. On this occasion M. Chassaignac describes the mode in which he is in the habit of dressing a stump, so as to prevent, as far as possible, the irritation and penetration of the skin by the cutting edge of the tibia.

"I take a strip of adhesive plaster of a full finger's breadth, applying the middle behind, so as to support the pendulous soft parts at the posterior part of the stump. The two ends are brought as obliquely as possible, from below upwards, and from behind forwards, crossed, and carried to the anterior part of the leg, three or four fingers' breadth above the level of the section of the bone. Other strips overlap the edges of this, and of each other in succession, and are applied in such a manner as always to carry the stress of the apparatus towards the anterior part of the leg, at some distance above the level of the amputation. These strips so applied have the effect of bringing the integuments forward, so as in some measure even to fold them over the anterior part of the limb, in place of allowing them to become painfully stretched over the edge of the osseous section." (p. 270.)

Gangrene seized the stump, and the patient died on the 15th. The vascular system was minutely examined. While, as a general rule, pathological changes are found most intense at the origin of the aorta, and gradually decrease towards the terminal arteries, the contrary was observed in the present case. The few osseous plates observed in the valves and upper portion of this vessel increased in number, and the coats of the bloodvessels augmented in thickness, in proportion to the distance from the heart. The left popliteal artery was completely obliterated by a firm coagulum, three or four fingers broad. Below this point the arteries became permeable for short distances, to become again obturated by dry, cylindrical, cretaceous concretions, as hard as stone, seeming, as it were, to plug up the cavity of the vessel.

"On cutting from without inwards, we seemed to open into the cavity of the artery, the walls of which we alone had separated. On everting these, we saw in the centre of the vessel thus extended, a complete osseous cylinder. The appearance was deceptive, for it was not in the cavity of the vessel itself that this concretion was formed. When cut lengthwise the cylinder presented in its axis a cavity evidently lined with the internal membrane of the artery. It became plain, that by the first incision we had divided the cellular tunic, and the most external layers of the middle one, while the innermost layers of this, converted into an osseous plate, retained the form of a cylinder ensheathed within the divided membranes, although at first sight we seemed to have opened into the cavity of the artery itself." (p. 272.)

This could only be made out in some parts, as the irregular compression exerted by the concretion often prevented the internal cavity being seen. The veins, also, presented a great number of calcareous concretions. In

* See the British and Foreign Medico-Chirurgical Review, vol. v. p. 262.

this case, M. Chassaignac believes that amputation of the thigh should have been performed in place of that of the leg.

"From this fact I wish to establish what I consider as a practical rule of great importance—namely, that when amputation is performed for senile gangrene, we must not be content to divide the parts at a greater or less distance from the gangrene; but must carry back the line of incision to a point where the arterial pulsation can be indubitably felt. Several facts which I have observed lead me to admit as a general rule, that in senile gangrene of the foot, there is always an obliterating coagulum in the corresponding popliteal artery." (p. 273.)

VIII. *On Pathological Relaxation of the Symphysis of the Pelvis after Delivery.* By M. Martin.

This paper only contains some additional facts illustrating the occasional occurrence of so considerable an amount of relaxation of the symphysis of the pelvis after delivery, as to prevent progression, and induce a variety of symptoms. The cases related by M. Martin, show that these symptoms may be mistaken for those caused by displacements of the uterus, which either do not exist at all, or are mere consequences or complications of the pelvic relaxation. He found cases which had long resisted misplaced treatment, at once yield to the compression of a broad steel belt passed around the pelvis, giving provisionally that fixity which the pelvic joints themselves re-acquired after an interval varying from some months to a year.

M. Danyau, reporting on the paper, observes that, at first sight, errors in distinguishing an affection of which the diagnosis is so easy, would seem very unlikely; but still, those who know how easily the mind is preoccupied, and how necessary it is to pay attention to the conditions of the entire organism to avoid being misled, will not be surprised at such being committed—especially as the affection to any considerable extent is a rare one, and a very rare one, compared with the frequency of uterine displacement after delivery.

IX. *On Gun-shot Wounds of the Bladder.* By MM. Demarquay and H. Larrey.

In the present analysis we must blend our notice of M. Demarquay's paper with that of the able report delivered in upon it by M. H. Larrey. M. Demarquay relates two cases which have occurred in his own practice, and furnishes abstracts of twenty-four others which have been related by other authors. He commences his essay with an account of some investigations he has instituted concerning the anatomical relations of the bladder:

"Wounds of the bladder are favoured by certain anatomical and physiological conditions, which it is of importance to notice, one of the most essential being the state of distention of the organ. These wounds are almost always received in the midst of some important action, when the individuals injured have the bladder full of urine. The bladder, rising from the pelvis into the hypogastric region, pushes up the peritoneum, and becomes to some extent superficial, and exposed more to the danger of being wounded, without however the peritoneum always participating in the lesion. . . . . The facts which I have observed, and those which I have collected from others, enable me to prove that frequently the anterior part and inferior fundus of the bladder may be wounded without the peritoneum being so at all. I believed that the bladder in becoming developed from

below upwards, and from before backwards, as regards its body, underwent a similar development in relation to its inferior fundus—this explaining how, when the organ is distended with urine, a ball may strike it sufficiently high in its lower part, either traversing or passing-by the rectum, but leading to no lesion of the serous membrane which unites them. To determine this point I made many researches, in order to exactly ascertain the distance there is between the peritoneal *cul-de-sac* and the surface of the perineum—and this the more willingly, since eminent men, who have entered upon this investigation, are by no means agreed as to the results. I examined subjects of different ages, and found the distance between the *cul-de-sac* and the perineum to be very variable according to the age, sex, and especially the *embonpoint* of the subject. But a much more important thing to determine was, whether the distention of the bladder, at the same time that it thrust the peritoneum upwards, did not also thrust it backwards upon the rectum, which would permit a lesion occurring moderately high upon its inferior fundus without the peritoneum being interfered with. I have been able to assure myself that the peritoneum covering the posterior wall of the bladder is in no wise modified by the distention of that organ, which is moreover sufficiently explained by the prostato-peritoneal aponeurosis described by M. Denonvilliers." (p. 293.)

A circumstance favouring lesions of the bladder is the fact of the organ being in contact with curved surfaces, as those of the abdominal walls and pelvis, which impress particular directions on the ball. In a case related by M. Baudens, a ball entered the abdomen opposite the last rib on the left side, and made its exit at a point opposite the right sciatic notch—probably glancing from the front of the intestines, and penetrating the bladder as it passed towards the sciatic notch. Urine escaped by the lower wound; but the urinary fistula was healed in about a month, a slight degree of paralysis of the limb only remaining. The reporter relates another case, in which the ball, entering below the symphysis, struck the anterior surface of the bladder, and passed out in front of the left ischium, the patient recovering with a urinary fistula, which continued open for some months.

It is of great importance to be aware of the *direction* which the ball takes, as a means of deciding upon the amount of danger attendant upon the wound. Thus, in a case reported by Fleury, the bladder was traversed directly from before backwards, as well as the rectum and peritoneum; and in another, reported by Jobert, the ball entered a little above the right trochanter, and penetrated the os ilii, abdomen, and bladder, coming out at the anterior superior iliac spine on the left side. In these cases the patients died of peritonitis. A ball may, however, enter the pelvis in front of the iliac spine, traverse the bladder, and make its exit on the opposite side, without the peritoneum becoming implicated, an example of which is related by Ponéyes in the 'Transactions of the Académie de Chirurgie.' (tom. vi.) When the projectile passes in below the bladder, it may injure the coccyx and wound the neck of the bladder and urethra, without inducing a lesion of the peritoneum, as in a case related by Baudens.

Several cases are on record, and especially one by Larrey, in which the ball, having spent itself, *remains in the bladder.* The reporter believes this to be due, not only to the diminution of the speed of the projectile and the resistance of the vesical walls, but also to the obstacle offered by an accumulation of urine, or even by the contraction of the organ opposing the exit of the body. He believes that this penetration of projectiles into

the bladder is of more frequent occurrence than is usually supposed; it being overlooked until certain mechanical or inflammatory symptoms exhibit themselves, either prior or subsequent to cicatrization. Sometimes other bodies are carried in by the projectile. In a case, related by Tulpius, of a soldier dying with symptoms of calculus, after the healing of a wound of the bladder, three calculi and a large fragment of the pubes were found. The ball sometimes becomes encysted in the bladder, a case of which is related by Larrey.

Sometimes balls which strike the bladder do not rupture it, but only produce a contusion of its walls; signs of lesion of the organ only appearing consecutively, when the eschar separates. Such would seem to have been the case in an observation related by Fleury.

The wound may not be a simple lesion, but complicated with other injuries, as *fracture of the pelvis.* Percy relates an interesting case, in which the ball not only pierced the os ilii, but traversed the hip-joint, breaking the head of the femur—the patient living to the fifty-sixth day. One of the commonest complications is *wound of the intestine*, whether the small, as in cases seen by Fleury and Kerauden, or the lower part of the rectum, as in those of Dupuytren, Baudens, Larrey, and the author. In other cases, the *genital organs* are frequently more or less injured; and in some of these, division of the epigastric artery has given rise to more or less troublesome hæmorrhage.

Symptoms.—Among the primary signs is the pain of the blow, especially if the peritoneum be concerned; although Larrey relates a case in which the patient did not know that he was seriously wounded. Desire to pass urine, or to go to stool, is evinced, and sometimes considerable hæmorrhage occurs. Usually catheterism is easy; though in a case related by Bourrienne, in which the organ had been traversed near its neck, this was impossible. To these symptoms succeed others due to inflammatory action of the bladder or of other organs wounded, or to the extension of this to the peritoneum, together with more or less discharge or infiltration of urine. The reporter observes, that while the escape of urine is a characteristic sign of penetration of the bladder, it is a very variable one, according to the seat, course, or parallelism of the wound, and the repletion of the bladder; as it is also in respect to the quantity which escapes, and its mixture with other matters. In place of flowing freely from the wound, the fluid too frequently meets with obstacles, and becomes infiltrated into the neighbouring tissues, giving rise to alarming symptoms. Hæmorrhage, which is so frequently observed externally, sometimes takes place internally, forming the intra-vesical hæmorrhage that has been so well described by Larrey. The blood tends to coagulate within the cavity, and then acts as a foreign body, giving rise to the successive and complicated symptoms due to internal hæmorrhage, cystitis, and retention of urine—catheterism, as in Bourrienne's case, being impossible. It is the infiltration of urine and internal hæmorrhage, that usually, in these accidents, give rise to inflammatory occurrences, peritonitis being the most to be apprehended of these. Cystitis is much less common, and less dangerous; being chiefly due to the presence of foreign bodies, and exhibiting itself especially towards the neck of the organ.

The *Diagnosis* of the accident is usually easy, although the distinctive

signs derived from the flow of urine from the wound may not always be present at first. With respect to the medico-legal question of the relative size of the apertures by which the ball entered or passed out, there is the same difference of opinion as prevails with respect to gun-shot wounds in other regions of the body. In the author's own two cases, the wound made by the ball at its entry was larger than that of its exit.

Prognosis.—Although all the cases which the author has collected, in which there was no lesion of the peritoneum, terminated in recovery, yet, he observes, as these have been recorded on account of their unusual character, we must not, from the contemplation of so many examples of cure, be prevented from considering the accident as one of a highly dangerous nature. The reporter observes that the experience of the moderns does not confirm the opinions of the ancient observers, that wounds of the bladder are necessarily of a fatal issue. One reason of this is, that wounds from cutting instruments, with which they alone were acquainted, seem to be much oftener complicated with dangerous accidents, than those from fire-arms. The former perforate and divide the tissues cleanly and largely, favouring the infiltration and effusion of urine, and the consequences. Gun-shot wounds induce tumefaction, and escharification of the tissues, and preserve them better from the effects of the effusion of urine. In these, too, the canaliculated disposition of the wound favours the issue of fluids, salutary adhesions are promptly formed, and yet premature cicatrization is not observed as in wounds from cutting weapons. Almost all the soldiers whose bladders become pierced by sabre or bayonet wounds, die within forty-eight hours, when the peritoneum is implicated, however prompt may have been the succour afforded them.

Treatment.—A principal indication is to favour the issue of urine, either by catheterism if possible, or through the wound, which for this purpose may require enlargement. The inflammatory symptoms must be met by vigorous depletion; and tepid injections should be thrown into the bladder, to favour the disintegration of the coagula. Where the rectum also has been injured, stools must be avoided, opiates being administered until the inflammatory symptoms have subsided, and the various complications have been attended to, especially the removal of foreign bodies from the bladder. When a foreign body is so located that it cannot be removed, the question arises whether lithotomy should be performed at once, or after the inflammatory accidents have subsided. By the first procedure, a source of irritation is at once removed, and a free egress for the urine is secured. Yet, in the cases recorded, surgeons have not adopted it, but have waited for the subsidence of the inflammation. The presence of foreign bodies is not indeed so dangerous as it might seem at first; for as an aperture exists in the bladder, the urine flows away; and the walls of the organ not contracting energetically, little suffering is produced. Exceptions to this may occur, as in a case of Larrey's, in which the severe pain caused by the arrest of a ball in the bladder, necessitated the immediate performance of lithotomy. The rule M. Demarquay would lay down is, that if the foreign body occasions only slight pain, and the urine flows away freely, we should wait until the primary symptoms are relieved. Sometimes a small ball retained in the bladder has been extruded through the urethra by the efforts of the patient, as in Bonnet's case; and in a case quoted from

Bartholinus, a piece of iron was thus voided with immense suffering. In the case of Lieutenant Burnet, cited by Haller, several splinters were thus passed. M. Demarquay condemns the high operation practised by Baudens, but the reporter believes that he does so too absolutely.

The reporter does not think that sufficient stress has been laid upon the *position* given to the wounded person, which, however, facilitates the displacement of foreign bodies, the due disposition of the lips of the wound, and the issue of the urine. In other cases, certain motions imparted to the body may favour the effects of position. Larrey attributes the well-doing of many cases to the introduction of a catheter, a full-sized elastic one being the best.

The following is a brief summary of the particulars of the two cases which came under M. Demarquay's own care. 1. A national guard was struck in June, 1848, by a musket-shot on the left of the linea alba, just above the pubes. The ball, traversing the pelvis, came out at the lower part of the right buttock. A catheter was introduced; but urinary infiltration took place anteriorly, and numerous abscesses followed, erysipelas also several times pervading the whole body. At the end of several months splinters of bone were discharged through the posterior wound; and an examination, induced by the sufferings of the patient, showed the presence of calculus. The operation of lithotrity was performed, causing great pain, and many fragments were passed, attached to one of which was a splinter of bone. The urinary fistula continued open, and the patient died worn out.

2. Oudiné, a national guard, received, on the 24th of June, 1848, a gun-shot wound in the hypogastric region, opposite the right external inguinal ring. Several fragments of the pubes were carried into the bladder, and the hypogastric artery, as well as a portion of the cord, was divided. The ball traversed the inferior fundus of the bladder, the rectum, and the left side of the sacrum, and came out at the left buttock, four fingers' breadth from the anus. Abundant fluid, consisting of blood and urine, flowed from the inguinal wound; and an urgent desire to go to stool prevailed. Next day the finger, passed by the aperture of the rectum communicating with the bladder, found the latter to be full of coagula. The hypogastrium was freely leeched, tepid injections were thrown into the bladder, and opium was administered for the purpose of relieving pain, and preventing the action of the rectum. Towards the end of June, when inflammation had seized the whole track of the wound, and pus flowed out, carrying with it eschars, adhesions having become pretty firm, a little castor-oil was given. Much fæcal matter was voided, but mostly by the wounds in the bladder and integuments. Very little urine was passed by the urethra, most coming through the wounds or the rectum. Early in July, the wound began to clean, eschars were detached, and blood was discharged from the bladder. Many splinters remained in the bladder, some being free and others adherent; which were felt by the finger introduced by the rectum. Forty-one had been extracted by strongly curved polypus-forceps; and with the aid of mild laxatives the patient had a stool every fifth day. At the commencement of August the wounds had closed up. On the 12th, however, when there remained only the intercommunication between the rectum and bladder, violent pains occurred at the site of the

inguinal wound; and the right testis, the cord of which had been injured, became inflamed. The cicatrix re-opened, and pus, urine, and sometimes fæcal matter, passed through the wound, additional splinters continuing to be removed either by the anterior wound or the rectum. Successive abscesses formed in the coverings of the testis. By the end of August, all had become quiet again, the anterior wound having re-closed; but the testis was atrophied. The opening between the rectum and bladder gradually narrowed; and in September the patient was able to walk somewhat, and seemed cured: but now the right testis suffered from inflammation, which however soon subsided. At the end of the year he could walk with only slight difficulty, but a small fistulous opening allowed a few drops of urine to pass through the rectum during micturition. The venereal desire was much diminished, and only a small quantity, apparently of prostatic fluid, was secreted.

X. *Report on M. Boinet's Memoir on the employment of Iodine Injections in Ascites.* By M. Morell-Lavallée.

XI. *On the Treatment of Congestive Abscesses, dependent upon Caries of Bone, by Iodine Injections.* By M. Boinet.

In his report, M. Morell-Lavallé reviews most of the cases which have been recorded, in which gaseous or liquid bodies have been injected into the peritoneal cavity for the cure of ascites. Among these substances, protoxide of nitrogen has been one of the most successful, as recovery or amelioration has followed in all the four cases in which it has been employed. The reporter, however, gives the preference to iodine (employed in eight cases), which he thinks well deserving the attention of surgeons. He considers that the following formula is that which gives rise to the least re-action, and consequently to the least danger: water 150 parts, iodide of potassium 2 parts, tincture of iodine 25 parts.

M. Boinet observes, that every practitioner is familiar with the ill results which have hitherto attended the attempts at treating abscesses connected with caries of the spine, or the large joints, such as the hip. He considers the method he now recommends as pre-eminently useful in this particular. All the other modes of treatment at best only evacuated the collections of matter, which were soon reproduced; and frequently were not able to effect even this, without a dangerous condition of the cavity ensuing from the admission of air into it. The iodine treatment, on the other hand, operates advantageously upon the seat of the disease and origin of the discharge, and prevents the resorption of the pus, whether this has undergone vitiation or not. The experiments upon this subject date as far back as 1839; and M. Boinet expresses his surprise, that, with the numerous facts now on record, demonstrating these advantages, the old routine practice should still be persisted in.

The numerous favourable results which he had obtained from the treatment by iodine of large abscesses, unconnected with caries, led him to extend the procedure to the more formidable cases. Referring to the *Gazette Médicale* for 1840, 46, and 49, for a detailed account of the action of iodine upon the various tissues, he contents himself here with indicating what takes place, when it is brought in contact with the walls of an abscess depending upon caries.

"Let us first call to mind the pathological anatomy of abscesses in general, and of congestive abscesses in particular. Their cavities are lined by a false membrane, behind which there exists a more or less thick layer of plastic lymph, infiltrated into the cellular tissue. At the same time that this membrane and lymph isolate the abscess from the surrounding parts, they preserve them from the immediate contact of the pus, the absorption of which they prevent. When we throw in an injection of tincture of iodine against the walls of these abscesses, its first effect is to cauterize, contract, and indurate the tissues it comes in contact with, and to render absorption no longer possible. Under the influence of its operation, a true fluxion supervenes, which may assume the characters of the mildest phlegmasia or the most violent inflammation, accordingly as the tincture has been more or less concentrated, or its contact with the parietes of the abscess more or less prolonged. Blood flows in greater abundance in the walls of the abscess, and a more abundant infiltration of serosity takes place into the surrounding cellular tissue. This becomes swollen, and tends to bring the parietes of the abscess nearer to each other, these exuding the uniting material which joins the whole into one mass. Then arises the adhesive inflammation, which acts as a barrier to the suppurative inflammation, by determining the union of parts, which, without that, would infallibly become themselves the seat of suppuration. If this adhesive stage of inflammation become developed throughout the entire surface, a radical cure is effected, and relapses are not to be feared. Sometimes only a partial result is accomplished, either because such inflammation has not been general, or the tumefaction has not been considerable enough to bring the walls in contact, or the inflammation in place of remaining adhesive has become suppurative, or lastly, because the carious surfaces, not yet having undergone favourable modification by means of the iodine, still continue to secrete pus. In such cases, when the inflammation has subsided, we must renew the injection, and proceed thus until the carious surfaces and walls of the abscess become so changed and modified, as to admit of the requisite adhesive process being set up. Each injection produces a marked modification in the nature of the liquid proceeding from the abscess; so that after undergoing various changes in appearance, it is at last transformed into a serous fluid or true lymph. After each injection the cavity is found diminished, so that each time a less quantity of fluid is required, while the re-action is less marked.

"Another fact that has been demonstrated is, that there is no necessity to close the puncture, the introduction of air into the cavity after the iodine injection not seeming to cause any inconvenience. This probably arises from the walls of the cavity being no longer capable of absorption, and thus no longer containing a purulent fluid capable of undergoing alteration by the air. We know how rapidly these purulent collections re-accumulate, and how great is the danger of leaving them in communication with the air; but such danger is not to be feared after the iodine injections, for if the walls of the abscess do not absorb, so also they do not secrete, or at least not much, for some days after the injection. It is doubtless proper to take all the precautions, by means of oblique and subcutaneous punctures, that have been advised; but most of the facts we have had under observation have taught us that such precautions are, at least, not indispensable, especially after the two or three first injections." (pp. 459—461.)

M. Boinet agrees with Boyer in recommending that the abscesses should be opened as soon after fluctuation is detected, as possible—the collection being then much smaller, and the caries more amenable to treatment. The quantity of iodine required to be injected will depend upon the size of the abscess. It should be sufficient to penetrate into all the anfractuosities, and be allowed to impregnate them for at least four or five minutes, after which the greater portion may be allowed to run out. The discharge of the tincture is sometimes difficult from the coagulating effect it produces on the pus and blood, and the consequent obstruction of the canula. When the

exit is easy, we may leave one-third or one-fourth of the injection within the cavity without danger. The strength of the injection must also vary. As a general rule, equal parts of water and the tincture may be at first employed, adding a little of the iodide of potassium (four parts to one hundred of the tincture). After two or three injections, the tincture has been frequently employed undiluted, and this in very large abscesses. The injections are differently employed, accordingly as the *abscess has not yet been opened*, or as it has become *fistulous*. In the first case the puncture is made at the most dependent part of the base of the tumour, traversing two or three centimetres of sound parts obliquely, before penetrating the abscess. The collection should be emptied as completely as possible, and the iodine then introduced; and in order to secure its coming in contact with the entire parietes of the abscess, we should, during the three or four minutes it remains in, gently knead the parts, and adjust the position of the patient so as to spread it everywhere, even to the carious parts themselves. In spite of all endeavours to remove the fluid, some always remains within the cavity, and hitherto no ill effect has resulted. All air should afterwards, as far as possible, be expelled by methodical compression, diachylon applied to the wound, and a poultice laid over the part. The degree of pain excited varies in different persons, and sometimes it is quite absent, notwithstanding that the iodine has acted beneficially. The sac gradually fills again, so that in a few days the tumour has nearly acquired its former volume; but though the abscess appears as large as before, it frequently does not contain above half as much pus, its cavity being diminished by the approximation of its walls through the tumefaction that results from their infiltration. As soon as fluctuation is felt, a second injection should be practised, whether inflammatory symptoms be present or not. If the cavity is partly obliterated, and fluctuation is only perceptible at certain parts, we must operate in the centre of such fluctuation; and in this way we can often more directly influence the carious part. After two or three injections, so little is the introduction of air to be feared, that the precaution of closing the aperture need not even be taken, the pus being allowed to flow out. Wherever it collects in any quantity, it must be again and again evacuated, whether this may be required every fourth or fifth, or every eighth or tenth day; as, among other ill-effects, the pus irritates as a foreign body, and gravitates upon parts that have not yet secreted it. In *fistulous abscess* and *fistulæ*, care should be taken to penetrate all the sinuosities, and retain the iodine in these, which it is not always easy to do. A gum-elastic catheter is sometimes of use in securing the conveyance of the fluid to the more distant parts; and the position of the patient during the injection is of great importance.

While recommending this local means of treating the abscesses, M. Boinet does not neglect the *general* treatment. All causes capable of lowering the powers, such as insufficient diet, are carefully to be avoided. The patients should be kept in bed as little as possible, and encouraged to walk as soon as their condition admits of their doing so.

In illustration of his views he brings forward four cases. 1. A case of caries of the hip-joint of several years' duration, which had baffled the most experienced practitioners, was cured (by anchylosis) in eight months. 2. An abscess from caries of the sacrum was cured by eight injections.

3. Several congestive abscesses, from caries of the cervical vertebræ, cured.
4. A case of spinal caries, in which the child seemed in an utterly hopeless state, was cured in three months by five injections.

XII. *On Two New Varieties of Hernia.* By M. Parise. *With a Report on the Paper.* By M. Gosselin.

1. *Intra-iliac Inguinal Hernia.*—This case occurred in a man, æt. 34, who, when seen, had suffered from pain, vomiting, and constipation during three days. A tumour, the size of an egg, at the upper part of the scrotum on the right side, was reduced with little difficulty, and fæcal evacuations followed an oily enema. After a while, aggravated symptoms of strangulation returned, and the tumour re-appeared and was again returned. Its re-appearance, and the persistence of the symptoms, induced M. A. Berard to operate. On opening the sac, a large quantity of fluid escaped, but no protruded part could be seen; but on passing the end of the little finger through a very narrow external ring, a round resisti g tumour was felt. On incising the anterior wall of the canal, the rounded tumour was found to contain the testis, which had not descended on that side; and the little finger carried beyond this could discover no strangulated part—a female catheter being also easily introducible into the abdomen. It was concluded that the intestine had been strangulated in the canal with the testis, but had become liberated. The symptoms continued, and the patient died on the fourth day after the operation, of acute peritonitis. At the examination, on turning the distended ilium to the left, a portion of it was found firmly engaged in an opening at the external side of the epigastric artery, the iliac fossa being distended by a tense tumour, which raised up the peritoneum and cæcum. On laying open the parts, a fold of the lower part of the ilium, about a foot long, and almost in a gangrenous state, was found lodged in a hernial sac, the lower surface of which lay on the *fascia iliaca*, its upper and inner part being covered by the peritoneum of the iliac fossa and the cæcum, and its fundus mounting up to the sacro-iliac symphysis. The neck of the sac was nearly annular, admitted the index finger, and closely constricted the pedicle without adhering to it. It was thin, but fibrous-looking, and resisting. Its plane looked almost directly inwards; the lower side being only a short distance from the epigastric artery and vas deferens, which was more posterior. Its upper side was continuous with the peritoneum of the iliac fossa, the anterior with that of the abdominal parietes, and the posterior corresponded to the external iliac vessels. Near its neck, the sac sent a prolongation into the inguinal canal, within which it widened for the reception of the testis, passing out of the ring and descending into the scrotum, where it again dilated to join the hydrocele tumour observed during life. The healthy testis adhered by its upper edge to the posterior and inferior side of the inguinal canal, being elsewhere free.

M. Parise observes that this is quite a different case from a strangulation by the neck of the sac, after a *reduction en masse;* for in fact the intestine having never passed the testis and narrow ring, and being too large to have been lodged in the canal behind the testis, had never been reduced at all. It was not a *bissac* produced by incomplete reduction, as described by Cruveilhier. Obstructed in its passage towards the inguinal canal, the

tumour took a retrograde course towards the internal iliac fossa, detaching the peritoneum lining this. The serous prolongation was, in fact, the tunica vaginalis forced beyond the ring by the water which resulted from the inflamed sac—it being an acute hydrocele, or, as Cruveilhier terms it, an aqueous hernia. This intra-iliac hernia is, indeed, a variety of congenital inguinal hernia, the intestine being primarily engaged in the tunica vaginalis.

M. Parise is not aware of another example of *primary intra-iliac hernia;* but cites a case from M. Demeaux's paper in the 'Annales de la Chirurgie,' tom. v., bearing a considerable analogy to it. Some cases of *incomplete reduction* of hernia also much resemble it, as that related by Cruveilhier, in the first volume of his 'Pathol. Anat.' p. 654. It consisted in a voluminous inguinal hernia, which was reduced, the walls of the abdomen near the ring rising or subsiding, accordingly as the protrusion re-entered or came out. The reduction was believed to be complete: but the patient dying, a small sac was found in the scrotum, and a very voluminous one behind the ramus of the pubes, at the anterior-internal part of the iliac fossa—this second sac communicating with the peritoneal cavity by a narrow orifice which strangulated the intestine.

2. *Ante-vesical Inguinal Hernia.*—M. Parise only knows of two examples of this, one of which occurred to himself. In this, the man was killed by a musket-shot. The omentum, stretched towards the right side, was observed to have entered the herniary opening opposite the inguinal canal. The neck of the sac was placed in the middle inguinal *fossette*, between the epigastric artery externally and the umbilical within. It admitted the middle finger, and was filled with non-adherent omentum. Its plane looked outwards and a little upwards, so that its internal side, resting against the umbilical artery, was thrown backwards. The sac, in place of entering the inguinal canal, was directed transversely inwards in front of the bladder, behind the lower attachments of the recti muscles. Its basis extended beyond the external edge of the rectus of the left side, corresponding to the level of the left external inguinal ring. There was no external appearance of a hernia. The *second* case is related by Hernu, in the 'Recueil de la Soc. de la Méd. 1801,' and was complicated with a strangulated inguinal hernia. At the operation, some non-adherent and healthy omentum was alone found. Three-fourths of this was removed, and the rest returned through the ring. It was, however, projected again, and had to be retained by a pad. The patient, after doing well for a time, died of peritonitis. Besides the sac which had contained the omentum, another was found on the same side, between the pubes and the bladder, containing some peritoneum embracing a fold of intestine—the two sacs representing a *bissac*, having its middle applied over the horizontal ramus of the pubes.

The following are the conclusions which M. Parise arrives at—1. Herniæ which derive their origin from hernia of the groin may become developed within the cavity of the abdomen.—2. These *intra-abdominal* herniæ may be directed into the iliac fossa, in front of the bladder, or within the substance of the broad ligament.—3. They may exist alone, or be accompanied by an inguinal or femoral hernia, constituting a *bissac* hernia.—4. In this last case the development of the intra-abdominal sac may be *primary* or *consecutive*—i. e., the result of an incomplete return of the

extra-abdominal sac.—5. Strangulation may occur at the common neck, or at that of the intra-abdominal sac; the viscera contained in the latter being especially affected.—6. The taxis is of no use for the intra-abdominal hernia. On the contrary, by thrusting back the viscera or serosity contained in the external sac, it increases the compression exerted upon the internal one, and aggravates the danger.—7. We may *suspect* intra-abdominal hernia, when the external hernia does not sufficiently explain the symptoms; when the hernia, though entirely or partly reducible, exhibits a great tendency to re-appear; when the reduction occurs without *gargouillement;* when it raises up the abdominal wall behind the ring; when, in spite of apparent reduction, the symptoms continue; and when these are not relieved, though an operation has detected no strangulation or contraction of the ring.—8. When we suspect such a disposition, we should carry the finger deeply towards the cavity of the abdomen, be well assured that it is really within this cavity, and not in a hernial sac, and direct it on all sides, especially *inwards* and *backwards*, this being the direction in which we should feel the internal wall of an intro-iliac sac, or the pedicle of an ante-vesical hernia. We should also employ traction on the external sac, in order to bring the inner neck towards the aperture, which may be at a greater or lesser distance from it.—9. If in spite of these researches he cannot find the cause of strangulation, the surgeon must act as if he had to do with an internal strangulation properly so called.

M. Gosselin, in reporting upon this paper, cites some additional cases; examples where an internal sac also existed being recorded in 'Arnaud's Traité des Hernies,' ii. 56, and by Fages in the 'Journal de Sédillot,' tom. vii. Examples of *bissac*, in which the internal sac co-exists with the external one, are related by Pelletan, in the 'Clinique Chirurg.,' iii. 359; by B. Cooper in 'Guy's Hosp. Reports, 1839;' by Cock in the same for 1847; and by Teissier in the 'Bulletin de la Société Anatomique, IX année.' These examples of *bissac* do not consist of sacs merely with two dilatations, one over the other, as in the *bissac* of Cruveilhier, but of real double sacs. Of these, two dispositions may occur. Either each sac may have its separate communication with the peritoneum, or such communication may exist only with the inner one, this again communicating with the outer one. Although the cases detailed are not all explicit enough on this point, the first of these conditions is the most rare, as it seems only to have prevailed in B. Cooper's case. The mechanism of the production of these *bissac* herniæ may be explained by one of four modes:—1. According to the view of M. Parise, the external and internal sacs are formed at the same time, by a simultaneous propulsion of the peritoneum in two directions.—2. According to Cruveilhier, Pelletan, and Cock, the repeated efforts to reduce the hernia, at a period when it was not strangulated, have pushed back the neck of the sac from the ring—the intestines, arrested by the pad of the truss, remaining in the interval between the ring and the neck of the sac. The peritoneum, gradually distended, forms an internal sac, separated from the outer by a new contraction occurring at the level of the ring.—3. According to M. Teissier, a hernia, whether strangulated or not, is returned *en masse*, and its partial re-descent outwards gives rise to the *bissac*.—4. The internal sac may have been produced by the reduction *en masse* of one hernia when two existed in the same region.

The existence of double sacs in the inguinal or crural passages is demonstrated by the cases of Arnaud, Lawrence, A. Cooper, Cloquet, and others. Of all these modes of production, the least plausible seems that of M. Parise. The second is that which has received most concurrence, and is that to which most of the cases are probably assignable. The progress of an internal hernia is entirely fortuitous, and does not observe the regularity of an external one, or of that which is implied by M. Parise's appellations, which, however, may be usefully retained, as directing the attention of surgeons to these deviations. The value of his communication does not, therefore, consist in demonstrating new varieties of inguinal hernia; but, joined to other cases on record, and to others yet to be recorded, it may assist in completing the history of internal hernia, hernia *en bissac*, and hernia having a double neck.

XIII. *On Cancer and Cancroid of the Skin.* By M. Lebert.

This is the most elaborate and lengthy paper in the present volume, in which M. Lebert states the results of his more matured experience, and lays down in considerable detail the marks of distinction between these two forms of disease—a distinction, he observes, that has been tacitly admitted, though not acknowledged, by all the classical writers who have left us accounts of their practice. He considers the views of this subject he has already published to have been strongly confirmed by the subsequent researches of Dr. Bennett, of Edinburgh, and of MM. Robin, Broca, and Follin, of Paris. He, however, warmly protests against receiving too implicitly the confident statements of those—the histosophers, as he calls them—who have merely examined specimens of disease presented to them. Himself for many years a provincial practitioner, he lays great weight upon the clinical history of the cases observed; and thus he does not attach the importance to the researches of MM. Bruch and Gorup-Besanez that their reputation as microscopists and analysts would seem to command, owing to the insufficiency of their materials, and the absence of clinical observation. Minute anatomical investigation suffices to determine the histological elements, but clinical observation alone throws light on the pathological value of these.

M. Lebert has now had the opportunity of observing, and bases the present essay upon, about 100 cases of cancer and cancroid diseases of the skin; a fifth of the number being examples of the first of these forms of disease. In noticing so lengthy a communication, we cannot do better than present an amplified summary of the sixty conclusions with which it is terminated.

A. *True Cancer of the Skin.*—1. True cancer by no means rarely attacks the skin *primarily* (as distinguished from examples in which it appears consecutively to cancer of other organs); and M. Lebert possesses 20 cases, of which number 11 affected the face (9 the lips), 7 the external genital organs, 1 the epigastrium, and 1 the leg.—2. Its form is intermediate between scirrhous and encephaloid, inclining to the latter. In two cases the tumours were cancerous in their substance, and cancroid at the surface. In a fourth of these cases the cancer was melanic, a form never met with in cancroid.—3. In the skin, as in other parts, cancer substitutes for the normal structure a new element and true adventitious tissue; while in

cancroid there is only a morbid exaggeration of the normal elements, epidermis being also found in parts where it does not naturally exist.—4. Cancer of the skin is composed of a fibroid stroma, containing, in all its interstices, a soft substance, which is generally infiltrated with a turbid lactescent juice, that is coloured black in melanic cancer, and is sometimes coagulated and gelatiniform.—5. Cutaneous cancer may increase by intussusception, or by the formation of multiple tumours spreading from the seat of the original tumour. Reaching a certain size, the tumour becomes congested, and then erodes the parts which cover it. In cancer of the penis, a fatty infiltration is sometimes observed, of a tubercular appearance, to which M. Lebert has applied the term "phymatoid."—6. The microscope exhibits, besides pale and elastic fibre, well-characterized cancer-cells, complete cells, and especially elliptical nuclei, with large nucleoli. In melanic cancer, the pigmentary element, in the form of granules or little globules, is observed, either external to, or within the cells. The granular or fatty infiltration is of frequent occurrence; hæmatic elements, and effused blood, are rare.—7. Cancer has a marked tendency to infect the entire economy; an infection which, far from confining itself to the primary seat of the disease, exhibits itself in distant parts, whether external or internal, and is notably distinguished from the simply local radiating infection which is observed in epidermic cancroid.—8. Relapse after cancer is not confined to simple reproduction in the same locality. This may occur in distant organs; the disseminated dermatic infiltration ordinarily preceding that of other organs.—9. The seat of the histological deviation exists within the dermis itself. At an early period it projects on the surface, by raising the epidermis. Recent tumours contain cancer-cells exactly similar to those of older and larger growths.—10. The local disease is manifested at first with a deceitful benignity, by reason of the little suffering it gives rise to. After a while, all the painful symptoms observed in cancer of other parts set in, and death occurs at a period varying from six months to two years, sometimes earlier and sometimes later.—11. Of 16 cases in which the *sex* was noted, 10 occurred in men and 6 in women; and of 10 cases of cancer of the face, 7 occurred in men and 3 in women. The mean *age* of the 20 cases was $57\frac{7}{16}$; cases being observed between the ages of 40 (very rarely in younger subjects) and 75.—12. The *causes* of cancer of the skin are as obscure as are those of cancer of other parts. M. Lebert has never been able to trace it with certainty to the operation of local irritation or prior disease. The *prognosis* is always bad, and especially so in melanic cancer, which possesses the greatest generalizing tendency.

B. *Cancroid of the Skin.*—13. Epidermic cancroid is essentially a homœomorphous condition, a disease of the normal tissue, and not the substitution of a new product. This is the capital distinction between it and cancer.—14. In relation to its *seat*, three forms are distinguishable: that occurring in the epidermic layer, properly so called (*epidermic cancroid*), that of the papillæ (*papillary cancroid*), and that which tends to propagate itself by infiltration to the dermis and deeper-seated parts (*dermo-epidermic cancroid*). Of these, the last is the worst form, as under its operation the disease may extend to the muscles, bones, &c. The three forms may be differently combined.—15. There may be observed every degree of the affection, from epidermic excrescence of undoubted

benignity, as simple warts, to cancroid of the worst appearance.—16. Warts are only a group of papillæ surrounded by a common envelope (from six to eighteen papillæ have been counted in a single wart), and increased in size. Their innocent character is explained by their slight vascularity. Cancroid, also, often commences with a small warty tumour; but this at once exhibits a more active circulation, a well-marked vascularity, and a tendency to a much more diffused hypertrophy of the papillæ in the vicinity of the primary tumour.—17. When the imbricated layer of the epidermis is the seat of the hypertrophy, it may acquire a thickness of five or six millimetres or more, the subjacent papillæ swelling and becoming hyperæmic. When the papillæ are the principal seat, they increase notably in size and colour, preserving, when the epidermis is removed, a granulated appearance, the morbid product having a more warty aspect when the epidermis is partly retained.—18. The most important form to recognise, from its greater severity, is that in which the epidermic hyper-secretion becomes infiltrated into the dermis itself, giving it a pale yellow appearance, slightly shining, and dull in places, of little elasticity, fragile, and easily torn. On compression, neither juice nor grumous matter issues, but oftentimes a dry whitish substance, having the consistency of soft paste, and not forming an emulsion with water, as cancer-juice does.—19. If a portion of these infiltrated parts be examined by the microscope, the same epidermic elements are found; young cells, with small nuclei, are observed; others somewhat more developed, the nuclei having some resemblance with those of cancer; and others, again, are undergoing degeneration, whether by drying up or becoming infiltrated with fatty matter.—20. There sometimes exists a peculiar element, found in no other morbid product than cancroid, and termed by the author *epidermic globes;* being round or ovoid bodies, of $\frac{1}{20}$, $\frac{1}{10}$, millim., or more in diameter, formed of a concentric accumulation of epidermic scales, possessing a fibrous appearance at the circumference, but manifesting their cellular nature in the centre. — 21. Epidermic cancroid has a marked tendency to ulcerate at the surface, becoming then covered with crusts, which sometimes assume almost a horny consistency. The pus often contains infusoria, besides the epidermic scales.—22. The epidermis, which is only a simple cellular and blastemic product, and not a true tissue, may become propagated around the primary cancroid (this disposition to infiltration adding much to the danger of relapse when the parts are insufficiently removed). It may thus attack the muscles (3 times in the 77 cases), the bones (5 times), and the lymphatic glands (3 times), of the vicinity. This, M. Lebert considers may take place by direct transport of the epidermis through the eroded lymphatics; a circumstance intelligible enough after the experiments of Œsterlen, exhibiting the transport of solid bodies by the lymphatics, and those of M. Follin, who found molecules of colouring matter in the axillary lymphatic glands after tattooing. This transport of cancroid, always to the glands of the same side of the body, is very different from true cancerous infiltration, which may take place by all the circulating passages, without any limit.

23. It is a capital point in the appreciation of Cancroid, to determine the mode of participation of the entire economy. M. Lebert states most positively that he has never observed secondary cancroid beyond the zone of direct propagation of the primary local disease—i. e., exactly the reverse

of what prevails in true Cancer. In the *whole* of 18 autopsies there was absence of secondary deposits, which are found in $\frac{1}{3}$ or $\frac{2}{3}$ of cases of true cancer, according to the organ affected. Of these cases, 7 were cancroid of the lip, 2 of the face, 4 of the penis, 4 of the vulva, and 1 of the hand. In 12 the disease came to its natural termination, the patient dying in the others in consequence of operations; but in both categories of cases, generalization of the disease was equally absent. Thus comparing Cancroid with Cancer, we observe *analogies* in the ulcerating and invading tendency, the possibility of infecting neighbouring glands, and fatal termination by exhaustion or putrid infection, when the progress of the ulceration is not arrested. On the other hand, we observe *difference* in structure and aspect, and in the absence of general infiltration, with distant and multiple depositions.—24. The *relapse* of cancroid, also, only occurs in the immediate vicinity of the portion of skin primarily affected, or in the neighbouring glands infected by epidermic infiltration. Relapse is therefore rather a continuance of the disease, than a diathesic and constitutional reproduction of it.—25. The epidermic infiltration of the glands may take place either by direct transmission through the broad, short, valveless lymphatic vessels, or possibly by the epidermic blastema becoming absorbed by endosmosis, the cellular formation only taking place afterwards.—26. There are three stages of the epidermic infiltration of the glands observable. In the first, a soft, semi-liquid, yellow, grumous matter is deposited in the middle of the gland; in the second, the matter is more abundant, and more compact, resembling tubercle; and in the third, the tissue of the gland inflames around the matter, and an extending ulcerative process is set up, epidermis being secreted amidst the sanies covering the surface, and the epidermic scales under the crusts differing much from cancer-cells.—27. However extremely destructive these cancroid ulcers may become, their progress is never found to be accompanied with the infiltration of cancerous matter. Cancroid does not degenerate more than any other morbid products.—28. Our chemical knowledge is too incomplete to determine the nature of cancroid; but M. Lebert details the effects of numerous reagents.—29. On several occasions, the spontaneous but partial fall of extensive cancroid of the penis has been observed; and in one case successive crops appeared in this way during some years, and died away, leaving a white cicatrix.—30. The cutaneous cancroid, which commences sometimes by a little warty tumour, and sometimes by a superficial crack, is accompanied by little pain during the first period of its growth. The more diffused it is in its development, the more is a serious extension to be dreaded.—31. During the second period, in which the increase of the tumour is accompanied by inflammation, the pain is more severe. The ulcer takes on an eroding form in the face, and the serpiginous form on a vegetating and hypertrophied base, in the genital organs. Both forms are observed on the lip.—32. The extension of the ulceration, with the sanious and abundant suppuration, may lead to the gradual wasting away of the patient; and when alimentation is impeded by the disease attacking the lip, death may take place more rapidly. The effect of cancroid on the general system is less injurious than that of cancer, and is in direct relation to the extent of the ulceration, and the importance of the organs implicated.

33. The mean *duration* of the cases was $6\frac{1}{2}$ years, the period varying

much according to the part attacked, being $3\frac{2}{5}$ for the lower lip, $3\frac{2}{3}$ for the penis, $8\frac{4}{4}\frac{3}{6}$ for the trunk and limbs, $9\frac{1}{6}$ for the vulva, and $9\frac{7}{12}$ for the face, excepting the lower lip.—34. In regard to *sex* there is no very notable difference; for among 61 cases, 31 occurred in men and 30 in women. Cancroid of the *lower lip* is, however, much oftener seen in men (15 out of 18 cases); while, as regards the rest of the face, female cases predominate (18 in 22).—35. The most common *age* is between 40 and 50; the eroding ulcer of the face being alone found oftenest in the aged.—36. Cancroid does not originate in syphilis, and the supposition of its doing so has arisen from erroneous diagnosis. It is a disease *sui generis*, capable of being excited or aggravated by external causes. Thus, phimosis frequently precedes cancroid of the penis, and the cancroid of the lower lip is frequently induced by the use of short pipes. Although for the operation of external causes a distinct predisposition is requisite, yet these exert much more influence in cancroid than in cancer.—37. The *prognosis* is more favourable for cancroid than for cancer, though very different from that of a benign affection. Cancroid of the *face* is slowest in its progress, especially when the mucous membranes are not attacked. When it invades the conjunctiva, nose, or mouth, the disease becomes far more serious, and its progress more rapid. Complete extirpation may cure it even then; but in some cases, notwithstanding every care, a recurrence takes place. Even in cases which seem incurable, the progress is slower than in cancer, and its continuance is often compatible with good general health (except when the sore takes on an unusual size); and it may be considered in such as truly a local disease, as is an atonic and varicose ulcer of the leg. The prognosis of cancroid of the *lower lip* is more favourable than is that of cancer, as patients are observed perfectly well for years after a first or second operation, the disease not generalizing itself as in cancer. Yet relapse is very frequent, probably because the disease, when diffused, has not been all removed. Such relapses are, however, always local; and the prognosis will probably improve in proportion as surgeons determine to eradicate all traces of the affection, and abandon superficial treatment. Cancroid of the *penis* is in the same condition as regards prognosis as that of the lip, while cancroid of the *vulva* is slower in its progress; and the prognosis of the disease is most favourable when it affects the trunk and limbs. By a variety of quotations from classical writers, M. Lebert proves that the difference in the prognosis in true cancer and cancroid, has been always admitted in practice by surgical authorities, although not stated upon the distinct grounds he himself advances.—38. The *curability* of cancroid is indubitable; but it is too often incurable, or reaches such an excessive extent as to render the prognosis very unfavourable.—39. The *treatment* is chiefly local. Early operation, and never touching the disease, save with the determination of eradicating it, are the primary indications. Excision must be performed in sound parts, both as regards surface and depth. As a general rule, the knife is to be preferred to caustic—the latter being especially applicable to the eroding ulcer of the face. Manec's arsenical paste (arsenic $1\frac{1}{2}$, cinnabar $7\frac{1}{2}$, calcined sponge 4) is the best caustic—a small portion of the sore only being attacked at a time, but the charge returned to with perseverance. Severe pain and erysipelas attend its application; but this

local reaction is one of the conditions of its beneficent modifying power. Other caustics may be substituted; but the weak ones are useless. — 40. The combination of excision and caustics will often effect what neither could do separately.—41. When the disease is too extensive to hope for a cure, palliatives, among which careful dressings, appropriate hygiene, and opiates are conspicuous, can alone be resorted to.

Cancroid of the Lip.—42. The cancroid affected the *lower* lip in the whole 20 cases; while out of 9 cases of cancer of the lip, the lower lip was affected in 6. Sometimes it manifests itself as superficial epidermic hypertrophy: but in most cases the papillæ are affected, whence it may extend to all the deeper structures, as the muscles and bones.—43. In 7 autopsies, the integrity of the internal organs, and of all parts of the economy, beyond the anatomical zone of the labial region, was proven.— 44. It is in the lower lip that the ill effects of local irritation as an exciting cause are best seen, as in the use of short pipes, scratching, &c., and afterwards in the employment of inefficient caustics. One-half the patients who come from the provinces, have only had the nitrate of silver applied.— 45. The especially spreading condition of the disease when seated in the lip, is favoured by the great vascularity of this structure; and owing to this invading disposition, a germ of the disease often exists unsuspectedly in the vicinity of its complete development.—46. The mean duration of life in the cases observed, was $3\frac{2}{5}$ years. The operation had become necessary at a period varying from 6 months to 3 years. Durable cures have been procured after one or more operations, the patients having been watched for 10 years and longer. During the 10 years M. Lebert has employed the microscope in the examination of tumours, he has not met a single case of *true cancer*, in which cancer cells were visible, in which the cure continued durable. Of 18 cases of cancroid of the lip, (15 men and 3 women,) the most frequent *age* at which the disease was observed was, as in cancer, between 45 and 50, it being as seldom met with after 50 as before 45. — 47. The lip must be largely removed, the classic V incision not being too closely adhered to. The engorged glands should be extirpated, as should be any portion of the jaw affected. When the cancroid is attended with superficial ulceration, the arsenical paste may be used, avoiding applying it on the inner side of the lips. After the operation, the patient should be desired to leave off smoking, and to apply early in the event of the disease re-appearing. At this second operation the surgeon should apply caustic after excising all the salient parts of the morbid tissue.

Cancroid of the Face.—48. Cancroid of the face (*Noli me tangere*, chancrous ulcer of the face) exhibits still greater difference from true cancer than cancroid of the lips does. It especially attacks the nose and eyelids; and while in this last it often assumes a papillary and epidermic form, the deep ulcer is oftener observed on the nose. In all cases the disease commences by a warty tumour, which is followed, first by excoriation, and then (perhaps after many years) by ulceration, which may reach a large size without inducing much pain, or sensibly damaging the health. It is not propagated to neighbouring structures, and is a more local disease than is the cancroid of the lip.—49. Its progress is *very slow*, so that patients usually die of other affections or complications; and it is suscep-

tible of cure. The mean duration of 18 cases was $9\frac{7}{12}$ years, the period extending from 18 to 33 years. In one-half of 21 cases, in which the *age* was noted, this was above 65; the mean age being $62\frac{10}{17}$. It especially attacks *females;* as, of 22 cases, 18 were women, and only 4 men. The prognosis is more favourable than in cancer of the lip, as the disease is more localized and is slower in progress; and even when not operated upon, it is consistent with prolonged life.—50. The disease must be energetically attacked by excision and repeated deep applications of caustic.

Cancroid of the Penis.—51. Two-thirds of the cases of reputed cases of cancer of the penis that have come under M. Lebert's notice, have proved to be cases of cancroid. The frequency of phimosis acting as a predisposing cause, is a point in which it differs from cancer.—52. The ulceration which succeeds to the hypertrophied papillæ is usually serpiginous, and the disease may extend to the corpus cavernosum and inguinal glands, which, too, may become ulcerated. In autopsies no infiltration of the economy beyond these glands is discovered; the results of 4 such being adduced by M. Lebert in proof.—53. The progress of cancroid is slower than that of cancer of the penis. It has continued from 15 months to 3 years; and in 3 out of 9 cases it was observed at an earlier age than other forms of cancroid—viz., prior to 40. The true duration of the disease may be hidden by the phimosis; and assistance is often not sought until an advanced period. By prompt operations, cures have been obtained; but when these are resorted to late, relapse is frequent. Except in very superficial forms, excision should be resorted to.

Cancroid of the Scrotum.—54. M. Lebert has never seen this disease attacking the scrotum; and he refers to the English writers for an account of the chimney-sweep's cancer. Bennett's observations upon this confirm his opinion of its cancroid nature, although in some cases it may consist of true cancer. He considers the asserted agency of the soot as highly problematical.

Cancroid of the Vulva.—55. This is usually vegetating and hypertrophic, and rarely puts on the form of eroding ulcer. Commencing in the labium, it may involve the whole vulvo-anal region; and it has usually been mistaken for syphilis.—56. Its progress is usually very slow, varying from 3 to 20 years in the 4 cases observed; and it is of more frequent occurrence during the first half of life, than during the second. If not operated upon betimes, the prognosis is very unfavourable.

Cancroid of the Trunk and Limbs.—57. The back of the hand and the heel are the sites of predilection; and this form is found especially in old persons, so that the mean age in 10 cases was $60\frac{7}{8}$. No example of glandular engorgement was observed among them. The progress is usually very slow; and where this has not been the case, it has often been due to local irritation. The general health is usually unaffected. Amputation has frequently been performed with success in cancroid of the limbs.

Epithelial Cancroid Tumours of Mucous Membranes.—58. These are often observed at the cervix uteri and surface of the tongue. Sometimes they assume the vegetating epidermic, at others the eroding ulcerated form, and in some cases a combination of the two. *Cancroid of the tongue* may infect the lymphatic glands. Cancroid eroding ulcer, frequently mistaken for cancer, is not rare in the digestive canal, especially the

stomach.—59. It is to be supposed that epithelial tumours are sometimes found in serous membranes, everywhere, in fact, where epithelium prevails. M. Lebert has found several small tumours at the inner surface of the arachnoid, consisting entirely of epithelial cells; and M. Robin has observed a similar appearance at the inner surface of the veins of a horse.

Keloid Tumours.—60. Keloid bears some relationship to cancroid, and especially in its disposition to local reproduction after operation. It is constituted of one or more projecting tumours of the skin, which may arise spontaneously, or become developed in cicatrices; and even when it occurs spontaneously, it is reproduced with astonishing facility, and this in the cicatrices which have succeeded to its extirpation. From this disposition to local as distinguished from constitutional reproduction, it may be considered as a link of the chain connecting the different forms of cancroid. Microscopically examined, there are no other elements than hypertrophied dermis found. The dense white tissue presents no cancer-cell, and it is usually composed of a mixture of fibrous and fibro-plastic elements in every degree of development. The juice pressed out of it is transparent, and exhibits only the cells, nuclei, and fusiform bodies proper to the tissue. When the juice and the amorphous uniting material are abundant, the tumour may take on a gelatiniform or fibro-colloid appearance, that has been mistaken for colloid cancer.

Besides the Essays in this volume that are here analysed, there are other papers which do not seem to us to call for more than an enumeration. These are, a case of Urethroplasty, by M. Ricord; a case of Fibrous Tumour of the Breast, by M. H. Larrey; two cases of Hydatid Cysts within the Cavity of the Abdomen, by M. Goyrand, (one of these being a hydatid cyst of the liver discharged by the bronchial and digestive passages, and the other an enormous acephalocyst of the spleen, which was opened externally); a case of Lachrymal Tumour, by M. Auzias-Turenne; and a case of Resection of the Superior Maxilla, by M. Michon, in which the palatine vault and alveolar margin were preserved, and a large osseous tumour was removed from the maxillary sinus. This case, regarded by the author as unique, in consequence of the large amount of the important structures he was enabled to leave behind, is very interesting, and reported in great detail; but it cannot be rendered intelligible by abridgment.

ART. V.

A History of Epidemic Pestilences from the Earliest Ages, 1495 *years before the Birth of our Saviour, to* 1848, *with Researches into their Nature, Causes, and Prophylaxis.* By EDWARD BASCOME, M.D.—*London,* 1851. 8vo, pp. 250.

THE history of Pestilences is almost the history of the world. There exists no people which has not testified by common sufferings to its common origin, and has not proved its brotherhood by sharing the evils which afflict mankind. War, famines, and plagues compose, in many cases, the dreary record of national life. The historian of pestilences should, therefore, know all countries, and should read all languages. To so great a

theme he must bring the energy of a settled purpose, and the labour of a life of thought. Without a great capacity and a firm resolve, he had better not meddle with a subject so impracticable and so vast. It is not every one who has strength enough to scale the Andes, or nerve to measure calmly the vast height of threatening avalanches.

The history of pestilences should not be a barren catalogue of diseases and their dates. The subject loses both interest and utility, if the only point of view is the chronological one. It may make men sadder, merely to learn how often their race has been afflicted, but it can scarcely make them wiser. It gives them no insight into the past, and promises them no guidance for the future.

The great writers on pestilences have considered their subject on various sides. Some, as Noah Webster or Schnurrer, have curiously sought out the great physical phenomena, as earthquakes, volcanoes, comets, famines, floods, &c., which may have been coincident with plagues. The object has been, to see whether from frequent coincidence it could be proved that these pestilences had any connexion with the grand aberrations of nature. Others again, of whom Hecker may be cited as the greatest instance, have endeavoured, by investigation into the habits and customs of a nation, and into the physical circumstances among which it dwelt, to trace the possible influences thence exerted over diseases. Or, rising to the highest point of view, other writers have endeavoured, from a consideration of every circumstance, from noting the vast changes of the physical world, and the apparently minute incidents which diversify man's private life, from the changes of empires and the migration of nations, from the shifting of customs and the versatilities of fashions, to deduce by strict analysis the genesis and spread of pestilences. Mysterious and inscrutable as they seem at first, who can doubt but that the key of the secret really can be found, and that epidemics are but the hieroglyphics which abstruse and hidden, but yet discoverable causes have stamped upon the earth.

The author of the work before us has not, we think, done justice either to his subject or himself. It is almost ludicrous to find the record of epidemics from the earliest times, in a small thin volume, with good broad print. The information is as meagre as the volume. Neither as a chronological record, nor as a comparison of physical changes or of national condition with epidemics, nor as a foundation for a searching induction and generalization, can we praise Dr. Bascome's book. His final chapters are, to our minds, especially distasteful; and with the exception of some general truths, there are few sentences with which we can feel perfectly satisfied. Yet the book bears abundant evidence that its author is a man of no little ability and of much learning, and that, if he had bestowed more time on his subject, he could not but have produced a work which would have been an honour to our national literature.

The book is divided into two parts. The first 183 pages are occupied with an enumeration of the various epidemics, and short notices of the different attendant circumstances. This part is imperfect and sketchy; and although authorities are cited, no reference is made to their works, and we are uncertain whether the quotations are from the originals or from copiers. Almost at random we shall select some passages for comment, which will prove that we have not spoken too harshly of Dr. Bascome's carelessness.

At page 3, Dr. Bascome cites from Plutarch the occurrence of a pestilence at Rome in the year 790 B.C. He has not given the reference to Plutarch, but the date must be a misprint, or a miscalculation. Rome was not built till the year 752 or 754 B.C., most probably at the latter period; and the first pest mentioned by Plutarch occurred in the year 738 B.C. The next recorded pestilence at Rome, in the days of Tullus Hostilius, is given by Dr. Bascome as in the year 694 B.C., the real date being 645 B.C. At page 6, he speaks of the pest at Athens as occurring 435 B.C. The exact date was in the second year of the Peloponnesian war, either in the year 430 or 431 B.C., and 321 or 322 years after the building of Rome. It happens that in this case the date is of material importance, as otherwise the fact is lost, that the breaking out of the Athenian pest coincided exactly with the period when apparently the same disease was at its height in Rome.

At page 9 and 10, several pestilences of the fourth century B.C. are recorded, but our chronology differs so much from Dr. Bascome's, that we recognise them with some difficulty. He omits, however, all notice of a disease which prevailed at Rome, 420 B.C., and was almost universal, but attended with scarcely any mortality, and which has been conjectured to have been influenza. Nor does he mention the widespread disease, which some commentators have put down also as influenza,—and which, if so, was the first known attack,—which had prevailed five years before (415 B.C.) among the Athenian army in Sicily.

We also read—"Annis 393 and 383 B C., the armies of Gaul and Rome were afflicted with sore pestilence." (p. 9.) Here is a singular confusion. The Gauls, after taking Rome, and while besieging the Capitol, were attacked in the former year with a most mortal plague, but the plague of 383 B.C. had nothing to do with them. It broke out most suddenly at Rome, without known cause, and lasted for some years. In the same page, the great plague of Rome, when Quintus Curtius leaped into the chasm, is erroneously put down as occurring in the year 366 B.C., whereas it commenced the year before. This is a trifling error, but in the next page is a mistake so grave, that if it existed only by itself, we should from that circumstance doubt Dr. Bascome's accuracy. He writes—"Annis 332, 296, and 291 B.C., Rome was again visited by pestilence, which was particularly fatal to breeding women and breeding cattle. A similar visitation affected Rome anno 272 B.C." (p. 10.) Now so far was the remarkable pestilence of 331 (not 332) B.C. from affecting women, that it attacked men almost entirely; and this peculiarity so terrified them that they accused the women of causing the plague, and 370 matrons were tried for sorcery and put to death. It was fifty-five years after this (276, not 272 B.C.) that the plague occurred which attacked women and beasts, and produced so many abortions that the Romans thought both men and domestic beasts were about to perish. To confound two such remarkable incidents in a common description, is indeed to make an "olla podrida" of history.

We observe also that the diseases which ravaged the armies of Alexander (about 322 B. C.) are altogether passed over, although several very interesting points should have been noticed, such as the tetanus which so afflicted them in Bactria, and the cutaneous disease which broke out on the banks of the Indus, and which, it has been supposed, was small-pox.

But let us take a description of one of these plagues. Most of them are so briefly referred to, that we might as well have remained altogether in ignorance. But one plague we might have expected that Dr. Bascome would have detailed more perfectly. Has any one ever written on Pestilences, and not paused to quote at length that marvellous description of the Athenian pest, which Thucydides, himself a sufferer, has pictured with the strength and colours of immortal genius? Dr. Bascome dismisses it in a page and a half, and the description of the actual disease occupies only twenty-eight lines, and is said to be drawn, not only from Thucydides, but from Lucretius, who 250 years subsequently gave a poetical description of it, all the main features of which were taken from Thucydides! Dr. Bascome says, that "the pestilence broke out at Athens when the inhabitants of the Athenian territory were crowded together into the city to avoid the ravages of the Lacedemonians." (p. 7.) He does not tell us, what is much more to the point, that the pestilence had ravaged Ethiopia, Lybia, Egypt, many parts of Persia and Italy, and afterwards Lemnos; that it commenced first in the harbours of the Piræus, to which it might have been brought by Egyptian ships; and that, although it was probably aggravated by the crowded state of the city, it did not arise, as has been too frequently stated, after Diodorus Siculus, from the pressure of the siege, as, apart from other evidence, the invaders only remained forty days in the Athenian territory, and then, terrified at the reports of the disease, retreated. The description given of the disease by Dr. Bascome would rend the heart of an historian. We find no mention of the blood-red tongue, of the burning at the chest, and heavy cough; of the great pain in the stomach which often accompanied the bilious vomiting; of the frightful cramp which attended the hiccup; of the reddish hue of the skin without marked heat, of the continual restlessness; and of the remarkable sequelæ—viz., gangrene of the genitals,* or of the hands and feet, destruction of the eyes, or the impairment of the memory which often lasted for so long a time afterwards. Thucydides also mentions that the disease attacked only once—a very important point, which every author who has quoted the historian has noted. As to the symptoms which he has noted, Dr. Bascome does not touch them forcibly enough; he speaks of "insatiable thirst," but Thucydides, to give a lively idea of the torment of the thirst, says, that the sick lay by the fountains, and, if not watched, plunged in. The violent fever is spoken of; but the description of the way in which the sick threw off all clothing and lay naked on the earth, is a mode of illustration which he might well have adopted. The diarrhœa is mentioned; but it is not stated that it was liable to come on after the seventh and ninth days, and killed many, although Thucydides remarks that it was sometimes critical.

In describing this pestilence also, Dr. Bascome has omitted all the attendant circumstances which, in the pages of Thucydides, give so vivid an impression of the horrors of the time. The dread of the disease, the fear of contagion, the unattended sick, the careless burying of the dead, the crowding of the temples with corpses, wild beasts and birds shunning the dead bodies or dying if they eat of them, the loosening of morality,

* Amputation of the penis was apparently performed—

"Vivebant ferro privati parte virili."

LUCRETIUS.

since men deemed that Destiny was accomplished, and that they all must die, and therefore passed in sensual joys their few remaining hours,—all these and other points Dr. Bascome deems of no value, although without such description half the reality of the picture must be lost.

Finally, Dr. Bascome very cavalierly remarks that the symptoms were "analogous to those of the bilious remittent and yellow fever of America and the West Indies." (p. 8.) We do not object to this opinion, which has received the support of Copland; but other hypotheses should at least have been mentioned. Thus the comparison of it with the "Ignis Sacer," which prevailed so extensively from the third to the eleventh centuries, or with the epidemic fever which ravaged Hungary in 1566, and with other diseases of the middle ages, brings out as many points of resemblance as that with yellow fever.

Let us, however, leave the Athenian plague, and take up some other portion of the book. The description of the diseases of the middle ages, and especially those of the fourteenth century, would test the power of any one. Yet, in some measure, the path has been cleared. Hecker's magnificent work on the "Black Death," would at any rate so far save a writer on the history of pestilences, all trouble but that of condensation. But singularly enough, though Dr. Bascome quotes Hecker, and uses the term "Black Death," in one place (p. 50), the grand features of the disease, its course, and principal symptoms, are so vaguely described, that we venture to say that no one, from perusing the account, would have the remotest conception of the actual extent of our knowledge on the point. So, also, after his description of the pestilential years in the middle of the fourteenth century, he passes on to describe, after Hecker, the dancing mania which immediately succeeded. Although he had got the dates before him, he post-dates the commencement of the great epidemic which was ushered in by the wild orgies of St. John's Day, by at least twenty-four years. He says it was evidently the "chorea" of the present day, to which notion he has been led probably by one of the terms given to the dancing mania (viz., "St. Vitus's dance," because it was cured by invocations to St. Vitus), and which term, lasting beyond the disease for which it was at first used, was applied by later medical writers to a disease which they fancied somewhat resembled the dancing plague. Another instance of singular oversight and carelessness occurs in this page (p. 56). After stating that the dancing mania began in 1374, he writes, "the disease also prevailed in France, and the sufferers were called 'convulsionnaires.'" The enormous anachronism of nearly 400 years is thus committed, since the sect of the convulsionnaires arose from the repeated miracles performed in Paris at the tomb of the Jansenist Paris, who died in 1724,* and the miraculous powers of whose ashes were not discovered till six or seven years afterwards. The 'convulsionnaires' have indeed an intimate medical connexion with the dancers of St. John, as in both cases it was a disease engendered by superstition, fraud, imitation, and communicated impulse,† but there is no other connexion between them.

If, leaving the first part of Dr. Bascome's book, we pass to the second,

* We quote the date from memory, but we are right within a year or two.

† We have used this term in the same sense as that of "suggestive idea," which has been lately employed.

on the nature, causes, and prophylaxis of epidemics, we find ourselves equally at issue with him. Here, however, we are dealing with matters of opinion, and Dr. Bascome may think his dictum as good as ours. But we cannot believe that many English practitioners hold opinions so vague and unsatisfactory. The "nature and causes of epidemic pestilences" is the title of the opening chapter of this part; but after an attentive consideration of it, we are quite unable to say what Dr. Bascome's meaning may be. But, so far as we can make out, he considers that the usual distinctions between various epidemic diseases, such as yellow fever, cholera, &c., are erroneous, and that all these presumed separate affections should be classed under the one head of "pestilence."

"We now hear," writes Dr. Bascome, "pestilence called plague in Egypt, yellow fever in America and elsewhere, bilious remittent and intermittent, and also yellow fever in the West Indies, and typhus or nervous fever in Great Britain; we read also of the same epidemics, which the ancients called pimples, pustules, apostunes, and gangrenous sores, now being called distinct and confluent small-pox, carbuncles, &c." (p. 188.)

But Dr. Bascome would not only call them by their old names, (if old they are, for we never heard before of an epidemic of pimples,) but evidently thinks that the distinctions implied by the several names are worse than useless. He speaks "of the wisdom and superiority of the arrangements of our predecessors, when compared with the confusion and more than uselessness of many of the nosological distinctions and classifications made since the days of Hippocrates." (p. 186.) And then just afterwards we have an instance of the "superior arrangements" he alludes to, since he tells us (though where he got the information from he does not say) that the ancients "classed all pestilential epidemic distempers under one general head or term—viz., pestilence, plague, or fever; under the head of consumption they noted all chronic diseases; and boils, scabs, pustules, blotches, carbuncles, &c., were included under that of skin diseases." (p. 187.)

If the distinguishing and essential point about a disease is simply the fact that it affects many persons at once, then we believe that Dr. Bascome is right in putting yellow fever, ague, typhus, cholera, and bubo-plague, &c., under one head; but if the characters of a disease are to be drawn, and if differences in causes are to be inferred, from the several constant effects produced on the human frame, then we need not say that Dr. Bascome's heterogenous classification appears the most singular jumble of causes and effects which can be conceived. This notion is not, however, a new one. It has been argued by several speculative writers, that the several forms of disease (which are generally considered as so many entities with specific peculiarities of origin, progress, and manifestation) are merely various forms of the same vague general disease, which presents itself now in one, and now in another shape. This opinion has never made progress, and probably never will, for it is contradicted by the first steps of inquiry, and can only be maintained by the most flagrant disregard of elementary facts.

In the next chapter, on the "causes of epidemic pestilences," epidemic diseases are attributed to atmospheric vicissitudes, such variations of temperature and of electrical conditions acting upon bodies predisposed to disease from defective sanitary conditions, such as want of light, impure

air, a scanty diet, and the "irregular and artificial life of man in a state of civilization." Dr. Bascome puts aside at once any notion of a specific material cause, even in the case of marsh diseases. To the obvious objections that epidemic diseases, as we see them, do not glide into each other, but are always distinct and easily distinguishable, and have been proved to have been so in some cases for many centuries, Dr. Bascome must, we presume, believe that he has already sufficiently replied by anticipation, when he made the assumption of the unity of "pestilence." To the next argument, which at once arises, that a specific material cause must be assumed for many epidemic diseases, because they are obviously transmitted from diseased persons to those near them, Dr. Bascome replies by making a series of assumptions, which are either expressed openly, or which naturally flow from the premises, and which we may express as follows.

It cannot, of course, be denied that some diseases, such as small-pox, being incontestably propagated by contagion, must have a material and specific cause, capable of transference; but Dr. Bascome separates all such contagious diseases from epidemics, although why this title should be denied to them we cannot understand. He then denies that the remaining diseases which compose his class of epidemics, or rather make less his conglomerate of "pestilence," such as cholera, yellow fever, influenza, continued fever, intermittents and remittents, are ever contagious. The argument on which he rests a conclusion so contradicted by facts, and especially by recent accurate investigations, is of a description which, without offence, we must take leave to call flimsy. Seven pages dismiss the question, and several of these have really no bearing on the point. As a specimen of this argument, we may quote one page out of the seven, which contains what Dr. Bascome evidently thinks the strength of his position.

"But that which I would urge in support of the NON-CONTAGIOUSNESS of epidemic pestilences, irrespective of every other authority, is the remarkable fact, that in our most ancient medical treatise, the thirteenth chapter of Leviticus, no mention whatever is made of epidemic diseases being reckoned contagious, although at the time when the Levitical code was being propounded there was no lack of experience in epidemic diseases; for in the days of Moses the times in Egypt were calamitous indeed—pestilence and famine ran riot through the land. Had epidemic diseases, then so common and lethal in Egypt, been considered contagious, the presumption is, that they would have been enumerated as such among those which were specified in character—viz., leprosy, scabies, lues,* &c.; and when we observe such minuteness displayed in the Mosaic ordinances to the very freeing of houses from damp previously to occupation, we cannot suppose that precautionary directions, as regards such universal and lethal maladies as epidemics, would have been omitted. With this remarkable fact before us, derived from sacred authority, I feel at a loss to conjecture the ground on which the idea of contagion is *at all entertained*, more especially as we have the occurrence of pestilential diseases not only foretold, but their very nature and mode of production positively conveyed to us from the same Divine source.

"The Lord shall smite thee with a consumption, and with a fever, and with an inflammation, and with an extreme burning. (Deut. chap. xxviii.) And it shall become small dust in all the land of Egypt, and shall be a boil breaking forth with blains upon man and upon beast, throughout all the land of Egypt. (Exod. chap. ix.) But the hand of the Lord was heavy upon them of Ashdod, and he

* What is meant by lues? Where is scabies mentioned?

destroyed them and smote them with emerods (violent dysentery), even Ashdod and the coasts thereof. (1 Sam. chap. v.) I also will do this unto you; I will even appoint over you terror, consumption, and the burning ague, &c. (Levit. chap. xxvi.) I will smite the inhabitants of this city, both man and beast, they shall die of a great pestilence. (Jeremiah, chap. xxi.) The sun and the moon standing still in their habitations, the mountains trembling, the waters overflowing causing famine and pestilence. (Habakkuk, chap. iii.) Behold, I will send a blast upon him. (2 Kings, chap. xix.) The Lord shall make the rain of thy land powder and dust. (Deut. ch. xxviii.) Thy heaven shall be brass and the earth iron. (Ibid.) And if the family of Egypt go not up, and come not, that have no rain; there shall be the plague, &c. (Zechariah, chap. xiv.)"—(pp. 213—14.)

Œdipus himself could not understand how these extracts are intended to bear on the position they are quoted to support. To us the whole thing is unintelligible, and we can only consider it as an entire misapplication of scripture truths and lessons.

Dr. Bascome does not consider it necessary to enter into the question at greater length, and we may very safely follow his example.

The last chapter in the book, the "prophylaxis" of pestilence, consists merely of the well-known sanitary rules as to the influence of light, air, water, &c.

We have now finished a review which it has given us no pleasure to write, for we would much rather, at any time, set about discovering excellencies than defects, and new truths than ancient errors. But on a subject so vast and important as that of epidemic diseases, if facts are inaccurately stated, and inferences illogically drawn, we have no choice except to abdicate our critical chair, or to express our dissent. We recommend Dr. Bascome, in all friendliness, to go again over the subject; to collect his facts with more care; to collate the accounts which we possess; to observe how these have been considered by the laborious and thoughtful writers who have written on epidemics; and when he has got his materials well before him, then, and not till then, to form his conclusions. He has either grudged the immense labour necessary for this, or he is not endowed with the accuracy and care requisite for such an inquiry. But unless he is prepared to investigate epidemics with a perseverance that knows no lassitude, and an acuteness that knows no failure, he should leave the subject altogether alone. He is evidently a man of talent; and we would hope that when we next meet with him, he may have remedied the defects of which we now complain, and may give us something in which we may be able to agree as frankly as in the present instance we have been compelled to differ.

ART. VI.

1. *A Compendium of the Law of Coroners, with Forms and Practical Instructions.* By JOSEPH BAKER GRINDON, Esq., Coroner of Bristol. —*London*, 1850. Fcp. 8vo, pp. 215.
2. *A Practical Compendium of the Recent Statutes, Cases, and Decisions affecting the Office of Coroner; comprising the New Enactments relating to the Poor, Police, Registration, General Board of Health, Removal of Nuisances, Prevention of Disease, Sewers, &c.; and the Orders and Regulations issued thereunder respectively. Also the Laws and Decisions in Relation to Burial Clubs, Riotous Assemblies, Steam-boat Navigation, Railroad Travelling, Collisions, &c. And the Explosions of Steam-boilers, Mines, and Gas; and Cases on Poisons; with Precedents of Inquisitions and Practical Forms.* By WILLIAM BAKER, Esq., one of the Coroners for Middlesex.—*London*, 1851. Fcp. 8vo, pp. 702.

IT is very probable that the titles of the above works will, at first sight, lead some of our readers to imagine that we have gone far out of our proper course, in order to notice matters which do not strictly belong to the medical profession; but we are quite sure that a little reflection will convince them that the duties of the coroner and medical practitioner are so frequently brought into juxtaposition, that a general acquaintance with the powers and functions of the one, cannot fail to be of value to the other. That this is true in respect of ourselves, is evidenced by the fact, that few indeed of our professional brethren are aware how they can best acquit themselves in the coroner's court; and hence it is, that bickerings and unpleasant altercations are frequently taking place, not only between the coroner and medical witnesses, but also among the medical witnesses themselves. If it were necessary to illustrate the truth of this by the citation of actual occurrences, we could easily do so by referring to the medical journals of the last few months; whereby it might be shown that the dignity of the coroner's court and the respectability of our own profession are both seriously damaged by the legal inexperience of those who appear therein as the principal witnesses.

If there were no other reason for undertaking a review of the volumes before us, we should, in our opinion, be justified in noticing them, on account of the principal fact, that in so doing we should be able to illustrate, in a very general way, the most important points connected with the law and practice of the coroner's court; but in addition to this, there are other circumstances which render these works acceptable to the medical public. Mr. Baker's volume, for example, contains all the new enactments relating to the poor, police, registration, general board of health, removal of nuisances, prevention of disease, &c.; together with all the orders and regulations issued thereunder respectively. So that it is hardly necessary for us to make further apology for discussing matters, which are, in the true sense of the word, medico-legal.

Nevertheless it would be altogether out of place were we to undertake a critical examination of these works; or even were we to follow out the

various judicial debates with which each of the arguments discussed in them is beset; for such a duty belongs entirely to the forensic journalist, and in our capacity of medical reviewers, we shall confine ourselves to those facts which relate to the duties and power of the coroner, in so far as they affect the interests of the medical profession. We may, however, inform those who wish to obtain further instruction on the subject, that they can do so by consulting the work of Mr. Jervis, now Sir John Jervis, Knt., and Lord Chief Justice of the Court of Common Pleas, 'On the Office and Duties of Coroner,' and also that of Mr. Richard Clarke Sewell, which is entitled 'A Treatise on the Law of Coroners.' The former publication appeared as far back as the year 1829; and although it is somewhat ancient in respect of the statutes referred to, yet it contains the basis of all that is worth knowing on the subject of "Quest law." Mr. Sewell's book was printed at a much later period—namely, in the year 1843—and it has the advantage of containing later enactments, and also of possessing a good summary of the facts of medical jurisprudence. Both of those volumes are regarded in the light of standard authorities; and hence, says Mr. Grindon, "I have no hope of improving on existing publications, except by adding the more recent alterations in the law, and offering that moderate amount of information which experience has given me." His object, therefore, has been, to provide a portable, as well as useful book on the *practical duties* of a coroner. Mr. Baker, also, acknowledges that he has endeavoured, in compiling his work, to interfere as little as possible with the publications already in print, but rather to furnish a supplement to them, so that coroners and others might thereby become acquainted with the new duties which have arisen under recent acts of parliament. We may add, moreover, that both of these volumes contain a great deal of valuable information in the shape of precedents, forms, examples, &c.

The term *coroner*, or *coronator*, is derived from *corona*, on account of the circumstance that the coroner chiefly had to do with the pleas of the crown. In the reign of Richard I. he was styled *coronarius*, but in *Magna Charta*, and subsequent statutes and law books, he is denominated *coronator*, or *custos placitorum coronæ*, because he originally had the custody of the rolls of the pleas of the crown. In Scotch law, before the destruction of his office in that country, he was called "*crowner*," and that is his vulgar appellation at the present time.

The origin of the office is very ancient, so much so, that it is lost in the darkness of antiquity. By some it is said to be coeval with that of sheriff, and to have been ordained with the latter office, to keep the peace when the earls gave up the wardship of the county; but this is doubted by others, and, as Jervis says, with reason, notwithstanding the many great authorities to the contrary; for, according to Sir Edward Coke, the sheriff was more ancient than the division of England into counties by King Alfred, and existed in the time of the Romans in this country as an officer of the consul, at which period we find no allusion to any officer whose duties corresponded with those of the coroner. But, whatever may have been the commencement of the office, it is evident that coroners existed in the time of Alfred, for that king punished with death a judge who sentenced a party to suffer death upon the coroner's record, without

allowing the delinquent liberty to traverse. This officer is also mentioned by Athelstan in his charter to Beverley. (Jervis, p. 2.)

There are three kinds of coroners at present in existence—namely, 1st, *Those who are coroners by virtue of their office*, as, for example, the lord chief justice of the court of Queen's Bench, who is supreme coroner over all England; the puisne judges of that court, who are also sovereign coroners; and there are certain officers of franchises who have, by a similar right, the power to execute this function.—2nd. *There are coroners by charter, commission, or privilege.*

"Thus the mayor of London is by charter coroner of London; and the Cinque Ports, from their great antiquity, have their own coroner; the dean and chapter of Westminster have their own coroner, who, by their appointment, is coroner for the city and liberties of Westminster. So, likewise, the bishop of Ely has power by charter to make coroners in the Isle of Ely; and in the Stannaries in Cornwall the wardens are coroners. The master of the Crown Office, or clerk of the Crown, is coroner of the Queen's Bench, and has jurisdiction over matters arising within the prison of that court and the Marshalsea. He holds his office by letters patent under the great seal. In addition to which there are many exclusive jurisdictions, and corporations for which coroners are appointed. But the two principal jurisdictions over which, by the Queen's grant, coroners may be appointed, are those of the Verge and of the Admiralty." (Jervis, p. 4.)

The former is sometimes styled the coroner of the queen's household, though his common designation is the coroner of the verge; he holds inquests within the palace, and has jurisdiction, in company with the county coroner, over a district of twelve miles around the court. By the statute of Henry VIII. (33, c. 12) this appointment is settled in perpetuity in the lord steward, or lord great master of the king's or queen's household for the time being. "The coroners of the Admiralty have jurisdiction in cases of death out of great ships in the main streams below the bridges near to the sea, but the county coroner has concurrent jurisdiction in great rivers within the bodies of counties, or where a man may see from side to side; and the statute does not extend to deaths in small vessels. In those cases the coroner who is first at the inquiry may take the inquisition, and the other has lost his opportunity." (Grindon, p. 2.) Finally, there are coroners of boroughs who are elected in accordance with the regulations of the Municipal Corporation Reform Act. (4th and 5th Will. IV. c. 76.) 3rd. *There are coroners by election*, who hold office under a statute which was passed in the third year of the reign of Edward I. These functionaries are denominated county coroners; they are elected by the freeholders of counties, and have jurisdiction only in the county or shire in which they are appointed.

At one time the duties of the coroner's office were very important, so much so, that no man was permitted to hold it who had not, at least, a knight's degree. In later times, however, the authority of the sheriff has encroached very considerably on the prerogative of the coroner; and, to use the words of Jervis,

"We may now look in vain for the individual, who, in the quaint language of Chaucer,

> 'At sessions ther was he lord and sire,
> Ful often time he was knight of the shire,
> A schreve hadde he ben, and a coronour,
> Was no wher swiche a worthy vavasour;'

for the office, whether in consequence of the rust and relaxation inseparable from ancient institutions, or of the inefficiency of its officers, has fallen from its pristine dignity into the hands of those who are in some instances incompetent to the discharge of even their present limited authority." (Jervis, p. 5.)

Sir William Blackstone, in his 'Commentaries on the English Law,' has made a remark to nearly the same purpose; but it must be remembered that a great and salutary change has been effected in the office of coroner, within the last twenty years, insomuch that it is now generally held by men whose respectability is unimpeachable. At the time that Blackstone wrote—namely, in the year 1765, it was a notorious fact, that the duties of coroner were performed by low, indigent, and corrupt men, who filled the office, not for the honour of the appointment, but merely for the sake of the perquisites which it gave them. This circumstance is rather surprising, when we consider that the statute of Westminster, which defined the high qualifications for the office, was, and still is, unrepealed; that statute was made in the fourteenth year of the reign of Edward III., and, although it did away with the absolute necessity for a knight's degree, yet it ordained that the candidate should have sufficient land and estate to take up that degree in case he was required to do so. It was likewise provided, that none should fill the office who had not sufficient land in the county to answer all people; and that in case the county elected an officer who was deficient in this respect, it was bound to pay his fines for him for having appointed an insufficient officer; in fact, there are instances of coroners having been dismissed from office on account of their poverty, and their having entered on the trade of a common merchant.

At one time the functions of the coroner were numerous. Coroners were magistrates and conservators of the king's peace; they held inquests of death; inquired of all kind of felonies, of treasure-trove, of royal fishes and wrecks; they recorded appeals of felony; proclaimed outlawry; executed process; took the confession and abjuration of felons; and, in fact, performed most of the duties which now devolve on the sheriff. It would appear, indeed, that formerly the sheriff was an inferior officer, for it is directed in ancient statutes, that he and his bailiffs are to attend upon the coroner, and to be obedient to his commands. Now, however, the duty of the coroner is so far reduced, that practically it is almost confined to inquests of death; though in law it extends to the following points—namely, to hold inquests on the bodies of persons dying by violence, whether the deaths have occurred accidentally or otherwise; to inquire into all cases of sudden death which have happened under circumstances of suspicion; and to hold inquests upon all persons who die in prison, or in lunatic asylums; to prepare and make return of his inquisitions; to apprehend persons charged with murder or manslaughter; and in the end, to bind over prosecutors and witnesses, according to law, to appear and give evidence in another court at the future trial of the accused. It is his duty, also, to inquire of treasure-trove, and to make a return of the same; to inquire of wrecks of the sea, of sturgeons, and of whales (royal fishes); and to attach and let to mainprize the finders, and to secure the finding to the queen's use; to execute the queen's writs, in case of disability on the part of the sheriff; to pronounce judgment of outlawry; to attend at the trial of prisoners charged upon his verdict; and to deliver into court to the

proper officers at the proper time all inquisitions of manslaughter, murder and *felo-de-se*.

One of these duties—namely, the inquest of death—concerns us so materially, that we shall make a full inquiry into the manner in which it is performed, and by this means endeavour to solve a few of the more important questions which naturally spring out of it.

1st. It might be asked *in what cases are inquests of death to be held?*—According to the statute of Edward I., the provisions of which are in operation at this present time, it would appear that the inquiries of the coroner are to be limited to the death of persons slain, drowned, or *suddenly dead;* but as this statute is merely directory, and in affirmation of the common law, it does not excuse him, or restrain him from the performance of any part of his duty which is incidental to his office at common law. It is proper, therefore, that the coroner should hold inquests on the bodies of all persons who die in prison, in order that the public may be satisfied that such persons came to their death by the common course of nature, and not "*per dure gard.*" "It would also seem," says Mr. Grindon, "that the same principle ought to apply in cases of persons dying in a lunatic asylum; but it is not customary to hold inquests in those cases, unless there be suspicion attached to them.

A very slight acquaintance with the statutes of Edward will show that it is the coroner's duty to inquire into every case of death, which has *manifestly* arisen from violence or unnatural cause. In fact, the statutes were framed for the purpose of protecting human life, and of putting a check to the then common crime of unjustifiable homicide, consequently it is the bounden duty of the coroner to satisfy the public mind on all such occasions, as to whether that violence was the result of accident or design. There are cases, however, in which it is difficult immediately to determine whether the death has been natural or not. These are the cases which come under the denomination of *sudden death*, and at the present time it is doubtful what construction ought to be put on these words of the statute. The coroner, who perceives in this mode of dying the operation of an unnatural cause, has ever been disposed to give them their fullest signification, and to hold inquests on the bodies of all persons "who are suddenly dead." On the other hand, the magistrate, who is the custodian of the public purse, sees in this liberal interpretation of the words a medium of gross and extravagant expenditure; he therefore endeavours to abridge the meaning which the coroner has given to them, and to show, from the very construction of the act, as well as from the nature and object of a coroner's inquiry, that inquests ought not to be held unless there is reasonable ground for supposing that death has been occasioned by violent or unnatural means. This, in fact, is the view which most of our legal authorities entertain on the subject. Jervis, for example, says—

"The dying suddenly is not to be understood of a fever, apoplexy, or other visitations of God, and coroners ought not in such cases, or in any case, to intrude themselves into private families for the purpose of instituting inquiry, but should wait until they are sent for by the peace-officers of the place, to whom it is the duty of those in whose houses violent or unnatural deaths occur, to make immediate communication whilst the body is fresh, and, if possible, whilst it remains in the same situation as when the person died. But, under whatever circumstances, this

authority must be exercised within the limits of a sound discretion; and unless there be reasonable ground of suspicion that the party came to his death by violent or unnatural means, there is no occasion, except in the case of a person dying in jail, for the interference of the coroner. In fact, coroners have on several occasions been censured by the court of King's Bench for holding repeated and unnecessary inquests, for the sake of enhancing their fees, where there was no reasonable probability that the deaths occurred from violence or unnatural causes." (Jervis, p. 24.)

In the case of *The King* v. *the Justices of Kent*, where a mandamus was applied for to allow an item in the coroner's account, the chief-justice, Lord Ellenborough, not only refused the application, but said, "There are many instances of coroners having exercised their office in the most vexatious and oppressive manner, by obtruding themselves into private families, to their great annoyance and discomfort, without there being any pretence whatever of the deceased having died otherwise than a natural death, which proceedings are highly illegal." Acting upon this and other subsequent decisions of a like character, the magistrates of counties have refused to allow the coroner's expenses in all cases where there was not sufficient ground for believing that deaths had resulted from unnatural causes; and we perceive, by the report of a special committee appointed by the Middlesex magistrates, in the month of October, 1850, to inquire into the duties and remuneration of coroners, that the justices have actually gone so far as to demand the particulars of the preliminary information on which the coroner issued his warrant or precept; and they have further resolved not to pay for any inquest where there was not a sufficient reason for supposing that death had been occasioned by some criminal act or omission. But who, let us ask, is to make these inquiries, which, by the way, are tantamount to an inquisition, without the solemn sanction or obligation of an oath? Already it is found that the coroner's constable is unable or unwilling to do so; and it would be highly imprudent, and contrary to all precedent and principle, for the coroner himself, the judge of the court, to engage in them. How could he, even supposing that he were the most unbiassed of mortals, keep his mind clear from prejudice in the conduct of a case which he himself had previously investigated; or how could he, as Mr. Baker remarks, with any consistency or propriety, caution the jury to abstain from inquiries out of doors, when he himself had been vitiating the injunction by prying into the facts, and gathering the crude and ill-digested statements of persons interested in the inquiry? No! If the coroner's court is to be reformed, it must not be done in this manner; and we may say, with *The Times*, which has already commented on the subject, "that such an absurdity is too patent to pass current for a moment, and that, whatever may be right, the proposition of the Middlesex magistrates is undoubtedly wrong."

But, to go back to our question, it is manifest that there is a great difficulty in giving a correct interpretation to the words "suddenly dead," and it is unfortunate that subsequent statutes have not tended to remove the difficulty. In the report of a Select Committee of the House of Commons, appointed to inquire into the measure, which had been adopted for carrying into effect the provisions of the Act 1 Victoria, c. 68 (which Act relates to the expenses of coroner's inquests), and also to inquire concerning the proceedings of the justices of the peace in relation to the

office of coroner in that county, it is said that so much uncertainty remains attached to the interpretation of the word *duly*, which occurs in the Act of 25 Geo. II. c. 29, authorizing the magistrates in quarter sessions assembled, to pay the coroners for all inquests duly taken, that in their judgment a clearer exposition of the term is desirable. Of the truth of this statement there cannot be a doubt, for the differences which it has created in the minds of coroners and magistrates have been attended with most mischievous results. Already it is known that coroners are disinclined to hold inquests in doubtful cases, for fear of their expenses being disallowed: such conduct cannot but foster the crime of secret murder. Indeed, this fact has been made the subject of comment by a home secretary in the House of Commons; for at the time of the Essex and Norfolk poisonings, Sir James Graham said, "there was reason to believe that in the county of Norfolk no fewer than twenty persons had died from poison administered by one individual, and in none of these cases had an inquest been held." He likewise informed the House, "that the magistrates in the county of Devon had even gone the length of passing a resolution not to pay the cost of any coroner's inquest where the verdict was, 'Died by the visitation of God.'" But let us for one moment reflect on the consequences of this ill-judged parsimony. Is it not a remarkable fact, judging from the parliamentary returns of the cases tried in the United Kingdom during the last ten years, that among the several counties of England there is a direct relation between the parsimony of the magistrates and the perpetration of that worst of crimes, secret murder? And does not this fact admit of explanation, when we consider that the coroner is really the magistrate of the poor, and that he is, strictly speaking, the conservator of their life and limb? "If," to use the language of *The Times*, "the rich man be on his deathbed, he is surrounded by relatives and friends; physicians must be called in; his condition from day to day is the theme of twenty mouths, and the occupation of twenty minds. But when life's fever is nearly at an end with the poor man, either by the process of natural disease, or because some secret hand has mixed poison in his dish, and the medicine has completed what the food had begun, who is to mark the event? What hand is near to wipe the death-sweat from his brow, but the one which in all probability laid him low in the pride of his manly strength?" And who is to expose the deeds of such an inhuman murderer, but the coroner and his jury? Look, again, at the influence which such inquiries have over the dark deeds of the workhouse and the prison, the madhouse, the factory, and, in short, the thousand other places where the life and health of the poor are but sadly cared for; and then let us ask whether it is prudent or merciful to weaken the power of so useful an officer.

2nd. It is proper to consider *in what manner the coroner's inquest of death is conducted.*—This is in great part regulated by the statutes of Edward I., and chiefly by the statute of the fourth year of that monarch's reign (st. 2), which orders that the coroner, on receiving information of a sudden or violent death, shall immediately go to the place where the slain, the suddenly dead, or the wounded, is lying, and shall forthwith command four, five, or six from the next towns, to appear before him in such place; and when they come thither, the coroner, upon the oath of them, shall inquire in this manner—that is, to wit, if it concerns a man slain,

whether they know where the person was slain; whether it was in any house, field, bed, tavern, or company, and if any, and who, were there; and so forth. The statute likewise directs that the inquiry shall relate to the persons actually concerned in the felony, and, if the death have been accidental, to the thing which occasioned it, so that it be forfeited as a deodand to the state. Subsequent statutes have, however, modified these proceedings, and at the present time it is customary to conduct the inquest in the following manner:—

The coroner having received the notice of death, causes inquiries to be instituted, in accordance with a certain form, in order that he may ascertain whether the death has resulted from violence, or is attended with circumstances of suspicion. Finding that it is so, and that an inquest must be held, he issues his precept or mandate to the constable or appointed officer of the place where the body lies, to summon a jury of at least twelve good and lawful men from among the householders of the district. On attending at the place appointed for the inquest, the coroner demands his warrant of the officer who is charged with its execution. He ascertains that at least twelve of the persons summoned are present, and then desires the officer to open the court, which he does in the following words:—

"Oyez! Oyez! Oyez! All manner of persons who can give any information on behalf of our sovereign lady the queen, touching the death of A. B., draw near and give your attendance."

And then he proclaims—

"You good men of the county (city or borough) summoned to appear here to-day to inquire, for our sovereign lady the queen, when, how, and by what means A. B. came by his death, answer to your names as you shall be called, every man at first call, under the pain and peril that shall fall therein."

The names of the jury are then read over, and a foreman, usually the first man on the list, is chosen; on which the coroner requests them to proceed with him to view the body. This being done, and the body identified, the coroner administers an oath, first to the foreman, and afterwards to the jury, as follows:

"You shall diligently inquire, and true presentment make, of all such matters and things as shall be here given you in charge on behalf of our sovereign lady the queen, touching the death of A. B. here lying dead. You shall present no man from hatred, malice, or ill will, nor spare any through fear, favour, or affection, but a true verdict give according to the evidence, and to the best of your skill and knowledge. So help you God."

In the case of dissenters, they are affirmed in the manner most binding to their consciences.

At this stage of the investigation, the coroner directs that all but the jury shall retire, and he proceeds to examine the body and make such comments thereon as may be of use to the jury in their future deliberations. The coroner and jury then retire to some neighbouring tavern, or other convenient place; and the constable having proclaimed that all persons who can give evidence in the matter are to come forward and they shall be heard, the jury proceed with the inquiry and hear the evidence.

Each witness, on his appearance, is sworn to tell the truth, and nothing but the truth. His testimony is written down by the coroner, and then

signed by the witness. If the inquiry can be benefited by delay, the inquest is adjourned. This is done in a formal manner; and on the re-assembling of the jury the court is again opened by proclamation, the names of the jury are read over, and the new witnesses called. When the evidence is completed, the coroner sums up; he directs attention to the main facts of the case, expounds the law of it, and requests the jury to consider their verdict. If the case be likely to involve the liberty of any person, the public are now requested to withdraw until the deliberations of the jury are at an end; after which the court is again opened, and the verdict is publicly announced. The coroner then draws up the inquisition in accordance with the finding, and having read it aloud, he and the jury sign it. If it so happen that the verdict charges any individual with homicide, the coroner immediately issues his warrant for the apprehension and committal of the person so charged; and he binds over the witnesses, by recognizance of 40*l.* each, to appear at the next court of oyer and terminer, or jail delivery, or superior criminal court of a county palatine, or great sessions, where the trial is to be, then and there to prosecute and give evidence against the party charged. And lastly, when the inquiry is at an end (or before if necessary), the coroner gives his warrant for the burial of the body, makes out a form for the registrar, and then dismisses the jury.

3rd. It may be asked whether *a person is bound to give his attendance at a coroner's inquest, when he is duly summoned thereto by the proper officer of the court.*—To this we reply that he is, or he exposes himself to a fine. In the case of jurymen and ordinary witnesses, the statute of 7th and 8th Vict. c. 92, sect. 17, enacts,

"That if any person having been duly summoned as a juror or witness to give evidence upon any coroner's inquest, as well of counties, cities, and boroughs, shall not, after being openly called three times, appear and serve as such juror, on such inquest, every such coroner shall be empowered to impose such fine upon every person so making default as he shall think fit, not exceeding forty shillings; and every coroner shall make out and sign a certificate containing the name and surname, the residence, and trade or calling, of every such person so making default, together with the amount of the fine imposed, and the cause of such fine, and shall transmit such certificate to the clerk of the peace for the county, riding, division, or place in which such defaulter shall reside, on or before the first day of the quarter session of the peace then next ensuing, and shall cause a copy of such certificate to be served upon the person so fined, by leaving it at his residence twenty-four hours at the least before the first day of the said next quarter session of the peace; and every such clerk of the peace shall copy the fine or fines so certified on the roll, on which all fines and forfeitures imposed at such quarter session of the peace shall be copied, and the same shall be estreated, levied, and applied in like manner, and subject to the like power, provisions, and penalties in all respects, as if such fine or fines had been part of the fines imposed at such quarter session: provided always, that nothing herein contained shall be construed to affect any power now by law vested in the coroner for compelling any person to appear and give evidence before him on any inquest or other proceeding, or for punishing any person for contempt of court, in not so appearing and giving evidence or otherwise." (Baker, p. 419.)

And in the case of medical witnesses, the penalty for refusal is still greater; for it is enacted in the 6 and 7 Will. IV. c. 89 sect. 6,

"That where any order for the attendance of any medical practitioner, as aforesaid, shall have been personally served upon such practitioner, or where any such

order not personally served shall have been received by any medical practitioner in sufficient time for him to have obeyed such order, or where any fresh order has been served to the residence of any medical practitioner, and in every case where any medical practitioner has not obeyed such order, he shall for such neglect or disobedience forfeit the sum of five pounds sterling, upon complaint thereof made by the coroner or any two of the jury before any two justices having jurisdiction in the parish or place where the inquest under which the order issued was held, or in the parish where such medical practitioner resides; and such two justices are hereby required, upon such complaint, to proceed to the hearing and adjudication of such complaint, and if such medical practitioner shall not show to the said justices a good and sufficient cause for not having obeyed such order, to enforce the said penalty by distress and sale of the offender's goods, as they are empowered to proceed by any act of parliament for any other penalty or forfeiture." (Grindon, p. 127.)

The coroner also has power to commit any one to prison for contempt of court, who refuses to answer the questions which are put to him by the jury, or who objects to sign his name to the depositions which he has made, or who refuses to be bound over to appear and prosecute at a future trial of the accused.

4th. *Of the mode of summoning witnesses and under what circumstances they are to appear.*—It is evident from the proclamation which is made at the opening of a coroner's court, that all persons who know anything of the matter at issue are to appear at the inquest, so that they may be heard; and although it was at one time doubted whether the coroner was justified in examining all witnesses who offered, as well for the interest of the crown as against it, yet it is now settled that he ought to do so, in order that he may have the evidence on both sides of the question. In case of a committal, however, the coroner need not bind over the witnesses for the accused party. At the present time it is customary not to trust to the chance appearance of witnesses on proclamation, but to take measures for their attendance before the court has met; and medical practitioners were formerly summoned and paid in the same way as ordinary witnesses, but by the statute of 6 and 7 Will. IV. c. 89, a better provision is made for the attendance and remuneration of medical men. By that statute the coroner is empowered to order the attendance of any legally-qualified medical practitioner who visited the deceased during his last illness; or in case he was not visited during that period by such a practitioner, the coroner can issue his summons for the appearance of any legally-qualified practitioner, who is, at the time, in actual practice in or near the place where the death has happened; and it shall and may be lawful for the coroner, either in his order for the attendance of the medical witness, or at any time between the issuing of such order and the termination of the inquest, to direct the performance of a *post-mortem* examination, with or without an analysis of the contents of the stomach or intestines, by the medical witness or witnesses, who may be summoned to attend at any inquest; provided, that if any person shall state upon oath before the coroner, that in his or their belief the death of the deceased individual was caused partly or entirely by the improper or negligent treatment of any medical practitioner or other person, such medical practitioner or other person shall not be allowed to perform or assist at the *post-mortem* examination of the deceased.

It is further enacted, that whenever it shall appear to the greater number of the jurymen at any coroner's inquest, that the cause of death has not been satisfactorily explained by the evidence of the medical practitioners or other witness or witnesses who may have been examined in the first instance, such greater number of the jurymen are hereby authorized and empowered to name to the coroner in writing any other legally-qualified practitioner or practitioners, and to require the coroner to issue his order for the attendance of such last-mentioned medical practitioner or practitioners as a witness or witnesses, and for the performance of a *post-mortem* examination, with or without an analysis of the contents of the stomach or intestines, whether such an examination had been performed or not; and if the coroner, having been thereunto required, shall refuse to issue such order, he shall be deemed guilty of misdemeanor, and shall be punishable in like manner as if the same were a misdemeanor at common law.

The third and fourth sections of this Act relate to the fees which are payable to the medical witnesses, namely, to each the fee of one guinea for attendance whereat no *post-mortem* has been made, and a fee of two guineas when there has been such an examination, with or without a chemical analysis: provided nevertheless, that no order of payment shall be given, or fee or remuneration paid, to any medical practitioner for any *post-mortem* examination which may have been instituted without the previous direction of the coroner; provided also, that the inquest is not held on the body of any person who has died in any public hospital or infirmary, or in any county or other lunatic asylum, or in any building or place attached thereto, and used for the reception of patients.

It is manifest from the wording of this statute, that a medical witness ought not to appear before the coroner to give evidence, unless he has received a medical summons to do so. There are indeed cases on record where the coroner or his deputy has refused to pay the medical witness, on the ground that he had not been properly summoned. Again, it must be evident to our readers that a medical practitioner is guilty of an indiscretion, and is likely to forfeit his fees, if he undertake the *post-mortem* examination of a body without the coroner's order. So also it is apparent that the witness will not lose his fees by not undertaking a chemical analysis of the contents of the stomach or intestines; for the words of the statute are to this effect,—that the fee of two guineas shall be paid to the medical witness for his evidence and examination, *with or without a chemical analysis.* Moreover, as it is at the present time customary for an analysis to be made of the liver, kidneys, and other organs of the body not mentioned in the Act, it is questionable whether the coroner can order the medical practitioner to undertake such an investigation; and it appears to us to be the duty of the medical witness to hand over the several parts which he has removed from the body, into the custody of one who is, from his pursuits, specially qualified to undertake such a difficult inquiry. By so doing, he will release himself from very heavy responsibilities, without in any way endangering the magnitude of his fees.

5th. *Of Evidence.*—The rules which apply to this branch of the subject are precisely the same as those which govern the testimony of witnesses in other courts of law; and as the coroner has to do with jurymen who are not always acquainted with the rules of evidence, he must take care that

nothing illegal appears on the face of the deposition, lest they be brought forward and used at a future trial.

If we take a general view of the matters which are discussed at an inquest we shall perceive that

"The usual evidence before the coroner consists—1st, of the dead body of the deceased, which must be examined and its identity proved;—2nd, of witnesses to the fact, or, where such witnesses are wanting, of—3rd, circumstantial or presumptive evidence;—4th, of the confession or admission of suspected persons;—5th, of the declarations made by persons in apprehension of approaching death;—6th, of written documents. The latter rarely come before a coroner's jury, but it sometimes happens that letters or memoranda found in the pockets of, or written by, persons who have destroyed themselves, are produced in evidence of sanity or insanity. If they are received in evidence, it should be upon proof of the hand-writing, and, if without date, of the time when they were written: in every case, however, they must be looked upon as a very apocryphal species of proof." (Grindon, p. 109.)

The depositions should be taken with great accuracy, and as nearly as possible in the words used by the witness; so that, at any future inquiry, the witness may not be confused by the introduction of words which he neither employed nor knew the meaning of. Medical witnesses should be particularly careful not to employ technical expressions, or to use words of doubtful meaning; for by so doing they are likely to mislead the jury, or else to prolong the business of the court by the explanatory discussion of matters which cannot in any way benefit the inquiry. Again, it is necessary that they should confine themselves, as much as possible, to matters of fact, leaving their deductions therefrom for after consideration. Another point to which the medical witness ought to direct his attention, is the method and order in which he details the circumstances of the case. He will find it best to recount the facts in the order in which they occurred; to say, for example, who summoned him to the patient, at what time he got there, where he found him, how he was lying, what was his condition, and so on until the party died. If a *post-mortem* has been made, the witness should say when it was done, and how long after death; he should describe the condition of the body, as to whether it was fat, muscular, and healthy-looking, or otherwise; he should say, whether it exhibited any marks of violence; and then proceed to inform the jury concerning the state of the different internal organs. Finally, if it be a case of suspected poisoning, he should conclude his evidence by saying, that he had handed over the contents of the stomach &c. to Mr. A— B—, for chemical analysis, leaving it with the jury to summon that person or not, as they may think proper. By adopting a course of this kind, every one of the facts will be particularized, they will fall together in their proper places, and the whole of the testimony will be understood and appreciated by the jury.

If the witness considers it necessary to make notes of the case, in order that the facts may not slip from his memory, he should make them at the time he is conducting the inquiry, or they will not be admissible in evidence in a future stage of the proceedings.

The medical witness frequently has an opportunity of learning many important particulars from the patient while he is in a dying state; and it is proper for him to know that certain rules have been laid down for the

guidance of those who are called upon to receive such evidence in a court of law. As a general rule, the declarations of a dying man are not receivable in evidence, unless it appear to the satisfaction of the judge that the deceased was perfectly conscious of his being in a dying state, and had no hope *whatever* of recovery at the time he made them. Under these circumstances, it is considered that he is truly sensible of his awful condition, and that he would not peril his soul by the utterance of a falsehood; consequently, the declarations are held to be as truthful as if they were made under the solemn obligation of an oath. This rule applies even when the deceased did not actually *express* any apprehension of danger, and when death did not take place until a considerable time after the declarations were made. But if there be any hope of recovery, however slight it may be, in the mind of the dying man, his statement will not be received by the judge, notwithstanding that the death might have occurred immediately afterwards. There are many cases in the volumes before us which serve to illustrate the truth of this; and we learn from them, that it is the duty of the medical attendant emphatically to inform the patient, when there is no hope of recovery, that his moments are numbered, and that he has but a short time to live, so that if he have anything to communicate or confess, he might do so while time remains.

If the declarations have been made in writing, and signed by the deceased, the original document must be produced and attested, for then no parole evidence will be received; and, finally, it is proper to know that the declarations made must have reference to the subject of the charge and to the cause of death, otherwise they are not admissible.

Again: much difficulty prevails in the law, in deciding under what circumstances a confession or statement made by a prisoner or suspected person is receivable in evidence; and a great number of cases are quoted by both of our authors, in the hope of elucidating this perplexing subject. Among the points which have been decided are these: that if a prisoner be before a magistrate or coroner, and he offers, or is asked, to make a statement, it is necessary that the magistrate or coroner should caution him to the following effect—namely, that what he says will be taken down and may be used against him, and that in speaking he is not to expect any hope of favour by confessing, or to entertain any fear by not doing so, but that he is to use his own discretion in the matter. It is admitted, moreover, that no confession made by a prisoner or suspected person can be used against him, if it be drawn out of him by the threats or inducements of an interested party. Now, as it is possible that a medical witness might be brought into relation with a criminal, and that, by reason of such professional connexion, the accused may entertain confidence, and offer a statement, or ask for advice, concerning the subject-matter of his guilt, it is important to know how, under such circumstances, the medical attendant ought to conduct himself. Judging from the cases before us, we may conclude that it is the duty of the witness to offer no inducements whatever to make the accused confess; on the contrary, he should inform him that he had better consider well what he is about, for that, if he says anything to criminate himself, it will unquestionably be a duty to give it in evidence against him. If the medical witness proceed in any other manner, it is probable that the confession will not be received by the judge; and that

his conduct will be severely censured. At page 301 of Mr. Baker's volume, there is a case which illustrates this part of the subject:

"At the trial of a servant for attempting to poison her mistress, a medical man, having denied that he had held out any inducement to the prisoner to confess, gave evidence of a confession, without which the prisoner could not have been convicted. Evidence was then given, that before she made her confession, he had said to her, in the presence of her mistress,—'It will be better for you to tell the truth.' The medical man was recalled, but did not admit this, and the judge left the evidence, including the confession, to the jury, but reported, that if the evidence had been given in the first instance he should have excluded the confession. It was afterwards held, that the confession ought to have been struck out, and that the conviction was wrong."

Lastly, it may be said that a statement made by a prisoner when he is drunk is receivable in evidence, even if the man has been made drunk in the hope of his saying something to criminate himself.

6th. *Concerning the Publicity of the Coroner's Court.*—It has frequently been asked, whether the proceedings at an inquest are to be likened to those which take place in ordinary courts of justice, or whether, from the peculiar nature of the inquiry, they are not more nearly allied to the business which is conducted before a grand jury. Now, the former is a public tribunal, and the latter is a private one. A little consideration, however, will show that the coroner's court is a court of record and not of accusation; its proceedings have but one object—namely, to ascertain the cause of death; and if the determination of that cause leads, as it often does, to the apprehension of a suspected person, those proceedings can never be regarded as final, but merely as preliminary to another inquiry, for judgment is never pronounced on a coroner's verdict. Taking this view of the matter, it may be affirmed that the coroner's court is not a public one. On the other hand, looking at the proclamation which is made at the opening of an inquest, it might be said that the public are invited to be present, and that therefore they have a right of access to it. This, in addition to other considerations, has led many authorities to conclude that the inquiry is at all times a public one. But Sir John Jervis, in reviewing the various arguments which have been advanced in support of the latter opinion, says, that "however strong those arguments may at first sight appear to be, they do not, upon examination, establish an universal right for *all* the public to be present; but, at most, they extend only to such as are summoned, suspected, interested in the result of the inquiry, or are inhabitants of the village where the body is found dead." (Jervis, p. 216.) He afterwards states, that as the coroner's inquiry sometimes leads to accusation, it is advisable, if not necessary, occasionally to conduct it in secret, lest the party, being informed of the suspicion which arises, eludes justice by flight, or by tampering with the witnesses. Cases may also occur in which privacy is necessary for the sake of decency, or for the respect due to a family; and it would be highly imprudent to allow the publication of things which might do mischief when they cannot do good. He adds, moreover, that even in cases where absolute privacy is not required, the exclusion of particular individuals may be necessary and proper. Of this, the coroner is evidently the best able to judge; and it is also manifest that the possession of such a power is actually necessary to the due administration

of justice: for it is impossible that the proceedings of any court can be conducted with due order and solemnity, and also with that effect which justice demands, if the presiding officer have not entire control over the persons present, and the power of admission and exclusion according to his own discretion. In point of fact, it has been decided in several cases, that the coroner has the right to exclude, not only particular persons, but also the public generally. He may even *forcibly* expel a contumacious individual from the room in which the inquest is held. Nevertheless, says Jervis, it is obvious that as in most cases publicity assists the investigation of truth and the detection of guilt, this power ought not to be exercised without just cause, and due consideration.

Again, it has been decided that the coroner can at any time refuse the interference of counsel learned in the law; and it is the custom at the present time not to allow an advocate to address the jury, or to plead in any way for a suspected party. He may, however, by permission from the coroner, assist the court in the examination of witnesses, for by this means he may help in the discovery of facts, and lead the jury to a correct verdict.

7th. It may be asked, *Whether the coroner's jury is a responsible body, and whether they are bound to attend to the opinions of the coroner?* To both of these questions we answer, No; for it appears, from the writings of Jervis and Sewell, that the jury are endowed with great discretionary powers, and that they are not accountable, even to the state, for the undue exercise of their judicial functions. In point of fact, they have such complete control over the proceedings of the inquest, that they are not even bound to attend to the remarks which fall from the coroner. They are not challengeable as other juries are, and they can call before them as many witnesses as they please, or they may examine any one witness as often as they please; for it is their duty to investigate the facts of the case, and, to use the language of Jervis, they are neither to expect, nor are they to be bound by, any specific or direct opinion of the coroner on the case generally, except so far as regards the verdict, which, in point of law, they ought to find as dependent and contingent upon the conclusions in point of fact. It has indeed been questioned by some authorities, whether the jury are obliged to submit implicitly to the directions of the court even in this particular; for the functions of the jury are altogether independent of the court, and have been imposed for the express purpose of exercising a wholesome check on the powers of an unjust judge. In most cases, however, unless the coroner shows a disposition, either to mislead the jury, or to hurry over the inquiries by merely taking so much of the evidence as may make a show on the face of his proceedings, it is the duty of the jury to manifest the most respectful deference to the advice and recommendations of the court, especially in all matters which relate to the law of the case; not only because the jury is avowedly under the superintendence of the coroner, but also because the experience of the latter must at all times be of great assistance in determining the value of evidence received by them, and in directing their attention to those facts which might, but for his interference, have escaped their notice. Nevertheless, the coroner is bound to accept their verdict, whatever it may be; for they have the finding in their hands, and he has merely to take it and record it. If that verdict be an unjust one, or

one not warranted by the facts, it may be quashed, and a new inquisition held; but the jury are not responsible for the error.

Having proceeded thus far with the subject, we may now ask,

8th. *What is an Inquisition, and of what does it consist?* "An inquisition, properly speaking, is," to use the words of Jervis, "a written statement of the verdict or finding of a jury, returned for the purpose of a particular inquiry (generally the cause of death), as distinguished from an indictment, which is an accusation by the oaths of jurors, returned to inquire generally of all offences within the county." (Jervis, p. 245.) If, however, the inquisition contain any subject-matter of accusation, it is then equivalent to an indictment, or the finding of a grand jury; and the parties charged may be tried and convicted upon it, notwithstanding that the grand jury, at a subsequent stage of the proceedings, may think proper to ignore the bill.

We are told by Grindon, that, under the latter circumstances,

> "The inquisition should contain a plain, brief, and intelligible narrative of the offence committed according to recognised words of art; but, except in particular cases, where precise technical expressions are required to be used, there is no rule that other words shall be employed than such as are in ordinary use; or that in indictments or other pleadings, a different interpretation is to be put upon them than they bear in ordinary acceptation." (Grindon, p. 132.)

The inquisition consists of three parts—namely,

1st. Of *the caption or heading*, which contains the venue or place of jurisdiction; relates where, when, and before whom, the inquest is held; gives a description of the deceased; says where the body lies; mentions the names and qualifications of the jurors; and states that their finding is on oath.

2ndly. Of *the verdict*, which mentions the fact of death, and if possible specifies the cause thereof; contains the name and surname of the party charged, if such there be; gives an account of his estate and degree; mentions his place of residence; alleges the time and place of the several acts committed; and finally, relates in a very precise manner the nature of those acts, and the manner in which they were performed.

3rdly. The inquisition contains *the attestation* of the jury—that is, their several signs, signatures, and seals.

As the trial of a person may be proceeded with on account of the charge mentioned in the inquisition, it becomes necessary that the most precise and accurate forms of expression should be used, in order that the indictment which is to be founded thereon, shall be valid in point of law. Hence it is that many technical expressions are introduced, which, in some cases, are absolutely necessary to the validity of the indictment. It is usual, for instance, in cases of murder, to say, that the individual charged with the act done, had not the fear of God before his eyes, but was moved and seduced thereto by the instigation of the devil; that he committed the offence in violation of the peace of God and our sovereign lady the queen; and that he did it feloniously, wilfully, and of malice aforethought. The following is an example of the manner in which this part of the inquisition is generally worded:--

Verdict of murder with a poker.

"That James Stout, late of the parish aforesaid, in the county aforesaid, labourer, on the first day of September, in the year aforesaid, not having the fear of God before his eyes, but being moved and seduced by the instigation of the devil, with force and arms, at the parish aforesaid, in the county aforesaid, in and upon the said William Little, against the peace of God and of our said lady the queen, then and there being, feloniously, wilfully, and of his malice aforethought, did make an assault, and that the said James Stout, with a certain iron poker, which he the said James Stout then and there had and held in both his hands, him the said William Little in and upon the head of him the said William Little, then and there divers times feloniously, wilfully, and of his malice aforethought, did strike and beat, and that the said James Stout did thereby then and there give unto him the said William Little in and upon the top of the head of him the said William Little divers mortal bruises, of which said mortal bruises he the said William Little then and there died. And so the jurors aforesaid, upon their oath aforesaid, do say that the said James Stout, him the said William Little, in the manner, and by the means aforesaid, feloniously, wilfully, and of his malice aforethought, did kill and murder against the peace of our said lady the queen, her crown and dignity."

In cases of murder by poison, the verdict is still more technical; but it appears from the statements made by several of our great legal authorities, that much of the language so employed is mere surplusage, and that, in so far as it is immaterial to the main facts of the case, it might be omitted, without in any way affecting the validity of the charge. Nevertheless, the words mentioned in the example given are generally employed, partly, perhaps, because their use is sanctioned by custom, and partly because they tend to aggravate the offence, and so to create a greater horror of crime. This they may do in some cases; for a story is told of a desperate criminal, who, after he had listened with astonishment and evident discomposure to the language made use of in the indictment, threw up his hands and begged for mercy, saying, that although he was without doubt one of the worst of characters, and had in his time committed many great offences, yet he had never been guilty of half the wickedness that had been set down to him, and that if they meant to go on in that way, there was evidently no hope for him on this side of the grave.

Several statutes have been made for the purpose of simplifying these formalities, and one of them—namely, the Act of 6 and 7 Vict. c. 83, especially directs that much of the language usually employed in indictments may be advantageously omitted; it, moreover, gives power to any judge of her Majesty's court at Westminster, or of assize or gaol delivery, to rectify any technical defect in an inquisition, which might have occurred by reason of a non-adherence to the old forms of accusation; and the new bill of Lord Campbell's has a still greater tendency to remedy the defects in question, inasmuch as it does away with the necessity for particularly describing the act or deed which constitutes the foundation of the charge. In the fifth report of her Majesty's commissioners for revising the criminal law, some important alterations have been made as respects the drawing up of indictments, with a view to the avoidance of those flaws which have afforded so great a scandal to the administration of criminal justice in this country, and also to the curtailment of prolixty; but it is not our province to enlarge upon this matter.

While speaking of the subject of verdicts, it may not be out of place to

remark, that there is a strong tendency in the minds of coroners to advise the return of verdicts which are too merciful in their nature, or else too indefinite in their character, for any practical good. It is usual, for instance, to return a verdict of *temporary insanity* in cases of self-murder. This, no doubt, arises out of a feeling of humanity, out of a consideration for the memory of the deceased, and pity for the mental agony of the survivors; but it is questionable whether, in many cases, such an act of humanity is not a mischievous extension of mercy; whether, in fact, it does not help to nurture the crime of self-murder, by creating a morbid feeling of sympathy for the condition and conduct of the deceased. Indeed, the highest authority on this subject says, that "coroners are too apt to strain this excuse to too great a length," (Jervis;) and the same writer remarks, that a notion too generally prevails, that he who destroys himself must be *non compos;* that the very act of suicide is evidence of insanity, and that no one in his senses would commit that which is so contrary to reason and nature. But this very argument urged in extenuation, is, in fact, the aggravation of the offence. If tenable, it would excuse every criminal equally with the suicide, and would apply more forcibly in proportion to the enormity of the crime. To murder a parent, or a child, is as much repugnant to nature as self-murder; but if none but madmen could commit such crimes, no one would be culpable in the eye of the law. The law very rationally judges that every melancholy and hypochondriac fit does not deprive a man of the power of discerning good from evil, which is necessary to form a legal excuse. And therefore, if a real lunatic kills himself in a lucid interval, he is *felo de se* as much as another man.

In our law courts a great difficulty exists in determining whether a person be insane or not. The usual test, by which the fact is determined, is founded on a principle or rule laid down by Lord Mansfield, at the trial of Bellingham for the murder of Percival. On that occasion, the learned judge alluded to is reported to have said,

"That in order to support a defence of insanity it ought to be proved, by distinct and unquestionable evidence, that the prisoner was incapable of judging between right and wrong; that, in fact, it must be proved beyond all doubt, that at the time he committed the atrocious act with which he stood charged, he did not consider that murder was a crime against the laws of God and nature; and that there was no other proof of insanity which would excuse murder or any other crime; that in the species of madness called lunacy, where persons are subject to temporary paroxysms, in which they are guilty of acts of extravagance, such persons committing crimes, when they are not affected by the malady, would to all intents and purposes be amenable to justice, and that so long as they could distinguish good from evil they would be answerable for their conduct."

Lord Erskine made delusion the test; but Mr. Baron Alderson has even gone farther than either of his predecessors, for, in his address to the jury at the trial of Pate, he is reported to have said,

"That it was not because a man was insane that he was unpunishable, and he must say that upon this point there was generally a very grievous delusion in the minds of medical men. The only insanity which excused a man for his acts was, that species of delusion which conduced to, and drove a man to commit, the act alleged against him. If, for instance, a man being under the delusion that another man would kill him, killed that man, as he supposed for his own protection, he would be unpunishable for such an act, because it would appear that the act was

done under the delusion that he could not protect himself in any other manner, and there the particular description of insanity conduced to the offence. But, on the other hand, if a man had the delusion that his head was made of glass, that would be no excuse for his killing a man. He would know very well, that although his head was made of glass, that was no reason why he should kill another man, and that it was a wrong act, and he would be properly subjected to punishment for that act. These were the principles that ought to govern the decision of juries in such cases. They ought to have proof of a formed disease of the mind—a disease existing before the act was committed, and which made the person accused incapable of knowing, at the time he did the act, that it was a wrong act for him to do."

This, therefore, is the rule which is adopted at the present time, in order to discover the fact of madness; and we may refer to the volumes before us, especially to that of Mr. Baker, for examples of the manner in which it has been applied by judges in the cases of Arnold, Blakely, Pate, &c. But a very little consideration on the part of those who are acquainted with the habits and dispositions of the insane, will be sufficient to show that the test is not applicable even to the best of cases; for it is a notorious fact, that some of the most mischievous occupants of lunatic asylums are well able to distinguish right from wrong, and good from evil. In many instances, indeed, they freely acknowledge the errors of their conduct, and make earnest promises of amendment; but it would be a fatal mistake to put any reliance on such admissions, and to trust them into society without control or supervision, merely because they had promised to do better; for it is well known, that under the influence of the least excitement, the reason would give way, the good intentions would be entirely forgotten, and violence and mischief would assuredly result. It has been truly said by the present Lord Chief Justice of the Court of Common Pleas,

"That a lunatic may be coherent in conversation, but insane in action; he may be rational when under restraint, but when released and at liberty to act according to the impulse of his hallucinations, will show by his conduct that he is really insane. In fact, it is frequently difficult, nay, even impossible, without considerable experience, time, and patience, fully to develop the delusions which exist in the mind of a crafty lunatic." (Jervis, p. 91.)

We may add, moreover, that most writers on the subject of insanity are accustomed to recognise a species of insanity, in which there is not any delirium, hallucination, or notable lesion of the intellect or reasoning faculties, but merely a morbid perversion of the natural feelings, affections, habits, or moral disposition. This kind of insanity is denominated "moral" insanity; and from the circumstance that there is never a manifest want of knowledge of right and wrong, it is entirely ignored by the members of the bar, and much mischief has doubtless arisen therefrom. We must not, however, pursue this subject any farther at present.

The verdict of *felo de se* is one which involves the conclusion that the murderer was in a sound state of mind when he committed the act imputed to him. This crime is wisely and religiously considered in the English law, as the most heinous of all offences; for, to use the words of Jervis, "as no man hath power to destroy life but by the commission of God, the author of it, the suicide is guilty of a double offence: one spiritual, in invading the prerogative of the Almighty, and rushing into his

presence uncalled for; and the other temporal, against the king, who has an interest in the preservation of all his subjects." (p. 110.) Hence it is, that this offence is ranked by law amongst the highest of crimes, making it a peculiar species of felony—a felony committed against oneself; and the punishment, though it be inoperative upon the dead body, is as great, under the circumstances of the case, as it can be; for it blasts the reputation of the felon, proclaims him the very worst of homicides—a self-murderer; it inflicts the disgrace of an ignominious burial, prohibits the gracious offices of the church, and, finally, makes a complete forfeiture of all that was possessed by the deceased.

The *verdict of murder* is returned when the facts show that a person of sound mind, memory, and discretion, unlawfully kills any reasonable creature in being, and under the protection of the laws, with malice aforethought, either expressed or implied; and the party killed must die within a year and a day after the stroke or cause was received. It is evident from this, that four conditions must be fulfilled, in order to justify the charge of murder—namely,

1st. *The person killing must be of sound mind.*

2nd. *The person killed must be a reasonable creature, in being, and under the queen's peace:* the words of the law are, that he or it must be *in rerum naturæ;* and therefore a child *in ventre sa mere* cannot be the subject of murder, although the contrary was formerly held to be the case. But if it be born alive and die from drugs given, or operations performed while it was in the womb, then it is murder. So again, if it breathe in its progress into the world, but is actually dead when it is clear of the mother, it is not murder; nevertheless, if it has been wholly produced alive, and has an independent circulation of its own, it is murder to destroy it, though it be still attached to the mother by the umbilical cord. This has been held good by the fifteen judges. (Grindon, p. 82.)

The difficulty of proving that the child was born alive, produced a legislative enactment in the twenty-first year of the reign of James I. (c. 27), which required the mother to prove by one witness at least, that the infant was born dead; in default of which she was charged with and punished for murder. By the statute of 43 George III., c. 58, that enactment was repealed, and the evidence required was of a contrary nature. Other changes were also made in the law at that time, so that, in an indictment for murder, the woman could, provided the evidence failed in this particular, be found guilty of concealment, and punished accordingly. In the ninth year of the reign of George IV. (c. 31), this statute was also repealed, but it was re-enacted in substance, except that the mother could be re-indicted for concealment only. With this, however, the coroner has nothing to do; for it is his place merely to find the cause of death.

Mr. Baker has written a great deal on the subject of infanticide, showing that the crime is a very common one, and that it is perpetrated chiefly in the manufacturing districts by the use of opiates. We must refer our readers to his chapters on child-murder, burial-clubs, &c., for full information on this important subject.

3rdly. *There must be malice aforethought, implied or expressed.*—This is the grand criterion which serves to distinguish murder from manslaughter. The word *malice* implies, in its common acceptation, a settled anger of

one person against another, and a fixed desire of revenge; but, in the law, it may merely mean an evil design, the dictate of a wicked, depraved, and malignant heart. The law rarely demands actual proof of the existence of malice, but in charges of murder it ordinarily takes it for granted that malice aforethought has existed. This, however, may at all times be determined by the nature of antecedent circumstances; and when it so happens, from a consideration of these, that malice aforethought cannot be inferred, the charge of murder cannot be sustained. Hence it is that the killing of a person in hot blood, on the spur of the moment, before the heat of passion has had time to cool, or the putting an end to the life of a human being by the incautious laying of poison for the destruction of vermin, or by the accidental administration of a pernicious drug by a surgeon, physician, or other person, is not murder. It is murder, however, to advise an individual to take any deleterious drug, knowing at the time that such a drug is dangerous. It is also murder for a person to kill another, after there has been time for the anger to subside. In the case of duelling and prizefighting, for example, it is evident that the parties meet for the purpose of doing each other an injury, and consequently, if death happens, all who are concerned in the matter are guilty of murder. By the statute 1 Vict. cap. 87, the crime of duelling is made a capital felony; for it states, that if in the act of fighting a wound is given, the party so wounding shall, upon being convicted, suffer death; and if no bodily injury be done, the parties who meet are liable to transportation for life, or for any term not less than fifteen years.

So, again, the persuading a lunatic to kill himself is murder; the turning loose a furious animal, by which the death of man results, and the killing of one person by mistake for another, are, in such case, murder. The taking a sick person against his will into the cold air; the exposing an infant to the elements, or to wild beasts; the submitting a servant or child to negligence, hard usage, or to bad and insufficient nutriment, and the inflicting of too rigorous a punishment on a prisoner in gaol, if followed by death, is murder; for the law will, under all such circumstances, infer malice.

It is proper, moreover, to say, that there must be a corporal damage done to the party killed, in order to constitute the charge of murder; and that it does not signify whether by skilful treatment he might have recovered, or whether he must have soon died; for no man can apportion his own wrong. It appears from this, that the working of a person into a passion of grief, fear, or anger, whereof he dies, is not murder, for then there is no corporal damage.

4thly, and lastly. *It is necessary for the validity of the charge that the death take place within one year and a day of the time when the stroke was received.*

By *the verdict of manslaughter* is meant the unlawful killing of another without malice expressed or implied; and it is either voluntary, from a sudden transport of passion, or involuntary, from the pursuit of some unlawful act, or a lawful act criminally or improperly performed. As we have already said, the great distinction between manslaughter and murder is in the question of malice. Among the examples given by Mr. Baker to illustrate this part of the subject, are the following:

"Giving a child an improper quantity of spirituous liquors heedlessly, and for brutal sport, is manslaughter, if death be thereby caused."

"If the driver of a carriage be racing with another carriage, and from being unable to pull up his horses in time, the first-mentioned carriage is upset, and a person thrown off it and killed, this is manslaughter in the driver of that carriage." (p. 193.)

It would also be manslaughter if either of the carriages went over a man and killed him. It may likewise amount to manslaughter if a person in frolic does that which kills another, notwithstanding that he did not intend to produce any serious result.

"A boy, in a frolic, without any intention to do any harm to any one, took the trap-stick out of the front of a cart, in consequence of which it was upset, and the carman, who was in the cart putting down a sack of potatoes, was pitched backwards on the stones and killed. It was held that the boy was guilty of manslaughter." (p. 194.)

"Where husband and wife are separated by consent, the husband granting the wife a stipulated allowance, which is regularly paid, he is not bound to provide her with shelter; but if he is informed or knows that she is without shelter, and refuses to provide her with it, in consequence of which her death ensues, he is guilty of manslaughter (even though the wife be labouring under disease which must ultimately prove fatal), if it can be shown that her death was accelerated for want of the shelter which he had denied." (p. 195.)

Mr. Baker has quoted several cases which illustrate the operation of the law in respect of collisions, accidents, &c., but it may be said, in a general way, that where any one, by negligence, by rashness, or by want of proper caution, in the performance of a dangerous but lawful act, has contributed to the death of another, he is guilty of manslaughter.

Of late we have been made acquainted with a number of cases where death has resulted from the incautious use of quack medicines; many of these cases have been made the subjects of investigation in criminal courts, and it will not be altogether profitless if we direct attention to the law of this matter.

At page 92 of Grindon we are informed, that any person, whether he be licensed or not, who deals with the health of her Majesty's subjects, is bound to have competent skill, and to treat his patient with care, attention, and assiduity; and if a patient die for want of either, the person is guilty of manslaughter.

"It matters not," said Mr. Baron Garrow, at the trial of St. John Long, "whether the individual consulted be the president of the College of Surgeons, or the humblest bonesetter of the village, he ought to bring into the case ordinary care, skill, and diligence. I am of opinion, that if a person, who has ever so much or so little skill, sets my leg, and does it as well as he can, but does it badly, he is excused; but suppose the person comes drunk, and gives me a tumblerful of laudanum, and sends me into the other world, is it not manslaughter? And why? Because I have a right to have reasonable care and caution. Why do we convict in cases of death by driving carriages? Because the parties are bound to have skill, care, and caution." (Baker, p. 205.)

On another occasion—namely, the second trial of Long—it was remarked by Mr. Justice Bailey, that,

"I do not charge it in *ignorance* merely; but there may have been *rashness*, and I consider that *rashness will be sufficient to make it manslaughter*. As, for instance, if I have a toothache, and a person undertakes to cure it by administering

laudanum, and says, 'I have no notion how much will be sufficient,' but gives me a cupful, which kills me; or of James's powder, and says, 'I have no notion how much ought to be taken,' and yet gives me a tablespoonful, which has the same effect,—such persons, acting with *rashness*, will, in my opinion, be guilty of manslaughter."

From the time of Lord Hale, downwards, it has been held by the judges, *that in order to constitute the charge of manslaughter, the person accused must have been guilty of criminal misconduct, arising either from gross ignorance or criminal inattention.* This is the rule which applies to all cases of death from the administration of medicines, and it seems to be a matter of no consequence whatever whether the person so charged be a regular practitioner or not, or whether he have had a regular medical education or not. There are opinions to the contrary; but in the judgment of Lord Coke and others, "these opinions may serve to caution ignorant people not to be too busy in this kind of tampering with physic, but they are no safe rules for a judge or jury to go by." Nevertheless, it was said on one occasion by Lord Lyndhurst (Rex *v.* Webb), that although there was no difference between the acts of the regularly-qualified practitioner and the unlicensed one, yet,

"If, where *proper* medical assistance could be obtained, a person totally ignorant of the science of medicine took upon himself to administer a violent and dangerous remedy, to one labouring under disease, and death ensues in consequence of that dangerous remedy having been so administered, then he is guilty of manslaughter."

Again, at the summing up of the evidence in the trial of St. John Long, Mr. Justice Parke told the jury, that,

"On the one hand we must be careful and most anxious to prevent people from tampering in physic, so as to trifle with the life of man; and, on the other, we must take care not to charge criminally a person who is of general skill, because he has been unfortunate in a particular case. It is God that gives, man only administers, medicine; and the medicine that the most skilful may administer may not be productive of the expected effect; but it would be a dreadful thing if a man were to be called in question criminally, whenever he happened to miscarry in his practice. I call a man acting wickedly, when a man is grossly ignorant, and yet affects to cure people, or when he is grossly inattentive to their safety." (Baker, p. 208.)

A slight acquaintance with the subject under consideration will show, that in all the trials for *mal-praxis* which have lately taken place, the judges have been guided in their address to the jury by the old opinion of Lord Hale—namely, that "if a physician gives a person a potion, without any intention of doing him any bodily hurt, but with the intent to cure or prevent a disease, and, contrary to the expectation of the physician, it kills him, this is no homicide; and the like of a chirurgeon." (Year Book, 3rd Edward.) This opinion, which is sound enough in its proper acceptation, has been distorted in a very extravagant manner, and applied to the case of uneducated quacks, in consequence of what was afterwards said by Lord Coke—namely, "I hold the opinion to be erroneous, that if he be no licensed chirurgeon or physician that occasioneth the mischance, that then it is felony; for physic and salves were before licensed physicians and chirurgeons." But if this is the law, it is high time that some alteration should be made in it; for at the present day, there is no occasion for

the interference of unqualified persons, seeing that regular practitioners are always to be found ready to give their assistance in case of accident or disease. When Lord Coke expressed his opinion, the poor in many places were entirely dependent on the skill of wise women and experienced bonesetters, but it is not so now; and we conceive it to be highly imprudent that the judges of the present day do not use their influence with juries to prevent that awful sacrifice of human life, which occurs from the unskilful practice of individuals who have no acquaintance with disease or the art of curing it. Within the last few years, or, as we might almost say, months, no less than ten cases of death from lobelia and cayenne pepper have, to our knowledge, been made the subjects of inquiry with a coroner's jury; and although in five of those cases a verdict of manslaughter was found, yet in no instance, at the subsequent trials of the prisoners, did the judge consider it to be his duty to advise the return of a similar verdict. How different would it have been if a man, ignorant of the structure and management of a locomotive, had undertaken the conduct of a train, and by his unskilfulness caused the death of a fellow-creature. In such a case the words of censure would flow freely enough, and there is no doubt that the unfortunate author of the mischief would be made to suffer the extreme penalty of the law. In our judgment, however, there is no difference in the criminality of these cases, and it is a misinterpretation of facts to say that the individual is culpable in the one instance, and only indiscreet in the other.

Our professional brethren will perceive, from what we have said, that in the eye of the law there is no difference between the misadventure of a quack and that of a regular practitioner. In point of fact, the conduct of the latter might, at any time, be made the subject of grave accusation. We refer to this, because we have reason to believe that the profession is not sufficiently informed on the subject, and that they therefore consider it to be a piece of gross interference on the part of a coroner, to hold an inquest on their patient, merely because the friends of the deceased are dissatisfied with the practice employed. But it is proper to know that the coroner is bound, by the laws which govern him in his official capacity, immediately to institute an inquiry whenever a statement is seriously made to him, to the effect that the person lying dead has not been properly dealt with, and that his death has been accelerated, if not caused, by improper medical treatment. If such a charge be groundless, the result of the investigation will be the very best acquittance that the practitioner can possibly have. It is a piece of folly, therefore, to say nothing of its illegality, for a medical man, be he ever so well qualified, or ever so exalted in his position, to oppose the inquiry of the coroner, or to annoy him by any kind of interruption while he is engaged in the prosecution of his duty. We make these remarks, because several cases have been recorded within the last year or two, where gentlemen, who have been unjustly charged with *mal-praxis*, have seriously damaged the aspect of affairs by their injudicious interference with the proceedings of the coroner.

Among the other verdicts recorded by coroners, are some which are merely verdicts of fact, as, for example, "Found dead;" "Found drowned;" "Died by the visitation of God;" "Died by accident;" and so on. These are considered to be so unsatisfactory, that in the year 1845 a

letter was addressed by the Registrar-General to every coroner in England, urging him to be more careful in the conduct of his inquiries, and begging of him to improve upon such findings as those to which we have referred.

Before we leave this part of our subject, we might say, that at one time the law took cognizance of every species of casual death which happened without the default, concurrence, or procurement of any rational creature. It did this in order that a greater abhorrence of murder should be raised, and that more pains should be taken to prevent the sacrifice of human life. The penalty in such cases was the forfeiture of the thing which actually and immediately caused the death. This forfeiture was denominated a deodand (*res Deo data*), from the circumstance that it was originally given to the church as an offering for the repose of the dead person's soul. But as it was often difficult to determine how much of the thing in question was actually concerned in the death, and as the forfeiture was manifestly an unjust impost on those who, in the generality of cases, were not in any way culpable, or even concerned in the matter, this part of the coroner's duty has been abolished. It was done away with in the year 1846, by the Act of 9 & 10 Vict. c. 62.

9th. *Can the proceedings of the Coroner's court be traversed, quashed, and amended?*—This we in part alluded to at the termination of our seventh question. It may now be stated, that the inquest of the coroner is never regarded as a final proceeding, but may be traversed to a higher court. It may also be quashed when the inquisition is so defective that no judgment can be given on it, or when the verdict is not warranted by the facts, or when there has been misconduct on the part of the coroner or jury, or when there has been any informality in the proceedings; as, for example, when the jury have not viewed the body. This, indeed, is considered to be so essential to the validity of the inquisition, that no return can be made if the body is not seen by the jury. It appears, in fact, from the words of the statute (4 Edw. I. stat. 2), that at one time the body was lying before the jury during the whole of the inquiry; and although this practice has fallen into disuse, yet the viewing of the body and its identification are so essential, that, according to Jervis, if the body cannot be found, or if it have lain so long before it is viewed that no information can be obtained from the inspection of it, or if there be danger of infection by digging it up, the inquest ought not to be taken by the coroner unless he have a special commission for that purpose. Indeed, it would seem that coroners may be amerced for taking up a body which has lain so long, that, from its state of decomposition, no information can result from the view. Under these circumstances, the investigation of the facts ought to be made by the magistrate, who conducts the inquiry by evidence alone.

An inquest may also be quashed, if, in case of murder, or manslaughter, or *felo-de-se*, the return is not made on parchment.

When an inquisition is quashed, a new inquiry may be instituted by leave of the Court of Queen's Bench; and if there be any imputation on the character of the coroner, he is not permitted again to interfere in the matter, but a *melius inquirendum* may be awarded to take a new inquisition by special commissioners, who are authorized to proceed by the testimony of witnesses, without viewing the body.

We have already said that by the Act of 6 & 7 Vict. c. 83, the judges

have power to order the amending of an inquisition when it is defective merely in technical points—that is, when it is good in substance, but imperfect in form.

10th. *Liabilities of Coroners.*—A coroner is subject to a fine of five pounds sterling, if he be remiss in making an inquisition on a person slain, and returning the same at the next gaol delivery. He is also liable to a fine of forty shillings if he fail to do his office on any person dead by misadventure. He may likewise be amerced and imprisoned at the Queen's pleasure for not doing his duty; or for making a false return; for concealing felonies; for not delivering into court to the proper officers at the proper time, the evidence and finding of the jury in cases of murder or manslaughter. It appears, moreover, that a coroner may be removed from office for being confined in prison for twelve months; for living at a distance from his place of jurisdiction; for being so poor that he cannot answer his fines; or for being disabled by reason of sickness or old age. And, as we have before said, he may have his fees disallowed by the magistrates in those cases where he has held inquests unnecessarily.

11th. *Fees of Coroners.*—In ancient times, when the office of coroner was filled by men of substance and title, there were no fees attached to the appointment; but the coroner merely received a penny from each visne for and towards expenses of his travail, &c. This tribute was made, not as a fee, but out of respect to the dignity of his office. In later times, however, the duties of the appointment have become more onerous, and the men who perform them less wealthy; in consequence of which certain fees or emoluments have been created. By the statute of 3 Hen. 7, c. 1, it is enacted that a sum of thirteen shillings and fourpence shall be paid to the coroner for every inquest on a person slain, the money to be levied on the goods and chattels of the slayer, or on the township in case the murderer escape. The Act of 25 Geo. II., c. 29, directs that a fee of twenty shillings shall be paid to the coroner for every inquisition *duly* taken; and it is further ordained that he shall be entitled to the sum of ninepence per mile for every mile which he travels from his usual place of residence to where the body lies; but no allowance is made for the return journey, or for more than one distance, namely, that of the extremes, however many inquests may have been held on the road. More recently it has been enacted (1 Vic. c. 68) that the coroner shall be paid six shillings and eightpence for every inquest, over and above all fees, &c, to which he is by law entitled; so that the remuneration which he now receives for each inquest duly held is one pound six shillings and eightpence, together with the chance fee of thirteen and fourpence, when the murderer has any goods to forfeit. It is said that some coroners, in remote districts, insist on receiving fees from the relatives of the persons on whose bodies they hold inquests; but this conduct, if made known, would inevitably expose them to the danger of removal from office. (Grindon, p. 32.)

12th. The last question which we propose to consider is the following:—*Would it be advisable to abolish the coroner's office, and to transfer the duties of it to the jurisdiction of others; or would it be better to reform the office, and to strengthen the hands of the coroner by the appointment of a new officer—namely, a medical inspector or post-mortem examiner?*—These questions have already received a considerable amount of attention from the Middlesex

magistrates, who do not hesitate to say that, in their judgment, the office of coroner might be advantageously abolished, and the duties of it thrown into the hands of the police. The chief data upon which they have founded this opinion are the following: 1st, that the proceeding is expensive; 2nd, that it is never final or conclusive; and 3rd, that, considering the number of inquests held, it rarely leads to any practical results. Nothing, however, could be easier than the task of rebutting the arguments which have been advanced upon these data; but we believe that such a duty is unnecessary, for the public mind is sufficiently informed on the subject to make its own conclusions. Nevertheless, we may remark, that in respect of the first of these objections, although the figures employed so skilfully by the magistrates in their report of 1850, may, at first sight, appear to warrant the conclusion which has been drawn from them—namely, that there is an enormous outlay of public money for such inquiries, yet a little examination will show that, after all, the expense of the business is not so great a matter; for, according to the calculations entered into by the editor of *The Times,* in a leading article for January the 29th, "one halfpenny per head is the exact charge upon the population of the western division of the county of Middlesex for the services of its coroner."

With regard to the second objection—namely, the inconclusiveness of the inquiry, it is difficult to imagine how such an objection could ever have been raised, for were we to act upon it, we should be justified in doing away with the magistrate's functions, because in criminal cases they also are never conclusive; so again, it would be advisable to set aside the duties of the grand jury—that pride and boast of an Englishman's heart—for they, likewise, are not in any instance final. To entertain such an idea, however, is to entertain the idea of abolishing the whole system of judicial proceeding as it is now practised in this country: and that, we need hardly say, is a piece of folly far beyond the contemplation even of the Middlesex magistrates.

Thirdly, it is the very effectiveness of the coroner's court which begets that want of practical results—that fewness of convictions to which they have alluded; but we might as well say that an engineer might do away with the water-gauge, or any other tell-tale apparatus in the construction of a steam-boiler, merely because, in all his examinations of the instrument, he never, or rarely, found it indicating anything wrong. But what would be the result of such an insane proceeding? Why, that there would then be no guarantee to the public that the machine was ever in a state of danger. We regard the coroner's office as one of the safety-valves of society, and the public would be guilty of the highest kind of indiscretion if they were to do away with it.

Again, we might say that the transference of the duties of the coroner to the hands of the police, would be the worst change that could possibly be effected. For though every respect is due to the integrity of the gentlemen who now occupy the bench, yet we are quite sure that it is not in human nature to oppose itself to its own interests; and, knowing the manner in which those functionaries are elected, we say that it would be the height of imprudence to transfer to their hands a power, the exercise of which would often run counter to their individual interests. It has been truly said, that the police magistrate has no will of his own when his

views differ from those of the government. We will give an instance of this, which is recorded by Mr. Payne, in his letter to *The Times*. When Sir Robert Baker was chief magistrate of Bow-street, he had orders from the government not to allow the funeral of Queen Caroline to pass in a certain direction; but knowing that if he obeyed those orders, bloodshed and loss of life would certainly result, he did allow the funeral to pass; and for this act of disobedience he was instantly dismissed. Now, supposing that a less merciful man than Sir Robert Baker had been in his place, and that, by obeying the orders of the government, bloodshed and loss of life had resulted, what would have been the consequence? Why, that if the coroner's power were vested in the magistrate, no inquiry whatever would have been instituted, and the public would never have received satisfaction for the wilful murder committed under the indiscretion of an obedient officer.

We will take another case.

"When Mr. Wakley battled so courageously against the inhuman practice of army-flogging, it is well known that the whole influence of the Horse Guards would have been exerted to prevent, or patch-up, the Hounslow case, had not the coroner been resolved to pursue the iniquity into the most secret recesses of its guilt. But what would have become of Mr. Wakley, had his appointment been at the mercy of the government, of which the Horse Guards form part? A word from the commander-in-chief to the home-secretary would very quickly have blown the coroner from his course; and barbarity would have found shelter with tyranny. And so it would be, if, under the proposed arrangement, another Hounslow case were to occur; no magistrate, however spirited or conscientious, would dare to array his individual person against the overwhelming *power* of the Horse Guards, for indeed to do so would be the excess of foolhardiness. What, then, would become of the claims of humanity? Where would they find a protector or an advocate?" (Baker, p. 40.)

Or suppose that a death took place at a police-station, or in prison, from the violence or neglect of those who had authority there. Who is to investigate the facts of such a case? or who is likely to have the hardihood to make the truth of it public?

But this is not the worst part of the scheme; for, if we were to transfer the duties of the coroner to the police, we should not only be giving the magistrate the power of sitting in judgment, in cases of death, and deciding in such a manner as may best suit his individual views, but we should also be centralizing in one or two very subordinate individuals, perhaps the least important members of the force, the absolute power, either of suppressing inquiry, or of unnecessarily promoting it. This is a discretionary power which ought not to be possessed by any irresponsible individual, and we cannot perceive, viewing all the facts of the case, that any good whatever could result from such a change in the coroner's office. On the contrary, there is every reason to believe that it would be attended with the most serious disadvantages; for at the present time the coroner's inquiries are conducted in public; they are instituted before twelve men of the neighbourhood—men who are, from their local knowledge, best able to understand and appreciate the facts of the case. Besides which it must be remembered, that the coroner is a public officer; that he is not answerable for his conduct to any but the public, and is, therefore, most likely to protect its interests. Again, the institution itself has its foundation on

the prejudices of the nation; its importance and value are deeply rooted in the public mind; and its usefulness has been sanctioned by the experience of ages. How, therefore, can we rashly venture to set aside so noble an office, unless its removal is actually called for by the people themselves? No; in our judgment it would be more advisable to reform the court, and if possible to strengthen the hands of the coroner. This may be done in various ways:—by giving a more precise meaning to the word *duly*, by defining the circumstances under which inquests ought to be held; by increasing the dignity of the coroner's office; by giving him a respectable court-house; by not calling his acts in question, unless there is good reason for so doing; by abolishing fees, and paying him as other judges are paid—namely, by a fixed stipend; and finally, by appointing an officer, whose duty it should be to make the preliminary inquiries as to the necessity for holding an inquest, to conduct *post-mortem* and chemical examinations, and, if possible, to act as a public prosecutor. The functions of this officer would be somewhat allied to those which are performed by the procurator-fiscal in Scotland, and the like officer on the Continent; but there would be this important difference between them—namely, that in the case of the medical inspector, there would be no power of pronouncing judgment; for the whole duty of investigating the facts would rest, as now, with the coroner and his jury. By adopting some such a reform as this, we are convinced that the hands of the coroner would be greatly strengthened; that the cause of death would be more frequently determined; that the expenses of the investigation would be considerably reduced; that the officiousness of unfeeling constables would be entirely set aside; and, in fine, that the confidence of the public would be completely secured, and its safety fully provided for.

We are glad to know that a proposition to this effect was unanimously adopted by the recent committee of Middlesex magistrates; and we trust that ere long we shall see it put into full operation. For there can be but one opinion on this subject—namely, that *post-mortem* examinations are not conducted as often as they should be, and that the non-detection of crime is assuredly the result thereof.

We now find that we have devoted so much attention to the *lex coronaria*, and the duties of the coroner's office, that we have not space for a consideration of the many other important matters which are noticed in Mr. Baker's volume. This is a subject of regret to us; and, as we cannot surmount the difficulty, we must content ourselves with advising all of our readers who are interested in these questions to obtain the book for themselves; for we can assure them that it will be found serviceable, inasmuch as it not only contains a good practical exposition of the coroner's duties, but it also contains most of the acts of parliament which refer thereto, and many of the instructions and notices issued by the Poor-law Commissioners and General Board of Health. All of these subjects affect the interests of the profession; and we are not aware that the same body of information is brought together in any other publication.

Art. VII.

Guy's Hospital Reports. Vol. 7. Part II.—*London,* 1851.

I. *On a Certain Affection of the Skin, Vitiligoidea Plana et Tuberosa.* Two Plates. By Thomas Addison, M.D., and William Gull, M.D.

The disease of the skin here described is a somewhat rare one. The authors have selected the above name for it, because it bears some resemblance to the affection denominated *Vitiligo* by Willan. It presents itself under two forms—namely, either as tubercles, varying from the size of a pin's head to that of a large pea, isolated or confluent; or, secondly, as yellowish patches of irregular outline, slightly elevated, and with but little hardness. Either of these forms may occur separately, or the two may be combined in the same individual, in which case the connexion of the two may be traced through an intermediate series of gradations, which clearly demonstrate their essential relations.

Four cases are related. In three of these, the skin-disease was evidently connected with hepatic derangement. In the fourth, the patient was the subject of diabetes—an interesting fact, seeing that modern pathology points to the liver as the faulty organ in that disease.

We extract one case by way of illustration:

"Eliza Parachute, æt. 33, of middle stature, moderately well nourished; mother of six children; catamenia regular. Her present illness began in 1848; she attributes it to fright, and to a blow received in the left groin whilst attempting to separate two men who were fighting. Two days after this she became jaundiced, and had from time to time severe paroxysmal pains about the hypochondria, lasting for a day or two; the liver being also enlarged and tender. Four months after the commencement of the jaundice (August 4th, 1848), she was admitted into the hospital, under the care of Dr. Hughes. She remained in until the 26th of Sept., and left much in the same state she was in when admitted. There was at this time nothing complained of beyond the itching and irritation of the skin common in jaundice. The present affection began after the jaundice had continued fourteen months, when she again came under the care of Dr. Hughes. It first appeared in the hands, spreading across the flexures of the joints of the fingers and palms. Soon afterwards, a yellowish patch of discoloration began near the inner canthus of the eyelid, and then a precisely symmetrical one at the same part on the opposite eyelid. These patches are very slightly raised, and not obviously indurated; they have extended very slowly. In the early part of the year 1850, two models 2733th 2733th were made of the case. At this time the patches on the face existed as above described. Along the ridges bounding the flexures in the palm and about the joints of the fingers, there were yellowish, opaque, irregular, and somewhat raised lines. About the thumb, first joints of the fingers, and inner and interior parts of the wrists, there is a gradual transition to a tubercular prominence of the affected parts, and some distinct tubercles exist on the elbow and knee. The diseased parts are tender, so as to give her pain in using a knife to cut bread. The whole surface of the body is of a dull lemon tint. Various means were employed, without avail, the disease showing a tendency to progress slowly. Through the kindness of Mr. Startin, under whose care the patient now is, we have been able to observe it up to the present time. The jaundice still remains, occasionally deepened by the exacerbation of the hepatic symptoms. The skin is of a dull lemon hue. During the last seven months the affection has become more tubercular, especially about the back of the joints of the fingers of the right hand.

The patch of confluent tubercles on the elbow has much increased since the model was taken. Both elbows are similarly affected. There are also tubercles on the right knee, on the superior surface of the great toe, and on both ears. On the hands, the gradations from the plane to the tubercular variety are well marked, and the essential relations of the two forms demonstrable. This case has been of the greatest value, in enabling us to connect together the cases which had previously occurred. The tubercles about the ears, elbows, joints of the fingers, &c., are of the same character they were in Sheriff's case. They are firm, rather irregular on the surface; have much the appearance, at first sight, of small compound follicles, but on closer inspection are proved to depend upon a change in the cutis. On the surface, small venous capillaries may be here and there seen, producing a mottled appearance. In the hands, we pass insensibly from the tubercles on the back of the joints to the state described in Mrs. B——'s case—namely, the slightly raised, opaque, yellowish lines about the flexures of the palms and fingers. The further identity of the disease in the two cases is shown by the presence of similar patches about the eyelids in both." (pp. 269—271.)

The progress of the disease is very slow, and treatment appears to have little influence over it.

II. *Cases of Fracture of the Cervix Femoris.* With Four Plates. By J. B. Hodgson, Chesham, Bucks.

This paper contains the report of four cases, all in aged subjects, in which perfect bony union took place. They were all treated by the double-inclined plane, constructed of pillows, and were kept in that position for a long period, from fourteen to sixteen weeks. To this the author ascribes his success, and we believe he is right.

III. *Cases in Surgery, with Remarks.* By John Birkett. Four Plates.

The first case is the record of spontaneous atrophy of a very large nævus of the right arm of an infant. Nearly the whole of the upper arm was affected, and the disease extended on to the shoulder and part of the chest. During the time it was under observation, the child had measles, and then, immediately afterwards, hooping cough, so that its constitutional powers were very seriously reduced. Probably enough this low condition of the powers of nutrition had a good deal to do with the cure of the local affection.

The second case is one of subcutaneous nævus on the side of the face, treated by subcutaneous ligature. The cure appeared to be complete for some time, but subsequently the disease recurred.

The next case is an example of abscess forming in a large adipose tumour of the thigh.

This is followed by a very interesting and rare case of *hydatid cysts voided during the act of micturition,* which we extract.

"H. B——, æt. 34, a strong, healthy-looking man, first came under my observation in July, 1851, bringing with him a small number of hydatids, which he said he passed from the urethra during micturition. He furnished the following statement of his case:—Belonging to the corps of Royal Sappers and Miners, he was on duty at Beyrout, in Syria, in Nov. 1840, where he had the yellow fever. He recovered, and continued in good health until August, 1841, when he was, at Mount Carmel, attacked with plague. When sufficiently convalescent, he was sent back to Beyrout, and thence to Malta, where he was put into a room in the quarantine harbour. After being here a few days, he first felt pain in the left side,

for which he was blistered, &c., but never felt any relief. Upon the completion of quarantine, he was sent into the Malta Hospital, and was under medical treatment until sent to England in the spring of 1842. He was then put on convalescent duties, being sometimes in hospital, at others on sick furlough, until August of the same year, when he joined the works, although he continued to suffer very much with the pain in the left side, and now, for the first time, he discovered a swelling in the same region. In June, 1843, he was very ill for several days, and experienced acute pain, indeed, a sensation as if something had broken away from his left side. The next time he made water after this sensation, he passed, to all appearance, blood and corruption, intermixed with 'bladders, skins, &c.,' several of which were coated with a sort of yellow sand (crystals of uric acid?) These attacks have troubled him at different periods, at intervals varying from one to six months, to the present time. He is conscious of an attack coming on, some days before, by feeling a depression of spirits, and constant bearing-down; after violent exertion, as playing at cricket, he has often passed several hydatids, which, I suppose, may be attributed to the more violent compression of the cyst by the abdominal muscles.

"The last attack commenced on Sunday, June 29th, about 1 A.M., and he consulted Dr. Lever, by whom he was kindly sent to me a few days afterwards. He experienced great pain in the left lumbar region, and, as he describes it, a bearing-down pain, extending over the surfaces of the left leg and testicle, until the following Wednesday night, July 2nd, when he passed some hydatids during micturition. He has continued to expel a few nearly every time he micturated, and on this last occasion he may have passed sufficient to fill a half-pint mug. Sometimes the urine would be turbid, at other times clear, and occasionally the urethra was blocked up when he was in the erect posture, but the urine would flow when he reclined or assumed the recumbent posture.

"When I first saw this man, he was recovering from the last attack. He brought a few bladder-like bodies, which were immediately recognisable as hydatid cysts. Some, still larger, were ruptured, and they had been voided in this condition, but the smaller were entire and perfect. Some were unchanged; others appeared to have undergone some decomposition. After a long search, I discovered a single tentacle of echinococcus, and something which had the appearance of the entozoon itself. The cysts were all of them more or less encrusted with fine crystals of uric acid, and there was likewise a plentiful deposit of phosphatic crystals. I passed an elastic catheter into his bladder, and drew off some turbid urine, which contained the *débris* of hydatids, cysts, and blood-discs. After a most careful examination of the man's abdomen and lumbar region, in every attitude and position, I was quite unable to detect any tumour, or even enlargement, nor is there, to the touch, even in the part where the tumour he mentions once existed, the slightest indication of anything abnormal.

"It is, of course, useless to speculate upon the position of the parent cyst, which, in all probability, is seated near the left kidney, having a communication either with the pelvis of this organ, or its ureter.

"Cases of this kind are, I suppose, of rare occurrence, for I am only able to find the following instances: Dr. Copland, in his valuable work (Dictionary of Practical Medicine, art. 'Kidneys,' § 243), states that he once recognised the *débris* of hydatids voided with the urine of a patient, but he gives no further particulars, and a case is related by Dr. Durrant (Provincial Medical and Surgical Journal, March, 1851). My friend, Dr. Hannover, of Copenhagen, had once a case under his observation, and I have heard of another instance occurring in the practice of Mr. Parry, of Docking, in Norfolk." (pp. 300, 301.)

A case of *insuperable constipation*, with a small tumour in the femoral region, which, upon operation, proved to be a nodule of fat, perhaps a part of the omentum, and which was cured by incision of the crural ring, is next related. And the paper concludes with an account of a *glandular*

mammary tumour and cyst, with intracystic growths, which is chiefly valuable for the histological description. We could, however, scarcely make this intelligible without the plate.

IV. *Select Surgical Cases from the Out-patients of Guy's Hospital.* By A. Poland.

The first four cases will well repay perusal; but a mere abstract would be of little value. The concluding group affords a good illustration of secondary syphilitic affections of the mouth, alæ nasi, &c.

V. *Observations on the Treatment of Anasarca, or General Dropsy, by Puncturing the Legs.* By John Hilton, F.R.S.

We strongly commend this paper to the notice of our readers, because we believe that the treatment here recommended is one of much value, and that if the operation be performed in the way Mr. Hilton advises, the risks of mischief will be greatly diminished. The instrument he employs is a long, narrow, sharp-pointed lancet, fixed upon a cylindrical steel shoulder.

"The punctures should be made along the outer side of the leg or thigh, or both, about two or three inches apart—four in the leg, and the same number in the thigh, will be amply sufficient, or, perhaps, more than necessary; and, no doubt, the fewer the better, provided the intention be obtained. Indeed, I may say that I have found two punctures in each part of the limb, oftentimes quite enough for the accomplishment of the intended drainage.

"The instrument, after perforating the skin, should be passed very obliquely from without to within, across the limb, or in a direction from below upwards, through the cellular tissue, between the skin and fascia covering the muscles, so as nearly to reach, but not to wound, the fascia; to intersect the inter-fibrillar meshes of the areolar tissue to the length of an inch or more; it should then be withdrawn through the same track, care being taken to disturb the parts as little as possible, by external pressure with the hand. Extensive subcutaneous incisions of the cellular tissue, by a lateral movement of the instrument, are, as far as I have seen, to be avoided; they are not only not required, but are liable to the objection, that an injury unnecessarily extensive to a structure, little capable of being repaired, or ill-disposed to repair itself, in a constitution already depressed by the same cause or causes which may have led to the general anasarca, would most probably be followed by the dangerous consequences usual in such cases.

"I prefer the outer to the inner side of the leg, because that part is less occupied by lymphatics and veins, and because when the patient is lying upon the back, or sitting up in bed, with the lower extremities everted as usual in a state of repose, the artificial apertures for draining the body so situated, occupy a lower level than could be obtained on the inner side of the limb.

"Immediately after the punctures have been made in one or both limbs, each limb should be invested with or wrapped in a separate blanket, for the purpose of keeping up or retaining the proper temperature of the limb or limbs, and to prevent any abrasion of the skin by friction of the two limbs upon each other; the blankets should be changed for others, warm and dry, before the skin of the limbs becomes chilled.

"Should the delicacy of the patient's skin, or any apprehension regarding the quality or chemical character of the effusion into the areolar tissue, suggest the probability that the skin may become excoriated or much irritated by the fluid running for a considerable length of time over it, the fluid may be made to pursue an exact course away from the limb by fixing upon the limb worsted threads (capillary conductors), so arranged that after crossing over the punctures they shall

enter gutta percha or glass tubes, which can conduct the fluid into receptacles placed within or outside the bed.

"If any doubt exists as to the propriety of puncturing the limb below the knee, from the feebleness of the general circulation, or, as sometimes seen, from a passive congestion of the bloodvessels of the skin (although I may here add, that this latter condition has not prevented my puncturing the legs with great advantage, and without any of the anticipated bad consequences), then it is better that the punctures should be confined to the thigh, whilst the leg, wrapped in blankets, should be a little raised above the level of the thigh.

"When it happens that the limb below the knee is so hard and unyielding (from the character of the effused fluid) as to constitute what has been termed an œdema durum, with the skin in an unhealthy, scabrous state, or thickly crusted over with flakes of unhealthy cuticle, it is advisable then to puncture the thigh only, and to have the leg below the knee bandaged in flannel, and slightly raised upon an inclined plane of wood, placed under the blankets or the whole bed, so as to facilitate the gravitation of the fluid in the leg towards the aperture in the thigh.

"If it be important, from the urgency of the symptoms, to drain the body quickly of its serum, it may be advisable to puncture both limbs at the same time, otherwise I prefer puncturing one limb and raising the other, so that its fluid may descend to the groin on its own side, and thence reach, by crossing the abdominal parietes, the punctures made in the limb on the opposite side of the body. When it seems likely that the puncturing may be required to be repeatedly done, it is well, if the two limbs be in an equally favourable condition for it, to puncture them alternately." (pp. 361—363.)

VI. *Remarks on Death from Strangulation.* By A. S. Taylor, M.D., F.R.S., Lecturer on Chemistry and Medical Jurisprudence in Guy's Hospital.

This is by far the most valuable and important communication in the present number, and as the points of interest are numerous, we shall analyze it with some minuteness.

At the Chelmsford Lent Assizes for 1851, Thomas Drory was tried for the murder, by strangulation, of a female named Jael Denny. He was the son of a farmer of great respectability, and resided within a short distance of the cottage where the deceased lived. Both were about twenty years of age, and the girl, who was pregnant by the prisoner, had reached the ninth month of her pregnancy. On the afternoon of Saturday, October 12th, 1850, the prisoner and deceased were seen conversing together for about twenty minutes in the neighbourhood of the prisoner's cottage. This was about half-past five P.M. The evidence respecting *the deceased*, showed that about six o'clock on this day she had tea with her parents as usual, appearing to be in good health and in high spirits. She told her mother that she had made an appointment with the prisoner to meet him at a stile very near their cottage *at half-past six o'clock;* and the prisoner, it was supposed, had led her to expect that at this interview he would make some arrangement regarding his marriage with her. At, or about, this time, the deceased left her tea half finished, dressed herself hastily in some of her mother's clothing, left the house, and was not again seen alive. She was found next morning at, or about, *eight o'clock*, lying dead in a field at a short distance from the stile at which she said she had made an appointment to meet the prisoner on the previous evening.

"When her body was found, the head was cold, and the arms and legs cold and stiff; but the body (the abdomen) was perceptibly warm to the hand. It will be remarked, that from the time deceased was last seen alive, thirteen and a half hours had elapsed.

"The *attitude* of the body, when found, is thus described by the different witnesses:—The deceased was lying on her face, a little inclined on one side, owing, probably, to the prominence of the abdomen. Her lower clothes were arranged in a straight and orderly manner, and her fur-tippet was lying on the ground, two or three yards from the body. Her bonnet was on her head, but much crushed and broken. It was flattened in front, as if from pressure from behind, while the deceased was on her face. Her face was flat on the ground, and her nose pressed down tightly. The nose is described as being quite flattened, and turned a little to the left side by pressure; it was impossible, in the opinion of one witness, that the mere weight of the head could have produced either this degree of pressure or the indentation observed in the ground. The features were so altered, that although this witness had known the deceased for four or five years, he could not recognise her. When the body was turned over, blood escaped or bubbled from the mouth, nose, and eyes; and the face was observed to be black, and much swollen. There was half a teacupful of blood on the spot where the face lay—under the mouth; and more blood in another spot, about a foot from the head; the hair was matted together with blood and dirt. The right arm was lying bent at a right angle underneath the body, and pressed down by its weight; the left was raised, with the hand directed towards the left shoulder, but partly covered by the body. There was a cord on the neck, which was twisted round it three times. One of the witnesses took the third turn from off the neck, and observed that this turn was a little loose; but on putting his finger to the throat, he found a knot of cord lying in front of the neck. The remainder of the cord was very tight, a portion being actually imbedded in the neck, and the cord was drawn so tightly, that the skin of the neck had swollen up between the coils. From other evidence it appeared, that the knot which formed the loop of the rope was pressing on the fore part of the neck, while the bite of the noose was at the back part, a little behind the left ear. There were *three coils and a half* of rope round the neck, and, with the exception of the last half-coil, all were tight; the two innermost coils being so tight as to indent and cut the skin. The end of the cord went over the back of the left shoulder, and about an inch of its extremity was lying loosely (without being grasped) between the thumb and finger of the *left* hand of the deceased, which was raised towards it. One witness described this hand as being stretched out a little, so that the end of the cord could be seen lying in the hand, before the body was moved or turned over. The deceased was *right-handed*; there was no mark of grasping, laceration, or indentation on either hand; and from the position of the bite of the noose and the direction of the coils, the cord could have been tightened only by pulling to the *left* of the deceased. The cord was stout, and of the thickness of a window sash-line. At the part where the noose had been tightened, the pressure had been so great that the cord was condensed to about half its thickness, and some of the fibres had been cut through by the force used. There was no blood upon it, except just at the end, where there was a small spot. The second coil had, at the back part, tightly locked in a portion of the apron of the bonnet and handkerchief of the deceased.

"A woman, who undressed the deceased six hours after the body was found, stated that she examined her face, and found the mouth bubbling with blood; her tongue protruded out of her mouth, and was clenched very tightly with her teeth. Blood oozed from her eyes, mouth, and ears. Her body, from her head to the shoulders, was very black (livid). There were two marks where the cord went round the neck, quite lacerated through the skin. Upon the back of her left wrist were marks, apparently, of a bite, from both rows of teeth,—the impressions were quite distinct before they were washed, and blood was oozing from them.

On the right elbow a piece of skin had been taken off, about the size of a shilling, and the patch was very black. The elbow had a bruised appearance.

"A *post-mortem* examination of the body of the deceased was made by Mr. Williams, surgeon, of Brentwood, on the second day after it was found. The eyes were much distended and suffused with blood, and the pupils were dilated. There was a general lividity and swelling of the face; and the tongue, which protruded from the mouth, had been bitten by the teeth. There was a superficial laceration of the skin covering the lower part of the throat, on both sides; and there were two deep marks, as if from two cords, or from two impressions of one cord, tied tightly round the neck. The two impressions were both situated over the trachea, and the skin had swollen up between them. The trachea had been flattened by strong pressure, but had regained its shape; it had a bruised appearance in the parts corresponding to the two marks on the neck, and its structure there was softer than natural. There was extreme ecchymosis on the upper part of the chest, such as might have been produced by a heavy blow, or by the pressure of a person kneeling upon it. There was a contraction of the fingers, which were drawn in to the palms of the hands. There was an abrasion of skin at the back of the right elbow. There were marks, apparently of teeth, on the back of the *right* wrist, and there were also scratches on the back of the left arm and hand. On opening the head, there was great congestion of the whole of the brain. The heart was healthy, but much distended on the right side with blood in a coagulated state. The lungs were congested to an unnatural degree; the right pleura was adherent,—a result of previous inflammation. The stomach contained ordinary food, and the coats were in a healthy condition. The intestines were healthy. On opening the uterus it was found to contain a male fœtus in the ninth month; and this was probably alive at the time of the deceased's death." (pp. 372—375.)

Mr. Williams, on examination at the trial, gave it strongly as his opinion that the death was not an act of suicide. He was followed, on the same side, by Professor Taylor, whose examination is so important that we quote it at full:

"Professor Taylor, examined by Mr. James.—I have attended to the evidence given by the various witnesses as to the appearance of the deceased's body. I have particularly attended to the mode in which this cord was found round the neck of the deceased, to its being in her left hand, and to the fact of her right hand being under her; and, from the appearances, I am decidedly of opinion that it was not a case of suicide by her own hand. It could not have been done by herself while lying down, for she could not, in that position, have exerted force sufficient to form such marks on the neck. Taking the upright position, if the first coil was so tight round the neck as described by the witnesses, it would have speedily produced insensibility and loss of muscular power, so that a second coil could not have been tightened round the neck by her own hand, to have produced a second compression and a second laceration on the sides of the neck. Acting with the *left* hand, there would be a very speedy loss of the power of tightening a cord with a noose already formed, unless the cord had a single end with which she could draw it, and it were firmly turned round the hand several times to give a firm hold. From the immediate insensibility which would have followed the complete compression of the windpipe, she would have been found dead, with the end of the cord twisted round the left hand, or some mark on the left hand, of the cord having been so round. I do not believe that the marks on the windpipe could have been caused in any other way by herself than by the end of the cord being thus coiled around her hand or attached to some fixed point; and in this case the last coil ought to have been just as tightly drawn as the two first coils. In self-strangulation, so soon as the windpipe is firmly compressed, by whatever cause, the individual becomes unconscious, and the person can neither tighten the cord nor loosen it. We thus have the power of judging, from the position of the body and

the state of the cord, how far it could have been an act of suicide. I give it as my firm opinion that this could not have been an act of suicide.

"If she were lying down or standing up and the cord was round her throat, she could not cry out or make any noise. Directly the windpipe is compressed an individual becomes powerless, and incapable of making any cry. Assuming a person lying down with a cord tightly bound round her neck, in half a minute she would become quite powerless. . . . . .

"Mr. BODKIN.—Assuming this young girl to have been standing up, and that she died by self-strangulation, would she not fall forward?—*Witness.* If she was pregnant there would be a disposition to fall forward.

"And in consequence of its being sudden, the fall would be likely to be heavy?—It would.

"Would it be likely for a person falling under such circumstances to injure her nose?—Possibly on a hard surface; on a soft surface I should say no.

"The nose would strike against the ground, and the state of injury would depend on the nature of the soil?—It would; but it would cause ecchymosis, not flattening—I should say that in falling there would probably be the mark of a bruise on the face, according to the force of the fall.

"You assume the description given of the state of the neck and of the cord to be perfectly correct in the opinion you have given?

"LORD CAMPBELL.—Of course he does,—the question was put on that hypothesis.

"Mr. BODKIN.—The statement is, that the last coil of the rope was not so tight as the other two?—*Witness.* Yes.

"Have you heard it stated by any person that the three coils of rope were equally tight?—I heard one witness say so; but when I spoke about the tightness of the third coil, I alluded to its being held firmly in the hand or fixed to a certain point.

"Supposing the person used both hands, would not the injury to the neck be greater?—If the party used both hands, and there was only one coil round the neck, there might be great injury to the neck produced, but if there were three coils the injury would be probably less, although sufficient to produce death.

"I believe the strength exerted in cases of determined suicide is very great frequently?—It is.

"Supposing a person determined on suicide to wind a small cord of this kind round the neck three times, and to pull it (i. e., one end of it) with both hands till insensibility was produced, and then fall, is that a state of things which, in your judgment, is likely to produce the injuries described?—I do not think that the violence found could be produced even if the person took both hands,—I do not think that with either one or both hands those injuries could have been produced. As I understand, the end of the cord was to the left, and the end of the cord must have been pulled in that direction, and consequently the right hand would not come much into play at all to exert any degree of force.

"Supposing the hand to grasp the cord tightly in the act of strangulation, is it not possible to relax it some hours after death?—Not where there is a tight convulsive grasp on the cord.

"Would the jar of the after-fall not have a tendency to make the hand let go of the cord?—I think there would be such an amount of power required to effect the injury by the twisting of the cord round the hand, that the cord would not be released from the hand by any accident of that sort.

"Is that an opinion theoretically given, or did you ever see such a case?—I only know of one instance of self-strangulation in this way, and then the cord was found in the hand.

"Have you ever known a case in which the grasp had been subjected to this test of the after-fall?—No, I do not know that I have.

"I think you said, if you had found the end of the cord once round the hand of the woman tightly, it would have made a difference in your opinion?—It might,

because it would have had a fixed point to draw from; that is, if it had been firmly stretched.

" By firmly stretched, do you mean from the hand to the neck?—Yes.

" Would not a ligature of this sort cause the parts around it to swell?—It would, if tightly pressed.

" Would not that swelling present the appearance to a non-professional person of greater indentation than there really was?—As I understand Mr. Williams to state, there were two depressions, and the skin was greatly raised up between each; and there must have been great compression, as well as swelling, to force it up between the two.

" These questions of strangulation, I believe, are very difficult ones?—They are difficult, but not so difficult if we know the exact state of the body and cord; for then, I think, they may be distinguished. Suicidal might be easily mistaken for homicidal strangulation—homicidal strangulation cannot be easily mistaken for suicidal, because it is scarcely possible to place the body, or the cord round the neck, in such a way as to deceive those who have attended to these subjects.

" In a case of decided homicidal strangulation, is it not easy to simulate suicide?—No, because in such cases too little or too much is done; in this case there was too much done. There may be cases in which it is difficult to distinguish between them, but there are others in which there is no difficulty; every case must be judged of by its own circumstances.

" In your book I find you refer to a celebrated case, which was pronounced by some to be suicidal and by others homicidal; there was a handkerchief tied round the neck so tightly that it was difficult to remove it. [He read the case.] I believe that gave rise to considerable discussion, and you in your book allude to it?—Yes: but in that case there was a knot to keep the handkerchief fixed, and the persons to whom it was referred, formed an opinion that it was a case of suicide from the moral circumstances.

" In another case stated in your book, a woman was found lying on her stomach in her bed, and a woollen garter tied twice round her neck with two knots, and the circumstances of that case, you state, left no doubt that it was a case of suicidal strangulation?—No doubt of it; but the circumstances were widely different from those observed in this case.

" Would not the first knot of the ligature round her neck have had the effect you speak of in this case of depriving her of consciousness and power?—No, because the first knot, it appears, was put on the throat above the larynx, and the second knot was over the first; I imagine that the first constriction was not very tight, and was on that part of the neck where it allowed a certain degree of respiration.

" But it caused death?—It was sufficient to cause death, although probably not immediately, because death may be produced by any partial obstruction of respiration, as by a stocking tied round the throat.

" I will call your attention to another case here, that of a lunatic—the patient was found with his head hanging over the side of the bed; and when it was attempted to raise the body, it was found that respiration had ceased, and that the neck was constricted by a handkerchief folded up like a cravat; the first end of the handkerchief was exceedingly tightly tied with a knot, and then carried round and fastened by another knot; there was no doubt that this was a case of suicide, but the reporter says if deceased had been found in a lone place, it would have been taken for a case of murder?—Yes; but the first turn of the handkerchief did not compress the *windpipe* so as to prevent respiration entirely, and it would therefore allow the possibility of a second turn and a knot in the same way.

" That fact does not appear in the report of the case?—No; but in both cases it is rather too loosely stated in the original reports where the pressure was. If it was above the larynx the person might breathe a minute or two; it is only where it is *on the windpipe* that we have that immediate and entire loss of power. I never tested it on myself; but an eminent Austrian practitioner has tested it on

himself, and he finds that the effect materially depends on the part compressed by the ligature.

"I believe a few years ago it was generally received as an opinion that persons could not strangle themselves?—Not generally, but only by those who had not studied the subject.

"Is there not another case where a person effected suicide with a stick forced into a handkerchief, and twisted round and fastened in his bosom?—I have heard of it, but I am not prepared to admit that the first turn of the cravat or ligature was calculated to produce death immediately. It is a very common sort of suicide for persons to put a ligature loosely round the neck, then introduce something to tighten it, like a tourniquet, and so produce death; but that, in such cases, the first turn of the stick necessarily produces immediate insensibility or death, I am not prepared to admit.

"Re-examined by Mr. JAMES.—Must there not in this case have been a great deal of violence used to make blood flow from the nose, mouth, and nostrils of the deceased?—I think so decidedly.

"Are you still, from the appearance of this body, of opinion that it was not an act of suicide?—I am. The cases quoted are all cases distinguished from *this*—there the ends of the ligatures were either tied in knots, and thus secured, or the ligatures were mechanically tightened by twisting with a stick.

"Do you give in this case a decided opinion?—I do; that it was caused by another hand, and was not an act of suicide. There are cases in which there is no doubt, and I think this is one of them—it is an act of homicide by the violence of another." (376—380.)

The evidence which connected the prisoner with the alleged act of murder was as follows:

"It was distinctly proved that, for a considerable time, he had been in habits of close intimacy with the deceased—she was pregnant by him,—it was reported he was about to be married to another person, while he had deceived the deceased by leading her to suppose that he would marry her; and the last fatal interview, it was presumed, was had with her under the pretence of making some positive arrangement on this subject. Hence her great anxiety to keep her appointment, leaving her meal half-finished. The prisoner had lately found her an incumbrance, and was evidently desirous, in some way or other, of shaking off the connexion. Some time before her death, he had said to one witness:—'he would like to shove her off, for he was afraid she would bring him into trouble.' On the 29th of September—i. e., fourteen days before her death—he caused her to write a letter, affirming that her pregnancy was not by him. On the 1st of October, two days after this paper had been procured, while ploughing in a field, he told a witness that he had this letter in his possession, and said—'You will hear something very serious about Jael Denny: she told me on Sunday morning she would make away with herself.' At this time he appeared confused, and did his work badly. When arrested on the charge of having committed the murder, he stated that he had a letter to show that the child with which deceased was pregnant was not his. He said to a witness voluntarily, that he had heard her mother say she (the deceased) would destroy herself, and added—'the mother came to me, about fourteen days ago, and asked if my razors were all right; I said I would go and see, and I went and found them all right, and I asked what she asked such questions for,—she replied,—'I heard Jael say she would destroy herself, and I thought she would get at your razors.'' The evidence of the mother at the trial proved that the whole of this statement was untrue, and a pure invention on the part of the accused. Such is an outline of the *moral* evidence against the prisoner; and it was alleged by the prosecution to supply a sufficient *motive* for the act, and a desire to conceal it when perpetrated, under the pretence of suicide. The mode in which the cord was found lying near the left hand of the deceased, also tallied with the view that a rude attempt had been made by the party who had perpetrated this crime to simulate the act of suicide." (pp. 380, 381.)

Besides this, there were stains upon the prisoner's clothes when he was arrested, which, as will be more fully seen afterwards, were proved to be mammalian blood; while he referred them to a vegetable substance which could not possibly have produced them. And lastly, the prisoner's time was unaccounted for during the period which would have corresponded to the appointment and the time of her death. We here again quote from the paper before us.

"In cases of this kind, questions of time are of vital importance. For reasons to be hereafter stated, it was inferred that, considering the state in which the body was found, the death of the deceased might have taken place soon after she left her house to meet the accused, or at a later period of the night—i. e., from about seven o'clock in the evening up to half-past ten or eleven o'clock. The evidence for the prosecution brought out the following facts regarding the prisoner:—When asked by the mother, on the morning of the discovery of the body, where the deceased was, he said:—'I don't know, I have not seen her.' The mother said:—'You did see her yesterday, between four and five o'clock, and talk to her, and appointed her to meet you at the stile near my house at half-past six; she went out to meet you, and where did you leave her?' He replied:—'I have not seen her *since five o'clock*. I went to Brentwood.'

"It was proved by the evidence of one witness that deceased and prisoner were seen together on the Saturday evening at the stile at half-past five o'clock; they remained there about twenty minutes, which would bring the time to about ten minutes before six o'clock. The witness who deposed to the fact, left his work about six o'clock, and at that time received his weekly wages from the prisoner, who was then in his own house. He told the prisoner he was going to Brentwood, and the prisoner asked him to bring a saddle from that place for him, saying that he (the prisoner) should not go to Brentwood that night. This witness returned from Brentwood at ten o'clock that night, and left the saddle at the kitchen-door of the prisoner's house. He saw no more of the prisoner from the time his wages were paid; but it is to be inferred from the deposition of this witness, that the prisoner would have occupied another half-hour in his usual duties. This would bring the time to *half-past six o'clock*, the hour of the appointment made with the deceased.

"The next date is fixed by himself. When taken into custody, he asked a witness named Hammond what he was doing in his field so late on Saturday night. The witness Hammond did not see the prisoner, and would not have known that he was near the field, but for the question thus put. This field is situated between the spot where the body of the deceased was found, and the town of Brentwood; and there is a road by the side of the field along which the prisoner must have passed. This witness was in his field from *eight o'clock* to twenty minutes past eight. A little after *eight o'clock* the prisoner was seen entering Brentwood with a basket of eggs, which soon after that time he delivered at the house of a person who had ordered them, and he remained in the house of this person about twenty minutes, when he went out into the town. The time was fixed by the prisoner himself looking at his watch and comparing it with the clock.

"Evidence adduced in the defence for the purpose of establishing an *alibi*, only confirmed the testimony of these witnesses. A woman who acted as housekeeper to the prisoner, as well as her husband, deposed that he came home from his work between five and six o'clock on Saturday afternoon,—that he had his tea, paid the men their wages, occupied himself in dressing, and that it was between *half-past six* and seven o'clock when he went out. The prisoner said he was going to Brentwood, and took with him a basket of eggs, which, in the course of the week, he said he should require on Saturday night. He took the key of the house with him, according to his usual practice. This witness went to bed at eight o'clock, and saw nothing more of the prisoner that night. She got up the next morning

about *six o'clock*,—the prisoner got up about the same time, and she saw nothing unusual in his conduct. He brought some sugar and other articles from Brentwood, but she knew nothing of the hour of his return." (pp. 382—384.)

For the defence, two surgeons, Mr. Thorpe of Maldon, and Mr. Pollock of Hatton-garden, deposed—the first that he thought there was a *doubt* as to whether the deceased committed suicide or not; the second, that he would feel *considerable difficulty* in forming an opinion as to the cause of death, whether suicide or homicide. Both of these opinions were founded upon cases which they had met with, and to which reference will be made presently.

The prisoner, however, was found guilty, and before his execution, made a confession, in which he stated that he met the deceased by appointment at the stile about half-past six o'clock on Saturday evening; but in the meantime he had gone to a cellar in his father's house, and taken from it part of a rope which was lying there. On meeting her for the second time, he said that he and deceased talked and walked about, after which, at her suggestion, they sat down on a bank. She had come to urge him to marry her. He passed the rope gently round her neck as they were sitting, and had got the end of it into a loop before she perceived it. She jumped up at once, and put up her hands to save her throat (at which time the marks of injury found upon them must have been inflicted), but he pulled hard, and she fell without a struggle. He then left her lying in the field, and went to Brentwood.

The medical evidence in this case resolves itself into the five following divisions:

1. *What was the cause of death?* This need not detain us. It was manifestly *strangulation.*

2. *When did death occur?* The evidence showed that the deceased left her home at half-past six P.M., and was found dead at eight A.M. the next morning, at which time the abdomen was still warm, but the head, arms, and legs were cold, the latter rigid. The average period of the cooling of the body of an adult, in a room at a temperature of about 60°, is about fifteen or sixteen hours; but many examples of slower cooling are on record. In this case the body was exposed to the external air, and would cool more quickly; but nothing definite could be drawn from the observed circumstances.

3. *Was the act of strangulation homicidal or suicidal?* This was the most important question; and we think it well to quote the series of facts and inferences which led Dr. Taylor to give the very decided opinion which he pronounced, because they exhibit, in a remarkable degree, the careful and minute observation, and the clear reasoning, which are so essential in the investigation of cases of this nature.

"1. The deceased was *right*-handed; and on the hypothesis of suicide, she must have made the tension with her *left* arm and hand. From the position of the loop or noose, any traction to the right would not have tightened but have loosened the cord.

"2. That, supposing her to have exerted such a traction at all, she must have been in the erect or sitting posture. The force used, indicated by the great local violence to the neck, could not have been exerted by a person attempting to tighten a cord by drawing it to the left while in a recumbent posture, whether prone or supine.

"This hypothesis would, besides, leave wholly unexplained the flattening of the nose (obviously from direct pressure, not from a fall), and the fact that deceased had bled in *two* places, one spot being a foot from the other.

"3. That the cord must have been pulled with excessive violence in a horizontal direction by *one end only*, as the mark was *circular* round the neck.

"The other end of the cord formed a noose or loop, and was tightly fixed at the back of the neck. Thus, then, all the force of traction must have been exerted to the left, in which direction the right hand of a right-handed person could not act horizontally, so as to produce the amount of violence found on the soft parts of the neck.

"4. That the fact of there being three coils and a half of rope round the neck, formed an obstacle to the tightening of the cord by pulling *one* end to the left so as to imbed the two inner coils in the skin, and to leave the outer or third coil loose.

"On the supposition that the deceased produced the constriction by her own act, it follows that the three coils must have been round the neck at one time, and the two inner coils sufficiently loose to allow of respiration before traction was commenced.

"5. The double indentation found on the trachea could not have been produced by the two inner coils (on the supposition of suicide), except by the great tightening of the outer coil.

"6. As insensibility and loss of power must have immediately followed the complete compression and obliteration of the trachea by the two inner coils, the outer coil ought not to have been found loose or unconnected with the object by which the force of constriction had been produced.

"To suppose that the deceased could have produced the intense constriction by the first coil, and afterwards retained sufficient power to pass a *second* coil from right to left around her neck, indenting the skin, and flattening the trachea as much by the second as by the first coil, involves, in my judgment, a physiological impossibility. There was therefore, on the suicidal hypothesis, no explanation to resort to,—but that all the three coils had been placed at once round the neck *loosely*,—that *one* end only of the cord had then been so pulled to the *left* as to produce the great amount of violence found and to tighten equally the two inner coils; while the outer coil and extremity of the cord, by which this immense force must have been applied to the two inner coils, was found lying *loosely*, without any attachment either to the hand of the deceased or to any other fixed point.

"7. To have indented the neck, compressed and bruised the trachea in two distinct places, to have caused effusion of blood to the amount of a cupful from mouth, nose, and ears,—this effusion being found in two distinct places a foot distant from each other,—would have required a very considerable tension of the outer coil, and, at the same time, a *continued* tension,—lasting sufficiently long for the head to move a foot after a cupful of blood had been lost as a mechanical result of the first constriction.

"8. Admitting such conditions of the body and cord to be compatible with suicide, the act could only be conceived to be possible in this case, by the fact of the end of the cord being found tightly wound round the left hand of the deceased.

"9. On the suicidal hypothesis, it would have undoubtedly required a very firm grasp of a rope to produce such effects as were here observed; and from the rapid production of unconsciousness by the compression of the trachea and the arrest of respiration, it would have been impossible, on the part of the deceased, to relax the grasp. Hence the cord should have been found either firmly held in the hand in the rigidity of death, or wound round it in a state of tension.

"Unless we adopt this view, we must suppose that after having used an enormous amount of violence by a rope in the left hand, the dead body had the power of relaxing the grasp,—of loosening the outer coil of cord, and so moving the hand that the end of the cord should be found lying between the finger and thumb and barely touching the palm.

"Such a condition is not only physiologically, but in this case, as it will be presently shown, from the length of the cord, physically impossible.

"10. Admitting the improbable supposition that the end of the cord had been wound round the hand of the deceased, or grasped by it with the necessary firmness to cause laceration of the soft parts of the neck, and an effusion of blood from mouth, ears, and nose,—and that it had fallen from the hand subsequently to death by a spontaneous relaxation of the muscles, it is reasonable to expect that the soft parts of the hand would present some indication of such a force of traction having been employed by the deceased; but the palms of the hands and fingers presented no appearance whatever of such an extraordinary exertion of muscular power having been made by the deceased. There were marks of violence at the *back* of the hands and on the arms, out of the course of the cord; but these were of such a nature and in such a situation that they could not have been, by any possibility, produced by it.

"11. The *length* of the cord renders it impossible to suppose that such a force could have been exerted by the deceased herself. I ascertained, by measurement, that its total length was *fifty-nine* and *a half* inches. It has been already stated, that there were three and a half coils round the neck, the first quite tight and close to the skin; the second also tight, but enclosing part of the apron, of the bonnet, neckerchief, and cloak; and the third more loose than the other two. Making due allowance for these circumstances, and the fact that the circumference of the lower part of the throat of a full-grown woman like the deceased, cannot be taken at less than *fifteen* inches, the three coils and a half would have consumed fifty-two and a half inches of the cord, leaving only *seven inches* for the traction! This was barely enough to reach the finger and thumb of the raised left hand, and not enough to allow of such a firm grasp by the hand as would be necessary to the production of so much violence to the soft parts of the neck. I find, by measurement, that the circumference of a small female hand in the adult is rather more than *seven inches*. This measurement includes the palm of the hand only without the thumb, and embraces that part of the hand around which a coil would be placed when the object of a person was to produce firm traction.

"Hence, then, the hypothesis of suicide involves one of these physical conditions. Without a firm hold of the cord, which could not have been had with less than one coil round the hand, it is impossible to conceive that such violence to the neck could have been produced by the act of the deceased; and if one coil had thus been spontaneously wound round the hand, it would have consumed the whole length of the cord up to the last half coil, and left no portion whatever to give a purchase for pulling with so much violence. Either condition is a physical impossibility; and no theory will suit the facts or explain them, excepting that which admits that the act was not the result of suicide, but of manual violence applied by another person." (pp. 391—395.)

The following are the cases which led Mr. Thorpe and Mr. Pollock to give evidence that the strangulation of Jael Denny might have been suicidal:

"Mr. Thorpe's Case.—'C. H.—, æt. 58, left his home at 7 A.M., for the purpose, as he stated, of going to London to present a bill. On his return home, he effected self-destruction in the following manner: he passed a noose of cord over his head, and then inserted a stick, about fourteen inches long, between the cord and his neck. Having done so, he, with the assistance of the stick, twisted the end sufficiently tight so as to cause, I should say, almost immediate suffocation (strangulation?) Still it appeared there was time for him to insert in the lower end of the stick in the inner side of the waistcoat, and the upper end was accurately adapted to the internal jugular vein and carotid artery. The date of this suicide was the 11th of February.' (1851.)

"In this case, it will be seen, the deceased committed suicide by employing a circular ligature and tightening it with a stick, on the principle of the tourniquet.

The cord was tightened by the twisting of the stick, sufficiently to obstruct respiration, and the stick was retained in the inside of the waistcoat in such a position as to prevent it from slipping or loosening the cord. Here, then, was a fixed point found for the constriction, and sufficient to explain it. There was nothing in the position of the stick or the cord incompatible with the fact of the man having perpetrated the act himself. There was no more violence to the neck than the stick and cord could have produced by the deceased's own act. There was no effusion of blood from nose, mouth, and ears. The man was not found with his dress torn, his nose flattened, and other marks of recent violence on his person. Had these appearances existed on the body, and had the stick been found loose, or lying at a distance, with no fixed point from which the constriction could have originated, it is probable that the opinion of suicide would not have been entertained in this instance, or at any rate considerable doubt would have arisen, whether it might not have been an act of homicide. As it is, the case merely proves that a person may strangle himself by the aid of a noose and a stick: and there are several such instances on record. In no one point, except in the cause of death being the same, does it bear any analogy to the case of Jael Denny.

"Mr. Pollock's Case.—'Pizzala, an Italian, about fifty years of age, employed as a porter, was found dead, in the forenoon of the 3rd of January, 1851, in an attic of the house of his employer. He had been missing from his employment thirty hours. When found, he was lying on his back, rather inclining to the left side, with a piece of ordinary sash-line coiled *four times* around his neck,—two of the coils so tight, and imbedded therein, that there was some difficulty in undoing it. The right hand held one end of the line, and the left hand the other, with a turn of the line around each, to hold it the more securely. The right arm was extended,—the left flexed. I made a *post-mortem* examination of the body, on the fourth day after it was found. *Externally*, the face was swollen and purple; the vessels of the conjunctivæ were congested,—the tongue protruded towards the left side,—bloody froth issued from the mouth, and the lower jaw was slightly twisted towards the left side. The skin of the neck was abraded in a nearly continuous line around it, about five-eighths of an inch in width, and presenting the appearance of being produced by two coils of the line. There was considerable ecchymosis above and below the line of abrasion. Each hand retained the impression of the line being coiled around it. *Internally*, the vessels of the brain and its membranes were greatly congested.

"'The evidence before the coroner left no doubt of this having been a suicidal act.'

"This case proves that a person may strangle himself, and that he may accomplish strangulation by pulling the two ends of a cord coiled several times round the neck; and that some degree of local violence to the neck may thus be produced by the ligature used. These are points which could not reasonably be disputed, even supposing that no such case as that of Pizzala had occurred. In what way, however, would the facts of this case justify a medical opinion that the deceased, Jael Denny, had destroyed herself? She was on her face with her nose flattened, and other marks of violence on her person. The cord was not held in, or wound round each hand, and there was no mark or impression to indicate that it had ever been coiled around either hand. In Denny's case blood had been forced out of mouth, nose, and ears in two places, and the amount of local violence was much greater than in that of Pizzala. It is clear, then, that in the left hand of Denny, if this had used the force, a much stronger impression should have been found than in either hand of Pizzala. In the case of Denny the cord was not long enough to form even a coil round one hand, while in that of Pizzala it was long enough to form a turn round both hands, the right arm being extended. In Pizzala's case both ends were strongly pulled to tighten the coils; in Denny's case there was only *one end* which could be pulled, and this was of insufficient length to allow of that amount of traction even with *one* hand which would correspond to the local violence!

"There is, therefore, no parallel between the two cases, except in the mode of death, and the means used to strangle. Had the facts regarding the body and the cord in Denny's case been the same as that in Pizzala's, no medical witness would have been justified in asserting that the act was one of homicide. It was simply because the facts were *not* the same, that a decided opinion was given by the witnesses for the prosecution, that the act could not have been suicidal. I have met with such a case as that of Pizzala; one coil of the cord only being round the neck, and the two ends being firmly wound round both hands. The person was found dead with the ends of the cord in both hands. I do not know that we can possibly have any better indication of suicide than this; for no murderer who has used a cord to strangle another would be able by winding the ends round each hand of the dead body, to imitate the tight convulsive grasp (passing from muscular contraction into cadaverous rigidity), by which alone the act of suicide under such circumstances could be accomplished. If he turned the cord around each hand, how is he to imitate or produce the impression which is left by the force used (in suicide), on the living hand? The existence of these impressions led justly to the inference of suicide in the case of Pizzala; their absence, with the other circumstances mentioned, to the inference of homicide in the case of Jael Denny." (pp. 397—400.)

Dr. Taylor relates some other cases very interesting and instructive, but we have not space to notice them.

4. *Within what period of time may death from strangulation take place?* This depends much upon the point at which the neck is compressed.

"On this subject, the experiments of Fleischmann made upon himself are of great interest. It was to them I referred in my evidence. This gentleman found that when he placed a cord round the neck, in the space just *below the chin*, it might be strongly drawn, either laterally or behind, without the act of respiration becoming perceptibly disturbed, and a person may, under these circumstances, inspire and expire for a long time. This is what might have been anticipated, since a ligature so placed nowhere compresses the air-passages. But under this constriction, the face becomes red, and the eyes congested and protruding; the head feels hot, there is a sense of weight, followed by vertigo, and there is suddenly a hissing noise in the ears. This last is a symptom of impending danger; and unless the experiment be discontinued at this time, the result may be fatal. These effects show an interruption to the return of blood from the head.

"Similar effects are produced when the cord is applied *over the larynx*. In this case the symptoms come on with greater rapidity, and there is from the first some difficulty in breathing. The previous experiments could be carried on for two minutes: but in this case, scarcely thirty seconds had elapsed before the hissing noise in the ears came on, and with this an indescribable sensation in the brain. The variation in the seat of pressure explains the difference in the latter case; there would be a greater mechanical interference with the act of respiration, and a greater obstruction to the return of blood from the head.

"When the pressure is over the *thyroid or cricoid cartilage*, there may be a slight degree of respiration. When it is *below* the cricoid and over the trachea, the interruption to respiration is instantaneous, and the pressure can be borne only for a very short period.* The effect of a complete obliteration of the lower part of the trachea by a ligature, and consequent constriction of the neck, was not tried by Fleischmann, as from the effects produced by slight pressure at the upper part of the trachea in connexion with the larynx, such an experiment might have proved fatal.

"These experiments clearly prove, that the power of continuing to respire, without which, in some degree, acts of volition, consciousness, and muscular exertion cannot be performed, depends on two circumstances:—1. The precise seat of

* Devergie, Médecine Légale, vol. ii. p. 370.

compression, and 2. The amount of compression. It is worthy of remark, that, according to Fleischmann's previous observations, if the compression should be immediately over the os hyoides at a time when the person is in the act of expiring, the dangerous effects come on very rapidly,—obviously from a closure of the glottis." (pp. 404, 405.)

The Spanish punishment by the *garrotte* affords evidence of the rapidity of death by strangulation, when there is a close constriction of the trachea. A collar is placed round the neck, which is capable of being tightened by a screw from behind. In the late execution of General Lopez, at Havannah, his head is stated to have dropped forward, and he was lifeless simultaneously with the *first* turn of the screw.

5. *Evidence affecting the accused from stains on his clothes.*—When the prisoner was arrested, some dark stains were observed on the front of a pair of corduroy breeches which he wore, and which he admitted he had worn on the evening of the day that he went to Brentwood. The evidence of the superintendent of police was to the following effect:

"The blood appeared quite fresh, and the patches were moist or damp. I pointed them out to the prisoner, and asked how they came there. He said it was not blood, but some stuff he had had to give the calves, and if I went with him to his house, he would show me the pot where it was kept. He took me to an outhouse, and, pointing to a pot, said, 'that is the pot where the stuff is.' On taking it down from the shelf the mouth of the pot was open, and covered with cobwebs. There was a wooden spoon in it, the handle of which did not reach to the edge of the pot, but was quite inside. On showing the prisoner the cobwebs on the mouth of the pot, he said he had not used any of the stuff for a long time.

"The stains in front of the small-clothes were not so well marked in colour or appearance as some small spots at the back part, in the bend of the ham; these had evidently escaped notice. The stains in front were of a red-brown colour; they had evidently been wiped, so that the blood had been removed from the prominences of the cords, and carried into the deep grooves of the stuff. It was in this situation, and by the aid of a powerful magnifier, that small, solid coagula were seen, having the colour and glistening aspect of fresh blood,—the fibres of the corduroy around being stiffened. In a few of the small spots at the back part of the clothes, which had not been disturbed, the coagula were well marked. The stained fibres cut from the grooves of the cords, and examined microscopically, under a power of from 300 to 400 diameters, presented the peculiar form of cotton deeply stained at the tips by a red colouring matter, having the crimson-red appearance of blood; and in the spots which had not been interfered with, the apex of each cotton fibre was coated with a dark-red clot or solid coagulum." (pp. 411, 412.)

On chemical examination it was found that the whole of the corduroy had been dyed with iron. On destructive distillation of an *unstained* portion, acetic acid only was evolved. By the same process applied to a *stained* piece, abundant evidence of the presence of nitrogen and sulphur was procured, showing that here animal matter was present. By maceration of a portion of one of the clots, in the corduroys, in water, holding a small quantity of white sugar in solution, as prepared by Dr. Rees, blood corpuscles were discovered by the microscope. Dr. Taylor enters fully into the question as to the possibility of determining whether the stains were produced by human or other mammalian blood, whether it was arterial or venous, living or dead; but on these points, none of which can, we suspect, be determined with any accuracy, we must refer to the original.

We must also follow the same course with reference to his examination

of the changes produced in red colouring matters by a common article of dress dyed with oxide of iron.

VII. *Two Cases of Pregnancy, with Cancer of the Cervix, in one of which the Cæsarean Section was successfully performed.* By Dr. Oldham.

The first of these cases deserves especial notice, both from its own interest and from the evidence it affords, that *sweeping condemnations*, to which certain authorities are somewhat too prone, are not always the most judicious.

"Sarah L——, æt. 28, a woman of middle stature, light hair, and blue eyes, was admitted into Guy's Hospital, under the care of Dr. Oldham, June 4th, 1851.

"She was the wife of a chimney-sweep, and had had five children, the last two years since. Her labours had been favourable, but only one of her children had survived, the others having died when young.

"Her health was good, and, as far as she knew, the uterus was free from disease up to the time of her present pregnancy, which was now advanced more than seven months. At the commencement of it she had lumbar pain, of a dragging character, followed by a slight protrusion of the anterior wall of the vagina, which she ascribed to lifting some heavy furniture. As gestation progressed, and especially after quickening, she suffered in increasing severity from uterine pain, occasional hæmorrhage, and a more or less copious discharge, sometimes thin and sanious, and at others thick and yellow, the result of a rapid development of malignant disease of the os and cervix uteri; for this she was attended by Mr. Wilson, of Cannon-street East, who sent her to Dr. Oldham, at whose especial request the treasurer admitted her into Guy's Hospital for the purpose of delivery.

"On admission her general health was not materially affected. She was neither emaciated nor anæmic; her appetite was good; her pulse 80, of good volume. The bowels were constipated, and unless relieved by medicine, she suffered additional pain.

"The uterus is the seat of almost constant suffering, aggravated at times into agonizing distress, affecting the loins, lower belly, hips and thighs, with sharp lancinating pains centrally through the pelvis. The discharge was abundant, of a sanio-purulent character, mixed with the desquamations from the diseased structures, and of a faint but not very offensive odour.

"When lying on the back the uterus was felt about two inches above the umbilicus, its outline throughout well defined, and it inclined to the right side. The fœtal heart was readily heard, and the active contractions of the fœtal limbs were frequent and short, and excited much pain *internally*. The whole of the lower segment of the uterus, which could be felt by the finger, was converted into a mass of malignant disease, which, in extent, appeared nearly to fill the upper part of the pelvis without even a margin of normal tissue. The substance of the disease was perfectly hard, and it was broken into large lobes by deep ruts and fissures, into any one of which the finger readily entered, and penetrated to at least an inch in depth; but the true os uteri could not, from amongst them, be with certainty ascertained, and the presenting part could not possibly be felt. The anterior wall of the vagina protruded beyond the vulva, and on attention being drawn to it by the patient, it was seen to have a diaphonous appearance from extreme thinness. She was often obliged to press this part inwards before passing water. Sexual intercourse had been abandoned for three months on account of the discharge.

"During the month which elapsed before labour, the treatment of this patient was almost entirely directed towards relieving the extreme local suffering, her powers being sustained by a good diet, which she was able to take, and the bowels being habitually regulated by small doses of castor oil, which we found could be counted on as a certain and valuable aperient. She was kept in bed; and the principal sedative was morphia, which acted most favourably. Stramonium was on

one occasion given, but its effect was unsatisfactory; chloroform with camphor and small doses of opium was prescribed, but it was abandoned at her request. Some local relief was obtained by belladonna plasters, and a liniment of tincture of aconita with extract of belladonna. From the first chloroform was ordered to be inhaled whenever the pain was more than usually severe; and Dr. Holman, the clinical clerk, at whose discretion and by whom it was always administered, reports:

"'The chloroform acts beautifully, evincing but slight influence over the pulse or respiration, and taking complete effect in about a minute. It was administered on lint. It causes an entire cessation of the pain for about two hours, and a gentle slumber for half an hour to an hour. A profuse sweat appears to be the only unusual effect.'

"During this time the contraction of the body of the uterus, as in labour, was frequently noticed, and probably caused much of her suffering. When this was the case the uterus became smaller and perfectly hard; the fœtal heart was at once heard with great clearness, and often the uterus would be observed to have moved from a distinct right lateral obliquity, either towards the centre, or even with an inclination to the left. Morphia, or a starch-and-opium enema, would relieve this; but the fœtal movements, and the irritation from the diseased cervix, would re-excite it, and after a time the uterus would remain in a state of tonic contraction for hours, both it and the abdominal surface becoming tender to the touch; when again it would slowly relax and the fœtal limbs again be felt.

"On the last two days of June, and the first two of July, these uterine contractions were more than usually severe, having at different times the appearance of labour.

"July 2nd, 3 P.M.—After intense suffering, for which she inhaled chloroform, and took morphia, some dribbling of fluid, which was supposed to be liq. amnii, occurred; and this continued during the evening and night, accompanied with pains of a more regular character, denoting, beyond doubt, the existence of labour.

"July 3rd, 2 A.M.—Dr. Oldham, after examination, found that the effect of labour had been to press low down into the cavity of the pelvis the cancerous mass, so that the anterior portion almost reached the external parts. The deep clefts were more patent, and one, which was considered to be the os uteri, allowed the finger to penetrate to its full stretch, but without reaching the preceding part. The solid lobulated structure, like lumps of hardened mortar, appeared to offer an insuperable impediment to any mode of delivery by the vagina; and he determined, with the ready consent of the patient, that the Cæsarean section should be performed, and Mr. Poland, the assistant-surgeon, who had previously been apprized of the probability of the operation, was sent for. At this time, 3 A.M., her pulse was 100; the fœtal heart was heard, but the uterine sound was not detected, the restlessness and pain of the patient forbidding any long search for it. The temperature of the room was sufficiently warm without artificial heat, although a fire was lighted for the convenience of warm water, &c. The bladder was emptied by the catheter: and the patient was brought to the edge of the bed on the right side, with the shoulders slightly raised, and a tailed bandage adjusted loosely around her, and then was placed under the influence of chloroform. The size of the uterus was so much reduced by the escape of the liquor amnii and its powerful tonic contraction, that it did not reach more than two inches above the umbilicus itself. This was so marked as to limit the upper boundary of the external opening to the umbilicus, from whence a straight incision was made, shutting in the median line to within an inch of the pubes. The length of this incision was between six and seven inches, and the peritoneum was divided by the aid of a director. The uterus, when exposed, was of a deep claret colour, firmly contracted, and feeling particularly solid and condensed. A straight incision through the anterior wall was now made, which, from the great resistance of the muscular structure, was comparatively superficial in depth, and it required seven or eight cuts to divide the entire wall, which was nearly, if not quite, an inch in thickness. As this was being done, the

slight irregularities in each sweep of the scalpel was made very conspicuous by the contraction of the divided fibres, which by elevating one portion over another, gave the divided surface a jagged appearance. During this part of the operation, the length of the incision was so much reduced by the contraction of the uterus, that it became necessary to prolong it slightly towards the fundus; and in doing this, the bleeding, which hitherto had been but moderate, greatly increased, and a copious flow of venous blood, into which the florid blood of a largish artery could be seen to stream, showed that the most vascular part of the uterus had been divided. Even now the size of the opening into the uterus was just large enough to admit Dr. Oldham's right hand, who at once broke through the membranes, and dipping low into the womb, tried to raise the head out first. It was at once apparent, however, that this could not easily be done; and raising his hand along the side of the womb, he first turned out an arm, then pressed out the pelvis and lower extremities of the fœtus, and, supporting the extricated trunk, endeavoured to withdraw the head. The opening, however, was so lessened by the contraction of the uterus, as he had anticipated, that this could not be done, and at once, therefore, having put the lower edge of the opening on the stretch by the hind part of the fœtal head, Mr. Poland divided with a mere nick of the knife a few of the fibres, which liberated the head. The hand was again introduced between the membranes and the uterus; and the placenta, which was attached to the upper and posterior wall near the left tube, was readily separated and turned out. The child, which was a female, strong and well made, though rather small, cried freely; and having since had a wet nurse, has done well. The emptied uterus sunk towards the pelvis, and the omentum slid through the upper part of the opening, with the outer edge only of a short coil or two of the small intestines. The divided edges of the opening in the uterus kept apart, and the body of the uterus itself was kept back towards the spine, losing much of its mobility by the mass of disease fixing it below. There was a good deal of bleeding from the upper part of the wound, which ran over towards the back part of the pelvis and the adjacent peritoneum. Dr. Oldham held a sponge firmly against the part, and with a second warm sponge cleared the extravasated blood away from behind the uterus, and carefully wiped the intestines and peritoneum, which had been soiled by it, while Mr. Poland applied sutures to the upper part of the wound. By this means, without disturbing the uterus too much, the bleeding was moderated, and the external opening was closed by a series of silk sutures at the distance of half to three quarters of an inch from each other. Some thin strips of plaster were drawn across between the sutures, and some longer and broader slips secured two vertical cushions of lint placed on each side the wound. About an inch of the lower part of the wound was left unclosed to allow any discharge to escape. The bandage was drawn over the plaster, and the bed cleared up without moving the patient. Throughout the operation the patient was kept insensible and perfectly tranquil by chloroform; her pulse and respiration was not affected by it; and at 4 P.M., when she had just recovered from the chloroform, she said that she had not felt the operation, and, excepting some smarting, she was free from pain 'and in heaven.' Subsequently she was attracted by the cry of her child, and inquired doubtingly whether it could possibly be hers. Ordered 3 grains of opium immediately, and 1 grain every 3 hours.

"1st day.—Ice and iced water, with a little tea or beef-tea. 11 A.M. She slept uninterruptedly for three hours after the operation, and awoke wishing for nourishment, when a few teaspoonfuls of beef-tea were given. She had been dosing since, and was in every respect comfortable; six ounces of urine were drawn off by the catheter; some fœtid sanies from the vagina, but no discharge from the wound. Rept. opii. gr. j 3^{tis} horis.—10 P.M. She had passed a quiet day, but now complained of some pain in the right iliac region. The abdomen was more distended from tympanitis, pulse 125; skin moist but hotter; there had been no rigor or vomiting, and she spoke well of herself. She consumed half a pint of good beef-tea during the day in teaspoonfuls. Ordered catheterism; omit beef-tea; small quantities of Wenham ice frequently. Rept. pil. 3^{tiis} horis.

"2nd day, morning.—During the night there had been more febrile heat, which went off towards morning. There was no abdominal pain, but some uneasiness and tension about the lower part of the abdomen, which was partially relieved by the catheter, by which from four to six ounces of clear urine were drawn off every six hours. She looked well, spoke cheeringly, and was most obedient to every injunction; skin moist and cool; pulse 120; tongue clear but rather dry.—Evening. In the afternoon a free discharge of a thin lochia oozed from the wound, which at once relieved the abdominal uneasiness. She was in every respect comfortable. Go on with ice and opium, catheterism as before.

"3rd day.—She had passed a better night than for the previous eight months, a dark lochia ran from the wound, and a fœtid sanies from the vagina. At noon she vomited, with scarcely any effort, a small quantity of water, tinged with green bile. In the evening no return of vomiting. and her general condition was favourable; pulse 110; the mammæ beginning to swell; Pergat;—arrow-root and beef-tea in small quantities.

"4th day.—This morning the bowels acted spontaneously and without effort, and she voided urine. The abdomen was but little distended, and she was doing well. The mammæ were so full as to require to be strapped. In the afternoon she was frightened by a thunder-storm, which had always alarmed her on account of a brother having been killed by lightning. Her pulse rose to 180, and she appeared much exhausted. In the evening she was still low-spirited and far more feeble than before, with a rapid pulse and a dread of death. She had had a loose stool in the afternoon. A loud and incessant hiccup had been most troublesome. Ordered brandy ʒiij in arrow-root, mustard-poultice to epigastrium; to continue the opium; and if the hiccup continues, some of the following medicine:—Magnes. carb., gr. viij.; vin. opii, ♏ v.; sp. ammon. c., ʒss; ex. jul. menth.

"5th day.—Much improved; hiccups less, but again brought on by some students accompanying Dr. Oldham into the ward. Wine ʒx; continue opium.

"From this time the reports recount a steady progress. On the 7th day the plaster and lint were removed, as they had been soaked with the constantly oozing lochia. The upper two inches of the wound were closed by adhesion, but the remainder was open. Some long and broad strips of plaster were applied and a bread poultice.

"On the 9th day the ligatures were removed, and water dressing substituted for the poultice. The fundus of the uterus in front appears to have adhered to the closed upper part of the wound, whilst the cavity of the uterus is open by the permanent gaping of the divided edges, which correspond to the separated margin of the external opening, which is three inches in length, and affords a ready exit for the lochial discharge. Gradually this opening contracted; and the lochial discharge having ceased, healthy pus from a granulated surface was alone secreted. Up to the present time, August 11th, the process of repair has gone on well, and the external wound is about an inch and a half in length, the granulating surface sloping inwards, and all but closing the uterus itself. In the course of a week or so, there is every probability that the granulations will have filled the remaining space, and cicatrization will be complete. The disease itself has somewhat shrunk since delivery; the discharge is not very abundant; she has been very free from the ordinary cancer pains, and her general health is well supported. A moderate menstrual bleeding has lately occurred. The opium was gradually reduced in quantity; and beyond occasional medicines to meet particular circumstances, the only treatment necessary, of late, has been to keep her at rest, to dress the wound, and to sustain her powers by a good diet, with wine and porter. I cannot close this case without acknowledging the incessant care which, at the critical time, was bestowed upon it by my clinical clerks, Dr. Holman and Mr. Butler.

"The very brief remarks which this case suggests are purely of a practical character, and have reference to the mode of delivery, which the particular complication required. When first I saw this patient she was more than seven months gone in pregnancy; the child was active and its heart beat strong; and the first question was, whether labour should be artificially induced, in accordance with the

practice unreservedly recommended by Dr. R. Lee. With respect to this, I would observe, that the operation itself, of puncturing the membranes, with such a mass of cancer, would have been attended with great difficulty, and could hardly have been accomplished without considerable hæmorrhage from the breaking-down of the diseased structure. But even supposing this preliminary difficulty overcome, there was still the same solid obstacle to delivery through the os uteri, which the mere difference between the size of a fœtal head at the seventh month and at term, would almost inappreciably lessen; and it appeared to me, that to bring on labour by my own act, would only additionally complicate the case, compromising, to a great extent, the life of the child, and anticipating a perilous crisis for the mother. On these grounds, therefore, I determined to leave labour to come on by itself, and to direct the treatment to alleviate pain, and quiet the disposition to premature contraction, so as, if possible, to carry the patient on to term. In accordance also with the views which I had expressed in a paper in the 'London Journal of Medicine' for March 1851, I resolved to watch the first effects of labour on the diseased os and cervix,—marking, at the same time, the signs of fœtal life; and if the former, as I anticipated, showed that the obstacle to delivery by the vagina was insuperable, and the latter, that the child's heart beat, to have recourse without delay to the Cæsarean section. The remedies which were employed to mitigate the sufferings which attended the enlargement of the uterus have been detailed, and the only one which needs special mention, was the inhalation of chloroform, which certainly was of great value.

"The obstacle which a mass of malignant disease, infiltrating the surrounding cellular tissue and disposed all around the cervix, offers to the labour of dilatation, is most formidable, and is incapable of being relieved by any operation for its reduction by simple section or other surgical means. The object which I had in view in allowing labour to go on for some time, was more as a tentative means to try the natural forces before having recourse to an extreme surgical operation, than with any expectation of their ultimately overcoming the difficulty; and I did this the more willingly, because a moderate amount of labour is favourable to the Cæsarean section, and because we had, in listening for the fœtal heart, a certain guide to the life of the child.

"It will be noticed in this case that the usual fœtid discharges and hæmorrhage from a mass of cancer had not been so great as materially to impair her constitutional powers, and this was most favourable with reference to her sustaining the immediate shock of the operation, and for the subsequent process of repair. When, therefore, after a few hours' labour, I found that the only effect had been to press down the lower segment of the uterus into the pelvis, and not to open the os uteri itself; and at the same time I found that the fœtal heart was beating; it appeared to me that the Cæsarean section was the only means to rescue the mother from impending danger, and to save the child, and that humanity and sound practice required its performance.

"The operation itself was performed in the usual manner, and the only point to notice in it is, that the incision was from below, not above, the umbilicus, and that the uterine opening was comparatively small. The former prevented any large escape of the intestines, which is usually embarrassing to the operator; and I believe it to be a matter of importance to make the uterine opening as small as is compatible with delivery, and to increase it as the occasion may require, rather than at once to undertake a major operation, with its attendant risks of greater hæmorrhage." (pp. 426—435.)

VIII. *Ophthalmic Cases.* By John F. France.

IX. *Cases of Lithotomy in India, in which the operation was performed.* By Mr. Coles, H.E.I.C.S. Communicated by Bransby B. Cooper.

This paper is chiefly valuable, as showing that diet and climate have small direct influence over the chemical nature of calculi.

Art. VIII.

1. *Anæsthesia, or the Employment of Chloroform and Ether in Surgery, Midwifery, &c.* By J. Y. Simpson, M.D., F.R.S.E., Professor of Midwifery in the University of Edinburgh, Physician-Accoucheur to the Queen in Scotland, &c. &c.—*Philadelphia*, 1849. 8vo, pp. 248.
2. *A Treatise on Etherization in Childbirth, illustrated by Five Hundred and Eighty-one Cases.* By Walter Channing, M.D., Professor of Midwifery and Medical Jurisprudence in the University at Cambridge, U.S.—*Boston*, 1848. 8vo, pp. 400.
3. *On Anæsthesia and Anæsthetic Substances generally.* By Thomas Nunneley, Esq., F.R.C.S.E., Transactions of the Provincial Medical and Surgical Association.—*London*, 1849. 8vo, pp. 381.
4. *Traité de la Méthode Anesthésique appliquée à la Chirurgie et aux differentes Branches de l'Art de Guérir.* Par le Docteur E. F. Bouisson, Professeur de Clinique Chirurgicale à la Faculté de Médicine de Montpellier, Chirurgien en Chef de l'Hôpital Saint Eloi, &c.—*Paris*, 1850.

Treatise on the Anæsthetic Method applied to Surgery and to the different Branches of the Healing Art. By Dr. E. F. Bouisson, Professor of Clinical Surgery to the Medical Faculty of Montpellier, Chief-Surgeon of the St. Eloi Hospital, &c.—*Paris*, 1850. 8vo, pp. 560.

5. *Ether and Chloroform: their Employment in Surgery, Dentistry, Midwifery, Therapeutics, &c.* By J. B. Flagg, M.D., Surgeon-Dentist, Member of the Rhode Island Medical Society.—*Philadelphia*, 1851. pp. 189.
6. *On the Inhalation of the Vapour of Ether in Surgical Operations; containing a Description of the various stages of Etherization.* By John Snow, M.D.—*London*, 1847. 8vo, pp. 88.
7. *Surgical Experience of Chloroform.* By James Miller, F.R.S.E., F.R.C.S.E., Surgeon in ordinary for Scotland to her Majesty, and to H. R. H. Prince Albert, Professor of Surgery in the University of Edinburgh, &c.—*Edinburgh*, 1848. 8vo, pp. 60.
8. *On the Employment of Chloroform in Dental Surgery, its Mode of Exhibition, &c.* By Francis Brodie Imlach, Dentist.—*Edinburgh*, 1848. 8vo, pp. 9.
9. *Des Effets Physiologiques et Thérapeutiques des Ethers.* Par H. Chambert.—*Paris*, 1848. pp. 260.

We must remind our readers, that in the article on this subject in the preceding number of this journal, we treated principally of the history and progress of Anæsthesia, and of the physiological action and comparative merits of Anæsthetic agents; occasionally, however, making such practical applications as seemed to come in our way. In the present article, of a more practical character, we shall in like manner not hesitate to introduce, where the subject seems to warrant, whatever deductions from the facts related may tend to elucidate the physiology of anæsthesia.

This part of the subject may be divided into the following heads:—1st. The mode of administration of anæsthetic agents; 2nd. The applications of anæsthesia to surgery (A), to midwifery (B), and to dentistry (C); 3rd. Other therapeutic uses of a more general character; and 4th. The relations of the subject with legal medicine.

But before entering on the consideration of these points, we think it right to make an addition to our former article, by referring to the views of Dr. Snow regarding the successive stages of anæsthesia; not because we consider our division of the process into three stages erroneous or insufficient, but simply because of Dr. Snow's eminence as an authority. Dr. Snow divides the complete anæsthetic state into five stages. In the first degree, he includes—

"The various stages of feeling that a person may experience, while still retaining a consciousness of where he is, and what is occurring around him, and a capacity to direct his voluntary movements. In the second degree, mental functions may be exercised, and voluntary actions performed, but in a disordered manner. In the third degree, there is no evidence of any mental function being evinced, and consequently no voluntary motions occur; but muscular contractions, in addition to those concerned in respiration, may sometimes take place as the effect of the ether, or of external impressions. In the fourth degree, no movements are seen except those of the respiration, and they are incapable of being influenced by external impressions. In the fifth degree (not witnessed in the human being*) the respiratory movements are more or less paralyzed, and become difficult, feeble, or irregular."

Dr. Snow considered that a middle-aged man would consume about two drachms of ether per minute, if supplied with a sufficient quantity in the proportion of forty-five of ether-vapour to fifty-five of air; and that every minute during which the inhalation went on would correspond to one of his stages, until the fourth minute had expired, when, having consumed an ounce of ether, he would be in the fourth degree of etherization. If the etherization were then discontinued, the man would remain in this stage for one or two minutes, pass gradually back into the third degree, which would last three or four minutes; "at the end of which time he is in the second degree, which lasts about five minutes, to give place to a feeling of intoxication and exhilaration, which lasts for ten or fifteen minutes, or longer, before it entirely subsides." Dr. Snow classes the stages of action of chloroform as he does those of ether. In speaking of the fourth degree, when produced by the former body, he says—"I am better acquainted with this degree as induced by ether than by chloroform, for with the latter agent, the third degree appears to encroach somewhat on this; chloroform, seeming to differ from ether, and approaching somewhat in its effects to benzin and bisulphuret of carbon, which we have seen are not attended with muscular relaxation at any stage of their effects." In the fourth degree of narcotisms from chloroform, he says that the sensibility of the glottis continues when that of the pharynx appears to be suspended, as in operations on the nose and mouth, the blood sometimes finds its way into the stomach, without any visible act of swallowing. Stertor hardly occurs in chloroformization, except during the fourth degree. Finally, in the fifth degree, which is the commencement of dying, he has seen convulsions of

* This was written in 1847, before a fatal case from chloroform had occurred. Dr. Snow admits the reality of fatal cases.

the limbs in animals poisoned by chloroform, never in those killed by ether.* We now proceed to the topics more immediately before us.

1. *Mode of Administering Anæsthetic Agents.*—When ether-inhalation was first introduced, no one seems to have thought of using the agent otherwise than through an apparatus. To describe all the forms that were invented, would far exceed the limits of this article; and indeed many of them could not be understood without illustrations, their arrangements being so excessively complicated. But as there are still advocates for the use of apparatus, we shall refer to some of the more conspicuous of those inventions. In most of them, the ether and chloroform were contained in pieces of sponge. Mr. Morton first used a spherical reservoir, with two wide and short tubulures; one of these had a valve, permitting the entrance of atmospheric air; the other was connected with a tube having two valves, one near the reservoir, opening on inspiration, and another on the side, which opened on expiration. Then came the more elaborate apparatus of Mr. Robinson, of London: here there was a reservoir with two tubulures, one of which was connected with a long flexible tube, supplied with valves similar to those of Morton, and a stop-cock in addition, so that the communication between the reservoir and the patient might be instantly cut off. Besides this, there was an apparatus for compressing the nostrils. Then Dr. Snow invented an apparatus, nearly similar in principle with the others, as far as its stop-cock and valves were concerned; but different as regarded the construction of the reservoir, which consisted of a spiral chamber in a tin box The portion of the box not occupied by the spiral chamber was filled with water, never quite approaching 65°. The effect of this apparatus was to keep the air which passed through the spiral chamber saturated with ether, and at the same time to mix the ether vapour with a sufficient quantity of air. And as Dr. Snow finds, that at the temperature of 64° air will take up about an equal quantity of ether vapour, or rather more than its own bulk, the temperature offers a guide to the amount of air with which the agent is mixed. Thus from an apparatus of this kind, or on this principle, if we know the temperature, we can estimate the extent to which this or any volatile body of the kind is mixed with air; and so knowing the diffusibility of the agent from the quantity of it removed during inhalation, we may estimate the amount of air which has been respired with it.† The face-piece of Dr. Snow's apparatus was so constructed as to allow of respiration by both nose and mouth. Afterwards Dr. Snow rendered his apparatus more portable.

These instruments were made still more complicated in Paris. Of those of MM. Charrière and Luer, only pictorial representations could give an adequate idea, with such a number of tubes, stop-cocks, and valves are they provided.

The constitution of the mixture of air and ether thus inhaled, is such as

* Medical Gazette, vol. vii. 1848, p. 413.

† The researches of Dr. Snow on the amount of air and ether-vapour which mix at various temperatures do not correspond with those of MM. Doyère, Lassaigne, and other foreign experimentalists. Thus, according to M. Doyère, the quantity of ether-vapour which the atmospheric air may take up, may fall in winter below 17 per cent., and in summer may ascend to 30; and while, according to Snow, 100 parts of atmospheric air, at the temperature of 70°, will absorb no less than 138 measures of ether-vapour, Lassaigne, at 59° Fahrenheit, only found 35·4 per cent., whereas the proportion, according to Snow, should be 80 of ether to 100 of air.

might render it inflammable on the approach of a light; and even the presence of aqueous vapour is not sufficient to destroy the inflammability: but when a light is brought near the mouth of an animal whose breath is so constituted, immediately after the withdrawal of an ether apparatus, the only effect of the explosion which takes place is to singe slightly the hair about the mouth; the flame does not pass into the lungs.

Similar instruments to those invented for ether have been used for chloroform, although they have generally been of the simplest description; and yet, perhaps, of the two agents, there is more reason for using an apparatus in the case of chloroform; but Dr. Simpson having from the first set the example of dispensing with the use of any instrumental aid for inhaling the new agent, an idea came to prevail that chloroform was more independent of such assistance than ether. Dr. Snow has particularly advocated the use of an inhaling apparatus for chloroform. His argument is, that the fatal cases have arisen in great part from the use of the chloroform vapour in a too concentrated state. His chloroform inhaler is constructed upon a similar principle to the one already described for ether, only it is of a different form; this principle is, as we have just seen, that air at a certain temperature will dissolve or mix with a certain volume of chloroform; and the object is sought to be obtained by surrounding the chloroform reservoir with a bath kept at a certain temperature.*

An apparatus a good deal used on the Continent, and partially, we think, in this country, and much praised by M. Sédillot of Strasburgh,† for the inhalation of chloroform, consists of a round hollow vessel of wood, pierced with holes for the admission of air, and having a tube for the introduction of chloroform, and connected with a caoutchouc mouth-piece, furnished with a valve for the egress of air in expiration. Similar instruments are much praised by Dr. Murphy in midwifery,‡ also by Dr. Channing; by Mr. Clendon and others in dental surgery.§ The various inventions which they approve of are all simple, and vary a little in form, but are similar in principle, and of the same kind as the only form of chloroform-inhaler now generally used in our hospitals; consisting of a small tin reservoir, with drilled plates, containing a sponge, and an adjoining mask or muzzle for the mouth and nose, with a valve at the top for expired air. The chloroform can be conveniently poured from a measure-glass on the external plates, in quantities of a drachm at a time.

We must notice another mode of inhalation which is not devoid of merit, although the use of it hardly deserves the pompous title sought to be given it, of the Italian method. Porta of Pavia, the inventor of this plan, recommends the patient's nose to be stopped with cotton. The operator is then to take a pig's bladder, slightly softened and distended; the opening is to be enlarged to the size of the mouth of the patient, two or three teaspoonfuls of pure sulphuric ether, or a drachm of chloroform, put in, and the opening of the bladder is to be fitted as exactly as possible to the mouth, so as to hinder the entrance of atmospheric air. One assistant keeps the bladder to the lips, while another supports it at the bottom. In

* Medical Gazette, vol. vii. 1848, p. 843

† De l'Insensibilité produite par le Chloroforme, &c., p. 45.

‡ Chloroform in the Practice of Midwifery. By Professor Murphy. London, 1848, p. 14.

§ On the Use of Chloroform in Dental Surgery. By J. Chitty Clendon. London, 1849, p. 18.

this way, Signor Porta says, the patient respires naturally, and gets the right proportion of ether and air; and he states, that he never fails to produce complete anæsthesia about the sixty-second second; the stupor lasts about three minutes, and can be renewed at any time by reapplying the bladder, with its contents replaced if necessary. An ox-bladder does not answer so well, the quantity of air being too large. On this principle, chloroform inhalers have been constructed, and made of various kinds of material.*

We are no advocates for the use of inhalers. These instruments interfere, as Professor Simpson very properly observes, with the movements of the patient's head, often prevent him coughing freely, and so forth; and as patients frequently at first reject the vapour, and toss their heads from side to side, we cannot, as with the handkerchief or the sponge, follow the motions of the head, and gradually and easily overcome the resistance to the entrance of the anæsthetic vapours. Deaths, as our previous article shows, have occurred with the use of inhalers as without them. The great argument for inhalers is, that thereby the quantity of chloroform given and mixed with the air may be more readily measured, and its admixture with a sufficient proportion of air secured. Now, from the researches of Dr. Hutchinson on the capacity of the chest, it is clear that the amount of air which a person may inhale, and consequently the quantity of the anæsthetic vapour taken in with it, will depend almost entirely on the height of the individual, and will have very little to do with his physical strength; so that all attempts to regulate the dose of chloroform must be liable to a considerable fallacy. And the entrance of a due amount of air can be secured by approaching the sponge or handkerchief gradually to the face; and keeping two or three fingers interposed during the inhalation. The only valid objection to the use of these simple means, is the occasional contact of chloroform with the lips and nostrils, whereby irritation and sometimes vesication of these tender parts is produced. This can generally be guarded against with care; and if the precaution is deemed necessary, it may always be prevented by previously rubbing the parts exposed with a little olive oil, or other oily or unctuous substance. The remarkable success of Professor Simpson is sufficient to show that instruments are not necessary.

Some difference of opinion prevails, as to whether it is better to bring the patient rapidly under the anæsthetic influence, or to accomplish this more slowly. Professor Simpson is an advocate for the former mode. A somewhat interesting case occurred lately at Strasburgh, where one more death from chloroform subjected the medical man who operated to an accusation before the correctional police. The operation was tooth-drawing on a married female. The quantity of chloroform used was small, and the death sudden, almost before the completion of the operation. M. Sédillot, who gave evidence, stated, that in his opinion sufficient precautions had been taken to ensure the entrance of a due amount of air into the lungs; but he condemned the inducing of anæsthesia with chloroform in a shorter period than ten or twelve minutes.†

In surgery, some prefer placing the patient in an anæsthetic state before his removal from the bed to the operating table. Such appears to be the

* Bouisson, pp. 123—129. † Journal des Débats, Jan. 31, 1852.

common practice in the Edinburgh and Aberdeen Hospitals; and it is strongly advocated by Professor Miller. (p. 25.)

In midwifery, Dr. Simpson's mode is to render the anæsthesia as deep as in surgery, at the time when operative or surgical measures are necessary in parturition; but in ordinary labour he induces a much less profound degree of insensibility, increasing, however, its intensity as the pains become more severe, and keeping the patient constantly asleep towards the last and otherwise most painful part of the progress. He believes that the great secret of its exhibition in midwifery, is to give it instantly as each uterine contraction commences, and to withdraw it entirely during the intervals between the pains, during which a state resembling natural sleep will usually continue.

In dentistry, Mr. Imlach, who has extracted several thousand teeth under its influence, has given some excellent directions for the use of chloroform. He always uses a large dose, and judges by the effect, not by the quantity used; a cotton handkerchief is his only inhaler; and to prevent the patient clinching his teeth when insensible, he places in the mouth a gag of ivory or gutta percha, which causes no inconvenience. (p. 5.)

The great points in the administration of chloroform are, to watch the pulse, *and especially the respiration.*

2. *Applications of Anæsthesia to Surgery, Midwifery, and Dentistry.*—(A.) Of the desirableness of the subjugation or annihilation of pain in surgical operations, considered in itself, we cannot, on the whole, for a moment doubt; to disarm the operating table of a great portion of its terrors, is indeed a triumph of which our age may be justly proud. Not only is the actual pain of an operation thus removed, but also, in great part, that indescribable horror which often torments the patient for some time previously. Men of the greatest courage in other respects, and who have faced danger and death in many forms, have yet shrunk from the prospect of the slow and cold-blooded torture they had before them from the knife of the surgeon. Indeed, however man may summon his fortitude to meet physical pain, or any other dire misfortune, we must all bow to the laws of humanity, and *feel* the severity of fate, in spite of the efforts of our moral nature to rise above it. But when we find that this great relief which anæsthetic agents afford, is to be obtained almost without risk, and on the whole with very beneficial results otherwise, we should receive this great discovery with gratitude and exultation. So complete is the general use of anæsthetic agents, that the element of pain as an obstacle or source of danger or of terror in surgery, is for ever almost destroyed. Manual and instrumental therapeutics, as a branch of materia medica, now proceeds, says M. Bouisson, "in the silence, as it were, of vegetative life, and its salutary mutilations are only made known by changes of form without any painful sensation having been experienced by the organism." True it is, as has been already shown, there are exceptions to the general rule; but they are so rare as not to militate practically against it.

Besides the applications of anæsthesia to operations, it may be employed in surgery most usefully to favour the diagnosis of some cases. Professor Miller recommends its use in the examination of some female diseases, to save the delicacy and modesty of the patients. There are

some diseases which cannot otherwise be diagnosed without pain. For instance, in some diseases of the eye there is such intense photophobia, with spasmodic contraction of the eyelids on the entrance of light into the eye, that it is very difficult to open the eyelids, so as to make a proper examination of the organ itself. In such cases, a moderate degree of anæsthesia will often overcome the resistance of the orbicularis, and destroy for a time the sensibility of the retina, so as to allow of the examination being made. In many accidents the pain renders it difficult to allow of the garments of the patient being removed, and the parts injured being properly examined. In burns it is often difficult to remove the scorched clothes, burned as it were into the skin. In many painful affections of the vagina, accompanied by constriction, it is often hardly possible to use the speculum. Cases of painful catheterism may also be adduced, and necessary exploration of the urinary canal and bladder.* In such cases, and in others which can easily be imagined, as in affections of children, when the struggles of the patient afford an obstacle, and in many instances of feigned disease, as we shall see under our fourth head, the services which anæsthesia may render to diagnosis are considerable. For instance, Dr. Simpson, in his letter to Dr. Meigs, says,

"My friend Dr. Andrew Wood has just made a beautiful application of it. A boy fell from a height and severely injured his thigh. It was so painful that he shrieked when Dr. Wood tried to handle the limb, and would not allow of a proper examination. Dr. Wood immediately chloroformed him—at once ascertained that the femur was fractured—kept him anæsthetic till he sent for his splints—and did not allow his patient to awake till his limb was all properly set, bandaged, and adjusted."

The surgical applications of anæsthesia may now be considered under the following heads:—(*a*) the general indications and contra-indications to its use; (*b*) the prognosis of operations performed under its influence; and (*c*) the applications to some special groups of operations.

(*a.*) Anæsthesia is generally indicated wherever there is much pain or *great* muscular resistance to be overcome. By overcoming pain, it has caused many operations which used to be as rarely performed as possible, to come more in the way of the surgeon: the removal of nails, and the operation of the actual cautery, need no longer inspire horror to the operator or to the patient. With the view of removing muscular resistance, its use has become general in the reduction of dislocations and the operation of the taxis.

The observations of M. Bouisson appear to us here, on the whole, so just and so truly eclectic, worthy in that respect of the reputation for critical talent which the school of Montpellier has long enjoyed, that we

* Cases of painful catheterism hardly come under the applications of anæsthesia to diagnosis; but of course the same rule applies to them as to the diagnosis of urinary affections. Professor Miller quotes an interesting case of a naval officer who had long suffered under a stricture, where many attempts at cure by bougies had been stopped by the extreme sensibility of the urethra, which always on any attempt to pass these instruments occasioned intense suffering and spasm, long-continued after-pain, and often fever. Hearing of chloroform, he came to Edinburgh, and placed himself under the care of Professor Miller. Under the influence of anæsthesia, the usual treatment could be adopted, and was attended with success. The same author has a very high opinion of the utility of anæsthesia in sounding for stone, especially in children, whose struggles and shouting often interfere seriously with the diagnosis. He mentions an instance where in sounding a boy under the influence of chloroform, he encountered a projecting portion of the bladder, coated probably by some sabulous deposit, and where, but for the facility which the anæsthesia gave to the examination, he might have wrongly inferred the presence of a calculus.

shall follow his exposition pretty closely. He commences by remarking, that the sources of the contra-indications to the use of anæsthetics are very numerous and various, although the sum of cases in which these agents ought not to be used, is very insignificant in proportion to the immense number in which they can be employed with perfect safety and advantage. He recommends that anæsthetic agents should not be used before the expiring of the first six months of life; and that only ether should be given up to the second year, reserving chloroform for the epoch when the vital powers have acquired more energy: and for a similar reason, he prefers the former agent after the seventieth year. He would, then, only employ ether up to the eightieth year, at which he considers that anæsthesia ceases to be applicable in any shape; not because there is absolute danger in departing from these general rules, but more prudence in obeying them. With regard to morbid states:

"Diseases of the nervous centres, and of the lungs and heart, contra-indicate the employment of anæsthetic inhalations, when the physical or functional lesion is advanced to a certain degree. This degree can only be determined by the sagacity of the practitioner. A simple catarrh would not oppose the practice; but it would be dangerous to have recourse to it in phthisis, with spitting of blood; a slight hypertrophy of the heart would not be a sufficient contra-indication; but anæsthesia should be renounced if the heart-affection were accompanied with irregularity and intermittence of the pulse. A recent nervous affection, far from being aggravated by etherization, might be ameliorated; an old nervous affection with a tendency to syncope, might be a source of the greatest danger. It must be understood that the appreciation of these differences is the province of the operator. Here we can only establish some general principles.

"Ether, then, and especially chloroform, should be renounced in the following cases:

"1st. In patients extremely weakened by hæmorrhage, or by a spontaneous chlorotic anæmia.

"2nd. In epileptic patients, and hysterical persons with weakened constitutions.

"3rd. In individuals who appear very much disposed to cerebral congestions, or who have softening of the brain.

"4th. In those who are very subject to fainting fits.

"5th. In such as are disposed to hæmoptysis, or to pulmonary apoplexy.

"6th. In those who have organic lesions of the heart and great vessels, with feebleness and intermittence of the pulse."

We would add, those who have *very great dread* of the anæsthetic influence; since in some of the fatal cases, and in the one which recently occurred at Strasburgh, this appears to have been present.

How far we are to be guided by contra-indications will depend, not merely on the extent to which these exist in themselves, but also on the nature of the operation to be performed. It would be absurd to run the same risk, for the sake of saving a patient from the pain felt in the extraction of a tooth, that it would be right to run in amputation of the hip-joint! Thus, we have a sort of compound relation to consider, between the benefit to be received and the probable danger incurred.

As far as regards the circumstances in the operations themselves, which may influence our use of anæsthetic agents, M. Bouisson ranges them under the following categories:

1st. Operations which are of short duration, and where the pain to be endured is trivial.

2nd. Operations which require the active participation of the patient.

For example, in several operations on the anus and rectum, the surgeon is assisted by the efforts which the patient is desired to make, and by the attitude which he maintains, as in excising or tying internal piles. In the extraction of foreign bodies, also, which have penetrated into the tissues, it is often useful for the patient to take a posture similar to that which he had at the time of the accident.

3rd. Operations where the sensibility serves as a guide to the operator. Some objections have been made under this head without sufficient foundation. M. Lallemand, for instance, expressed his fears that in tying arteries a neighbouring nerve might be included in the ligature, on account of the absence of the usual sensation during the anæsthetic state. But there are very few arteries where such a mistake is likely to occur; perhaps the only one being the subclavian, on account of the numerous nerves of the brachial plexus surrounding it, and its liability to anomalies of position. Lithotrity has with more reason been enumerated as subject to the objection, on account of the possibility of piercing the walls of the bladder without the consciousness of the patient. On the continent, the opinions of surgeons have been much divided on this point. According to M. Leroy d'Etiolles, etherization is especially beneficial in lithotrity, when the calculus is contained in a bladder whose walls are enlarged, and whose muscular fibres are thickened, and, as it were, embrace the stone. Here anæsthesia, he thinks, promotes a relaxation of the walls, and, by disengaging the stone from pressure, renders it more easily seized. But in such bladders we are also more liable to pierce the walls by the instruments used. M Serre, of Montpellier, for this reason and others, opposes the practice of anæsthesia in this operation; and relates the case of a custom-house-officer, who had already been subjected to lithotrity, upon whom it was impossible to operate under the influence of ether, but who was successfully operated on afterwards when not etherized. On the other hand, M. Bouisson mentions the case of a female, in whom there was much general nervous excitement and irritation of the urinary passages, and in whom anæsthesia was employed during the operation of lithotrity with obvious benefit. And M. Amussat has given the cases of several old men who had severe attacks of cystitis, after lithotrity had been performed without ether, and were free from any such affection after having been etherized in other operations. In this country, also, opinions are divided on this subject. Dr. Keith, in a communication in the 'Edinburgh Monthly Journal,' where he reports an interesting case of lithotrity, says—

"The influence exercised by the chloroform was altogether beneficial. Usually the sphincter of the bladder is so tightened by the patient's fears and struggles, that the movements of the instruments are very much impeded, the delicacy of touch needful in detecting small fragments quite destroyed, and a painful resistance offered to the withdrawal of the instrument, especially if that chance to be a scoop well filled with the debris of the broken stone. But under chloroform, fears are at rest, the bladder insensible, and the sphincter quite relaxed." *

He proceeds to refer to other advantages. At a meeting of the Medico-Chirurgical Society, when Dr. Keith read his paper previous to its publication, Mr. Syme is reported to have said, that he "feared the consequences likely to result from the performance of lithotrity on insensible patients by

* Edinburgh Monthly Journal of Medical Science, April, 1848.

operators less expert than Dr. Keith;" and from a recent communication in the 'Lancet,'* we find that he still retains this opinion. In general, it may be remarked, lithotrity is not a very painful operation; sometimes anæsthesia may be useful in injecting the bladder, and introducing the apparatus where there is much irritation, but the chief pain is encountered during the expulsion of the fragments.

4th. There are some few operations where the object is the production of pain. Such are the cases in which moxa or other means of counter-irritation are used, in order to rouse the sensibility of the spinal cord or of the limbs, not simply to produce inflammation.

5th. Operations performed in cases where there exist previous causes of torpor and insensibility. In the operation of trepanning, for instance, the patients are often plunged in coma, or nearly insensible to pain; in such cases it would be, to say the least, superfluous to have recourse to anæsthesia.

(*b.*) On the first discovery of etherization, fears were expressed that the state of anæsthesia might have a bad effect on the after-results of operations; that the healing of wounds might take place less readily, and that other accidents might supervene. But experience has proved the reverse; the general opinion of surgeons is as favourable, as regards the after-consequences of etherization in surgery, as of the immediate effects. The diminution of the shock to the nervous system seems to favour the healing of wounds, and altogether increases the chances of recovery. The shivering and re-active fever which often follow operations are greatly diminished by etherization; there is generally more sleep, and a more complete feeling of comfort. Contrary to what some have asserted, the pain which follows or accompanies the return of consciousness after an operation, is less intense after etherization; although, perhaps, in some few cases, the absence of pain during the operation may render the patient comparatively more sensitive to the after-pains. The most common sources of danger which follow surgical operations, are the nervous phenomena, violent inflammatory affections of the wounded surfaces, gangrene and purulent absorption, and hæmorrhages. Both the first and second of these are obviously diminished; indeed, the diminution of nervous irritation has a direct tendency to moderate inflammatory action. A few examples of hospital gangrene, which occurred in the practice of M. Roux, were made the most of by the opponents of etherization. When men are to be found capable of maintaining such a monstrous doctrine as that pain itself was beneficial in surgical operations, it is not surprising that every little instance of misfortune, from whatever cause arising, should be attributed to the influence of etherization. Professor Miller states, that at one time he was inclined to think a tendency to erysipelas, which existed in the Edinburgh Infirmary, was due to this cause; but a more extensive experience convinced him of his error. It is evident that purulent absorption cannot be promoted by etherization.

The only evil consequences, in our opinion, which can fairly result from anæsthesia in the ordinary way, are certain nervous phenomena, either of an hysterical character, or a kind of nervous asthenia or prostration, headache, and cerebral congestion, which may perhaps, in some rare cases, proceed to

* Lancet, Jan. 31, 1852.

assume an inflammatory character; and irritation of the air passages, which may likewise, in some rare cases, pass into a species of pneumonia. Of the fatal cases we have already sufficiently spoken; but in considering what weight should be ascribed to them, and to the ill consequences which may occasionally result from etherization, we should not forget the ill consequences which may result from pain itself, and the fright attending its mere expectation. Dr. Ranking has related a case where death itself appears clearly traceable to mere pain.* We ourselves, on one occasion, had occasion to experiment on a Russian poodle. On merely cutting the skin of the thigh, the animal, apparently in perfect health, suddenly expired, without other cause, as we believe, except pain and fright. But it is not by quoting one or two isolated cases, or trusting to mere opinions, that we can form a satisfactory conclusion on this subject. Statistical results are our best guides, and we have them to a sufficient extent to enable us to form a clear judgment.

Dr. Simpson has collected statistical results, which show the rate of mortality in surgical operations, with or without previous etherization. It is quite clear that the great difficulty in applying such results, consists in comparing the cases. Out of such cases, then, Dr. Simpson selects the great amputations of the limbs, such as the thigh, the leg, and the arm, which are generally fatal in hospital practice, in the proportion of 1 to 2 or 3. In the hospitals of Paris, according to the figures of M. Malgaigne, who founds his conclusion on 484 cases, there were 273 deaths, or 57 in 100. In the hospitals of Glasgow, according to Lawrie, of the same class of cases, out of 242, 97 died, or 40 per cent. Mr. Phillips, in a general collection of the whole of the cases recorded in the periodical literature of this and other countries during the present century, and giving therefore the results both of private and hospital practice, finds 437 deaths out of 1369 cases, or 35 in the 100. Finally, Dr. Simpson himself, from statistics of English hospitals, makes out 183 fatal cases in 618 amputations, or 29 per cent. The same author has collected the results of 302 operations of the same class, performed in English and Scottish hospitals, on etherized patients, and the result is only 71 deaths, or 23 per cent. But this is not all. Of the particular operations collected, the most numerous are those of amputations of the thigh. At the same time, there are no operations in surgery of a more fatal description. According to Mr. Syme, the mean mortality is 60 or 70 per cent. Of 987 amputations of the thigh, collected by Mr. Phillips, 435 died, or 44 per cent. Mr. Curling, in an account of the amputations practised from 1837 to 1843 in the London hospitals, finds a proportion of 41 deaths in 100 cases. In short, to conclude, and omitting details, Malgaigne gives in the Paris hospitals a mortality of 62 per cent. in cases of this description; Peacock, in the Edinburgh Infirmary, 49; Phillips, in his general table, 44; Lawrie, at Glasgow, 36; and Simpson, taking the whole reports of English and Scottish hospitals, 38; but in etherized patients only 25 per cent., collected from the same sources. Numerous statistical tables of the kind could be collected from the various minor works published on the subject of anæsthesia in different countries, most of which we have now before us; but we shall content ourselves with remarking, that they all tend to the same end—namely, to

* Ranking's Half-yearly Abstract, vol. v. 1847, p. 383.

prove, not merely the general harmlessness, but the positive benefits of anæsthesia. We have before us in M. Bouisson's work a table of 92 surgical operations performed under the influence of ether and chloroform. A fair proportion of these cases may be considered as belonging to the more formidable and unsuccessful class; and yet there were only 4 deaths! A complete analysis of this table would occupy some space; suffice it to say, in the words of the author, that it testifies wholly in favour of anæsthesia.

(*c.*) There are some special surgical operations, respecting which a few words may be said; since it is not in our power to pass in review the various considerations which belong to the application of anæsthesia to the varied phases that surgery may offer. It is in amputations, that the discovery of anæsthesia has achieved its highest triumph. It is curious, as M. Bouisson remarks, that Professor Burns should, before this discovery took place, have called attention, after speaking of the shock to the system produced by the removal of a member, or of a considerable portion of the living mass, to the benefits likely to accrue from the *partial benumbing of the nervous system, so as to destroy, more or less completely, the sympathetic relation of parts.*

Among special operations in which doubtful benefit is derived from anæsthesia, may be reckoned, 1st, those called for in several affections of the eyes. In cataract, the excitement which some etherized patients undergo, and that manifested on the return of consciousness, may interfere with the success of the operations; but in the great majority of operations on the organ, anæsthesia may be used with great benefit. In operations practised on the parts connected with the posterior fauces, there has been much fear of the consequences of the bleeding in etherized patients. We shall confine ourselves to stating, that in these operations extreme caution should be used; and that this caution should be increased as the principal seat of the disease to be removed, or of the operation, approaches the glottis. Nevertheless, Professor Miller has shown how, by careful management, the dangers to be encountered in cases of this kind can be considerably diminished.

It is not necessary to point out the occasional benefit which may be derived from the use of anæsthesia in the taxis, in the reduction of dislocations, and setting of fractures. In all this class of surgical operations, cases must be continually occurring, proving to the surgeon the immense results which he may reap from the new discovery. There remains one important operation, in which, at first, considerable doubts were apprehended with regard to the application of anæsthesia—viz., lithotomy. For instance, it was supposed that the relaxation of the vesical walls which occurs in profound etherization, would prevent the bladder from contracting after the incision of the neck, and so allow the calculus to escape. Other objections of as little value were made, which experience has fully demolished. Anæsthesia is now used in private and in public practice, as regularly in lithotomy as in other operations; and it is unnecessary to recall arguments which have almost been forgotten.

It seems almost unphilosophical to conclude this brief summary of the results of anæsthesia in surgery in a voice of triumph. Yet who that feels a pride in the progress of his profession, can refrain from rejoicing in the great benefit, which, within so short a space of time, it has conferred on

suffering humanity? Henceforth, even the cock-pit of a man-of-war, and the hospital after a field of battle, will be disarmed of half their terrors. Henceforth the medical profession, *as a body*, cannot be accused of an unwillingness to adopt new discoveries; as the invention, labour, and zeal, and even the critical care, bestowed on this subject by the profession abundantly testify.

(B.) Let us now proceed to consider the application of etherization to Midwifery, and the alleged advantages of the practice with the principal arguments brought forward in opposition.

We cannot do better than commence this part of our subject with the first recorded case of anæsthesia in midwifery. Professor Simpson says—

"The first case in which I employed the ether vapour occurred on the 19th of January. . . . . The pelvis of the mother was greatly contracted in its conjugate diameter, from the projection forwards and downwards of the promontory of the sacrum; the lumbar portion of the spine was distorted, and she walked very lamely. The present was her second confinement. Her first labour had been long and difficult; she began to suffer on a Monday, and after a protracted trial of the long forceps, was at last delivered by craniotomy, late on the subsequent Thursday night."

As, in her present confinement, the medical attendant had not been made aware of the fact until nearly the full term, it was too late to have recourse to the induction of premature labour.

"The pains of her second labour commenced in the forenoon of the 19th. I saw her with Mr. Figg, at five o'clock in the afternoon, and again at seven. The os uteri was pretty well dilated, the liquor amnii not evacuated, the presenting head very high, mobile, and difficult to touch; and a pulsating loop of the umbilical cord was felt floating below it in the unruptured bag of membranes. From five to nine o'clock, the pains seemed only to push the circle of the os uteri further downwards, without increasing its dilatation, or making the head in any degree enter into the pelvic brim. . . . . I, shortly after nine o'clock, made the patient inhale the ether vapour. She was soon under the influence of the vapour, which was continued for about twenty minutes, when the operation of turning was performed. The extremities and trunk were readily extracted; but great difficulty was experienced in extracting the head. With extreme exertion it at length passed the contracted brim with the anterior part of its right parietal bone deeply indented by pressure against the projecting promontory of the sacrum, and the whole cranium flattened and compressed laterally."

The child gasped, but soon died.

"The transverse or biparietal measurement of its head, at the site of the indentation, was, in its compressed state, not more than two inches and a half. Hence, we judged the conjugate diameter of the pelvic brim not to exceed this. The infant was large, and rather above the usual size."

The mother was insensible during all the time, until she heard, but did not feel, the head of the infant jerk from her. She rapidly recovered, expressed her gratitude for the exemption she had enjoyed, and was up and dressed on the fifth day after her delivery. This case was, it must be admitted, sufficiently encouraging; and accordingly Professor Simpson pushed the practice, both before and after his introduction of chloroform, with his well-known energy and resolution.

Previous to this it was known that the motor nervous powers concerned in

parturition belong to the ganglionic and to the spinal systems, and are not necessarily dependent on the brain or the state of mind. Cases of perfect paraplegia had been published, where the act of parturition had proceeded regularly without any consciousness of pain. Haller long ago adduced the authority of Harvey, Smellie, Lamotte, &c., to prove that uterine contractions and labour may go on with the mother *ignara stupida et sopita et immobili et apoplectica, et epileptica et convulsionibus agitata, et ad summum debili.* The same author quotes a case where a woman was delivered at Amiens, while in a state of unconsciousness from drunkenness, and where, on coming to her senses, she was so much delighted at the result, as to declare her intention of making herself drunk in future should she have the same process to undergo.

In considering the propriety of using etherization in *ordinary* midwifery, the first consideration is the amount of pain to be relieved. Some regard the general amount of pain as so bearable, as not to warrant the use of anæsthesia; while others even maintain the pain in parturition to be of a conservative character. Thus Professor Meigs of Philadelphia, says, "I have been accustomed to look upon the sensation of pain in labour as a physiological relative of the power or force; and notwithstanding I have seen so many women in the throes of labour, I have always regarded a labour-pain as a most desirable, salutary, and conservative manifestation of life-force."* And he maintains that women, if sufficiently sustained and comforted, may in general be made to support the amount of pain, incidental to parturition, without much suffering. On the other hand, the advocates of anæsthesia, with Professor Simpson at their head, maintain both that the pain is often so excessive as to warrant the use of means to annul it; and that there is nothing about the character of pain in ordinary labour, to authorize it being called natural and conservative. Professor Simpson quotes numerous authorities to show that the amount of suffering is often enormous.† Here we perfectly agree with him; and as this suffering does not occur with all races of mankind, but seems especially an attribute of high civilization, and perhaps of the increased dimensions of the fœtal head due to high civilization, certainly what civilization has brought about cannot be termed natural, least of all ought that method which takes it away to have the epithet *unnatural* applied to it, as by Dr. Ashwell and others. We wholly agree with the arguments of Professor Simpson on this point. The ridicule with which he has overwhelmed his opponents is as well merited as it is severe. This cuckoo-cry is precisely similar to that which was got up against vaccination, and might with equal sense be applied to the invention of steamboats or railroads. It is a very striking fact, that the benevolent Dr. Rush should have expressed a wish, so many years ago, that some means could be devised for freeing women from the torture they experience in labour. He first mentions the exemption which women have in some parts of the world, and then goes on as follows,

"I have expressed a hope in another place, that a medicine would be discovered that should suspend sensibility altogether, and leave irritability, or the powers of motion, unimpaired, and thereby destroy labour-pains altogether. I was encouraged

* See Dr. Meigs's Letter to Professor Simpson, American Medical Examiner, March, 1848.
† Anæsthesia in Surgery and Midwifery, pp. 95—97.

to cherish this hope, by having known delivery to take place, in one instance, during a paroxysm of epilepsy, and having heard of another, during a fit of drunkenness in a woman attended by Dr. Church, in both of which there was neither consciousness nor recollection of pain."*

The sole question we have to consider is, whether the relief of pain by etherization is beneficial or injurious to mother and child; common sense teaches us that pain is an evil, and if we can remove it without injury, it is vain to plead any fantastic notions.

Some have committed the very grave error of confounding the sensation of pain with *labour-pains*, or uterine contractions. That the contractions are essential to delivery is notorious; but the question is, whether they are necessarily attended with pain to the mother? It is true, in some cases, the uterine contractions have appeared to be lessened or suspended during the state of anæsthesia; but in others they have even been thought to be increased. It has appeared to many, that owing to the action of the will being suspended, and no voluntary movement made to impede the labour, this increase of the contractions is directly due to the anæsthesia. Dr. Channing thus expresses himself on this head:

"The whole uterine power is exerted regularly and uniformly, and progress almost at once declares itself, dilatation of the passage, as we have before seen, very soon becomes more perfect and determines the amount of contractions. The secretions are increased. In short, a state most favourable to easy, rapid, and safe delivery is produced and sustained, how successful both to mother and child the tables hereafter will show."†

As opinions are divided with regard to the precise degree of influence of etherization on the uterine contractions, it may be well to give the opinions of some of the authors whose publications are lying before us. Dr. Protheroe Smith says,—"It may momentarily suspend the natural contractions; but it does not suspend them, nor the contractions of the abdominal muscles, when energetically set up."‡ Professor Murphy, after referring to a case related by Dr. Shekleton of Dublin, where the administration of chloroform *seemed* to suspend the contractions, and to two similar related by Dr. Denham, comes to the following very important conclusions:

"1st. That chloroform does not impair the contractile power of the uterus, neither does it interfere with the action of the uterus, unless it be given in very large doses, or the patient be highly susceptible of its influence.—2nd. That the full anæsthetic effect of chloroform may be produced without paralyzing the uterus. The intervals between the pains may be lengthened, or the pains be suspended, without any loss of power. On the contrary, in such instances the moment that chloroform is withdrawn, the uterine contractions instantly return with increased force and efficiency. The renewal of the uterine action may occur when the patient is under the influence of chloroform.—3rd. Whether the action of the uterus be temporarily suspended by chloroform, whether it be increased or not interfered with, in every case where the patient had previously suffered agonizing pains, and her labour was making an unfavourable progress, chloroform has produced a most salutary change in restoring the proper action of the uterus, by which labour was brought to a happy conclusion.—4th. Those efforts that indicate want of power in the uterus—viz., great protraction of labour, hæmorrhage, retained placenta, &c.—have not been proved to be the results of chloroform; on the contrary, where some of these conditions have been observed,

* Channing, op. cit. p. 150. † Ibid., p. 108. ‡ Lancet, May 1st, 1847.

there were causes present quite adequate to explain them independent of chloroform.—5th. That degree of etherization which removes the intensity of pain without interfering with consciousness does not interrupt the action of the uterus."*

Professor Martin of Jena, concludes,—1st, that consciousness can be abolished in anæsthesia without in any way interfering with the activity of the involuntary muscles, and in particular of the uterus; but, 2ndly, that in very deep sopor the contractions do take place seldomer, without, however, their being prevented after an interval more or less considerable, from completing all that is expected from their agency.† M. Chambert quotes a case communicated by M. Bouvin to the Academy of Medicine, in which the uterine contractions had been paralyzed by ether, and labour suspended. The pains had lasted three or four hours, were very energetic, and returned every two or three minutes, when the woman was put under the influence of ether. As soon as she was completely etherized, the contractions immediately ceased, and did not return until a considerable time after she recovered her consciousness. It was about an hour before they resumed their former activity. According to the same author, Dr. Sieboldt communicated similar results observed in eight labour cases, to the Academy of Göttingen. Nevertheless, he does not attach much importance to these facts, regarding them as exceptional.‡ Dr. Merriman is unwilling to go beyond the third stage of Dr. Snow, in the administration of chloroform in midwifery. In the fourth stage he considers the uterine contractions to be weakened.§ Dr. Wise, who speaks from considerable experience, says, that the "labour-pains, or rather the muscular contractions, usually recur more frequently."‖ Mr. Clarke, an American writer, states, that "generally the duration and frequency of the labour-action were lessened."¶ The extreme opponents of the use of anæsthesia, like Mr. Gream, of course maintain, that the practice does diminish the force and frequency of the uterine contractions. Here again, instead of quoting more numerous authorities, we shall have recourse to the eclecticism of M. Bouisson. He says,

"The uterine sensibility is completely effaced, like all pains, by the influence of anæsthetic vapours. . . . . The action exerted by anæsthetic vapours on the contractility of the uterus is not so easily verified; so that it has given rise to contradictory opinions, to which practical consequences have come to be attached. The general fact most easily appreciated is, that under the influence of a moderate etherization the uterus can expel the product of conception even when its sensibility is completely annulled. But does the contraction remain in its normal limits? The reply should be in the affirmative, if we refer to the observations of Messrs. Simpson and Paul Dubois. It would experience, on the contrary, notable modifications from the evidence of some other accoucheurs. M. Stoltz, for example, has cited facts from which it results that the womb has a tendency to contract more energetically under the influence of ether, and that this increase of activity, by narrowing the cavity of the organ, hinders the obstetric operations which may be had recourse to. According to MM. Bouvin and Sieboldt, a contrary action is exerted by anæsthetic agents."**

* Edinburgh Monthly Journal of Medical Science, Dec. 1849.

† Uber die Künstliche Anästhesie bei Geburten durch Chloroformdämpf, ss. 50, Jena, 1848.

‡ Chambert, op. cit. p. 237.

§ Arguments against the Indiscriminate Use of Chloroform. London, 1848.

‖ Chloroform in Midwifery. By Dr. Wise. London, 1849.

¶ American Medical Examiner, March, 1848.

** Op. cit. p. 475, *et seq.*

He then goes on to quote the facts stated by these observers, to which we have already referred; and gives *his* opinion, that much of the difference of opinion on this point is due to the effects of the different stages of etherization being confounded. Thus the excess of action perceived by Stoltz may be due to the first effects of etherization; and the diminution to a very advanced stage of anæsthesia.

According to M. Dubois, while the muscles of the abdomen contract vigorously under the anæsthetic influence, those of the perineum are relaxed, so that in this way delivery is favoured. This difference is physiologically accounted for by M. Longet, who maintains that the abdominal muscles are comprised in the category of respiratory muscles; and that thus their motions survive those of the other muscles of animal life, while the muscles of the perineum belong to the common group of voluntary muscles, and are paralyzed by the anæsthetic action. M. Bouisson is of opinion, that as the abdominal muscles can only be considered as accessory respiratory muscles, and have more relation with the functions of the bowels than with those of the thorax, it is reasonable to suppose that the excitement, emanating from the uterus during parturition, is directly reflected from the spinal cord on the muscular walls of the abdomen; and what he thinks contributes to prove this is, that the "abdominal muscles may refuse the contingent of force which they bring to this act, if the etherization be sufficiently deep to abolish the reflex power, while they still continue to act as respiratory muscles, maintaining a residue of life."

From all this we may conclude, that etherization rarely interferes seriously with the contractions of the uterus in natural parturition, unless the anæsthetic state be pushed to its extreme limits, or the vapour be administered during the intervals *between* the contractions, instead of being only given *during* the contractions. Professor Simpson has combined the administration of anæsthetic agents with ergot. Our own opinion is, that on the whole labours are somewhat prolonged under the anæsthetic state, but not to such an extent as to interfere seriously with the use of the agents.

Let us now proceed to consider the principal arguments adduced in favour of anæsthesia in midwifery, and those on the other side; since we have cleared the ground, as it were, and shown that pain can be removed without any serious interruption of the natural process of parturition, if not with actual benefit to it.

The arguments in favour of the employment of anæsthesia may be summed up as follows:

1st. The removal of the pain is beneficial to the mother by preventing the nervous excitement and shock which physical pain is apt to excite, and the nervous and inflammatory reaction which, in some constitutions, is apt to result.

2nd. It renders many operations, requisite in complicated labours, easier of performance, and more beneficial to the patient.

3rd. Statistics prove the practice of anæsthesia to be beneficial to the mothers, and nowise dangerous to the children.

The first proposition appears almost self-evident, unless the opponents of anæsthesia could prove specific objections which we shall consider. In many delicate females, the excessive shock to the nervous system during delivery is a most serious source of injury. Most practitioners must have

seen cases where a kind of hysteria generated in this way has terminated in puerperal fever; and even where a puerperal epidemic was present, there is every reason to believe the exciting cause often to lie in the nervous system. The feelings of women who have been subjected to the anæsthetic state, after previous deliveries not under anæsthesia, cannot be disregarded; almost all of them are strongly in favour of the practice. Indeed, in general, they speak enthusiastically on the subject.

Under the second head, it may be observed generally, that the more midwifery becomes surgical, the less reason there should be for objecting to the practice of anæsthesia. And accordingly it is chiefly in preternatural labour, that its advocates become most decided in expressing their conviction of its utility.

Under the third, Dr. Channing observes,

"I have before alluded to the advantages of etherization in operative midwifery. In none of its departments are these advantages so signally declared as in preternatural labour, especially that variety of it in which the upper extremity presents. Such cases are remarkable for the power manifested by the womb in its spontaneous efforts to expel its contents. So violent and efficient sometimes are these, that they accomplish delivery by an evolution of the fœtus, which consists in the forced descent of the nates, and delivery of the breech along side of the arm; for that remains the presenting part through the whole of this marvellous process. In cases in which evolution does not and cannot occur, and in which the operation of *turning* is resorted to, to save the patient's life, the womb grasps its contents with an inconceivable power, and still further opposes to the operator the increased contraction which his manual effort almost necessarily produces."

Add to this the suffering which attends this operation in all its stages, and the voluntary resistance and efforts which the consciousness of what is going on produces; and we can understand how nearly all midwifery practitioners admit the very great benefit of etherization in such cases.* In instrumental labour, the facilities introduced by the practice are very great. Dr. Channing gives a table of twenty forceps cases under etherization, which all did well; there were only five still-births, and the convalescence was very rapid. The experience of numerous distinguished accoucheurs has fully confirmed this view. Among other cases of instrumental labour, in which we have ourselves witnessed the advantages of anæsthesia, was one in which a cancerous structure existed between the vagina and rectum. It was necessary to incise the neck of the uterus, and the patient was safely delivered, agonizing suffering saved, and the greatest benefit derived from the use of chloroform.

The following is quoted from Dr. Channing, since it bears on one of the objections to etherization in labour—namely, its asserted liability to cause convulsions:—

"There are nine cases of convulsions. Four of these were fatal. The whole history of these cases allies them to the gravest of their kind, and presents in a very striking manner the advantages of etherization in their management. In one, there was suspension of fits for three and a half hours, during which the child was born alive. Convulsions recurred, but the patient did well. In another, the convulsions were absent for three hours, during which a state of most promising results existed. The labour steadily advanced. The pulse, respiration, skin, all

* Velpeau may be added to the number of those who testify to the value of anæsthesia in cases of turning.

showed great improvement, and what I have not observed before, consciousness returned. But so deeply impressed was the whole system by the disease, so powerful was the action of its exciting causes, and so strongly marked were the symptoms of predisposition to it as they existed during pregnancy, that convulsions recurred before delivery, and though this was accomplished with great ease, were fatal. If the question should be asked, why was not etherization continued during the three hours of absence of convulsions? I answer, that there was nothing in that condition which seemed to me to require or to warrant its employment; and further, had fatal convulsions followed its use, would not this have been regarded as the direct result and effect of needless etherization? But it was not to escape any such possible issue as this, that the use of ether was omitted. It was discontinued, simply and wholly, because it was not thought necessary. This statement enables us to avoid erroneous inferences, and thereby a groundless objection to etherization is removed."

Dr. Channing, in one of his tables, presents a series of 28 cases of instrumental, preternatural, and complicated labours, in which etherization was not used. Of these 15 died! Of the 7 cases of convulsions, 6 were fatal, and there were 16 still-births! While of 51 cases of the kind, where etherization was employed, only 4 were fatal, and there were 19 still-births! Had any practitioner met with such results as the former of these tables exhibits, at the outset of the practice of etherization, doubtless the practice itself would have been to blame in his eyes.

For the statistics which have been published of the results of etherization in midwifery, we must refer principally to the works of Dr. Simpson and Dr. Channing. Dr. Channing's tables are too extensive to be transferred to our pages. The works of these accoucheurs contain, also, a large correspondence with numerous practitioners, which proves, not merely the safety, but the benefits, of the new practice. Dr. Simpson, in 1848, communicated the results obtained in 1519 cases, and in our opinion established the utility of anæsthesia upon incontestable grounds. But let us consider some of the objections which have been made.

These are chiefly; 1st, the so-called religious objections; 2nd, the assertion that etherization in pregnancy is immoral, and apt to excite improper feelings in the patient; 3rd, that in some cases it renders the use of the forceps dangerous; 4th, that it is injurious or dangerous to the patient; and lastly, that it is injurious to the child.

Our readers are well aware of the mode in which Professor Simpson encountered the argument from the third chapter of Genesis—namely, by showing that the true meaning of the Hebrew word used to express sorrow, did not offer any obstacle to the practice of anæsthesia. We should have preferred to have encountered the objection in another manner. Surely if the Almighty had intended woman, or rather ordained her, to suffer physical pain in giving birth, what effort of man could have availed to subdue his will? But it must be remembered, that there are certain prejudices, which, however we may be disposed to regard them as prejudices, ought still to be respected on account of their origin. And therefore perhaps Professor Simpson acted wisely in taking up the ground he did.

Of the second objection very much has been made.* Undoubtedly, anæsthesia does occasionally give rise to erotic sensations, but these are so very rare as not to amount to an objection. Indeed, it remains

* See especially the works of Mr. Gream and Dr. Barnes.

to be shown, whether the occurrence of the emotions referred to be more common in obstetrical than in surgical practice; so that if there be anything in the objection, it is applicable to the entire use of anæsthesia. On this point Dr. Simpson says—

"I have *never* once witnessed any trace of indecency, either in word or action, in any obstetric patient under the use of chloroform; and the evidence of one and all of my obstetric brethren of whom I have inquired on the subject, is to the same effect." (p. 148.)

The third objection has been principally urged by Dr. Meigs, who maintains that the sensations of the patient afford us our best guide for the introduction of instruments. He says, in order to introduce the forceps with the greatest safety to the mother, "the best guide of the accoucheur is the reply of the patient to his interrogatory 'does it hurt you?' The patient's reply, 'yes' or 'no,' are worth a thousand dogmas and precepts." Dr. Simpson answers the argument of Dr. Meigs by asserting, that every accoucheur possesses a much greater anatomical knowledge than is here supposed. Certainly the argument does look exceedingly like a plea for ignorance; and its only value arises from the very high authority from which it emanates. We disposed of Lallemand's analogous argument about the danger of including nerve and artery in the same ligature, in a similar manner.

The arguments under the fourth head, if these were true, would be fair; but the vast amount of testimony of a favourable character now before the world, renders it unnecessary to examine them in detail. The most has been said, that could be said on the subject; and time, if it has not fully resolved the question, is rapidly disposing of it. With regard to the alleged injurious effects on the children, Dr. Channing observes,

"Etherization, we are told, may mentally or physically affect the infant. It may be weakened in body or mind; and particular diseases are named, which may be looked for from the agency of the remedy of pain. This objection rests upon hypothesis alone. It has no facts for its origin, and none for its support."

Nevertheless, M. Amussat, in some experiments which he made on the females of animals in a state of gestation, observed, when etherization had been carried so far as to produce a commencement of asphyxia—the last stage of Dr. Snow—that the fœtuses were stupified to some extent, and their blood of a darker colour than usual.* M. Dubois, also, has noticed some acceleration of the pulse in new-born infants. He has observed an increase to the extent of 160 or 170 beats, instead of 130 or 140 ordinarily observed. M. Bouisson regards these facts as exceptions, and concludes generally that etherization exercises no deleterious influence over the health of the child.—This, moreover, does not constitute a valid objection against chloroform; for we have been informed by obstetric practitioners who had made trial of both agents, that while the influence of *ether* was frequently exerted upon the infants, as shown by their anæsthetic condition at birth, no such result has ever been witnessed by them since they have employed *chloroform*.

The same general rules which are applicable, as contra-indications to the occasional use of anæsthesia in surgery, hold good in obstetrics. In the latter branch prodigious quantities of chloroform have been given, and the

* Bouisson, p. 484.

anæsthetic state kept up for days without injury; thus Dr. Channing relates a case where in five days twenty ounces and a half were given; and Dr. Simpson has, we believe, gone even to a greater extent. The evil consequences which may result are similar to those mentioned as liable to take place occasionally in surgery.

M. Bouisson sums up in the following manner the results of his experience and his criticism. In all cases of forceps-application, frequently in difficult contraction of the placenta, in turning, in serious obstetrical operations, such as the Cæsarean section, symphyseotomy, craniotomy, and such like, and generally in all cases of painful labour, he recommends the use of anæsthetics. But in cases of ordinary labour, where the pain experienced is slight, he hesitates.

Dr. Channing has given some excellent general rules,* the principal of which is, not to employ anæsthesia in forceps cases until the instrument is applied, so as to mitigate the objection of Dr. Meigs; but this rule necessarily exposes the patient to considerable suffering during the previous part of the labour, and should not, therefore, be practised by one sufficiently conscious of his own resources.

We cannot quit this part of our subject without paying the illustrious professor of Edinburgh that tribute which he deserves for the courage, energy, and perseverance, with which he has carried out the application of anæsthetics to his own peculiar branch of our art. Nor is less credit due to him for the moderation and patience with which he has encountered some most harsh and wholly undeserved attacks. If he has sometimes used ridicule, it was where ridicule was the most efficient argument. Surely, even had the first to apply anæsthesia in obstetrics somewhat highly coloured the very brilliant results which he obtained, something of the kind might be pardoned; since enthusiasm is the very pabulum which supplies the inventor with energy. But Professor Simpson has been as exact and careful in stating his facts, as he has been firm in maintaining his views and sagacious in developing them. He has gained a niche in the temple of Fame among the great DISCOVERERS of new truths in medicine, beside the Harveys, the Hunters, and the Jenners.

(C.) Anæsthesia in tooth-drawing need not detain us long. Of course, the precautions stated to be necessary in giving chloroform or ether in surgery and midwifery apply to their use in the various operations on the teeth. It should not be forgotten, that of the fatal cases which have occurred, several have been cases of simple tooth-drawing. But it should also be remembered, that thousands of patients have been operated on in the anæsthetic state without accident. In the very excellent paper of Mr. Imlach, which has already been referred to, that gentleman states, that four years ago he had already used chloroform in three hundred cases of dental surgery without bad consequences, but the reverse. Other dentists have confirmed his statement. Those patients who have once had chloroform for the extraction of a tooth, almost invariably demand a repetition of the application, if another tooth is to be extracted. Mr. Imlach is also of opinion that anæsthesia enables the dentist to perform his work with "more satisfaction, certainty, and success." We can only say, that the use of anæsthesia in dental surgery appears to be fully warranted by its success.

* Pp. 94, *et seq.*

3. *Therapeutical Applications of Anæsthesia.*—It is now evident that the use of anæsthetic agents is capable of an extension beyond the bounds of merely operative medicine. It has been transported into medicine itself; and perhaps this circumstance may attract the attention of the profession to the advantages which may accrue from the use of other remedies in the form of inhalations. When we consider the great extent of the pulmonary mucous membrane, and the facility with which vapours may be introduced through the respiration into the blood, it seems extraordinary that this mode of administering medicines has not been more exactly studied. Dr. Beddoes, as is well known, established an institution at Bristol for the purpose of investigating the therapeutical effects of gases; and, independent of other results, we have seen that to this very institution we owe, through the genius of Davy, the first hint of anæsthesia by means of nitrous oxide. Pneumatic medicine may, perhaps, now receive a fresh development. Pain exists in a vast number of diseases, where even opium is insufficient to afford relief; in such cases a field is opened to the use of anæsthetic agents.

The first class of diseases which present themselves to our notice, are neuralgic affections. In several cases of intense facial neuralgia, benefit has been obtained from the inhalation of chloroform, when all other remedies have failed. In pains of the bowels, gastralgia, and in nervous colics, similar results have followed. Many observers have pointed out the benefit derived from doses of chloroform in the liquid form, in relieving the pain in the early stages of cholera. We can ourselves confirm this, with the addition that the doses ordinarily administered of chloroform in the liquid form have been much too small. We have ourselves given ten or twelve drops for a dose, or even more, instead of the doses of three or four drops recommended by some. Frictions of chloroform along the vertebral column have also been recommended. Ether at one time was announced, before the discovery of anæsthesia, as a specific in cholera; but, in fact, remedies of this kind can only be considered as palliatives. It has been said that the spasms of angina pectoris have been averted by anæsthesia. A. M. Aubrun "was able by employing it to lengthen to eight days the intervals between attacks (of the kind) which were formerly renewed several times in the twenty-four hours." M. Bouisson particularly points out the benefit which has been derived from anæsthesia in nephritic colic; and the probability of its being equally useful in relieving the torture which is occasionally felt during the passage of a biliary calculus. The relief experienced from the internal administration of ether in such cases had been before noticed, and ascribed to the solvent action of the ether on the fatty constituents of the calculi.

There are many spasmodic affections likewise, closely allied to the class of which we have been speaking, where the benefits of anæsthesia have either been verified, or are likely to be so; although in some such affections there have been very different results. For instance, in some cases of hysteria, etherization has produced a calmative effect—in others the reverse. In some patients, it has even appeared to bring on a fit of the disease. In epilepsy the evidence is, on the whole, unfavourable to the use of anæsthetic agents, and especially of chloroform. Where the symptoms seem to be accompanied with organic disease of the brain, this, of course, is particularly

the case; so much so, that in such cases the practice should be abandoned altogether. Hooping-cough, nervous asthma, chorea, and hydrophobia, may be particularized as diseases in which much was hoped from the inhalation of these agents, and where the results as yet have been insignificant. The cases in which most benefit may be expected, are, as Dr. Simpson has remarked, those in which the spasmodic affection results simply from undue excitability or super-polarity of the spinal centre. He has recently published a very interesting case of convulsions in an infant of only a fortnight old, in which the very best effects accrued from the use of chloroform, after all other remedies had failed, and the child seemed rapidly sinking; and he refers to the recent experience of this agent in pneumonia, as indicating that it may be freely used in the spasmodic stage of hooping-cough, without fear of inducing or aggravating pneumonic inflammation.*

In tetanus many favourable cases have been reported; although one of the first trials terminated fatally; and the patient was supposed to have perished partly from the effects of ether. This was in the practice of M. Roux. M. Bouisson states that he has been able to collect 15 reported cases of etherization in tetanus; and of these 10 terminated favourably. In 7 of these cases, ether was the agent employed; and in 3, chloroform. It is worthy of observation, that ether having been employed in the liquid form, administered by the stomach in the treatment of tetanus, we have thus a means of comparing the respective energies of the gastric and pulmonary mode of administration; and the result shows that the former is decidedly less efficacious than the latter. M. Hutin, surgeon-in-chief to the Hospital of Invalids, says, that in Africa he had observed 60 cases of traumatic tetanus; and that of these a third had been treated by ether in the liquid form, administered by injections and frictions along the vertebral column, without favourable results. However, it might be well, in the treatment of such a disease as tetanus, not to rest content with one mode of applying ether or chloroform, but to use these bodies in various ways, by way of inhalation, friction, or otherwise.

In mental alienation, anæsthesia has been a good deal used. Mac-Gavin of Montrose, has especially pointed out its beneficial effects in cases of madness with much excitement. One great use of anæsthesia in lunatics would be, that by means of it their indocility could be overcome, and thus they might be brought to undergo operations to which otherwise they would be refractory. It has been thus used to administer food through tubes in the œsophagus, to patients who were resolved not to swallow, but rather to die of hunger.

In meningitis and delirium tremens, the use of anæsthetic agents is of very doubtful advantage.

On the whole we may sum up with M. Bouisson as follows:—1st. Lesions of sensation are those in which inhalations of ether and chloroform are especially useful, as in the various forms of neuralgia; 2nd. In lesions of the motive power, the beneficial results of anæsthesia are less evident; and, 3rd. Affections of the intellect appear the least susceptible of benefit of the whole.

Anæsthetic agents have been applied locally in the way of frictions, in nervous and rheumatic pains, in painful ophthalmia, and in orchitis. Under

* Edinburgh Monthly Journal, Jan. 1852.

the hands of some it has been found exceedingly successful in relieving pain and subduing inflammation, and in the dressing of ulcers. The first effect of the local application is to produce some degree of redness; but this soon subsides; and the secondary local action is decidedly anæsthetic. It is very plain to us that we are only at the beginning of the medicinal use of these agents; and we would refer in illustration to the remarkable results of the use of chloroform in the treatment of pneumonia, which have been obtained by Wachern, Baumgärtner, Hebling, Schmidt, and Varrentrapp, who have altogether treated 216 cases in this mode.*

4. *Applications to Legal Medicine.*—In order to show the use which may be made of anæsthesia in feigned diseases, we extract the following from the work of M. Bouisson. He had for a long time thought of employing this means in the diagnosis of deafness supposed to be feigned:

"Chance," he says, "had not furnished me with a case of the kind; but having in 1848 to treat a soldier whom I suspected of counterfeiting deafness, and who showed himself very clever at his part, I chloroformed him slightly, and on the first appearance of anæsthetic sleep I asked him questions, in a low tone, to which he replied. This result may be explained by reflecting on the effect of these stupefying vapours on the understanding. The unconnected chain of ideas, as in drunkenness, do not admit of those precautions which are requisite for the success of the pretender."

Simulated dumbness and stammering can be detected in a similar way; and in pretended muscular contractions, the method is of easy application, and has been found very successful.

It is very obvious that anæsthesia might render very much easier such a plot as the celebrated one in which the Countess of Saint Geran was the victim; for whatever opium or any other poison of the kind could accomplish, ether or chloroform could certainly effect more surely. The substitution also of one child for another could thus be rendered easy. Such circumstances are no objections to the practice of anæsthesia. Every great discovery has some inconvenience inseparable from the lot of mankind. In the same way, no doubt, etherization might be employed by the libertine to accomplish criminal purposes; and young or feeble persons might be made to perish by means of ether, or especially chloroform. We have already spoken of the *post-mortem* appearances to be expected in such cases. We shall conclude, however, by quoting on this head the following observations of M. Bouisson. He says,

"Although etherized or chloroformed animals which have been killed by this form of intoxication, are exempt from constant and characteristic anatomical lesions, they often present the greater part of the appearances of asphyxia. The heart is distended with blood, the lungs are coloured of a deep red; there exist sometimes apoplectic spots in these organs, ecchymoses beneath the pleura, or traces of emphysema. The liver, of the colour of wine-lees, is gorged with black blood; the kidneys are of a violet-colour, owing to sanguineous congestion. The vessels of the meningeal membranes are distended; the pia mater is especially injected towards the lower part of the brain and the annular protuberance. The cerebral pulp is in general spotted with blood. The blood itself is blacker and more fluid than is natural. Bubbles of air are often remarked. But the truly important character, and which can distinguish anæsthetic from ordinary asphyxia, is the characteristic odour of ether

* See Henle's Zeitschrift für Rationelle Medecin, Neue Folge, band i. heft 1; and Medical Times, Oct. 18, 1851.

and chloroform preserved by the blood, the fluids, and the tissues. M. Flandin, by analyzing the blood, has extracted ether from it; and M. Lassaigne, while declaring that it is difficult to obtain much, does not the less point out the decisive marks of its presence."

We observe that Dr. Glover, in his 'Experiments on Chloroform,' published in 1842, remarks upon the power which chloroform and its allied bodies appear to possess of penetrating the tissues. In speaking of one experiment, he observes, "the smell of the chloroform was distinct in the chest, and in the urine passed a few minutes before death."

We conclude by congratulating the profession on the amount of zeal and energy which has been displayed in this interesting subject; and by hoping that anæsthesia will be carried to the furthest extent of which it is susceptible. We are no Sybarites; but unfortunately, with all our efforts, there will still remain in the world a sufficient amount of misery. Let us do what we can to mitigate human suffering.

Art. IX.

On the Nature and Treatment of the Diseases of the Heart, containing also an Account of the Musculo-Cardiac, the Pulmo-Cardiac, and the Veno-Pulmonary Functions. By James Wardrop, M.D., &c.—*London*, 1851. 8vo, pp. 587.

We intend to pay Dr. Wardrop a compliment, when we say that his book is not easily reviewed. It is clever, original, and suggestive; but it is rather loose, rambling, and we suspect in some places inaccurate. No one can read it without perceiving that it is written by no ordinary man; yet many ordinary men have written books which have much more won our confidence. It is a book which deserves no censure, and yet does not command unconditional praise. It displays, in part, evidences of unusual power, of genius even; in part, traces of very superficial reasoning and hap-hazard conclusions. Every one who reads it carefully will be instructed by it; yet few would like to adopt its statements without modification and reserve.

There is one great merit about the work. It is perfectly clear, whether right or wrong. Dr. Wardrop has stated his opinions with precision; and although rather lengthy in his exposition, we can give him nothing but praise for his manner of expression. Dr. Wardrop places on his title-page three terms, which have already, doubtless, arrested the attention of our readers. He speaks of certain functions, which he designates musculo-cardiac, pulmo-cardiac, and veno-pulmonary. These are terms which, so far as we know, have been used only by himself; and a short exposition of them may not be without interest.

The nature of the "muscular-cardiac function" will be best explained in Dr. Wardrop's own words.

"When the respiratory apparatus is healthy, and the circulation through the right heart undisturbed, the pulmonary veins afford a sufficient supply of arterialized blood to the left heart for the ordinary purposes of the systemic circulation. During sleep this must be strictly true, but if the heart is suddenly required to act with an additional vigour—a circumstance which is constantly taking place whilst we are awake,—an increased supply of blood then becomes necessary, the energy of

the heart depending on the quantity of blood with which it is supplied. This important office we shall find is fulfilled, not only by accelerating the flow of the venous blood into the right heart, but, as I shall endeavour to show, also by *impeding* the exit of the arterial blood from the left heart, both which purposes are admirably effected, simply by the change of form which the muscles undergo during their contractions." (p. 23.)

In other words, Dr. Wardrop believes that arteries are continually undergoing pressure from the action of the adjoining muscles, and that the diminution of calibre, and the consequent impediment to the circulation, keeps the left heart well supplied with blood. At the same time the veins are also pressed upon; but on account of their valves, the blood in them is forced onward to the heart.

Without entering into the various arguments which are advanced in support of this view, we may object to it on the grounds, that in the majority of cases, there is no evidence of muscular compression of arteries, and that if there was, it is difficult to see what useful purpose this could serve. Nothing that we can find in Dr. Wardrop's book alters our opinion, that most arteries provided with firm coats, and disposed as they so frequently are in thick sheaths, and in protected situations, are removed designedly from the influence of muscular movements. So also are the large veins, as any constant pressure upon these would necessarily impede, and not accelerate, the flow of blood. Moreover, on Dr. Wardrop's hypothesis, the very activity of the muscles would tend to obstruct the supply of blood which is required as the condition of that activity.

The "pulmo-cardiac function" is still more doubtful. Dr. Wardrop assumes that the lungs, in addition to their usual duties, perform the office of a reservoir for receiving any surplus quantity of blood, either venous or arterial, which the cavities of the heart cannot admit. The blood is supposed to accumulate in the pulmonary arteries and veins, until the overburdened heart is ready to receive it. This hypothesis has evidently arisen from the former one; for Dr. Wardrop has seen, that if in every muscular movement the arteries are compressed and the exit of blood from the left ventricle impeded, it would be necessary to provide some means of counteracting the great accumulation which would sometimes inevitably result.

But as the lungs might evidently be unequal to this duty of a reservoir, or might be damaged by excess of it, Dr. Wardrop carries the reservoir a step further back into the venous system, and supposes that as the lungs receive the surplus blood of the heart, so the subcutaneous veins receive the surplus blood of the lungs. This constitutes the "veno-pulmonary function."

"That the subcutaneous veins perform the office of a *receptaculum*, by admitting any surplus quantity of blood, which cannot be admitted into the pulmonary vessels, in like manner as we have seen that the pulmonary vessels themselves receive that blood which the cavities of the heart cannot admit, is illustrated by the phenomena which accompany different degrees of distension of these vessels." (p. 65.)

Now, that any great impediment in the arterial system, or at the mouth of the aorta, will cause accumulation in the left ventricle, the auricle, the pulmonary veins, and lung-substance, and that this pulmonary congestion carried to a great degree may cause impediment in the right heart, and in the great veins and venous system generally, are well-known facts. But that such an accumulation is physiological, not pathological, and occurs in a

moderate degree many times a day in every one, appears to us a doctrine which it would be waste of time to refute. We do not find, either, that Dr. Wardrop advances a single good argument in support of his hypotheses; but as he is ingenious here as everywhere, we beg to refer our curious readers to his book, to judge for themselves of the value of his observations.

After discussing these "functions," Dr. Wardrop passes on to some additional preliminary matter, and has a chapter on the "reciprocal influence of the heart and nervous system." He then enters on the consideration of the diseases of the heart. The arrangement is very systematic and complete, although the several items are by no means discussed with equal precision. The phenomena, causes, and symptoms of heart-diseases generally, are discussed; first, the symptoms of diseases of the heart derived from remote parts, such as from the cerebro-spinal, the respiratory, the digestive, or the uterine system; then follow the diagnosis, prognosis, and treatment of heart-diseases generally; and, lastly, the different diseases of the heart are considered. In this part of the work are many serious omissions, many important remarks, many observations with which we cannot agree, and many, of whose accuracy we feel doubtful. We shall not attempt to go through these several chapters; but shall notice a few points which appear to us of especial interest. Chapter 26 is on the "Gouty inflammation of the heart." As far as we can see, Dr. Wardrop considers that any cretaceous or chalky deposit in the endocardium or pericardium, is the consequence of gouty inflammation. "The chalky or earthy deposits," he says, "so frequently met with in the membranes of the heart, may be legitimately considered as the produce of an arthritic inflammation." Some of these deposits, he then says, are composed of urate of soda; at least we presume he is referring to the cretaceous deposits of the heart. If this view were admitted, then inflammatory exudation and atheroma, of which the calcification is an educt, are also to be considered as produced by gouty and not by rheumatic or other forms of endocarditis.

Of this, however, there is no proof, and Dr. Wardrop has certainly jumped very hastily to his conclusion. At the same time, the exact effect of frequent gouty attacks upon the heart is well worthy of investigation. Dr. Wardrop alludes to the last illness of George IV., who died, he says, from gouty deposits about the heart. From the official account of the *post-mortem* examination which he quotes, it appears, however, that the heart was fatty, and that the aortic valves were calcified; and the only reason for considering this to be gouty appears to be, that the king had had repeated attacks of gout, and that this disease was hereditary in the royal family.

In the following chapter, on the Rheumatic inflammation of the heart, it is stated that the urate of soda is never found in the exudations. If this be asserted from sufficient observation, it is a very interesting point; but from the way in which Dr. Wardrop uses the terms urate of soda and cretaceous deposits as synonymous, we are inclined to hesitate before receiving it.

The chapters on Treatment are practical and useful; but we do not find anything which calls for remark. We therefore leave Dr. Wardrop with the hope that our short notice may induce some of our readers to peruse the entire work.

Art. X.

1. *On the Fallacies of Homœopathy, and the Imperfect Statistical Inquiries on which the results of that Practice are estimated.* By C. H. F. Routh, M.D., M.R.C.S., &c. &c.—1852. 8vo, pp. 85.
2. *Report of the Speeches on Irregular Practice, delivered at the Nineteenth Anniversary Meeting of the Provincial Medical and Surgical Association, held at Brighton, August* 13 & 14, 1851.—8vo, pp. 32.
3. *Speech at the Medico-Chirurgical Society, relative to Homœopathy: with Notes on the peculiar Theological Opinions of some Disciples of Hahnemann, &c.* By James Y. Simpson, M.D., President of the Royal College of Physicians, and Professor of Midwifery in the University of Edinburgh.—*Edinburgh*, 1851. 8vo, pp. 27.

From the authorized list of the homœopathic practitioners of medicine, it does not appear that they exceed above fifty in London, and seventy or eighty in Great Britain and Ireland. Considering the smallness of their numbers, they make a wonderful noise, especially when criticized. Clamorous outcry is evidently part of their system, as well as a diligent propaganda through the press. If one of their body be subjected to criticism, a general croaking or quacking is heard from all sides. All suffer with him, whether pastry-cook or pure. All are despised with him; but, like Maw-worm, they like to be despised, for it helps them in their vocation, which is, to get money by empirical practice to an infinitesimal amount. They are earnest, solemn men, for the most part; they never laugh in their sleeves—at least, are never seen to laugh. Laughter would be unbecoming in men who suffer a daily martyrdom from the drug-and-poison-dealing "allopaths." They are religious, nay, they are professedly devoted Christians; but then, they acknowledge Hahnemann as a prophet. They are charitable; for so soon as one of the body settles in a large town, he opens a homœopathic dispensary, and dispenses globules to as nearly an infinitesimal an amount as he can, on "Mondays, Wednesdays, and Fridays," or probably "every morning," of which a Brobdignagian inscription on a Brobdignagian board gives legible announcement. But they are thrifty with their charity, and require those who avail themselves of the dispensary facilities, to pay "half-a-crown per month," if they can. They cultivate the agreeable, especially to the ladies, to whom they are lavish in presents of homœopathic tracts and globules; but they are also rigid logicians and skilful dialecticians. The world is dunned and dinned with their "Reasons" for "embracing" homœopathy (that irresistible syren), in which the great and potential reasons of neglected merit, and the consequent *res angustæ domi*, are faithfully omitted. As martyrs to their much vilified, because elastic faith, they are really sublime. Like all *other* great and good men, they also suffer for the Right; but associated with Harvey, Jenner, and the like, as "searchers after truth," they can suffer with resignation, especially as their martyrdom facilitates their success. A "universal law" is the burden of their plaint, which, by directing "the proper use of dynamic medicines," will "eradicate all those psoric tendencies, which cause or

increase all our aches, pains, ill tempers, obstinacies, rebellions, cachexies, and all chronic diseases"*—a law which strikes potently at the mortality caused by destructive pestilences, and disarms them of more than half their virulence, as well as at all other severe affections. "Je veux des maladies d'importance," says the homœopath, with Moliere's quack, "des bonnes fiévres continuées avec des transports au cerveau, des bonnes fiévres pourprées, des bonnes pestes, des bonnes hydropisies formées, des bonnes pleurisies, avec des inflammations de poitrine. *C'est là que je me plais, c'est là que je triomphe.*"

This boast is, indeed, the crowning point of their art. Their appeals to popular opinion, their soft blandishments, their globulistic charities, their religious power, their solemn energy, their sublime martyrdoms, their cynical abuse of their brethren,—all would be comparatively useless, were it not for the loudly-proclaimed and ever-repeated announcement, that it is their system alone which is effectual against serious or dangerous maladies. "C'est là que je me plais, c'est là que je triomphe."

But although they repudiate rational medicine, and that "old experience" which in medicine has attained "to something like prophetic strain," because they found it impossible to practise their profession with a good conscience under its directions,† still they cling to its skirts, and would, by any possible method, secure its approval. Although, in their declared judgment, the old system is based on mischievous principles, and leads to none but mischievous results, and although they are snubbed and scouted, for their abusiveness and folly, by the practitioners of the old school, as not of their professional republic, but aliens to their ethics and creeds, still the homœopathists tenaciously cling to the legitimate body, long for its honours, and, by the fact of sending their publications to us, earnestly implore its criticism. The criticism desired they shall have; but not on the ground chosen by themselves. We grant them nothing. They are not to us disinterested inquirers after truth, patient endurers of unmerited contumely, followers of a faith which the dictates of a deep conscientious conviction required them to adopt. They are rather men who follow their profession, and who suffer their pseudo-martyrdom, for no higher or holier motive than the need to make money. Their motto is "Rem." No sophistry, however elaborate; no manifestation of scientific research, however apparently profound; no protestations of deep conviction, however loud; can blind the observer of their proceedings to the damning fact, that before they embraced homœopathy, they had no success, pecuniary or curative, in ordinary practice. Their failures in the former sense are patent to the world, in the latter are loudly proclaimed by themselves. It is their asserted curative success in their adopted *art* that we have alone to consider; and we will now examine whether they have been as successful as they boast to have been.

In investigating the alleged authenticity and weight of recorded facts, it must not be forgotten that some inquiry is requsite into the fidelity and truthfulness of the recorder. When the ordinary practitioner communicates to the press the result of his experience, for the benefit of his brethren, he

* Published Sermon of the Rev. T. R. Everest, Vice-President of the Hahnemann Hospital, &c.

† The Union of Hydropathy with Homœopathy: an Address read at the Homœopathic Congress in London.

is not biassed by the one great motive to error to which the homœopathist is liable. He subjects his doctrines and his facts to an *exclusively* professional tribunal, and consequently to judges competent to decide. His pecuniary success is only so far dependent on his record, as he succeeds in convincing his brethren of the accuracy of his facts and the soundness of his conclusions. It is not so with the homœopath. His first and main appeal is to the public at large. That is the incompetent and unfit tribunal from which he seeks favour; and if he appeals to the professional tribunal, it is solely that he may the more strongly influence the public mind. The latter appeal is only a trick of fence. Such a tribunal as he prefers, he knows well, cannot sift and investigate his bold assertions, or the bolder statistics he advances in support of his assertions; and it therefore follows that there is a constant temptation to fraudulent practices, to which the ordinary practitioner is not exposed. It is not surprising, then, that a vein of sophistry runs through the homœopathic writings; that facts are habitually distorted; and that even direct frauds—real palpable breaches of good morals—are treated with absolute tolerance.

The pamphlet by Dr. Routh is an able exposure of the scientific fallacies and sophistries of the homœopaths; and the other pamphlets before us contain exposures of actual frauds. It would be manifestly unfair to tax a whole body with the delinquencies of individuals, if these delinquencies were fully and absolutely repudiated in its authorized publications; but what can be said of the men and the literature of the sect, when we find fraud not merely *not* repudiated, but apologized for? It is fully in the recollection of our readers that a tradesman of Taunton, named Blake, managed to get admitted to examination at the College of Surgeons by the production of fraudulent certificates; that the fraud was detected by the profession resident in Taunton; and that the name of the offender was struck off the college lists. Such an individual could not, of course, be met in consultation by any honourable man, in whatever way he professed to practise. Now Mr. Blake had the misfortune to attend a case upon which an inquest was held; and under the head "Mr. Blake and the Coroner," the editor of 'The British Journal of Homœopathy'* thus comments on the conduct of Blake's "medical adversaries."

"They have two reasons for their hostility: a personal feeling against Mr. Blake because he was once a tradesman in their town, and the fact of his being a homœopathist. We just advert to the fact, that after having passed a very creditable examination, and obtained his diploma, the council of the College of Surgeons caused his name to be struck off from the list of their members, on the ground that he had obtained the favour of a premature examination by fraudulent certificates of his previous course of study. . . . . Having, we think injudiciously, chosen Taunton, where he had previously sold comfits, as his place of residence as a homœopathic practitioner, he necessarily encountered the dire rage, and bitter opposition, of his professional brethren of the allopathic school. There is nothing so disgusting as the meanness, the baseness, of this kind of hostility," &c.

To those who consider the concoction of a fraudulent certificate to be the deliberate utterance of a damning lie, and constituting, therefore, an indelible stain on the character, these remarks of the editor of the 'British Journal of Homœopathy' must appear singularly conclusive as to

* No. 29, p. 431.

the reliance that may be placed upon anything printed or published by the sect. Unfortunately, this kind of practice is by no means rare; or rather, it seems to be the rule with homœopathists to obtain diplomas by ingeniously misrepresenting themselves or their views. Thus a person of Norwich proceeded to graduate at St. Andrews; and when examined, gave perfectly orthodox answers to the requirement that he should "specify the mode of treatment he is in the habit of adopting, and the doses of the medicines which he prescribes." Yet he was at the time a homœopathic practitioner, and did *not* specify the doses of the medicines which he prescribed. On Dr. Day reproaching him with what appeared to Dr. Day "the grossest deception," this person's feelings are quite hurt! and he says—

"I shall simply state that it was not my business to declare myself a homœopathist, and that I think your expression of 'grossest deception' is wholly inapplicable either to me or to any one who, under such circumstances, *practises such reserve* as I did. . . . . I cannot imagine that you, or any right-minded man who considers the present state of medicine and of medical parties in these realms, would, after due consideration, apply the term you have used to *a reserve so necessary as that I practised* in not putting forward my therapeutical views."

A somewhat similar case occurred at Edinburgh. A student went up for graduation, and "a plain question" was put to him by Dr. Christison, with the hope of eliciting "a downright answer." The question was, "Do you intend to become a homœopathic practitioner after you graduate?" The individual thus pointedly addressed, had avowed privately three years previously, that he had already practised homœopathy, and came to study for his degree, in order to settle *honourably* as a homœopathic practitioner. He therefore answered Dr. Christison's plain question "with the reserve necessary in the present state of medicine," and said, "I am not now a homœopathist, but after graduation I mean to inquire into the truth of it."*

We might multiply instances of similar obliquity of moral perception; but it is perhaps in the compounding of doses of remedies, in the nomenclature of cases, and in the published results of treatment, that this convenient system of mental reservation and ingenious equivocation is most apparent. The sect aims at pecuniary success. To secure this, it must demonstrate the therapeutic success of its principles and practices; and with a view to this demonstration, we have, in addition to the venial errors of an insane enthusiasm, the deliberate concoction of fraudulent proofs. However much we may apologize for and excuse unintentional error, we are staggered at the outset by the damning fact, that homœopathic practitioners have been detected in the administration of doses so large that they have proved destructive to life. In private practice this fraud is easily practised, since powerful doses of remedies may be combined in a pilule. Dr. Routh's remarks on this point are important:

"Occasionally, however, it becomes necessary to show that homœopathic medicines have really an effect, and this is especially useful to unbelievers. Our strongest active principles are put in a homœopathic pill or pilule, and administered. It is owing to such treachery that the Duke of Cannigaro [Cannizzaro, formerly known in London as Count Antonio] was killed by three homœopathic globules. The

* Edinburgh Monthly Journal of Medical Science, August, 1851.

death of Mr. Horace Green, of New York, who swallowed in sport a number of homœopathic globules, is explained in the same way. Indeed, chemical analysis has proved to what extent this fraud is carried occasionally; $\frac{1}{3}$ of a grain of morphia and appreciable doses of mercury have been found. When we remember that $\frac{1}{50}$ of a grain of elatin will purge, $\frac{1}{2}$ a grain of strychnia will kill, $\frac{1}{50}$ of a grain of aconite may kill, &c., it is easy to understand how a globule so medicated would have a sensible effect." (p. 10.)

In the nomenclature of their cases also, the homœopathists evidently adopt that "reserve which is so necessary in the present state of medical science" to be practised by them. The profession has been so long accustomed to the strictest rectitude in diagnosis, and to consider it a most imperative duty, that it is hard to conceive that any duly qualified physician would deliberately and wilfully pervert the truth in this respect. Yet we have very suspicious examples of statistical data and of recorded cases of this kind. Dr. Routh quotes instances of the latter (which are sufficiently numerous in homœopathic literature), and demonstrates the "cookery" of the statistics. As it is to the latter that homœopathists appeal in the last resource, and as they constitute the citadel within which they take refuge when their absurd theories are subjected to the *reductio ad absurdum*, it is worth while giving a short synopsis of the results at which Dr. Routh has arrived, and of the inferences to which they necessarily lead. The more important hospital statistics of homœopathic practice are derived from certain hospitals in Germany, especially Fleischmann's in Vienna, and those of Liege, Dresden, and Leipsic.

In the first place, the homœopathists assert that their method of treating diseases diminishes very considerably the *general* mortality. In ordinary hospitals, the mortality ranges from 7 to 10 per cent. of the admissions, while in the homœopathic it is shown to be as low as 1·4 per cent. in the Leipsic hospital during the years 1842—1849, and in the Vienna hospital it did not exceed the maximum of 6·4 per cent. The difference is great, but it is easily and perfectly accounted for. They select their cases, and with that "reserve so necessary in the present state of medicine," do not reveal the principles of selection. Dr. Routh has, however, unravelled the mystery. In the first place, they do not include the persons who are admitted moribund; now this, as Dr. Routh shows, makes a difference of 4 per cent. Then they do not admit incurable cases, and they adopt a different nosology, so that mild cases read as if they were severe examples of disease; or they give a preference to neuralgic cases—a class in which a fatal termination rarely occurs. Dr. Routh observes,

"Are the cases in both allopathic and homœopathic hospitals identical in nature? I do not hesitate to say they are not. I remember having once seen a young lad admitted in Fleischmann's hospital at Vienna for simple headache. On the visit the next day he was well, and yet had not seen any physician, or been prescribed for. Yet, on his visit, *the physician could not pass him over*. A globule was ordered; and no doubt in the annual returns the case was recorded as a cure. I do not say this was otherwise than an accident, but many such accidents would materially affect the mortality. I can honestly affirm, that the serious cases are few and far between." (p. 39.)

Dr. Routh adds the statements of the hospital returns in corroboration, and shows that during the years 1835—43, the cases of hysteria, hypochondriasis, spasms, chlorosis, headaches, dyspepsia, catarrh, &c., were 622

not to include 270 very mild surgical cases, and such diseases as tonsillitis; pertinently adding, that in the face of such numbers admitted, having diseases rarely, if ever, ending fatally, it is very difficult to believe that the cases are not selected. In Leipsic, the proportion of cases of headaches, amenorrhœa, and chlorosis admitted, was 29 per 1000; in the Glasgow Infirmary only 4 per 1000:—the proportion of cases of odontalgia, or toothache, 56 per 1000; a disease actually not known at all on the *books*, at least, of ordinary hospitals. In this way it is very easy to have a *hospital* population actually presenting a lower rate of mortality than a healthy agricultural population; in the latter it is hardly ever below 2 per cent., whereas in the Leipsic policlinik in 1839, it was only 0·5 per cent., or 1 in 200, instead of 1 in 50. The result of the method followed by the homœopathic hospital authorities is well illustrated by some interesting statistics in No. 8 of the London weekly tables of mortality for this year. The mean mortality of the general London hospitals for 34 days was 7·59 per cent. of the cases treated. The maximum in any particular hospital was 10·5 per cent., the minimum 4·47. It is obvious that the severity of cases, and the number of severe cases admitted, will cause a considerable variation between particular hospitals; but in the military hospitals, all the cases of illness in the army, whether mild or severe, are treated, and the result is, that the mean mortality is only 2·108 per cent.; the maximum being 2·66, and the minimum 1·11 per cent. If the severe cases, such as phthisis, &c., were excluded from the military hospitals, the mortality would be quite homœopathic. Now the *selection* of favourable and curable cases implies the *exclusion* of the unfavourable and incurable from the homœopathic hospitals. Dr. Routh takes phthisis as an illustration, and gives the following table, that "figures may speak:"

"*Cases of Phthisis.*

	Number.	Ratio per cent.
Glasgow Infirmary, 4 years	481	..... 4·0
General Hospital, Vienna, of 51,709 cases . .	366	..... 4·5
Dresden, 1821—43, of 27,067 cases	1854	..... 6·8
Strasburgh, 1841, of 1324 cases	128	..... 9·6
In the homœpathic hospitals—		
Fleischmann's (Vienna), 6501 cases	98	..... 1·5
Leipsic, 1841—48, 6507 cases	101	..... 1·6." (p. 40.)

Dr. Routh may well remark, "this is a singular coincidence!"

Another fallacy (or might we not say fraud?) is demonstrated by Dr. Routh. It would be easy to diminish the proportion of deaths from any given disease by classifying with them mild cases, not altogether symptomatically, although pathologically dissimilar, such as bronchitis with pneumonia, cerebral congestion with cerebritis, pleurodynia with pleuritis, &c. In considering the *Io pœans* of the homœopaths in treating "des bonnes pleurisies, and des bonnes inflammations de la poitrine," Dr. Routh again makes figures speak. First, as to pneumonia, the comparative success of the new lights and the old is thus set forth by the homœopaths:

	Cases.	Deaths.	Mortality.
Under homœopathic treatment .	783	... 45	... 5·7 per cent. or 1 in 17
„ allopathic „ .	1522	... 373	... 24·5 per cent. or 1 in 4

A result apparently most favourable to homœopathic treatment.

"But, first, the question presents itself, *are these cases occurring in the homœopathic hospitals genuine instances* of pneumonia? Let us make figures speak. I find that in the two years 1848 and 1849, there were admitted into the General Hospital at Vienna, 51,709 cases. Of these, 3884 were cases of bronchitis, or 7·5 per cent. of the whole, and 1134 cases of pneumonia and pleuro-pneumonia, or 2·1 per cent. Applying this test to Fleischmann's Hospital, out of 6551 cases admitted between the years 1835 and 1843, there were only 59 cases returned as bronchitis or catarrh, or 0·8 per cent., and 300 as pneumonia, or 4·5 per cent. . . . . The review of these facts admits but one of two inferences; the cases are either picked out or selected, or the diagnosis is wrong. In the same town we should expect a similar number of cases. Taking the General Infirmary of Glasgow, the proportion of bronchitis cases, out of 12,007 cases of all diseases, was 423, or 3·6 per cent., of pneumonia 141, or 1·1 per cent. of the whole." (p. 47.)

Such are the extraordinary revelations made by a collocation of facts. Our readers will, we think, go along with Dr. Routh in his conclusions, and fully comprehend some additional statements.

"The above conclusions seem to point out that the principal reason of the homœopathic success is to be found in the incorrect diagnosis or selection of cases. Diseases are not called by their proper names. Thus, in the Leipsic (1841—42) returns, we have 7 cases of peritonitis *muscularis;* in 1840—41, 1 case of peritonitis muscularis, another of pleuritis *muscularis;* again in the same year, 3 cases of muscular peritonitis, and muscular pleuritis, &c. In their cases of pneumonia, instances of *pneumonia*-hypostatica are recorded. This improper nomenclature, it is clear, as including diseases perfectly different, and in no way fatal, must materially affect the cipher of mortality when comparison is made for special diseases." (p. 48.)

Not content with this exposure, Dr. Routh hunts the lying statistics of the homœopaths through other fallacies, as of age, sex, primary or secondary inflammation, &c. He also compares the statistics of ordinary methods of treatment under favourable circumstances with the homœopathic, and conclusively demonstrates their worthlessness.

Pleuritis is another disease marvellously obedient to homœopathic treatment. In ordinary hospital practice it destroys 13 per cent. of those it attacks, homœopathy reduces this figure *apparently* to 3 per cent. But, curiously enough, as in pneumonia so in pleuritis, the relative proportion to other cases is more than double that which occurs in ordinary practice. "The reality is," observes Dr. Routh, "that most of the cases returned are cases of *pleurodynia;*" and he shows, by quoting recorded cases, that their own histories of so-called cases of pleurisy prove this indubitably. Indeed, we cannot but be struck with the gravity and importance often attached to cases of really trifling moment, detailed by the homœopaths, and with the easy credulity they display in believing their patients to be seriously ill. In the thirty-first number of the 'British Journal of Homœopathy,' we accidentally stumbled upon an illustration of this habit. A Dr. Cockburn sends an instance of a cure of hydrophobia, in which he "gave merc., to be followed by bell. next day," and then "gave laches," with the happiest results. "Having made the subject of hydrophobia one of special investigation," remarks the historian, "*I have not the slightest hesitation* in giving my opinion that the above was a decided case of that disease not fully developed." Yet the editors of that journal, who are so indulgent as to characterize the impious and insane vagaries of Mure in Rio, as not being "in all cases the most judicious," cannot

swallow the absurdity of this hydrophobia case, and although weak enough to print it, have sufficient regard for their own character to "confess we cannot share the confidently expressed opinion of our esteemed correspondent!"

To show what may be done in the way of obtaining fallaciously successful results, even when there is *bona fides* in the diagnosis, Dr. Routh quotes some numerical results of treatment by rational medicine, in cases of undoubted pneumonia. He observes,

"In 1824, Laennec did not lose one pneumonia case. In 1825, out of 28 pneumonia cases, simple or double, with or without pleurisy, one only died, and this an old man of 70. In 1825, out of 34 cases, 3 died; two females brought *in articulo mortis*, and an old man above 70. Altogether, Laennec concludes, that out of 57 pneumonias, he in reality lost but 2 patients [aged] above 70. Louis lost, out of 20 cases, 3 patients, all from 60 to 70. In 1831—2, Trousseau lost 2 patients out of 58. Some of these were bled in the hospital; but five had been so before admission. Since that period, owing to a change of type, he had not been so successful. Professor Bang, out of 54 cases of pneumonia treated at Copenhagen, lost only 2. These were generally bled at the onset. Dr. Wolff did not lose any out of 10 cases (Grisolle). Out of 75 cases treated by Bouillaud, recovery was observed as follows—55 cases of simple pneumonia, 2 died; 16 cases of double, 11 died. M. Husson, out of 43 cases, bleeding each patient from 1 to 11 times, lost only 3 cases. Grisolle collected 44, for the most part severe; in 2 cases the pneumonia was double; 6 only died. The fatal cases occurred among weakened individuals, who were generally above 50 years old. Were I to imitate in this compilation the homœopaths, I could conclude I had collected some 300 cases with a mortality of about 3 per cent." (p. 57.)

The homœopathic sect boasts its success in the treatment of FEVER as a most valid argument in favour of its doctrines and methods of cure; and in support of its boast, wearisome columns of statistics are produced, into the authenticity of which it is really a most useless labour to inquire. But to show what unassisted nature will do in fever, in spite of the most defective hygiene, and of the total neglect of anything like the ordinary attentions shown to the sick in hospitals, we quote the following extract from an article in the first volume of this journal, in which we treated of the epidemic fever of 1847. In reference to this malady we observed,

"At its outbreak, there was frequently no treatment at all—absolutely none; this was in consequence of the suddenness and the extent of its prevalence. . . . . Four or five fever patients might often be seen stretched on heaps of straw placed on the damp brick floor, in apartments under ground, so close and ill-ventilated, that neither air nor solar light could approach the wretched inmates in any proportion whatever to the requirements of the human organism." (p. 303.)

Such was the hygienic condition of our "allopathic" patients. What was the treatment?

"It was truly remarkable how small a proportion of patients so distressingly situated actually died. . . . . Having found, upon a moderately large scale, that patients labouring under the epidemic fever would, in a great majority of cases, recover *without any treatment*, positive or negative . . . . we discountenanced all active or powerful interference, ... being content, in the bulk of instances, simply to withdraw *lædentia*, where we could detect no satisfactory clue to positive *adjuvantia*. Keeping patients in bed from the beginning, and insisting that they remained there until the establishment of convalescence, exclusion of injurious stimuli, cleanliness, and free ventilation, with farinaceous and diluent drinks

sufficed in most cases to bring them successfully through the disease, without resort to active measures at all."*

Surely globulism cannot claim the merit of cure here!

We need hardly refer to the homœopathic statistics of the treatment of Cholera by globulism. Dr. Routh quotes a table from one of their writers, in which the mortality, under homœopathic treatment, is put down at 4·4 per cent., under ordinary treatment at 49·3 per cent. This, as Dr. Routh observes, carries upon the face of it the stamp of misrepresentation; so that we need not weary our readers with the effective analysis, which Dr. Routh gives, of the real value of these absurd figures.

Our analysis of homœopathic statistics would not be complete, if we limited it to the returns from homœopathic hospitals, and omitted the more numerous and more interesting returns from individuals. Homœopathic literature abounds with these, for the sufficient reason that they are intended for popular circulation; and while they are always most unanimously in favour of the sect and its practices, they also help, by their influence on the public mind, the individual homœopath, and this is an immense advantage. It is somewhat embarrassing to make a selection from the choice examples of "statistics" from homœopathic private practice, but we think the report on "The Cholera at Huddersfield," by a Mr. Brady, is, upon the whole, the choicest. It appeared in 'The Homœopathic Times' for December 1850. Cholera, like all pestilential diseases, is dear to the quack; and we have therefore numerous statistical details, in the homœopathic journals, of its successful treatment homœopathically. We are inclined to think that no report is so decisive as Mr. Brady's homœopathic. Dr. Atkin, of Hull, has the following notice of the results of his treatment in 'The British Journal of Homœopathy:' "Of 77 cases treated, there were 57 recoveries, and 20 deaths. Mortality, 25 $\frac{75}{77}$ per cent." This is modest; because, in the first place, rational treatment presents occasionally quite as favourable results, while the average mortality in a series of years does not exceed 38 per cent.; and because, in the second place, the "cooked" statistics of the Vienna homœopathic hospital make it so low as 5·3 per cent. Mr. Brady, however, is not content with this homœopathic cookery; for we find the following analysis of the cholera cases treated homœopathically in Huddersfield: "Cases treated by Brady and Ramsbotham 8; recoveries 8; deaths *none!*"

Ramsbotham infallibly cures hydrophobia; Brady and Ramsbotham infallibly cure cholera. These two men must be a greater blessing to Huddersfield, than bushels of Parr's or Morrison's pills. We question, indeed, whether the feats of Coffinism, or brandy-and-saltism, or mesmerism, should be mentioned on the same page with those of Ramsbotham and Brady, for it does not appear that *any* of their patients die. Mr. Brady, after triumphantly relating six cases of cholera cured, and calmly observing, "they must be regarded as highly satisfactory, as evidencing the value of homœopathic treatment in this disease,"† adds, "Many cases of choleraic diarrhœa and English cholera came under our treatment at the

* Vol. i. of this journal, p. 303. Dr. Routh quotes this article, and by interpolating the words "in Ireland," places the locality of our practice "in that country;" which is nowhere stated, and which was not, in fact, the sphere of our labours. Dr. Routh, in other sentences, also fails to quote us literally. He also designates our journal, "Dr. Forbes's Review," which is a complete misnomer.

† Homœopathic Times, *loc. cit.* p. 310.

same time, and fully established the pre-eminence of homœopathy in such affections, all having been speedily cured." So that not a single case proved fatal—*not one*—of the MANY attacked. Death was scared away by the advent of Messrs. Ramsbotham and Brady;—Death his dart shook, it is true, in their presence, but dared not strike.

Such statements as to the results of medical treatment carry with them their own confutation, and as contributions to medical science or art, are really beneath contempt; the slightest possible notice of them or their authors is far more than they deserve. But when, in addition to these outrageous boasts, Mr. Brady adds adverse statements respecting the modes of treatment followed by others, and their results, as far removed from truth in the opposite sense, there is an element added, which cannot be passed without notice. It is well known that the effect of medical treatment and the recovery from cholera are much influenced by various complicating circumstances. Dr. Routh amply demonstrates statistically, what is obvious to all who have had any experience of the epidemic, that cholera is not an exception from the general principle, that the mortality of an epidemic is less (after a certain period) in proportion to its duration. Thus, at the commencement, more than one-half of those attacked with cholera die, but at the close, not one-fourth. In Glasgow, for example, the deaths were in the second week 60·5 per cent.; towards the close of the epidemic 22·2 per cent. Mr. Brady, who pithily observes, that "if statistics afford valuable aid in the advancement of science, they are no less useful to those who wilfully essay to pervert facts," publishes in 'The Homœopathic Times' a table of so-called "Allopathic" cases, in which the deaths are 18, the recoveries 2, or, in other words, a mortality of 98 per cent.! The essay containing the allopathic mortality, and the homœopathic immortality, with the names of the respective fallible and infallible practitioners, was reprinted from 'The Homœopathic Times,' and circulated by hundreds amongst all classes of the population in Huddersfield, with comments admirably calculated to deceive the public. The following is an example of the kind of thing Mr. Brady thinks it right thus to circulate:

"I have now to relate an incident which occurred on the occasion of my afternoon visit to my patient. Mr. Allatt, a surgeon, who with Dr. Taylor and Mr. Clark have had the not very enviable honour of having attended the patients before-named, as having been under allopathic treatment (which treatment presented such unheard-of per-centage of deaths), during my visit to my patient, entered the room, and stated that he had been invited to see the patient, Aaron Taylor, in the interval between my two last visits. . . . . On Mr. Allatt pointing to his beautiful yellow mixture, I inquired if he seriously expected to cure Asiatic cholera with *that?* He replied, very innocently, that he did, especially when combined with Dr. Ayre's calomel treatment. I then inquired if he was not aware that such treatment was proved so unsatisfactory, as to have been exploded by almost every intelligent practitioner?"

And thus this Mr. Brady goes on in the true literary style of the genus quack. But that our readers may thoroughly understand this most flagrant departure from truth and the commonest principles of sound morals, we extract Mr. Brady's table at length, names, residence, and all.

"Subjoined, I append a table of cases of epidemic cholera, treated in Huddersfield and its vicinity. So far as I could gain information on the subject, I pledge myself for the accuracy of my report. The deaths were copied from the books of the registrar in Huddersfield.

TABLE OF CHOLERA CASES TREATED IN HUDDERSFIELD AND ITS VICINITY, WITH RESULTS.

NAME.	AGE.	RESIDENCE.	TREATMENT.	RESULTS.
William Moss . .	23	Paddock Head	Allopathic . .	Died.
Hannah Taylor . .	55	Ditto . . .	Ditto . . .	Ditto.
Sarah Tomlinson .	78	Ditto . . .	Ditto . . .	Ditto.
A. Micklethwaite .	17	Ditto . . .	Ditto . . .	Ditto.
Sarah A. Taylor .	24	Ditto . . .	Ditto . . .	Do. (pregnant).
Thomas Dyson . .	1 1/3	Ditto . . .	Ditto . . .	Ditto.
Jane Dyson . . .	4 3/4	Ditto . . .	Ditto . . .	Ditto.
J. Micklethwaite .	44	Ditto . . .	Ditto . . .	Ditto.
John Alpin . . .	—	Ditto . . .	Ditto . . .	Ditto.
Charles Potter . .	40	Ditto . . .	Ditto . . .	Ditto.
Jas. Micklethwaite	—	Ditto . . .	Ditto . . .	Recovered.
Mrs. Micklethwaite	—	Ditto . . .	Ditto . . .	Do. (supposed cholera).
William Leonard .	38	Quay-street .	Ditto . . .	Died.
James Atkinson .	49	Fountain-st. .	Ditto . . .	Ditto.
Elizabeth Wilson .	50	Lane . . .	Ditto . . .	Ditto.
George Jagger . .	4 1/2	Ditto . . .	Ditto . . .	Ditto.
Benjamin Berry .	66	Folly Hall .	Ditto . . .	Ditto.
Mary Goodyear. .	39	Cropper's-row	Ditto . . .	Ditto.
Abraham Smith .	39	Ditto . . .	Ditto . . .	Ditto.
Eli Wilson . . .	58	Lane . . .	Ditto . . .	Ditto.
Mrs. Greenhough .	40	Paddock Head	Homœopathic	Cured.
John Briggs. . .	50	Ditto . . .	Ditto . . .	Ditto.
John Briggs' child .	7	Ditto . . .	Ditto . . .	Ditto.
Thomas Cliffe . .	40	Ditto . . .	Ditto . . .	Ditto.
Mrs. Richardson .	35	Marsh . . .	Ditto . . .	Ditto.
James France . .	17	Ditto . . .	Ditto . . .	Ditto.
Mr. Beavers . .	35	Sheepridge .	Ditto . . .	Ditto.
Mrs. F——tt . .	60	Clare Hill . .	Ditto . . .	Ditto.

Results: number of cases treated allopathically 20; recoveries 2; deaths 18. Number of cases treated homœopathically 8; recoveries 8; deaths 0."

Will our readers credit us when we assure them that this table is a lie—ingenious, it is true—but a lie from beginning to end? Yet it is so. Few literary members of the profession are ignorant of the zeal, industry, and truthfulness of Dr. John Taylor, formerly professor of clinical medicine in University College, and now a physician in Huddersfield. Dr. Taylor has published a report on the cholera, which is characterized in the Report on Cholera in England lately issued by the registrar-general as "a model which the local inquirer should consult." To secure as complete accuracy as possible, he obtained personally the particulars of the cases from the families of the sufferers and the practitioners in attendance; they amounted in all to 93, of which 39, or 40 per cent., were fatal. Dr. Taylor drew up a table of the cases and their termination; and on reference to it he finds, that in or near to the places named in Mr. Brady's table, there were FORTY-SIX RECOVERIES. Mr. Brady pledges himself (what the pledge is worth may be inferred from that damning fact)—Mr. Brady "pledges" himself for the accuracy of his report; yet of forty-six recoveries he could only hear of TWO!

Was Mr. Brady blind and deaf when he inquired as to the fate of John Goodyear the husband, and John Goodyear the son, of Mary Goodyear, whose death he chronicles from "allopathic" treatment? Could he possibly remain in ignorance, that these two persons were attacked on the day following that on which Mary Goodyear was attacked, and that they both recovered? How did it happen he could state that Mary Goodyear died, but could not say, also, that her husband and son recovered? And these are the cholera statistics of homœopathy! Perhaps the reader may think this *suppressio veri* a solitary instance, and possibly an error. It is neither the one nor the other. The reader will observe, that Mr. Brady mentions 12 cases of cholera as occurring under "Allopathic" treatment at Paddock Head, of which, 2 are reported recoveries, although, with a cunning pretence to great accuracy, he adds the remark, "supposed cholera," to one of the recoveries, positively making the mortality 99 per cent. Yet, in reality, there were 25 cases in that locality, treated by three or four practitioners and by Dr. Taylor himself; the deaths were 11, the recoveries 14, several of the latter being in the same houses as those cases of which Mr. Brady recorded the fatal result!

Monstrous as the crime of falsification of recorded facts must appear to the right-minded practitioner, and despicable as that falsification must appear to the man of honour when used for the purpose of vilifying a professional brother by circulating it amongst the general public, Mr. Brady descends even into a lower deep, being apparently anxious to exhibit in his own person the capabilities of homœopathic frauds and falsehoods. We extract the following passage from Mr. Brady's paper, with profound shame that any member of the profession could publish so mean and cowardly an inuendo (as we must consider it to be) against the medical officers of the Cholera Hospital at Huddersfield. The italics are not ours, and we can only interpret the passage, after carefully considering it, into an insinuation of drunkenness, if not of peculation, against the *medical "friends"* of the nurses.

"I may here add that a temporary cholera hospital was built at Paddock, by order of the board of guardians, but was not completed until the last wave of the ebbing tide of cholera had receded; so that, I believe, the only cases of diseases ever treated in it, were two of simple fever. The Allopathic medical men, ordered for the use of the nurses, and I suppose, by way of a prophylactic (!!!) a barrel of beer; and laid injunctions on them to 'drink plentifully of it, and keep themselves in good heart.' These directions they most religiously observed; for while the beer lasted, the nurses and their *friends* were in a state of uninterrupted intoxication. In truth, every member of the Allopathic faculty, from the medical men down to the nurses, will have reason to remember the liberality of the board of guardians of the Huddersfield Union."

The historian of the sea-serpent, we are satisfied, is as worthy of belief as the writer of this detestable paragraph. The entire scope and wording of it displays a malignant ingenuity in composition. The italicized term, *friends*, may or may not refer to the "Allopathic medical men" who supplied the nurses with the prophylactic; but there can be no doubt of the gross imputation upon their integrity and judgment in the assertion as to its employment; for it could only be with the sanction or connivance of the medical staff, that the nurses "were in a state of uninterrupted intoxication." The designation of the nurses, too, as "members of the allopathic faculty," is a choice illustration of the insufferable impudence and

vulgarity of the homœopaths. Yet the man who writes this ribaldry can whine most innocently, because he is kicked out of professional society.

It appears, then, that in whatever way we view the proceedings of this new medical sect, we come to the conclusion, that there is a large amount of knavery practised by its members. As to their folly, it is patent to the world; for their expressed belief is the perfection of foolishness. The rational writers before us have amply illustrated the monstrous absurdities implied in the adoption of the doctrine of infinitesimal doses and "dynamical" medicine. We shall not, however, cull from their pages any illustrations, but rather turn to the authorized writings and to the records of the sect itself. Dr. Madden, we hope, when he finally returns to the corps of rational practitioners (and he is evidently on the road), will entertain us with the doings of the *gobemouches*, who gaped in wonderment at the vagaries of the "magnetoscope," invented by a Mr. Rutter, by which wonderful instrument an alliance was to be established between mesmerism and homœopathy, and the "dynamical" powers of a decillionth of a grain of chalk rendered visible to the naked eye. None is better able to describe the energy and enthusiasm of his victims—none their blank dismay at his frank confession, "that the motions of the instrument were the result of every slight motion of the operator's hands!"

Our readers may not know that there is in the lowest depth of homœopathy a lower deep, termed *isopathy*, of the true character of which the princes of homœopathy differ much in opinion. It was discovered by a veterinary surgeon, named Lux, and the formula of his therapeutic principle is *æqualia æqualibus curantur*. This Lux (*Lux a non lucendo*) counsels all contagions, as sheep-pox, cow-pox, grease, itch, syphilitic poison, &c., to be "potentized," and to be administered as remedies for those diseases. He adduced as proofs of his views the cure of diseases caused by sulphur, mercury, and bark, by those substances. This doctrine Hahnemann did not deny, but considered it to be rather an encroachment on his domain; and he termed it, treatment by *simillima* and not *æqualia*. But we will let the 'British Journal of Homœopathy' speak as to the views of homœopathists on "isopathy."

"Stapf, while admitting the facts of isopathy, alleges, that they do not speak for *æquale* but *simillimum;* he thinks homœopathy is thereby enriched. He does not very well like the application of the process to other morbid products than those of contagious diseases, and considers we should only use the morbid product for the patient from whom it was taken, and hence such products should not be kept as medicinal preparations. Hahnemann is very cautious on the subject; he will not altogether deny the power of isopathic preparations—especially psorine ['potentized' itch-matter]—but will not admit the practice to be anything but a kind of homœopathy. Helbig rejects isopathy *in toto*. Rau dislikes it, but will not deny its efficacy altogether, at least in contagious diseases. Thorer sees, in the isopathic preparations only *simillima* not *æqualia*, limits its employment to contagious diseases," &c.*

The names of half-approvers, like those just mentioned, follow in lengthened file, and are all, doubtless, infinitesimal sages, or their opinions would not be recorded. We might quote Dufresne, Müller, Kammerer, Veith, Kurtz, Buchner, &c. The author of the paper himself seems to agree with Hahnemann in attributing efficacy to psorine (scrapings

* British Journal of Homœopathy, No. 29, p. 338.

of itch-patients), and to that extent in sanctioning the principles of these isopathists. Let that same author describe their nasty doings.

"The person mentioned above as having potentized his blood, did the same by his tears, and let his son smell the preparation, who forthwith was affected with pain in his lachrymal gland. The evacuations, both upwards and downwards, of cholera patients have been 'potentized.' To get scarlatinine, milk-sugar was tied on to the body, and to obtain morbilline, measly patients held globules in their hands. According to Attomyr, 'potentized' itch-matter (psorine) developed lice in the head of a healthy person who proved it. Teeth affected with caries, and the matter from fistulous ulcers, were potentized, and termed *caries dentium* and fistuline. Other curious matters that have been 'potentized' by our isopathists are, ascarides, lumbrici, tape-worms, the water from dropsical swellings and hydroceles, the expectoration of phthisical patients," &c.*

This "potentizing" is as absurd a thing as the materials used by the isopathists, and is a process purely Hahnemannic. It is founded on the dynamization theory of the founder of homœopathy, who asserted that by merely carrying about the medicines in a fluid state we "potentize" them. He says, that from continued succussion and trituration of very diluted medicines, "a great, hitherto unknown, undreamt-of, change—the setting free and developing the dynamic powers of a medicinal substance so prepared, resulted." The dynamization will be altered by shaking the bottle in which the medicine is contained, six or eight times before giving the patient each successive dose. The writer in the 'British Journal of Homœopathy' amply shows that Hahnemann was an arrant dunce or impostor, with his succussions and dynamizations—continually contradicting himself, and retracting in one page what he had said on the preceding! Nevertheless, the believers in his self-evident absurdities were and are numerous; and some of them, as Weber, Mure, and Müller, invented complicated machines for perfecting the dynamization. One simpleton pretended that he infected an unlimited number of unmedicated globules with medicinal power by one dry medicated globule; and another "communicated 'blood-power' to thousands of globules by shaking them up with one dry globule of a dilution of his own blood, and with these he effected wonders in congestion."—"Whilst Hahnemann warned against giving drossera in hooping-cough that had been prepared with twenty shakes" (we still quote from the organ of the sect), Jenichen's preparation of that drug in the 500th dilution has been shaken 6000 times, in the 5000th dilution 60,000 times!" The 'Sketch of the Progressive Development of the Homœopathic System,' published in the Journal from which we have quoted, is perhaps the most vivid picture of the absurdities, contrarieties, contradictions, bewilderments, hallucinations, and (we ask pardon for using the word) tom-fooleries, into which Hahnemann and his followers have fallen, that can be desired by the most earnest foe or insane admirer of infinitesimally mysterious mysticism.

But what are the results of these follies on humanity? Is it possible that they can be carried into practice without inflicting, positively or negatively, much suffering? The organ shall again speak by the pen of Dr. Madden:

"About a year ago, I was consulted by a lady who had been long an invalid, and had latterly been treated according to our system; her attendant, however, was

* British Journal of Homœopathy, No. 29, p. 338.

among the most rigid adherents to all the dogmas of our great master, and accordingly he allowed no other means beside an occasional lavement to be employed to overcome the great tendency to constipation under which she laboured. By degrees the enema lost its effect, and she was then directed to increase the quantity used, and the frequency of its employment. A number of pelvic symptoms which at this time manifested themselves, were attributed by her attendant to uterine congestion, and she continued under his care. When I first saw her, she was exceedingly weak, complained of constant dragging pains in the back, which prevented her taking almost any exercise, and the bowels never showed any symptoms of acting, unless she took two or three enemas consisting of a quart each. On examination, I found that though the uterus was slightly congested, it was by no means sufficiently so to account for her symptoms; whereas the rectum was enormously enlarged, extending entirely across the posterior wall of the pelvis, and being fully three times its natural size. That this most unpleasant morbid condition is traceable to the treatment, I think there can be little doubt."*

The next case of homœopathic *mala-praxis* recorded by Dr. Madden presented still more disastrous results.

"An elderly gentleman who had lived freely, and suffered much from gout, was induced to consult a homœopathist, on account of the shattered condition of his general health. As he had been transgressing the laws of health in many ways, the first effects of the treatment were decidedly beneficial, as his medical man enforced temperance and regularity. But after a time, the system began to succumb under the too rigid abstinence which had been recommended, and the bowels ceased to respond to the homœopathic remedies; and the medical man, fearing that one or two slight divergencies from the strictest homœopathic rules were the cause of this failure, urged a still closer adherence to every particular, and the result was, that after a few weeks of unsuccessful treatment, an attack of paraplegia ensued, which has proved incurable. . . . . I feel satisfied that the paraplegic attack might have been delayed, if not warded off, had the patient been put upon a more liberal diet, and occasional mild aperients administered when necessary."†

That is to say, if the so-called "allopathic" or rational method had been adopted. Really we think, in these cases, Dr. Madden illustrates a remark made by Gil Blas, when carrying out the practice of Dr. Sangrado, "Quelqu' excellente que soit la pratique d'un médicin, elle ne sauroit manquer de censeur." However, Dr. Madden is perfectly impartial; for he frankly details the history of a case which he treated homœopathically for six months, when the patient's health began to suffer so much that he advised his "patient to ask the opinion of an allopathic friend," who, after applying leeches a few times to the organ affected, and practising "a little manipulation," soon put the patient on the way to complete convalescence. Dr. Madden's paper is "On the Use of Auxiliaries to Homœopathic Treatment," and these cases are illustrations of the necessity of such auxiliaries to successful homœopathic practice. With admirable simplicity, Dr. Madden fully demonstrates "that the occasional use of auxiliaries does not, of necessity, destroy the good effects of *pure* [!] homœopathic treatment!"

So much for the most curious system of quackery modern times has seen, which is more adapted to the era of declining Greek and Roman civilization, than to the culmination of the European. If it were not that conflict with error is the imperative duty of every man who values truth and common sense, we would leave this system to its inevitable fate. Quetelet has

* Dr. Madden, in British Journal of Homœopathy, No. 23, p. 315.
† Ibid., op. citato.

shown with what constancy circumstances occur in the same ratio year after year—nay, how even the same proportion of letters are yearly dropped into the post-office undirected; so it is with quackery. A certain proportion of weak people are inevitably preyed upon by the cunning, and a certain proportion led by the fanatical and enthusiastic, to the loss of health, station, fortune, life. Homœopathy and Mormonism, Mesmerism and Socialism, Coffinism, Perkinism, and Millerism, Chronothermalism, Hydropathy, Kinesipathy, Isopathy, and a variety of minor manifestations in religion and politics of this cachectic condition of society, are singularly suggestive of the extent to which the evil has grown, and to which the well-being of society is really endangered. If all the methodical madness now spread through a hundred channels were concentrated by some hardy impostor on one point of our social system, its buttresses would be endangered. Perhaps the remedy for this state of things is, after all, war. But the calm observer of passing events cannot fail to notice how every branch of human knowledge is pestered with fools and knaves to such an extent, that it appears as if a moral leprosy had infected society, and the love of honest, manly, vigorous TRUTH was in danger of extinction. We are satisfied, however, that the profession of medicine will protect its noble art and science from debasement, and will only derive fresh energy and power from its conflict with false philosophy and an untiring duplicity.

In noticing the occasion of Dr. J. Y. Simpson's discourse, and the steps that have been taken by the Edinburgh medical authorities, we cannot but congratulate the profession that the call which we made upon the medical corporations two years ago, has been so soon responded to; and we think we may do service by once more expressing our sentiments:

"The medical corporations have a duty to perform in this matter. It is a new thing to find men seeking the diplomas and licences of public bodies, that they may the more effectually injure the established system of medicine; it is a new thing, we say, to find individuals who have studied medical science, and been cherished by a medical *alma mater*, expressly claiming the ear of the public in virtue of the licences or diplomas they have obtained, that they may attack, and—if such an impossibility were possible—destroy the source of their knowledge and authority. It is a new thing to find such men banded together for the purpose of disseminating as widely as possible detestable sentiments regarding the orthodox system of medicine, like that which Hahnemann penned, and which we have quoted." *

The active steps taken by the Provincial Medical and Surgical Association deserve the thanks, as well as the support, of the profession. We hope the day is not distant when a more comprehensive and more definite code of ethics will be put in operation by the governing bodies of the medical polity; and the quacks of the profession placed in the ranks of those men with whom they have really feelings in common.

In expressing our thanks to Dr. Routh for his able statistical details, we would, in the most friendly spirit, caution him against a slovenly and incorrect style of composition. He is evidently an inexperienced writer, and the hint may be of use. We think, too, it is eminently injudicious to adopt the impertinent and false designation by which homœopathic writers characterize the rational and orthodox system of medicine. It is a term invented by Hahnemann, with that skill in fence for which he is remarkable,

* British and Foreign Medico-Chirurgical Review, vol. v. p. 309.

and it is only serving the purpose of the enemy to accept and use it. The practitioners of rational medicine should sedulously, and on all occasions, repudiate the term "Allopath," as applied to themselves, and "Allopathy" as applied to their science and art.

ART. XI.

1. *On the Physical Diagnosis of the Diseases of the Lungs and Heart.* By HERBERT DAVIES, M.D., Fellow of the Royal College of Physicians, Assistant Physician to and Lecturer upon Materia Medica at the London Hospital, &c.—*London*, 1851. Pp. 288.

2. *Abhandlung über Perkussion und Auskultation.* Von Dr. JOSEPH SKODA. Vierte Auflage.—*Wien*, 1850. Pp. 324.

Treatise on Percussion and Auscultation. By Dr. JOSEPH SKODA.

3. *Theorie und Methodik der Physikalischen Untersuchungsmethode der Athmungs und Kreislaufs Organe.* Von Dr. GEORGE WEBER, Praktischem Aerte in Kiel.—*Nordhausen*, 1849. Pp. 128.

Theory and Practice of the Physical Methods of the Examination of the Respiratory and Circulating Organs. By Dr. GEORGE WEBER.

4. *Neue Forschungen über die Schallerzeugung in den Kreislaufs Organen.* Von Dr. KIWISCH, von Rottereau, ('Verhandlungen der Phys. Med. Gesellschaft in Würzburg.') Erster Band, 1850, p. 6.

New Inquiries into the Production of the Sounds arising in the Organs of Circulation. By Dr. KIWISCH. ('Transactions of the Phys. Medical Society of Würzburg.')

5. *Die Discussion über die Gefassgeräusche.* ('Verhandlungen der Phys. Med. Ges. in Würzburg.') Erster Band, p. 75.

The Discussion upon Sounds arising in Vessels. (Op. cit.)

6. *Beiträge zur Diagnostik der Klappen-affectionen des Herzens.* Von Dr. GEORGE RAPP. (HENLE'S 'Zeitschrift,' Vol. viii. p. 147.)

Contributions to the Diagnosis of the Valvular Diseases of the Heart. By Dr. GEORGE RAPP.

7. *Traité Pratique d'Auscultation, &c.* Third Edition. Par MM. BARTH et ROGER.—*Paris*, 1850. Pp. 706.

So vast was the stride, out of deepest obscurity into comparative daylight, which was made by the immortal discoverer of auscultation, that it at first seemed as if he had not only commenced, but had completed, the investigation with which his name is inseparably blended. For a long time, to follow, was apparently to repeat Laennec; the originality of the leader seemed to reduce the disciples to copyists; the mine, no sooner discovered, appeared exhausted, and it was even supposed that the genius of a single individual had reached the height to which any human intellect could mount.

There was nothing surprising in this belief, when the extraordinary extent and general accuracy of Laennec's observations are considered. In

the history of medicine, no other instance can be found of a man both forming a great conception, and working it out into its minutest details with such wonderful success. We must look to other sciences, to Davy moulding chemistry into form, or to Faraday calling a crowd of sciences into light, if we would find a parallel to Laennec. It was no wonder, indeed, that his authority was, for a time, considered paramount, and that his statements were held as absolute and infallible.

Since then we have seen another illustration of the ever-recurring lesson, that the intellect of man cannot grasp fully a single point of any subject, but as he pursues it finds it stretch out into never-ending distance, and perceives at last that his exertions and his progress must be infinite. In a few years after the death of Laennec, we find that the promised land, which he was fortunate enough not only to gaze over but to enter, has yet to be explored; that even some of its grander features are not yet fully known, and that its deeper recesses will demand the labour of generations before the light of day shall fully reveal their proportions and their forms.

Among the chief of those who have most assiduously followed the path opened by Laennec, and who have added most largely to the truths he announced, and have corrected most carefully the mistakes into which he necessarily fell, must certainly be placed the members of the great German school of Auscultation, of which Skoda is the acknowledged head. In this country it was, for some years, the fashion somewhat to decry the labours of Skoda, and to throw aside the statements of German writers as if some national inaccuracy appertained to all of them. Of late years, however, as auscultation has been more carefully studied, and its difficulties better appreciated, the opinions of Skoda and his compatriots have been received with greater favour; and they are now beginning to exert an extensive influence in this country, and to modify considerably the stereotyped creeds which, with some important exceptions, have been professed by English auscultators.

The work of Dr. Herbert Davies, which we place at the head of this article, may be said to be Skoda in an English dress, with additions and comments by one who is evidently an excellent auscultator. It is a clear and readable book, very carefully written, and, we should hope, likely to be extensively read. There are several points which have been worked out by Dr. Davies with much ability, and which through him reflect credit on the English school of auscultation generally.

A great portion of the work consists necessarily of elementary matter, to which it would be a work of supererogation to refer. But there are many parts of it which are of great interest, and some of these will amply repay consideration. A systematic analysis of the book being unnecessary, we shall commence with the discussion of those points which have most attracted our attention.

In speaking of Percussion, we observe that Dr. Davies draws no distinction between a tubular or amphoric and a tympanitic note, although the two things are, in well-marked cases, very distinct, and often point unequivocally to different conditions. Thus, the tubular and amphoric notes are almost always attended in their production with an increased sense of resistance to the percussing finger, whereas the reverse is the

case with the tympanitic note. And whereas the tubular implies a sound transmitted from a somewhat limited space with rigid walls, such as the trachea, a large bronchus, or a cavity presenting, as nearly as possible, equal physical conditions, the tympanitic note occurs in many other, and in widely different circumstances. Thus, to take only one illustration: a tubular note at an apex, if it does not arise from a cavity, denotes the transmission of a tracheal or bronchial note through condensed lung, and is sometimes a valuable sign of such condensation. A tympanitic note, on the other hand, in the same situation, would denote either pneumothorax, or, more rarely, local emphysema, or an empty cavity with thin and flaccid walls, or, as we shall observe immediately, condensation and impermeability of the lower part of the same lung. The separation of these two sounds is, then, not an empty refinement, but a real and useful distinction.

In speaking of the tympanitic note (which of course with him includes the tubular) Dr. Davies refers to the fact, that in pneumothorax it is sometimes absent, or ceases after having been present. He adopts the now generally received opinion of Skoda, that extreme tension of membranous walls abolishes the tympanitic character. He then proceeds—

"Whatever the rationale of the sound may be, the true tympanitic tone is the usual indication of the presence of air in the cavity of the pleura. This sound is also stated to occur independently of pneumothorax in two other conditions of the lung.* First, in the emphysematous portions which frequently surround hepatized lung parenchyma; and, secondly, when the lung is gradually recovering from the compression of fluid previously effused into the pleural space. The explanation of the latter case is not very evident, but having several times observed it, I have no doubt of its truth; my attention was first drawn to it by Professor Skoda during my attendance at his cliniques in the hospital at Vienna." (p. 46.)

As Dr. Davies does not formally distinguish between the tympanitic and the amphoric or tubular percussion sounds, we are uncertain to which sound this last observation refers. A hollow note, which has usually been termed amphoric, has been observed by Williams, Walshe, and others, under the circumstances detailed—viz., during the absorption of pleuritic effusion; but we should doubt whether this ever has the true tympanitic character, unless Dr. Davies expressly means to affirm that it has, of which we are not certain. But there are several other cases in which a true tympanitic note does occur, besides those mentioned by Dr. Davies, and one of the most striking of these has been also described by Skoda. When there is extensive pleuritic effusion, so that the lower part of a lung is completely compressed, and the upper part, still being air-holding, is somewhat reduced in volume, and is in contact with the thoracic walls, a perfectly tympanitic note is given over this upper portion of the lung. This observation we have repeated many times, and are satisfied with its correctness. So clear is the sound, that it is at first difficult to suppose that air has not been generated in some way and collected in the upper part of the chest; but the comparative frequency of occurrence of the sound, and the evident rarity of such a spontaneous generation of air, render it much more likely that the clear note is given by

* The tympanitic note occurs also in partial emphysema of the lung, as stated by Dr. Davies himself, at a subsequent page. In general emphysema the note is clear and full, but not necessarily, not even perhaps generally, tympanitic.

the lung itself.* In other cases, again, an analogous condition is said to give a tubular note, which, however, we suspect to be more uncommon than the tympanitic note.

The occasional occurrence of a tympanitic note over hepatized lung has been observed in this country by Graves and Walshe, as well as by Skoda. Dr. Walshe† refers this sound to the generation of air in the cavity of the pleura; but the position of the sound, and the apparent absence of pleura-disease in some cases, seem opposed to this explanation as universally true.

Another point of interest connected with the tympanitic note is its occasional existence, at least to a certain extent, in chests in which the ribs and cartilages are unusually elastic, as in some children. In such a case, considerable solid infiltration does not much alter it; and, if the infiltration be in scattered granules, as sometimes in tubercle, does not change it at all.

On the interesting subject of Bronchophony, Dr. Davies is very full and clear, and we avail ourselves of the opportunity to enter rather more deeply into the subject than our space allowed us to do in our review of Dr. Walshe's book.‡ Dr. Davies, as a disciple of Skoda, adopts of course the theory of consonance, as advocated by that expert stethoscopist. On this point we agree with him to the fullest extent. Consonance must be as true under the conditions of the production of sound which are present in the chest, as it is under the same conditions external to the chest. We do not perceive how consonance, if true out of the body, can be untrue within it. Dr. Davies gives a quotation, which expresses very pithily the conditions and the effects of consonance :—

"From an extensive series of experiments, Mr. Wheatstone (Quarterly Journal, vol. iii.) has deduced the law, that a column of air may vibrate with consonance when the number of its vibrations is any multiple of those of the original sounding body. Thus when a tube closed at one end was furnished with a moveable piston, Mr. Wheatstone found that the tone of a tuning-fork was reciprocated by a column of air six inches in length. The column was then diminished to three inches, and the octave of the original sound was produced. By using forks of lower tone and very small tubes, and adjusting the length of the column of air, he obtained the octave, twelfth, double octave, and other concords of the original sound. The same experimentalist found that one column of air may be made to consonate with another; thus let two flutes which are in unison be placed near each other, let a certain note be produced on one flute, and the *intensity of the sound will be increased by the consonating vibrations of the air contained in the second flute* in a proportion dependent upon the distance between the two instruments." ('Philosophy of Sound,' by Higgins.) (Davies, p. 85.)

Precisely in the same way as the sound of one flute is intensified by the proximity of another, may the sound produced at the chordæ vocales (and which is augmented by consonance in the larynx) be intensified by

* This clear note may occur in two several periods of pleuritic effusion, as noticed by Skoda; and the observation, although minute, is very interesting. The tympanitic note may be found when the effusion is considerable, and compresses perfectly the lower lobes; then it is lost as the effusion increases and compresses the upper lobe; then, at an after period, it may return again at the apex, without actual diminution of the effusion. The explanation of this appears to be, that the diaphragm yields to such an extent as to lower the whole mass of fluid, and air then rushes into the least-compressed portions of the lung; or, as Skoda supposes, the lower lobe, under the influence of pressure and imperfect nutrition, may rapidly diminish in size, and then, if more fluid be not poured out, air is necessarily drawn into the upper lobe; or, on the other hand, the return of the tympanitic note at the apex may denote absorption of the effusion.

† Diseases of the Lungs and Heart, p. 64.

‡ See vol. viii. p. 285.

consonance in a greater or less degree at any point below the larynx, or above it, which presents the necessary conditions. In order that air may consonate, it must be enclosed in a circumscribed space; and besides air, those bodies only can consonate which readily produce independent vibrations, as tense cords, membranes, plates, &c. The intensity of the consonating sound depends on the form and size of the space, and on the condition of its enclosing walls. "It appears," says Skoda, "that the consonating sound of the enclosed air is so much the stronger, the more perfectly the enclosing walls reflect the sound." And illustrating this, among other ways, by the condition of the trachea and bronchial tubes, he thus writes :—

"The air contained in the trachea and in the bronchi can equally consonate with the voice, when their enclosing walls have, in respect of the capacity of reflecting sound, a similar or analogous property, as the walls of the larynx, the mouth, and the nasal cavities. In the trachea, whose walls consist of cartilaginous rings, the voice consonates almost as strongly as in the larynx. In not much lower grade than this, must consonance occur in the two bronchi into which the trachea passes. With the entrance into the parenchyma of the lungs, the bronchi are well known to have no longer closely contiguous cartilaginous rings, but the cartilages form irregular thin plates lying in a fibrous tissue. These plates become gradually smaller, thinner, and scarcer, as the bronchi divide, till at last the fine bronchi are merely thinly-coated channels. Therefore in the bronchi traversing the parenchyma of the lung, the voice consonates weaker than in the trachea, and the weakness is proportionate to the diminution and loss of the cartilages. The conditions under which the consonance of the air in the bronchi traversing the lung can be augmented are, that the walls of these bronchi must possess cartilaginous rings, or if they remain membranous they must be very thick, or they must be surrounded by condensed lung-tissue (in all these cases the walls reflect the sound more strongly than the membranous walls of the normal bronchus), and the air in these bronchi must be in communication with that in the larynx.

"When the air in a confined space is thrown into vibrations, the enclosing walls are frequently thrown into the same vibrations. The organ-pipes vibrate when the air in them sounds. The same thing is observed over the vocal tubes. The larynx vibrates with every sound, and its vibrations can be felt through fleshy layers of many inches thick. The walls of the bronchial tubes traversing the lung, when the voice consonates in their contained air, are equally thrown into vibrations, as those of the larynx; and these vibrations are transmitted through many inches of thick fleshy layers or strata of fluid to the thoracic walls." (pp. 39—41.)

The enclosed air does not consonate with every note, but, as Skoda observes, a body can consonate only with those notes which it is itself in a condition to give, or with aliquot parts of them*—that is, notes standing to them in harmonical relation.

Such is the theory of consonance, which has been almost universally adopted in Germany, and against which it appears difficult to bring a single valid argument. It may, indeed, happen that sounds within the chest follow different laws from sounds produced outside it, but if so, we should require a clear demonstration of so singular an exception. Some late English writers of eminence appear to have somewhat misunderstood Skoda's views, and to have attributed to him opinions which he appears to have carefully disclaimed. Thus Dr. Blakiston, in his very practical and useful work,† briefly alludes to consonance in these terms :

* Op. cit. p. 39.

† On Diseases of the Chest, p. 35.

"Dr. Skoda is of opinion, that vocal resonance at the surface of the chest results from consonance of the air in the bronchial tubes, rendered more adapted for such a purpose by solidification of the lung around them. He has thus confounded consonance with propagation of sound. According to this view, the air contained in the body of a violin consonates with every note of the gamut, rather than with one particular note. Waiving, however, this point, the exclusion of the sides of the bronchial tubes from any material share in the propagation of the sound of the voice cannot be maintained."

Dr. Walshe also remarks, in opposition to Skoda, that

"The theory of consonance does not seem to meet the difficulty, or rather, to go beyond it; for, in the first place, the air in any enclosed space does not consonate with every sound produced at its orifice, but only with the fundamental note of that space, or certain others having a fixed harmonical relation to that fundamental note. Now, when well-marked bronchophony exists, it is audible, as I have assured myself, with the various notes of the octave, though most with the low ones. Again, bodies consonate only in unison, or in certain fixed harmony, with the original sound. Now the pitch of the bronchophonic voice varies irregularly from that of the laryngeal, with which it co-exists. Further, it seems doubtful whether consonance could occur in a system of branching tubes, such as the bronchi; and, lastly, Dr. Skoda's exclusion of the tracheal and bronchial walls from all participation of the phenomenon is at variance both with theory and experiment."*

Dr. Blakiston must excuse us if we differ from him in his interpretation of Skoda's opinions. It is singular that Skoda should be accused of confounding consonance and conduction, when he was the first who distinctly separated the two phenomena; that the very fact he insists upon, of the consonating notes being in harmonical relation to the original note, should be advanced as an objection to his theory; and that it should be supposed that he excluded the enclosing walls from "any material share in the propagation of the sound," when he has expressly pointed the important part these play.

Dr. Walshe's objections are better founded, but do not appear to us conclusive. It may happen, that every or almost every note of an octave may consonate, although it is almost impossible that the degree of consonation should be equal in all. Dr. Walshe observes, in fact, that bronchophony is most audible with the low notes—a fact which is itself probably explicable only on the hypothesis of consonance; for if bronchophony were dependent merely on increased conducting power, there is no reason why the lower notes should be more audible than the higher. As to the pitch of the bronchophonic, varying irregularly from the laryngeal voice, this would, we admit, be an argument against consonance, were it certain that it is constant or even frequent; but we should like to see this point fully decided, before too much weight is given to it. In most cases of bronchophony, the different loudness of different notes is generally perceptible, and sometimes produces indistinctness of articulation; whereas if every syllable can be pronounced on the same note, articulation is distinct, for the degree of consonance of each note is equal. What, indeed, causes the extreme distinctness of what is termed "whispering pectoriloquy," except that generally the voice is kept at a single note, and therefore equally consonates?

If consonance be not admitted, how can intensification of sound within

* Op. cit., p. 129.

the chest be accounted for? Evidently not from increased conducting-power of the lung; for though this might, in rare cases, cause the voice to be almost as loud as over the larynx, it could never make it louder. Dr. Walshe has offered the ingenious explanation, that such intensification occurs from reflection of the waves of sound towards a focus. The same idea had been less carefully expressed by Blakiston, who speaks of sound increased by reverberation, and by Skoda and Weber. But Dr. Walshe has given this hypothesis a *locus standi*, and has, we think, made out a strong case for it. But we are not satisfied that it can be considered other than a secondary cause of intensification of sound, and we question if it can be applied to more than a limited number of cases.

Although we so completely agree with Skoda, as far as consonance is concerned, we yet fully admit, that we believe him to underrate the importance of the conducting-power of condensed lung. So differently do two hepatized lungs, apparently in the same physical condition, conduct sound, that Skoda appears to have distrusted any explanation based on the presumed existence of such a variable property. Dr. Davies, very properly we think, differs from Skoda, in attributing great influence to the degree in which sounds are conducted by diseased or healthy lung.

This discussion on consonance is not simply a barren or curious one, without practical fruits; had it been so, we should not have given to it so much space. But the great variations in the resonance of the voice, and the unexpected way in which other sounds, such as dry and moist rhonchi, occasionally present themselves at places distant from their origin, apparently from consonance, render it essential that an acknowledged physical law should receive its application in auscultation. An interesting example of consonating rhonchus is related by Dr. Davies, and illustrates well the importance of a knowledge of this subject. A medical student presented over the whole of the left lung a perfectly dull sound on percussion, and marked bronchial respiration and bronchophony. There was also present, over the entire of this side, a distinct subcrepitant râle,

> "So loud and distinct as to give the idea of the whole lung being riddled by small cavities. On post-mortem examination, it was found that two or three cavities of small size existed at the apex, while the remainder of the lung was converted into a solid mass of grey hepatization, no cavity, however small, being discoverable below the apex." (p. 136.)

In this case, the auscultation and percussion signs together, pointed to one of two conditions; either, as would have been the case in the vast majority of cases, to softening tubercle and fluid in the tubes, or, as actually was the case, to softening at the apex, and intensification of the moist rhonchus by consonance in the condensed remainder of the lung. Between the two states, auscultation *per se* could draw no distinction, except by finding manifest differences in the amount and quality of the rhonchus at the apex and base. The consideration of other signs, as those of percussion, of the amount of expectoration, or the progress and tendency of the subcrepitant râle to shade into other phenomena, would perhaps lead to an inference of the real condition. Other cases, apparently of the same kind, have come to our knowledge, in which good auscultators have predicted softening of tubercle, yet after death, nothing could be found at the spot to account for the moist rhonchus.

Dr. Davies adopts the nomenclature of Skoda, and discarding the distinction into bronchophony and pectoriloquy, speaks of strong and weak bronchophony. In our review of Dr. Walshe's excellent work, we expressed our opinions on this point, and need not now return to it; we wish, however, that Dr. Davies had retained Dr. Walshe's term of "pectoriloquous bronchophony," as it is a convenient phrase, with a definite meaning, and more correct, as well as more elegant, than the term "strong bronchophony.

As the intensity of vocal resonance is thus dependent to a great extent on consonance, so also the various forms of bronchial respiration, if this term may be used to include a great series of allied phenomena, are also produced in great measure by the same cause. When a portion of lung is solidified, the bronchi remaining open, the respiration resembles that heard over the large bronchi in the healthy chest, or is more intense than this, and passes into varieties which have been termed blowing, tracheal, and cavernous. Several explanations have been given of these sounds. Dr. Davies adopts the opinions of Skoda, and gives the following quotation from Zehetmayer as the exposition of his creed :—

"The vibrations which produce sounds in the larynx, trachea, and bronchi, are necessarily propagated to a column of air which stagnates in a bronchial tube surrounded by consolidated lung-parenchyma. This column is thrown into similar vibrations, the sound produced in the upper part of the air-passages being found to consonate in it with the same strength which it presents at its seat of origin. While now the respiratory sound is heard merely as a weak murmur in the neighbouring healthy parts (the normal lung-parenchyma being, on account of its spongy structure, a bad conductor of sound originating within it), the sound which extends from the larynx and trachea becoming concentrated in the blind-ending bronchus, is still further increased by reflection from the firm walls, and finding a good conductor in the solid lung-substance, reaches the ear of the auscultator with the same intensity as the sound at the larynx. Bronchial respiration is, therefore, the consonance of the respiratory sound of the larynx and trachea in the column of air contained in a bronchus which traverses a portion of consolidated lung parenchyma." (p. 116.)

Although this is probably true, we yet doubt whether it is not too absolute and partial. The bronchial respiration is thus partly a consonating tracheal or laryngeal sound; but is it not always also in part really bronchial, that is, a product in the affected bronchial tube itself? Over cavities, the entrance and exit of the air must produce, in some cases, sounds which are additional to that produced by the consonation of the laryngo-tracheal sound within the cavity, and by the vibrations which may be caused in it by the moving column of air. So also in the rigid bronchi; the air does not, for the most part, totally stagnate, but moves more or less readily, and must give rise to vibration of some part of the surface and of the projecting angles at the bifurcations.

Under the term bronchial respiration, Dr. Davies includes the "tubular breathing," "bronchial souffle, and blowing respiration," and as he holds the opinion that there is no positive auscultatory difference between the respiration of cavities and of bronchial tubes, he of course classes what used to be called cavernous respiration under the same head. "Bronchial respiration" is thus made to comprehend "every degree of intensity, from a weak, imperfect, and indeterminate tone, to a loud, distinct, and penetrating

murmur" (p. 113); in short every shade of bronchial and hollow respiration short of amphoric.

To this we make no objection, provided it be clearly understood that the term "bronchial breathing" is used in this comprehensive manner. This is the usual German acceptation of the term, and it has its advantages. But in England some writers restrict the term bronchial to a respiration like the normal bronchial, whereas Dr. Davies's highest grades present a much greater intensity than is ever heard over healthy bronchi. Dr. Walshe, whose book we consider one of the best ever published, uses the term "bronchial respiration" in the restricted sense, and employs the term "blowing respiration" for all the intenser grades, this blowing respiration being itself divided into three classes, simple (which includes "diffused and tubular blowing"), cavernous, and amphoric. Dr. Davies might thus use the term "bronchial," when Dr. Walshe would use "blowing," and would understand a different physical condition from that which could produce the "bronchial respiration." This is unfortunate, as it is absolutely necessary to use equivalent terms if observers are properly to understand each other's meaning.

With respect to the term "cavernous," Dr. Davies states, very correctly we believe, that—

> "The sound termed by Laennec cavernous respiration, cannot be always considered pathognomonic of the presence of a cavity, in consequence of the occasional difficulty, and even impossibility, of drawing an accurate and well-defined line of demarcation between it and an intense bronchial souffle; and the same remark can be equally applied to pectoriloquy and bronchophony." (p. 143.)

To this we may add, that we doubt whether any cavernous respiration not possessing a metallic character can be ever *pathognomonic* of a cavity. The mere *sound* cannot prove the cavity, but attendant circumstances may. Thus an "intense bronchial souffle" at an apex can hardly be produced by anything but a cavity, while the same sound in an interscapular region would not have the same signification. For in one case we know that the apex does not furnish the large tubes necessary for the production of such a sound, while the interscapular region does; and this, taken in connexion with the known tendency of tubercle to soften down, and with antecedent signs of softening, or with independent percussion signs (as the cracked metal sound) may prove the cavity, although the so-termed "cavernous respiration" *per se* could not do so.

We will now quit the first part of Dr. Davies's work, which treats of the physical diagnosis of the Lung, and pass to the second, in which the examination of the Heart is fully and carefully detailed. We shall here also select some special points for comment.

In reference to the exact relation of the orifices and cavities of the heart to the thoracic parietes, we may direct Dr. Davies's attention to two very good papers by Meyer, of Berlin,* who has lately examined this point, and seems to have certainly proved that we have placed the orifices a little too low, and too much to the right. The paper has appeared since Dr. Davies's lectures were delivered, but may be useful for the next edition.

The cause of the stroke of the heart against the side of the chest is a knotty point which is not likely soon to be solved. Without entering into

* Virchow's Archiv., vol. iii. pp. 265 and 399.

the question, we will allude merely to the two latest hypotheses, that of Kiwisch,* which is adopted by Weber, and that of Skoda.

In common with many pathologists, Kiwisch rejects the notion of an advance and recedence of the heart to and from the walls of the chest. The heart's stroke is not, he thinks, an approach of what was distant, but when the ventricles contract and become rounder, the swelling impinges upon the ribs, which of course cannot yield, and upon the intercostal spaces, which yield more or less, and by so doing gives rise to the impulse which is seen and felt. On the ribs themselves no shock can be felt; that the shock is felt only at the apex in one or two inter-spaces in healthy persons, results from the way in which the rest of the heart is covered by lung. To this doctrine, Valentin has made some well-founded objections.† The hypothesis adopted by Skoda, from Gutbrod,‡ is widely different, and has been much opposed; it is founded on the physical law, that fluid at rest in a vessel presses equally in all directions, but if allowed to flow out at an opening, the pressure is suddenly removed from this point; therefore, as the pressure on the point opposite to the opening still exists, and is no longer counterbalanced by the pressure which existed opposite to it before the opening occurred, the vessel will necessarily move in a direction exactly the reverse of that of the issuing fluid. When the aortic and pulmonary valves are pressed to the side of the vessel, and the heart suddenly drives out the blood at the arterial orifices, pressure is taken off at this point; but as it still exists at the apex, it tends to drive this part of the heart in a direction opposed to those of the issuing pulmonary and aortic streams. The beat of the heart is, therefore, a kind of recoil.

In addition to this, Skoda adduces other subsidiary causes, which we observe Dr. Davies has not mentioned—namely, after Gendrin, the pressure from above of the suddenly increased arterial columns, which tends to lower the apex, and the change in the form and in the position of the heart produced by the contraction.

We must satisfy ourselves with merely stating these views, which, although they have been promulgated for some years, have as yet made little way. We cannot now enter more fully into the question. After all, although we do not deny the utility, and admit at once the interest, of such physiological speculations, they are at present practically of little moment. As physicians, we are more concerned in determining the normal position of the apex and the normal force and rhythm of the impulse, and the diseased conditions with which changes in position, and in force or rhythm are coincident, than in debating on the causes of an obscure physiological action.

Another physiological question—namely, the causes of the sounds and murmurs in the heart and vessels, is, however, one of very great and practical interest; and has lately been especially treated of by Kiwisch, whose boldness, originality, and ingenuity, always command our attention, if they are not always able to gain our support. We need scarcely remark, that some time ago, Kiwisch§ stated, that the so-called placental bruit of the gravid uterus, did not arise in the uterus at all, but had its origin in the

* Prag. Vierteljahrschrift, 1846, p. 143. † Lehrbuch, 2te Aufl. p. 437.
‡ Op. cit., p. 142, *et seq.*
§ Beitrag zur Kenntniss der anatomischen Bechaffenheit der Placenta, &c. Prag., 1849

epigastric artery, and could be stopped by compression of the vessel. He has lately modified this view, and believes now that this murmur may also arise in the enlarged uterine arteries, though less frequently here than in the epigastrics.* Guided by this notion, Kiwisch proceeded to investigate the causes of sound in the heart and vessels generally, and employed the same methods of inquiry—namely, vivisections, experiments with tubes, with arteries, &c., removed from the body, as his predecessors in this path had already done. The following is a concise statement of his opinions:—

Vibrating bodies, according to the rapidity, equality, or special method of vibrating, give sounds of various quality, intensity, duration, &c. Liquids in movement, on the other hand, produce, *per se*, no sounds, as is proved by experiment. In the vessels, the moving blood, *per se*, gives rise to no sounds; but the sounds heard over them entirely result from the movements of the walls of the vessels, produced by the movement of the fluid. If the vessels contained air, the result would be very different, as air is especially fitted by its movements to produce sound. As the opinion that the movements of the particles of the blood, *inter se*, produce the sounds heard in vessels, appears to be erroneous, so also is the doctrine, that the qualitative alterations in the quantity of the blood produce any modification in the power of producing sound. It is a physical impossibility that the blood-corpuscles by any movement on each other can originate sound, and they cannot by any experiments be made to do so. In the circulating system, only certain elastic membranes can produce sound; and these membranes are, 1st, the coats of vessels; and 2nd, the valves distributed in various parts. These vibrations can give tone or murmurs (Töne on Geräusche) under two conditions; first, when, being in a certain state of tension, they are thrown into vibration by any external cause; or secondly, when, being relaxed, they are suddenly made tense. In this second method, the valves of the heart always give sound, and these sounds can pass into murmurs when the tension is unequal, and the vibrations become complicated; as, for example, in a valve permitting regurgitation, when in addition to the usual vibrations, others arise from the passage of the fluid over the free edges of the abnormal opening. In healthy vessels, sounds are not usually produced; but if a vessel is of unequal calibre, it is well known that vibrations in it and consequent sounds are produced, which, according to the rapidity of the vibrations, are either tones or murmurs. The explanation of this, according to Kiwisch, is the following: a stream of fluid passing through a tube, and assuming, of course, the shape of that tube, preserves temporarily its shape if it be driven with a certain force, when it passes into a wider part of the tube; the wider tube, therefore, if it be elastic, closes upon the column of fluid, then suffers expansion from the force of the current, then again contracts, and the succession of these movements, if it be rapid enough, produces sound. The kind of sound depends upon the thickness of the wall, and the width and form of the column, and is not produced at the narrow point, but in the wider part which succeeds it.

The causes of the heart's sounds are supposed to be as follows. The first or systolic sound arises solely from the extension of the auriculo-ventricular valves, which, as well as the points where they are inserted, are thrown into vibrations. The sound is in nowise derived from the mus-

* Verhandlungen der Phys.-Med. Gesellschaft in Wurzburg, Bd. i. p. 186.

cular contraction; Kiwisch supposes that the so-called muscular sound is an acoustic delusion, and arises from vibrations of air in the stethoscope and the meatus; if this error be avoided, no sound is heard during muscular contraction, as may be proved easily by listening over an uterus contracting in labour. The heart's stroke, also, is admitted by most writers to take no share in the production of the first sound, yet occasionally the stroke may produce sound by causing movements in the air in the stethoscope, and the meatus. That the first sound should be caused by the dashing of the blood on the heart's walls, or by the flattening of the sigmoid valves against the sides of the arteries, are "acoustic impossibilities." An empty space never occurs in any part of the circulating system, and there can therefore be no "dashing" of blood, but only a greater or less amount of pressure. The sigmoid valves, too, are not "beaten" against the sides of the vessels.—The second or diastolic sound is produced by the fall of the sigmoid valves, and has no other cause. The heart's sounds are therefore considered to be altogether valvular.

With regard to an arterial systolic tone produced by expansion of the vessel, Kiwisch believes this to be also erroneous, for the following reasons: 1st. It is found, after death, that to produce the necessary tension in arteries for sound, requires an extraordinary force. 2nd. From vivisection, the enlargement of the arteries during systole is found to be far too little to give rise to a loud sound. 3rd. Vivisection also proves that the seat of the tone is really in the heart, and not in the arteries. 4th. The impulse of a large artery sometimes gives a tone, as the heart's stroke does, by causing vibration of air in the stethoscope, but this can be distinguished from the true arterial tone. To the obvious objection of the loudness and variable characters of these arterial tones at some distance from the heart, Kiwisch replies that these changes occur from different conducting power and from consonance.

With regard to cardiac murmurs, the opinions of Kiwisch agree with those held in this country.

Extra-cardiac vascular murmurs are discussed, chiefly in reference to chlorotic and anæmic cases. In chlorosis, the seat of the murmurs in the neck is in the carotid arteries, and, as usually taught, in the deep-lying cervical veins. It is necessary to premise that, so far as blood is concerned, no alteration in its quality can have any *direct* influence, but only alterations in quantity and in force of movement. With regard to the production of these murmurs by changes in composition of the walls of the vessels, as they are local, the changes must also be local, but a local change has never been proved, and is unlikely. The murmurs in the neck, in chlorotic patients, are really derived only from compression; and this is true for all similar murmurs.—Kiwisch then enters into an argument in support of his assertion, of which we extract what appear the best portions. If in chlorotic or in perfectly healthy persons, but especially in children, the face be turned on one side, the shoulder drawn backward, and the chin raised, so that the omohyoid may compress the carotid, a systolic murmur is heard loudly in the vessel. If then the chin is brought down and round, and the omohyoid relaxed, the murmur gradually diminishes, and, when all pressure is removed, ceases. The variation in the amount of murmur depends chiefly on the ease with which the vessel is compressed. A weak vessel and a small quantity of

blood permit compression; a strong vessel, and a full, powerful current, resist the external muscular pressure. Pressure with the stethoscope produces the same result, and brings out murmurs, the pressure being equal, more easily in proportion to the compressibility of the vessel. The same facts are true for all arteries as well as for the carotid. In all, in variable intensity, according to circumstances, compression from any cause produces narrowing, and consequent vibrations. In exquisite cases of chlorosis, a murmur can be heard in the femoral artery, without stethoscopic pressure, when the thigh is drawn outwards; this arising from spontaneous compression of the artery against Gimbernat's ligament. As far as regards the vessel itself, the most important point, as already said, is the diminution from any cause of the peripheral tension of its coats, and the consequent ease with which these are compressed and thrown into vibrations. In chlorotics, the altered quality of the blood is not the immediate cause of the murmurs; but the loss of tonicity which appears consequent on this altered quality is probably the cause, so that, although not in the way often understood, the impoverished blood is the fundamental cause of the vascular murmurs. The murmurs produced in this way in the arteries can often be diminished or abolished by pressing on the artery in the further part of its course. A murmur in the subclavian may be often arrested by compressing the axillary artery. This arises from the fact, easily demonstrated on elastic tubes, that if an obstruction be offered by a stream of fluid (which has passed through a narrow into a wider tube, and has thereby given rise to sound), so as to cause a damming of the fluid, the vibration of the vessel, which previously has been seen, felt, and heard, ceases. The interruption to the circulation need not be complete to produce this effect.

With regard to the chlorotic-cardiac murmurs, Kiwisch states that these are only arterial, and suggests that they are produced by the relaxation of the vessel. Then the blood passing through the normal, narrow, arterial opening into the dilated portion, gives rise, as usual, to vibration and to murmurs.

Kiwisch then enters on the consideration of the murmurs in chlorosis, which are supposed to have their seat in the veins. He admits that veins, under certain conditions, might, like arteries, give rise to murmurs, but he asks, whether they are ever found under such conditions in chlorosis, and on physical and physiological grounds entirely denies it. According, then, to Kiwisch, the opinion of Ogier Ward, and of all succeeding pathologists, is erroneous, and the chlorotic murmurs are exclusively arterial. Four great arguments have been advanced in favour of the venous origin of these murmurs:—1st. They are continuous and not intermittent; 2nd. They can be abolished by pressure on the jugular; 3rd. Vibrations can often be seen and felt in them; 4th. The arterial sounds can be also heard and distinguished. Kiwisch declares, that all these arguments are worthless. In the veins, he says, the current of blood is continuous and uniform, and a venous murmur must be continuous and uniform, but in the neck of chlorotic females no murmur is ever heard, which is not augmented and strengthened during systole. This has been ascribed to coincident arterial murmurs, and it was also denied that the continuous murmur could be arterial. But Kiwisch affirms, that an arterial murmur

is frequently continuous, though the diastolic portion of it is much less marked than the systolic, which is blowing, piping, or roaring. He proves this by experiments on tubes and arteries taken from the body. By careful pressure also over an artery, an intermittent can sometimes be converted into a continuous murmur. In some diseased states, also, and in the epigastric arteries of pregnant women, a continuous murmur is heard. It is erroneous, then, to state that the diastolic rumbling murmur cannot occur in the arteries, and that venous and arterial murmurs can be distinguished by the character of continuousness and intermittence. By experiments on elastic tubes, it is proved that a certain force of current is necessary, in order that the fluid, in passing from a narrower into a wider portion, may retain its form for a certain time, without which vibrations and sounds are not possible. Now in the veins, the current is not sufficiently strong for this.*

As for the second argument, Kiwisch declares it is altogether a mistake. It has been supposed that the jugular vein was compressed, and the downward current arrested; but the simple fact was, that the carotid was compressed, the upward current obstructed, and the arterial murmurs thereby annihilated.—With regard to the third argument, visible venous vibrations, Kiwisch remarks, that two sets of venous vibrations are to be distinguished in the neck—namely, vibrations produced by regurgitation during the systole of the right heart, and an extremely delicate vibration felt in the external jugular, behind the insertions of the sterno-mastoid, which is simply a communicated vibration from the artery below. The first kind of vibrations are much too slow and irregular to give rise to sound, and the last is found to give no sound.

As to the fourth argument, that the sounds of the arteries and veins are diverse and can be distinguished, Kiwisch protests against what he considers an assumption, unsupported by any evidence, and contradicted by all the facts he can bring forward. He then enters into a critical examination of Cejka's cases of murmur in the innominate veins, and of Hamernjk's hypothesis of venous murmurs, which, although very interesting, need not now detain us.

Kiwisch then passes on to a full consideration of the so-called placental murmurs, and concludes his paper with the following summary:

1. Every sound in the circulating system arises from the walls, and never from the blood.

2. In perfectly healthy persons, sounds are produced only by the movements of the heart's valves.

3. All murmurs in vessels arise, principally, through unequal calibre of the vessel, and through unequal tension of its walls. Rapidity of circulation, and a relatively diminished peripheric resistance to the blood-stream, act as assisting causes.

4. The unequal calibre is most frequently produced through compression, less frequently through widening of arteries.

5. The murmurs in vessels, in chlorotics, are compression-phenomena, like those of the abdomen in pregnant women.

* In an appendix, Kiwisch alludes to some later experiments conducted with Kölliker and Virchow. The veins and arteries of large animals were laid bare and ausculted; no venous murmur could be obtained; an arterial one easily, which, however, could not be made continuous

6. Such murmurs are never seated in the veins.

7. Both systolic and diastolic vibrations occur in the arteries, and consequently continuous murmurs also.

8. In arteries with sound walls, the most important causes of murmurs are, diminution of the muscular contraction or tonicity of the vessel, and poorness of blood.

9. The murmurs in the abdomen of pregnant women arise solely in the vessels of the abdominal walls, and are not phenomena peculiar to pregnancy, but may occur in other enlargements of the uterus, and even, in some rare cases, from ovarian tumours, combined with enlargement of the uterus.

In the discussion which took place in the Würzburg Society after the reading of the above paper of Kiwisch, of which we have given most of the essential points, much difference of opinion prevailed. With regard to Kiwisch's explanation of the cause of the sound in an elastic tube of unequal calibre, Reinecker, Kölliker, Virchow, and others, objected to it, and seem disposed to adopt the opinion of Corrigan, that at an obstructed part the fluid is thrown into a whirlpool or eddy, which produces vibration of the walls. In speaking of experiments with tubes or arteries after death, Virchow very properly alluded to the difficulty of drawing conclusions from experiments on merely elastic tubes, as to the phenomena occurring in tubes endowed with both tonicity and elasticity.

As to the venous murmurs, their theoretic possibility was not disputed. It appears, from the statements of Reinecker and Virchow, that on neither side could positive arguments be brought to bear. The debate on this point brought the question before the Society, as to whether continuous murmur could occur in arteries without systolic augmentation; and it was also questioned, if all the phenomena of chlorotic murmurs could be explained by the theory proposed by Kiwisch. Rapp, who is so well known as an able stethoscopist, was decidedly in favour of the venous origin of some of these murmurs, and of their occurrence without the least pressure; and Reinecker, who introduced many chlorotic women before the Society, urged strongly the facts of the much greater frequency of the murmurs on the right side of the neck, where the anatomical arrangement of the veins but not that of the arteries is peculiar, and the very slight degree of pressure that is often necessary to stop the murmur, which yet could never have much affected the blood-stream of the carotid artery.

Our analysis of this paper has run to so great a length, as to preclude us at present from entering more fully into it. The observations of Kiwisch are of great interest; and his location of the so-called placental murmur in the arteries of the abdominal walls and uterus is an important fact, which most of the auscultators of Germany appear now to admit. We believe, also, that he is correct in stating that a diastolic, and more rarely a continuous murmur may arise in an artery; but we confess we cannot as yet see that he has made out a good case for the non-existence of venous murmurs.

He has not noticed, also, some very important points which are strongly opposed to his hypothesis. Thus, the strengthening, during inspiration, of the continuous murmur at the root of the neck, is readily explained by the supposition of its venous origin, but cannot be understood if the carotid be assumed as its source; the effect of position on the murmur

is more decided than would be the case were it only arterial; a similar murmur can be heard sometimes in the superior longitudinal sinus, in exquisite cases of chlorosis; and finally, as noted by Dr. Davies, in a remarkable case which occurred to Richter, the mammary veins were dilated in consequence of obliteration of the right jugular, and a clear continuous murmur was heard over them, which could not be attributed to an artery, as none was near them.

Assuming, then, that the so-called venous murmurs really have the origin usually given to them, without denying that continuous arterial murmurs may have been called venous, we will proceed to some interesting observations connected with venous murmurs, which are of great practical use.

Skoda, whose practical observations are always worthy of confidence, describes, after Ogier Ward, the venous murmur of the jugular veins; and, distinguishing the coincident systolic murmur of the carotid, remarks that for many years he has considered the venous hum not to be a sign of poverty of blood, as he has found it in healthy young individuals. This important remark has been fully confirmed, and the extraordinary frequency of venous hum in young persons, not only fully proved, but its frequency in adults demonstrated. Wintrich* has given the following summary of ten years' investigation of this point. The per centage of venous murmurs in healthy persons of both sexes is as follows:

Age.	Males.	Females.
1 to 5	97	98
5 to 10	94	95
10 to 15	89	95
15 to 20	86	88
20 to 25	82	88
25 to 30	80	86
30 to 40	80	86
40 to 50	77	78
50 to 60	72	75
60 to 70	68	71
70 to 80	40	39

These observations, if confirmed, prove that venous murmurs in health are the rule, and not the exception, in all periods of life till advanced age; although there is a gradual diminution from birth to death.

Dr. Davies has paid particular attention to this subject, and his researches confirm in some measure the above observation. In 802 healthy children (417 males, 385 females), he found the murmur present in both jugulars, or in 1 in 386 males and 364 females; there were traces of it in 11 males and 14 females; and it was entirely absent only in 20 males and 17 females.† Dr. Davies found the murmur in a number of adults, and thus sums up his opinions on the subject.

"1. The venous murmur does not necessarily depend upon any abnormal condition of the blood, nor upon any deviation from the health of the individual in whom it may be found, for we have observed it to be present in a large proportion of persons under the age of twenty-five years, and to exist occasionally in the aged, all in the most perfect health. It is not, therefore, an anæmic or chlorotic murmur,

* Göschen's deutsche Klinik, 1850, No. xv. § 159, 160; and Canstatt's Jahresbericht, Bericht von Valentin, for 1850, p. 84.

† These figures make the number of females 395, an immaterial error, which, however, we cannot correct.

although uniformly present in those conditions of the system which are marked by an impoverished condition of the blood, inasmuch as it has been observed in a multitude of instances to co-exist with the ruddiest complexion, and the most perfect health.

"2. The venous murmur is not entirely the result of pressure, although some portion of the sound may be fairly attributed to that cause. The existence of a sternal venous murmur, at a spot upon which no pressure can be exerted by the stethoscope,* is a sufficient proof that sound can originate in the venæ innominatæ, independent of any compressing cause; and if in these veins, why not in the jugulars also? If, then, these murmurs can neither be attributed to the transit of thin and impoverished blood through the veins, nor to the effects of external compression upon the parietes of these vessels, in what mode are we to explain their origin? I believe very easily. There can be no doubt that the rapidity of the blood in the large veins is usually sufficient to establish a friction capable of causing a sound, which is more or less audible according to the readiness with which the parietes of the veins take up the vibrations, and the facility with which the latter are conducted to the outer surface of the body." (p. 283.)

These observations support strongly, we believe, the opinions of Kiwisch, that the various murmurs arise alone from vibrations of the walls.

It should be distinctly understood, that although venous murmurs are thus frequent, the anæmic and chlorotic condition is that in which they are most intense and general. The term anæmic murmur is a truth, although a similar though less intense murmur may be present in a healthy person.

Our remarks have run to such a length, that we must omit all consideration, for the present, of Rapp's able paper. The subject is important enough to demand an article to itself, and we hope shortly to consider it fully.

A former edition of the work of Barth and Roger has already appeared in an English dress; and if it had not, we should hardly think it necessary to do more than notice the issue of a third edition. The work is a very good one, but partial and too national; and the ignorance of Skoda's opinions, and of the German press generally, has been with justice commented upon by the neglected parties.

Weber's little book is chiefly devoted to the circulating system, and is a highly practical and useful publication.

The appearance of so many superior works on Auscultation must be held to be a gratifying proof of the cultivation of this important branch of diagnosis. The subject is, however, still capable of indefinite development; and we trust that English observers will henceforth, as heretofore, be conspicuous and honoured labourers in this field. England has produced some of the best auscultators; and the works lately published by Walshe and Davies prove that she can sustain the reputation gained for her by Forbes, Williams, Stokes, Clark, and Latham.

It is necessary to recognise, that while facts can never alter, their relations to each other and their interpretation continually change with the progress of inquiry; and although it is but right to pay every respect to the dicta of former observers, it is equally our duty to remember that every subject will bear re-investigation, and that all statements, from whatever source they emanate, cannot be too often tested and proved by repeated researches, even in directions that have been supposed to be fully explored.

* Dr. Davies might have alluded also to the murmur in the superior longitudinal sinus.

Art. XII.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XXXIV.—*London*, 1851. 8vo, pp. 361. With Two Plates.

In resuming our critical analysis of the contents of this volume, we think it right to observe, that there is now no ground for the strictures which we felt it our duty to make on the typographical imperfections of the previous issue. On the contrary, the Society has evidently benefited by the change in its editorial hands; and the present volume, in place of being discreditable to those who prepared it for the press, is remarkable for its accuracy and freedom from blemishes.

The papers themselves are of an average degree of merit; some of them are little else than narratives of cases, which a little additional trouble and research bestowed upon an examination into the histories of similar cases, would have rendered much more valuable; and there are one or two communications to which we can refer, as examples of the mode in which a narrative of an individual case may thus be made the foundation of a most important memoir.

Having fully discussed, in our last number, the first two papers in this volume, which relate to the question of ovarian section, we pass them by on the present occasion, and proceed at once to—

III. *A Case of Softening of the Spinal Marrow, in a Boy affected with Chorea.* By Robert Nairne, M.D., Physician to St. George's Hospital, &c.

This case presents many points of interest. In the first place, it is "an additional instance of the frequent occurrence of chorea in rheumatism or in those of rheumatic diathesis, to which attention has been of late years directed." But its chief value lies in the fact, that softening of the spinal cord was discovered after death, without having been indicated during life by any other symptoms than those which were thought referable to the choreic state. The patient, a lad of seventeen, when first seen by Dr. Nairne, was obviously suffering from an attack of chorea, which had commenced about five weeks previously; and rheumatic inflammation of the joints of the lower extremities was also present, from which he had suffered at times since the age of twelve. Up to five days previously, however, he had been able to walk; and it did not appear that his subsequent loss of power was due to anything else than an increase of the chorea and rheumatism. He was treated for rheumatism, but grew rapidly worse; delirium soon came on, and the convulsive movements became more violent; and he died in a state of coma within a week after admission. On *post-mortem* examination of the spinal canal, it was found that "a portion of the spinal marrow, an inch at least in length, and situated opposite the third and fourth dorsal vertebræ, was white, and so soft that the slightest pressure of the finger broke it up. It was almost in a fluid state. The whole thickness of this portion was in this state. (In the central part of this softened portion, we are told in a note, there was fluid, and the remainder was thoroughly disorganized.) In consistence, firmness, and

colour, all the rest of the cord was strongly in contrast with this part." There was a congested state of the cerebral and spinal vessels, but no other morbid appearance about the nervous centres. The heart presented evidence of previous attacks of rheumatic inflammation.

We quite agree with Dr. Nairne in regarding the softening of the spinal cord as an accidental complication, and as having no causative relation to the choreic state. But the case is not the less important, as adding another to those already on record, in which there have been manifestations, not merely of automatic power in the lower part of the cord, but of the continuance of connexion of the parts supplied by it with the encephalic centres, notwithstanding a most evident disintegration of its substance. In this case, we are without adequate evidence of the degree of lesion of voluntary motion which had taken place in the legs before death; and we know no more of the state of their sensibility, than that the patient felt pain in them. They continued, however, to be subject to the involuntary movements characteristic of chorea; and these were increased in violence, even more in the lower part of the body and in the inferior extremities, than in the face and superior extremities, by slight emotional excitement, such as that caused by the approach of any one to the patient's bed, or by a question being put to him. Dr. Nairne concludes by remarking upon those anomalies, "which prove," he says, "that there is yet much for us to learn respecting the functions of the spinal marrow." No doubt there is; but we shall not learn much by examinations so slightly made as this of Dr. Nairne's. The question is, what amount of disintegration is compatible with the continuance of the transmitting power of the cord; and instead of a careful microscopic analysis of the diseased part, all we are told is, that the disorganization was such, "that it was thought unnecessary to put any of it under the microscope." We are consequently left quite in ignorance of the nature of the changes which had taken place in the cord, and of the degree of preservation of any of its elementary structure; and the information which this case might have yielded in the hands of a competent observer, is absolutely *nil.*

IV. *Case Illustrating the Difficulties of Diagnosis of Morbid Growths from the Upper Jaw, with Remarks.* By Prescott Hewett, Assistant Surgeon to St. George's Hospital.

This is another of those valuable contributions to practical surgery, which in former volumes have reflected so much credit upon Mr. Hewett. The case is also important, as affording another example of death occurring during the employment of chloroform. It would be difficult to condense Mr. Hewett's narrative, without making it unintelligible; but it may be stated that the patient, a young man twenty-five years of age, presented himself at St. George's Hospital with a large tumour, seemingly of the upper jaw, which filled up the superior maxillary region, and encroached upon the orbit, where it presented itself in the shape of small flattened bodies lying beneath the conjunctiva. A portion of it was also detected in the left nasal fossa, and a round mass projected into the pharynx, which could be distinctly felt by the finger curved round the posterior border of the palate. The gums and all the region of the mouth were free from disease. The patient stated that, six years previously, what appeared to

be a polypus was extracted from the nose, after which the cheek began to swell, and the tumour to make its appearance in the orbit, without causing pain.

It having been decided to remove the upper jaw, the operation was performed by Mr. Hewett in the usual manner, and under the influence of chloroform. In removing the superior maxillary and the malar bones, however, it was discovered that the diseased mass was lying behind, and was unconnected with the upper jaw, but nevertheless admitted, in great part, of removal. Whilst this was being done, the patient became faint, but rallied on being placed in the horizontal position; again, however, the pulse failed, and this time so completely, that in spite of stimulants it could not be restored. An opening was quickly made into the larynx, but without avail, and in a brief period the patient breathed his last.

After death the tumour was found to be of a purely fibrous character, originating in the roof of the left nostril, and especially on the inner side of the pterygoid process, whence it had passed backwards into the sphenoidal sinuses, and into the spheno-maxillary and temporal fossæ.

"The trachea and the bronchial tubes, even to their minute ramifications, contained a quantity of frothy blood. The structure of the lungs was crepitant throughout, but each section presented numerous small, dark spots of ecchymosis, produced by some of the air-cells having been also filled with blood; these organs were otherwise free from disease. The heart was healthy; its cavities contained some small black clots, but the greater part of the blood was thin and fluid, and did not coagulate on exposure to air. The other viscera were quite healthy." (pp. 47, 48.)

These appearances Mr. Hewett believes, and no doubt correctly, were due to the circumstance that blood had passed backwards through the glottis, when its irritability was suspended by the action of the chloroform; and he inculcates the necessity of caution in using this agent during operations about the mouth. In aggravated cases of hæmoptysis, when the patient has been suffocated by the sudden eruption of the blood into the mouth, it is common to find the trachea, bronchial tubes, and lung presenting precisely the same appearances as here detailed, arising of course from the driving of some portion of the fluid downwards by the inspiratory current of air.

In reference to the disease itself, Mr. Hewett makes the following observations:

"That a polypus of the nose, of a purely fibrous character, should take the course which I have just described, is, I believe, of very rare occurrence; some cases have, however, been recorded, which show that these growths do sometimes make their appearance in regions where one would little expect to find them. Such was the case in a patient lately operated upon in St. George's Hospital by Mr. H. Charles Johnson, where a fibrous tumour, lying in the orbit, was ultimately traced, by its pedicle, into the nose, through a hole in the inner wall of the orbit; and yet there was nothing about this patient to lead to the supposition that the disease had originated in the nostril." (p. 49.)

To this paper is appended a short communication from Dr. Snow, written with the object of proving that the chloroform had nothing to do with the patient's death; for that at no time was it administered beyond the second degree, or "that state in which the mental functions are confused, but not suspended." We cannot at all agree with Dr. Snow in his conclu-

sions; and think he would have consulted his own credit more by remaining silent, if he could not bear that any evil results should have followed the administration of chloroform in his hands.

V. *Cases of Rupture of the Liver or Spleen; with Remarks.* By Athol Johnson, F.R.C.S. &c.

The object of this communication is to prove that there is a possibility of recovery after rupture of either of these organs; for in some of Mr. Johnson's cases the laceration was found after death to have been healed, without, apparently, any amount of inflammation having been set up. In one instance, where the patient died from the effects of a totally distinct injury (fracture of the spine), it was discovered that the liver as well as the kidney had been lacerated, and that union had taken place without apparent inflammation. The practical conclusion drawn by the author from these premises, is in favour of keeping up the state of collapse consequent on the injury, by abstaining from the administration of stimulants, and rigidly enforcing the most perfect rest.

As the pain in those cases is often very violent, morphia or some other opiate is recommended.

VI. *Account of a Case in which the Cæsarean Section was Performed; with Remarks on the Peculiar Sources of Danger attendant on the Operation.* By Charles West, M.D., Physician-Accoucheur to St. Bartholomew's Hospital.

This elaborate essay hardly admits of condensation; every word is of value; every precept, suggestion, or caution which it contains merits preservation, and will no doubt meet with careful study from all who seek instruction on the important subject of which it treats, or who delight in logical acuteness and painstaking research. Our account of it must necessarily be imperfect, yet such as it is, it may serve to stimulate our readers' curiosity, if it fail to gratify their thirst for information.

Elizabeth Williams, at the age of 15, began to suffer from pain in the back and pelvis, almost constant in its character, yet aggravated at each menstrual period. When 25 years old, she married, but did not become pregnant until fifteen months afterwards. From this date, until the termination of her pregnancy, the pains which formerly distressed her became worse, and she experienced so much difficulty in walking, that for some weeks before her confinement she scarcely moved about at all, but remained the whole day lying on the sofa. There was nothing in her appearance to induce the belief that she was the subject of pelvic deformity; and Mr. Wren, her medical attendant, did not discover her condition until the 7th of May, 1850, when labour had set in. In two hours from the commencement of the pains, Dr. West saw her, and took charge of the case in conjunction with Mr. Wren.

Examination showed that—

"The spine was perfectly straight, but the sacrum was bent into an almost semicircular form, with a great convexity projecting backwards; the pubic arch was a little wider than would suffice to allow of laying one finger between the rami of the pubes; the tuberosities of the ischia did not seem to be much above an inch apart, and the rami of the pubes ran out into a sort of beak; the bones being

bent at the junction of the pubis and ischium, and being at this point not more than an inch apart.

"The os uteri could be reached with difficulty; it was open to about the size of half-a-crown; its lips were still thick, but soft." (p. 63.)

Under all the circumstances it was determined by her attendants, in consultation with Dr. Ramsbotham and Dr. Murphy, that the case was one calling for the performance of the Cæsarean section; and accordingly Mr. Skey undertook the operation. For the reasons which appeared to justify this proceeding, we must refer the reader to Dr. West's paper, where they are stated at length; nor can we afford space for the account of the operation itself.

There was much hæmorrhage as soon as the placenta was removed; but after a time the uterus contracted under moderate pressure with the hand, and the bleeding ceased. After the closure of the wound, a good deal of inconvenience was experienced from attempts at vomiting, induced (it seems not improbable) by the chloroform which the patient had insisted upon inhaling; and the stomach continued so irritable, that the necessity arose of supplying her with nourishment and medicine by means of enemata. For the first twenty-four hours she dozed at intervals, but continued in a state of great depression, from which she can hardly be said at any time to have rallied. The vomiting was very troublesome, and prevented her from taking more than a teaspoonful or two of any nourishment at a time; and she continued much in the same state until the 12th of May, when she died, 108½ hours after the operation. During the whole period, with the exception of an interval of a few hours, opium was administered by the mouth and rectum.

The abdominal cavity and the pelvis were examined 33½ hours after death. The external wound, which was six inches long, had united for two inches only; and there was a condition of sub-acute peritonitis, but no effusion of fluid into the abdominal cavity. The rest of the *post-mortem* appearances shall be given in Dr. West's own language:

"The bladder was empty and flattened, and appeared to have adapted itself to the altered shape and relations of the parts, being almost completely out of the pelvis. It was quite pale and healthy.

"The uterus was moderately contracted, but not the slightest effort had been made to close the wound, the external edges of which were retracted, and lay at least an inch apart, while the internal edges were but just in apposition.

"At the wound, the substance of the uterus looked swollen and infiltrated, the wall there measuring 1·2 inch in thickness, while at the fundus of the organ it was only ·7 thick. The length of the external uterine wound was 3·65 inches; of the inner 2·5. The edges of the wound were covered by a thin layer of a dirty brownish matter, in all probability altered blood; beneath which they were of a pale, dirty straw-colour. Elsewhere the substance of the organ was pale and bloodless, looking as white as veal; presenting no sign of inflammation, no thickening of its veins, neither coagula nor blood in their channels.

"The placenta had been attached posteriorly near the fundus of the uterus, but rather to its left side. The surface to which it had been attached looked quite healthy, as did the whole interior of the uterus, and also the vagina, in neither of which was there any blood nor any sort of morbid secretion.

"The pelvis was a very well-marked specimen of the deformity produced by mollities ossium.

"The lumbar vertebræ had been driven down into the pelvic cavity, so that, as the body lay upon its back, a line drawn from the symphysis pubis directly backwards, touched the upper part of the fourth lumbar vertebra.

"The length of such a line was 4·2 inches.
"Transverse diameter of the brim 4· „
"The rami of the pubes were projected forwards into a beak, the width of which was ·75 of an inch, while its length, measured from the inner surface of the symphysis, was 1·2 inch; by which the antero-posterior diameter was reduced to 3 inches.
"The bodies of the fourth and fifth lumbar vertebræ were much flattened, so that from the upper part of the fourth to the upper part of the sacrum they measured only 1·5 inch.
"The sacrum was so greatly curved, that from about its third vertebra it ran horizontally forwards. The depth of the bone to the point where it bent forwards, was rather less than one inch and a half; while from the point of the coccyx along the horizontal part of the sacrum it was 2·75.

"Distance between the two anterior superior spines of ilia . . .	8	inches.
„ „ tuberosities of ischia	1·2	„
„ „ point of coccyx, and summit of the pubic arch	2·8	„
„ „ rami of the pubes at the lower edge of the beak	1·05	„
Width at the upper part of the pubic arch	·6	„

(pp. 72, 73.)

Deeply interesting as is this narrative, we regard the author's reflections appended to it as of even greater moment. He has constructed a table exhibiting the cause of death in 147 fatal cases of the Cæsarean section, in which the account can be relied upon. This we are only prevented by want of space from transferring to our pages.

He states the dangers peculiar to the Cæsarean section to be as follow:

"1. The danger arising from hæmorrhage, which proceeds from a source different from that whence bleeding takes place in any other operation, and which is not capable of being arrested by the same means as suppress it under ordinary circumstances.
"2. That dependent on the shock inflicted on the nervous system, as well by the violent interference with the most important process that ever goes on in the organism within the same limited time, as by the injury to a part so important and so richly supplied with nerves as the uterus of a parturient woman.
"3. The hazard inseparable from extensive injury to the peritoneum, when unblunted in its sympathies and unaltered in its texture, as in cases of ovarian or other tumours, for the removal of which a similar exposure of the abdominal cavity is sometimes practised.
"4. That which results from the infliction of a wound on the uterus, at a time when, in the ordinary course of things, the processes which nature is prepared to carry on in it, consist in the disintegration and removal of its tissue; the very opposite, indeed, to those essential for the repair of injury." (pp. 74, 75.)

In respect to the first of these dangers—viz., that arising from *hæmorrhage*, it does not seem easy to determine why profuse bleeding has occurred in some instances and not in others. In 7 out of the 41 cases in which bleeding occurred, a wound of the placenta during the operation gave rise to it. Careful preliminary auscultation may probably avert such an accident in future. In 20 instances the bleeding arose at the time of the operation, and proceeded either from the edges of the wound, or from the seat of the separated placenta; and in the remaining 14 cases the hæmorrhage was secondary, having followed the closure of the wound. The great safeguard against bleeding—namely, the contraction of the uterus, must here be much diminished by the injury done to the organ in

the operation. The influence of chloroform in further diminishing this contraction is a problematical point; though there is little doubt that anæsthetic agents do retard uterine action. Dr. West also affirms that the risk of hæmorrhage is diminished by not operating until the liquor amnii has escaped.

The danger arising from *shock* is of very serious nature. In 33 out of the 147 fatal cases, this seems to have been the sole cause of death; in 11 more, it combined with inflammation, and in 9 others with loss of blood, to bring about the fatal result. Opium is probably the sheet-anchor in these cases.

The hazard of *peritonitis* is very great. In 37 per cent. of the cases, acute inflammation of the peritoneum and uterus was manifested during life and verified after death; and in 29 other cases, this inflammation was only modified in its intensity, not in its fatality, by the previous hæmorrhage. In 85 cases out of 147, therefore, or in 57 per cent., inflammation destroyed the patients. On this subject Dr. West truly exclaims, "How narrow must be the limits, which, in a patient who has undergone the Cæsarean section, separate the healthy action essential to repair, from the morbid action that tends to destruction!" (p. 78.)

The last source of danger enumerated by Dr. West is the special one caused by the wound inflicted on the uterus, at a time when its tissue is in a state of degradation and disintegration, and consequently in the most unfavourable condition possible for the repair of an injury. This source of peril has not received the share of attention which its importance deserves. In a large proportion of the fatal cases, no attempt is made to repair the injury; the wound remains widely open, or very imperfectly closed; and in two instances only is it stated that the edges presented a granulating surface when the body was examined. In other cases, the cicatrix is so weak and imperfect as to give way during a subsequent pregnancy; or there may, in fact, be no closure of the uterine wound at all, but merely a covering of it over by peritoneum, while its edges have become adherent to the abdominal wall.

Dr. West calls attention to the rapid diminution in the size and weight of the uterus after labour, as evidence of the activity with which this process of disintegration goes on; and remarks, that it is not a process of simple absorption, but of positive degradation of tissue, as is proved by the abundant presence of fat-globules in the lochial discharge, and by the condition of the tissue itself, as observed by Virchow, Kölliker, and Kilian. This consideration makes the favourable termination of Dr. Oldham's case (see p. 438 of our present number), in which the operation was performed on a patient suffering from extensive cancerous disease of the cervix, the more remarkable.

VII. *A Case of Cæsarean Section.* By Dr. Oldham, Obstetric Physician and Lecturer on Midwifery at Guy's Hospital.

The subject of the operation in this instance was a dwarf, 23 years old, who applied to Dr. Oldham when seven months' pregnant. The principal pelvic deformity was the projection of the promontory of the sacrum, which was so great as to shorten the conjugate diameter to about two inches; but the pelvic cavity and outlet were comparatively roomy. A few

days after seeing the patient, Dr. Oldham determined to induce premature labour, which was done by puncturing the membranes. The child was alive, and continued living for two days, but so little progress was made in the labour, that the child having by this time ceased to exist, on the fourth day Dr. Oldham proceeded to perforate the head and deliver it piecemeal. In this he failed; and therefore determined, in consultation with Dr. Lever, that the Cæsarean section was called for. The operation was performed by Mr. Poland, Assistant Surgeon to Guy's Hospital, and proved fatal by peritonitis on the evening of the second day.

For the reasons which rendered this proceeding justifiable, we must refer the reader to Dr. Oldham's paper, as it would be impossible to condense them in a satisfactory manner.

VIII. *Case of Extensive Necrosis of the Bones of the Cranium, and Removal of large portions thereof.* By John Drummond, F.R.M.C.S., Deputy Inspector of Hospitals, &c.

This is probably the nearest approach to a complete destruction of the bony envelope of the brain ever recorded. A sailor, serving on board ship at Sierra Leone, received a severe contused wound of the scalp covering the left side of the occipital bone.

In October of the same year, 1845, whilst in the Naval Hospital at Plymouth, he had an attack of erysipelatous inflammation, involving the whole of the head and face, leaving numerous purulent deposits under various parts of the scalp. During the next six years, by a co-existent process of caries and necrosis, the following portions of skull had perished: about five square inches of the right side of the frontal, parietal, and squamous part of the temporal bones; and the whole of the occipital, to within a short space of the foramen magnum, save a portion of the centre, of about two inches square, which was in process of being thrown off. At the time the description was drawn up, a large portion of the left side of the frontal, parietal, and temporal bones, remained firm, but in a diseased state, as was evident from numerous openings discharging pus of a fœtid nature, through which the bones could be felt in a state of caries. A portion of the posterior part of each parietal bone existed, as a slender arch over the vertex.

The spaces left by the destruction of these large portions of bone had been covered by a delicate and highly vascular integument; but in no situation was there the slightest tendency to ossific reproduction. The patient is stated to be "in robust health, suffering but little inconvenience from the extent of the disease;" but a doubt is expressed whether he can possibly resist the effects of complete destruction of the skull, which is likely to take place, sooner or later, if his life should be prolonged.

IX. *An Account of a Case of Fracture and Distortion of the Pelvis, &c.* By C. H. Moore, Surgeon to the Middlesex Hospital.

Mr. Moore's paper is principally remarkable for the care and accuracy with which he has described the distortions accidentally discovered to exist in the pelvis of a man, who died in the Middlesex Hospital without its being known that he was deformed. Two excellent woodcuts by Bagg accompany the paper; which, not being one of which an analysis would possess much interest, we must pass with this brief notice of it.

X. *Experiments on Chylous or Chylo-Serous Urine.* By John Mayer, Assistant Surgeon to the Second Native Veteran Battalion. *With a History of the Patient.* By G. Pearse, M.D., Garrison Surgeon of Bangalore.

We are glad to receive from India so interesting a contribution to our knowledge of a rare affection, to which, it will be recollected, attention was particularly directed by Dr. Bence Jones in the preceding volume of the Society's Transactions. The subject of this case was a young married woman, an Indo-Briton, aged twenty-two years, the mother of three healthy children, the youngest of whom, a particularly strong and healthy infant of ten months old, she was continuing to nurse at the time when she came under Dr. Pearse's care, though the supply of milk had latterly been scanty. Though tall and thin, her appearance was healthy; and, according to her own account, her general health was very good. When nursing her two elder children, which she had continued to do until they were fourteen or fifteen months old, she observed for some time previous to weaning them, that her urine had become white like milk, which peculiar appearance it lost soon after she discontinued nursing. During her third lactation, the urine assumed the white appearance when her infant was only five months old; and the medical officer under whose care she then was, observing that she was suffering from general debility, put her upon a course of tonics, and recommended that the infant should be at once weaned. This recommendation, however, was not attended to; and she continued to nurse for five months longer, when she was first seen by Dr. Pearse, having been sent to Bangalore (a distance of sixty miles) for change of air. The urine was then nearly as white as pure milk, and coagulated on cooling into a thick but not firm jelly; it was not unusual in quantity. When treated with a few drops of dilute nitric acid, it separated into a serum having a firm coagulum in the centre. The further examination of the fluid was placed in the hands of Mr. Mayer, whose results we shall presently give. The patient was caused to wean her child forthwith, and placed upon a course of sulphate of iron and quinine, with directions to be as much in the open air as possible. Two or three weeks subsequently, the urine was again examined; and although still retaining the milky appearance, it had more the aspect of healthy urine, and did not coagulate spontaneously until after it had stood for a long time undisturbed. The patient did not admit that there was anything the matter with her, save as regarded the condition of her urine; and being anxious to return home, she passed from Dr. Pearse's observation, so that he does not inform us whether, as on previous occasions, the urine returned to its normal condition after the suspension of lactation.

Mr. Mayer's inquiries were particularly directed to the determination of the question, whether the appearance of the urine was due to an admixture of the components of *milk*, or whether it was rather to be attributed to the passage of unassimilated *chyle* into the excretion. He found that on boiling it gave a copious white precipitate, on the surface of which, when separated by filtration, a fatty-looking matter was observable; and the filtrate, when treated with acetic acid, did not give any precipitate even when boiled. Hence he concluded that albumen was present, with fatty

matter; and that caseine was entirely absent. This conclusion was confirmed by the action of ether; which, when agitated with the urine, rendered it completely clear by dissolving the fatty matter, and caused a separation of the albumen as a tremulous jelly-like coagulum, leaving the urine in its natural state. The evaporation of the ether caused a deposit of fatty matter in a semi-crystalline state upon the sides of the phial. On the other hand, an artificial mixture of cow's milk and urine, made to resemble the chylous urine in appearance, having been examined by similar tests, was *not* cleared by ether, underwent no coagulation by heat (only the ordinary pellicle of caseine being formed), but gave a copious precipitate of white curdy matter on the addition of a few drops of acetic acid to the fluid when filtered after boiling. Finding, however, that the behaviour of human milk with re-agents, especially with acetic acid, was a point not distinctly determined by chemists, Mr. Mayer instituted a series of experiments upon the subject; the results of which were, that out of sixteen specimens of human milk from as many different females, five at once gave a precipitate upon being treated with acetic acid, whilst the remaining eleven did so only after being boiled. He found, however, that as *all* gave a precipitate after boiling, the presence of human caseine might be effectually determined in this mode; an admixture of one drachm of milk with half a pint of water, or of one grain of caseine in 3646 grains of water, being thus detectable. By this test, Mr. Mayer showed that the mixture of human milk with urine is as easily recognisable as that of cow's milk; and his conclusion that the urine in question contained albumen and ordinary fatty matters, not caseine and the fatty matter of milk, was thus fully confirmed.

XI. *Cases Illustrating some Difficulties in the Diagnosis of Pleuritic Effusions.* By T. A. Barker, M.D., Physician to St. Thomas's Hospital, &c.

The practical value of these cases obviously depends so much upon the due appreciation of their details, that we shall scarcely be able to give any useful summary of them within the narrow limits to which we must restrict ourselves. Their leading features, however, may be briefly noticed.

The first case was that of a labourer, aged 26, who had been subject to severe dyspepsia for three years, and was suffering from an attack of more than ordinary severity, which had commenced about two months previously to his admission. The right lung gave forth scarcely any respiratory sounds, and there was universal dulness on percussion over the whole of the right side. The left side, on the other hand, was very resonant in every part, including the cardiac region; and throughout the whole of the left lung, the natural breath-sounds were replaced by rhonchus, sibilus, and large crepitation. The ribs on the right side were almost immovable; those of the left were elevated as much as possible at each inspiration. It was difficult to obtain an exact measurement of the relative sizes of the two sides of the chest; but no discoverable difference existed. (Dr. Barker mentions that he does not attach much importance to this sign—as negative evidence, we presume; having had a patient from whose chest 160 ounces of serum were withdrawn by a trocar, and yet repeated measurements by three persons had not shown any enlargement of the

affected side.) The patient presented all the indications of the most urgent dyspnœa, and could only lie on the right side. Dr. Barker suspected that this might be a case of severe bronchitis of the left lung, with extensive effusion into the right pleura, the usual symptoms of effusion being somewhat modified by old adhesions; and with this view he caused a very fine trocar to be introduced between the fifth and sixth ribs, which did not, however, give exit to any fluid. Four days after the operation, the man said he felt more comfortable, and was sitting up before the fire, when he expired rather suddenly. On *post-mortem* examination, the following remarkable condition of the thorax was discovered:—the whole of the left lung was more or less emphysematous, and greatly enlarged, its edge completely overlapping the heart; the heart and mediastinum were pushed over to the right side, to such an extent that the central part of the mediastinum was not more than two inches from the right ribs; the right lung was collapsed to less than one-fourth of its natural size, forming a thin layer adherent to the thoracic parietes, and was pale and bloodless, containing very little air; and its surface was everywhere adherent to the costal pleura by delicate but perfectly-formed cellular tissue. The liver, though not enlarged or otherwise diseased, extended so high into the chest, that the diaphragm had narrowly escaped being wounded by the trocar, although the instrument had been carefully directed upwards. The following is Dr. Barker's interpretation of these phenomena:

"That there had, at some former period, been pleurisy with effusion on the right side; that the fluid had subsequently been absorbed, but that the compressed lung had not again expanded, and had become adherent to the ribs. Under ordinary circumstances, these changes would have caused great and evident contraction of the right side of the chest; but in the case now under consideration, this contraction had been prevented, partly by the opposite emphysematous lung and the heart, and partly by the liver, occupying the space which had previously been filled by the right lung." (p. 135.)

This case obviously presented great difficulties in diagnosis, more particularly as the heart's impulse could not be felt, and as the heart-sounds were very indistinct, for which the feebleness of its action and the intervention of the layer of lung gave a sufficient account. The knowledge of the possibility of such a condition, however, will obviously assist its recognition, should a similar case present itself.

In the second case, the symptoms chiefly consisted in excruciating pain at the lower part of the left side of the chest and abdomen, fixity of the left ribs, complete paralysis of the left leg, and great deficiency of power in the right. Careful examination failed to detect any symptom of disease in the viscera of the chest and abdomen; and Dr. Barker naturally drew the conclusion that the spinal cord was diseased in the lower part of the dorsal region, which seemed to be further indicated by the pain felt on percussing the lower dorsal and upper lumbar vertebræ. Little change in these symptoms took place until a few days before the patient's death; when the respiration became much more hurried, and there were indications of slight bronchitis in the right lung, and of obstructed respiration at the back part of the left, where also there was dulness on percussion; but there was no ægophony, nor did the change from the recumbent to the erect position occasion any alteration in the sounds heard, either on

percussion or auscultation, in the anterior parts of the chest. No other marked changes were observed during the patient's life, save that the respiration suddenly became much accelerated about two hours previously to his dissolution. On *post-mortem* examination, no appearance was found which at all accounted for the long-continued and severe pain, or for the paralysis; the only morbid change in the nervous centres being a white opaque thickening of the arachnoid covering the anterior and middle lobes of the brain, and a few white patches on the arachnoid of the spinal cord in the cervical region. The condition of the right lung and pleura was quite healthy; on the left side, however, the pleural cavity was found to be divided into two parts, by a line of adhesion continued from the root of the lung to the side of the chest, and then along its floor; this adhesion was firm in some parts, though evidently recent; in other parts it was very slight. The anterior portion of the pleura was perfectly healthy; but that lining the posterior pleuritic cavity was coated with a thin layer of loosely adherent granular lymph, and the cavity itself contained about three pints of dirty serum, mixed with granular matter, offensively fetid; there was also a considerable quantity of air. The lung itself was healthy, save at one spot at the lower and posterior part of the upper lobe, from which it was evident that a gangrenous slough had recently separated, whereby a small bronchial tube had been laid open. We quite agree with Dr. Barker in thinking it probable that this communication had only been formed shortly before death; and that it was through this that air had entered the posterior pleuritic cavity. As all the other viscera were healthy (with the exception of the heart, which exhibited slight indications of former valvular disease), and as the indications of pleuritis did not present themselves until after the abdominal pain had continued for many weeks, this pain seems quite inexplicable.

Dr. Barker refers to other cases that have fallen under his notice, which presented more or less of resemblance to this. In one of them (published by him in the 'Medical Gazette,' Nov. 10, 1843), the formation of a gangrenous perforation in the lung gave rise, within three days, to pleurisy with extensive effusion and pneumothorax, without any other previous signs of disease than slight sibilus and rhonchus, and without any such general disturbance consequent upon the occurrence, as usually leads to the detection of it. We cannot, therefore, doubt the possibility of the occurrence of the changes in the chest in the preceding case, during a very few days previously to death. In three other cases, again, Dr. Barker has met with the peculiar line of adhesion between the pulmonary and costal pleuræ, which in the recumbent position would be nearly horizontal; and he rightly (as we think) interprets this phenomenon as indicating that the effusion was not in the first place limited by pre-existing disease, but that the effused fluid having gravitated to the posterior part of the chest, the adhesions had subsequently taken place at the margin of its surface. Two other cases are referred to, in which all the symptoms of pleuritic effusion were present, excepting the absence of respiratory sounds; in one of them, *post-mortem* examination revealed the adhesion of the pulmonary and costal pleuræ into about twenty compartments, most of them communicating freely with each other; whilst in the other, the existence of such a subdivision of the pleural cavity was indicated during life by the fact that three distinct collections of pus were formed and drawn off.

We consider that Dr. Barker has done well to record those cases; and only wish that he had increased the value of his contribution by extending his comparative researches.

XII. *Case of Popliteal Aneurism treated by Compression; with some Remarks upon this Method of treating Aneurism, and a List of the Cases in which it has been employed in Dublin.* By O'Bryen Bellingham, M.D., F.R.C.S., &c.

This is a most valuable paper, and written in a truly modest and praiseworthy spirit, such as will no doubt advance the reputation of its author, as much as the estimation of the practice which it enforces.

Charles Maher, aged 42, an old soldier, having been recently employed in severe labour, complained in June, 1850, of pain in the sole of the foot, with swelling of the leg. In November of the same year he came under Dr. Bellingham's care, and then stated that he had felt pulsation in his ham for three weeks previously. On examination, an aneurism of the size of an orange was discovered, filling up the right popliteal space. It measured three inches from above downwards, and three and a half inches across, stretching the ham-strings and integuments over it. The pulsation and the *bruit de soufflet* were very distinct; pressure on the artery at the groin stopped the pulsation, and caused the tumour to collapse in a certain degree. The man's general health was good; and there was no reason to suspect the presence of any other vascular disease.

On the 30th November, the patient took a purge, and commenced the system of low diet, with rigid exclusion of all but a very moderate share of fluids. The diet consisted of two ounces of bread and two ounces of milk for breakfast; the same for supper; and the addition of two ounces of meat for dinner. On the 3rd of December, the pulse was smaller and softer, and the aneurism had diminished half an inch in circumference. The next day, at eleven o'clock in the forenoon, compression was commenced by means of two instruments, one on the artery as it crosses the horizontal ramus of the pubis, the other at the lower third of the thigh. At first the patient did not understand the object of the pressure, which was, to diminish the flow of blood through the tumour, without entirely stopping it, but never to permit any pulsation in it. In the evening, however, he was found clearly to comprehend the matter, and was therefore left to manage the instruments himself during the night, with the direction to alternate the points of pressure whenever pain was experienced, but never to allow any impulse in the aneurism. The next morning, at ten o'clock, on unscrewing the instruments, the tumour was found hard, solid, and circumscribed, without any pulsation in it. He remained in bed, and continued moderate pressure.

On the 10th of the month, it is reported that the instruments had not been applied for some days; but the aneurism was rapidly diminishing in size, being still quite firm and solid. The diet was now improved; and, on the 22nd, he sat up without inconvenience. On the 28th of the next month (January, 1851) the patient returned home cured of his disease.

Such an account as this of the cure of so terrible a disease as aneurism,

without danger to life, and even without pain, will sound to some of our elder brethren so like a fable, that we are glad to find a statistical table appended to the paper, giving the particulars of 36 cases of aneurism which have been similarly treated in Dublin by various surgeons since the year 1843.

Amongst them are six cases of femoral and twenty-six of popliteal aneurism; three cases of brachial and one of radial aneurism.

Of these, five of the femoral aneurisms were cured by pressure—the sixth was a diffused aneurism for which amputation was required; and Dr. Bellingham says that the ligature would equally have failed here.

Of the twenty-six popliteal aneurisms, twenty-one were perfectly cured; but one of these died suddenly of organic disease of the heart, forty-eight hours after the pulsation had ceased in the aneurism. Of the five remaining uncured, one patient returned to his employment before the pulsation had ceased; yet, though three years have since elapsed, he has never suffered any inconvenience from it. The second patient suffered ligature of the artery after the compression was discontinued; and recovered. In the third case, galvano-puncture was combined with the pressure; and the patient died of erysipelas. In the fourth, pulsation returned after pressure had stopped it for a time, and the limb was amputated. The fifth patient died of pulmonary disease; but the aneurism was found nearly filled with fibrine.

Two out of the three cases of brachial aneurism were cured by compression; in the third, there was a high bifurcation of the artery, which required the ligature of two vessels.

The radial aneurism, like the three brachial ones, was traumatic; and was cured by compression.

The following information is also afforded by Dr. Bellingham:

In three of the cases, the patients underwent the treatment by compression for aneurism in opposite limbs; one of them was treated three times —viz., twice for popliteal aneurism in opposite limbs, and once for femoral, each time with success. One of the patients had undergone the operation by ligature for popliteal aneurism in the opposite limb, some years previously.

Four of the patients have since died—two of aortal aneurism, one probably also of internal aneurism, and one of cerebral disease; another is at present labouring under aneurism of the aorta. Only two of the patients referred to were females.

In none of the cases marked cured by Dr. Bellingham, has there been any return of pulsation in the tumours; and there seems every reason to believe that the cure by compression is quite as permanent as that by the ligature; while no one, we should think, would prefer to use cutting instruments for treating any disease, if he can avoid it. We have been much struck with the modesty and propriety of the author's remarks upon the non-success which the plan of compression has met with in other places than Dublin; and we quite coincide with them.

It is unnecessary, however, for us to reproduce Dr. Bellingham's observations upon the precaution to be observed, and the form of instrument to be employed, in the treatment of aneurism by compression; as those of our readers who desire such information can easily refer to the original paper.

XIII. *Account of the Dissection of a Case in which two Popliteal Aneurisms had been treated by compression of the Femoral Arteries.* By Prescott Hewett.

A foreigner died in St. George's Hospital from the bursting of an aortic aneurism, whilst under treatment by compression for a popliteal aneurism in the right ham. He had also another aneurism in the left ham, which had been similarly treated in the Wexford Infirmary. In neither hospital does the apparatus employed seem to have been very perfect, or the pressure very sedulously employed. In both cases, however, the tumours diminished considerably in size; and the pulsation and swelling of the limbs lessened. After death, the man was found to have an exceedingly diseased arterial system, with many aneurismal dilatations.

The two popliteal aneurisms were blocked up to a considerable extent by coagula and fibrine; and the pressure had not produced any morbid effects upon the femoral vein or artery on either side.

XIV. *On the Relation of Sleep to Convulsive Affections.* By William F. Barlow, Resident Medical Officer to the Westminster Hospital.

Mr. Barlow is well known to have made many valuable contributions to the physiology and pathology of the nervous centres, from the clinical observation of disease; and in this paper he directs attention to a subject of great importance—the peculiar liability of convulsive affections to occur during sleep. This he attributes, and we believe with perfect justice, to the suspension of voluntary power, inducing a peculiar susceptibility in the nervous centres to be acted on emotionally, or by purely reflex stimulation. That many spasmodic movements occurring during sleep are to be attributed to the emotional state induced by unpleasant dreams, uncontrolled as this is by volition, we believe he is quite correct in affirming; and the illustrations he gives from observation of those disordered states in which the effect of emotions upon the movements of the body is peculiarly obvious, place this in a very striking point of view. One of these cases, showing the opposite influence of calm and of disturbed sleep, is as follows:

"A woman was affected with an almost perpetual tremor of the right arm and hand, which was extremely aggravated by emotion. I carefully noted that the arm and hand were completely still during perfectly sound sleep; but in imperfect or light repose there was a varying amount of tremor. The hand, no less in sleep than in wakefulness, became a delicate index of the condition of the mind. One night, when the patient was resting profoundly, I examined the hand as it lay by her side; not a muscle quivered; but shortly afterwards the slumber became manifestly imperfect, and then the hand and arm, influenced as it seemed by the emotion of dreaming, shook very forcibly. On a subsequent night I watched more minutely, and for a longer time. In calm sleep the hand lay in perfect rest, but anything which disturbed the repose served also to renew the tremblings. When it became very violent, she occasionally seemed about to awake, and even performed a voluntary act or two, and made a kind of complaining noise, as though annoyed by something; but instead of arousing completely, she relapsed gradually into her former complete unconsciousness, the slow subsiding of the tremors well marking its return. And so she would lie, without any movement of the voluntary muscles, the arm participating in the perfect quiet, until a noise partly discomposed her and renewed the tremors, which it was interesting to note, were frequently the only sign whatever of some degree of mental activity." (pp. 171, 172.)

In discussing the influence of the withdrawal of volition, Mr. Barlow lays great stress upon the principle which we hold to be of the highest importance, that when the activity of the will is suspended, there is a vastly-increased tendency to involuntary action of all kinds; so that, in fact, it is only after we have removed or paralyzed the seat of volition, that we can obtain experimental evidence of the independent reflex activity of the lower part of the nervous centres, whose ordinary operations are entirely under its control. This principle he illustrates by numerous examples drawn from the states of sleep, hybernation, paralysis, and coma, and also from the condition of the fœtus in utero; and he points out that even in the waking state, as in profound reverie, the withdrawal of the will leaves the same susceptibility. We believe that this principle is capable of yet more extensive application; being, in fact, the key to the explanation, not only of reflex or *excito-motor* actions in which sensation is not a necessary link, but also of those actions which became automatic by habit though guided by *sensations*, and of those, further, which are the expression of *ideas* suggested to the mind, without any emotional excitement.

We cannot at present, however, dwell longer upon this subject; and must only stop to mention that Mr. Barlow deduces from his theoretical conclusions some excellent practical hints, of whose value we entertain no doubt.

XV. *On Fatty Degeneration of the Placenta, and the Influence of this Disease in producing Abortion, Death of the Fœtus, Hæmorrhage, and Premature Labour.* By Robert Barnes, M.D. Lond.

We need scarcely remind our readers of the vastly-increased importance which the subject of fatty degeneration has assumed during the last few years; and it is very interesting, as well as important, to note the gradual extension of our knowledge of it, as the proximate cause of a large number of morbid phenomena. Our readers will remember that Professor Kilian has recently announced the discovery of this pathological change in the placenta (see vol. vii. p. 272); but two of Dr. Barnes's cases had been observed, and specimens transmitted to Dr. Hassall for examination, a year before he became acquainted, through our pages, with Professor Kilian's researches. In the first of these cases, flooding had come on during the sixth month of pregnancy, without any assignable cause; and recurred about three weeks afterwards, when premature labour followed, and the expelled fœtus presented every sign of having died some time previously. In the second case, premature labour, without hæmorrhage, had come on at the end of the seventh month, without any assignable cause; and the fœtus, as in the preceding case, appeared to have been for some time dead. In a third case, communicated to Dr. Barnes subsequently to the reading of his paper, a lady, when three and a half months pregnant, was subjected to considerable succussion; symptoms indicating the death of the fœtus followed, but miscarriage did not ensue; gestation went on to what would have been the full term, and the ovum was then expelled entire, presenting the grade of development which it would have attained at the fourth month, without any sign of putrefaction.

The placenta, in each of the first two cases, was studded, in different parts of the uterine or maternal surface, with fatty masses, varying in size from that of a pea to that of a walnut. These masses, however, were not circumscribed, like fatty tumours; and they obviously consisted of the placental substance itself, altered in various degrees by fatty deposit, or by actual degeneration. Where this deposit had taken place to the greatest extent, the substance was quite bloodless. The minute examination into its condition which was made by Dr. Hassall, showed that the diseased state chiefly affects the fœtal portion of the placenta, that is, the villi (or ultimate ramifications of the umbilical vessels) and the layer of chorion which invests them; and that it affects the maternal portion, which consists of the sinuses derived from the extension of the bloodvessels of the uterus, with their decidual investment, in a very subordinate degree. The fœtal villi are stated by Dr. Hassall to be thickly studded with innumerable minute spherules of oil; the chorion is thickened and destitute of its usual nuclei, having oil-spherules in its substance; the walls of the vessels no longer contain nuclei, but in their place are found spherules of oil, which are probably the products of their degeneration; and numerous spherules of oil are contained also in the space between the villi and the chorion. The cavities of the vessels are almost invariably free from fatty deposition; but they are also destitute of blood. Sometimes, however, the disease appears to have advanced still further, producing almost complete disorganization and disintegration of tissue; whilst in other parts of the placenta which present a normal appearance to the eye, the same changes may be detected in an earlier stage. It is to be noted, however, that a small quantity of fatty matter, in the form of minute spherules, is almost constantly present in the placenta, as we have ourselves also observed. In the third of the cases to which reference has been made, every part of the placenta was found to be pervaded by fatty deposit; but we are not informed whether this had advanced to the stage of disorganizing the normal tissue, and whether it was uniform throughout,—particulars which would have assisted in the discrimination of the source of the change. For it is obviously questionable, whether the fatty degeneration of the placenta was here the *cause* or the *effect* of the death of the fœtus; and we agree with Dr. Barnes in considering the latter to be the more probable view of the case, the conversion occurring subsequently, and thus taking the place of putrefaction, as in the *post-mortem* conversion of muscle into adipocire.

Dr. Barnes informs us that he has not unfrequently seen fatty masses in various stages of growth, in placentæ expelled at the full term of gestation; in these cases it is to be presumed that the balance of healthy placenta preserved was sufficient for the development of the fœtus. The rapidity with which this degenerating change may take place, as indicated by the occurrence of the whole series of changes before the seventh month of pregnancy, is another point of considerable interest; no similar data can be obtained with reference to the like changes in other organs. It seems probable, however, as Dr. Barnes remarks, that the state of pregnancy is one which specially predisposes to the production of fat.

The latter part of the paper gives a view of the bearings of this pathological inquiry upon obstetric practice; but we do not perceive that Dr.

Barnes has developed any other ideas under this head, than such as will occur to every well-informed reader.

XVI. *On some Secondary Physiological Effects produced by Atmospherical Electricity.* By C. F. Schönbein, Professor of Chemistry at Bâsle.

The object of Professor Schönbein's communication is to draw attention to the effects which atmospheric electricity secondarily exerts upon the living body by the production of ozone, and by its effects upon other substances; the notion that it directly exerts any powerful physiological influence being, according to him, no better than an hypothesis. On this point, however, no physiologist who has studied the relation of electricity to nervous force, and through it to other vital forces, is likely to accord with him.

Ozone, when first examined by Professor Schönbein, was considered by him as a peculiar peroxide of hydrogen; by Berzelius, De la Rive, and others, however, it has been regarded as a peculiar "allotropic" condition of oxygen; and Schönbein admits that his own later researches favour this view. It is generated in pure or atmospheric air, by passing electric sparks through it; and thus it is produced in the atmosphere, sometimes in considerable quantity, by thunder-storms and slighter electric discharges. This body is the most powerful oxidizing agent known to chemists; and it is highly poisonous to the living system, producing deleterious effects which are comparable to those of chlorine or bromine. It was early suspected by Professor Schönbein that the presence of an unusual quantity of ozone in the atmosphere might be a cause of catarrhal affections; and so far as his information extends, he has found it borne out in practice. It is, we presume, for the purpose of exciting observation and inquiry upon this point, amongst medical practitioners,* that he has brought this matter under the consideration of the Medico-Chirurgical Society, and that its Council has admitted the paper into its Transactions; for we can find scarcely anything in this first part of it, which has not been before the public for some years.

The second part of the paper contains some experiments that have been performed by him, with the view of determining the agency of ozone in the destruction of putrescent miasmata; and he certainly shows that, so far as *smell* can detect, they are most effectually neutralized by the ozoniferous atmosphere. So powerful, indeed, is this effect of the ozone, that ordinary atmospheric air charged with only 1–3,240,000th part of ozone is able to disinfect its own volume of air as fully charged as possible with the odour of putrescent meat. Hence he considers that the atmosphere, if sufficiently ozoniferous, will be speedily freed from miasmatic matters dispersed through it; but that a deficiency of ozone, or an excessive production of miasmata, may occasion an accumulation of these, so as to favour the propagation of epidemic diseases. He mentions, in corroboration of his view, that atmospheric ozone is most abundant in winter; but it must be remembered that during this period also there is least disengagement of putrescent emanations, in consequence of the low temperature; so that

* We are informed that the ozone test-paper may be obtained from Mr. Newman, the philosophical-instrument maker, in Regent-street.

the diminished tendency to the production of zymotic diseases observable during that season, cannot be fairly set down to the excess of ozone. His other observation is more to the point. He has remarked that the higher strata of the atmosphere are more ozoniferous than the lower; and as the ravages of many epidemic and endemic diseases are limited to a certain height above the level of the sea, it comes to be a very interesting inquiry, whether there is any such constant relation between the larger proportion of ozone in the higher localities, and their exemption from zymotic diseases, as would justify the conclusion that the latter may be attributed to the former.

As ozone may be easily produced artificially by the action of phosphorus on a limited amount of air, and as this artificial ozone appears to have all the properties of this natural atmospheric ozone, it may obviously be employed to great advantage as a disinfectant, should Professor Schönbein's expectations of its value be justified by more extended trials. It is with the view of calling the attention of our readers, and especially of such as are stationed in tropical climates, to this inquiry, which is obviously one of high importance, that we have placed before them this brief abstract of Professor Schönbein's views.

XVII. *On the Employment of the Heat of Electricity in Practical Surgery.* By John Marshall, F.R.C.S., &c.

Mr. Marshall is well known to have invented a very ingenious method of cauterizing fistulæ, by means of a platinum wire made hot by the agency of a galvanic battery. He gives a very interesting account of the reasons which induced him to resort to the plan, in the case of a man who had long laboured under an obstinate fistula in the cheek; and of the difficulties he encountered in bringing his invention to perfection.

We consider that he has added a very important means of cure to the very limited resources which surgeons have previously possessed for the treatment of these very troublesome and tedious cases; and that he deserves the thanks of the profession for this addition to its *armamentum*, the utility of which has, we understand, been abundantly proved since the publication of his paper. He states also that he has used the incandescent wire as a *cutting* instrument in the section of soft tissues; and considers that it will possess many advantages over the knife, scissors, or ligature, for the removal of redundant vascular parts. He promises the results of his experience on this point, however, in a future communication; and in the meanwhile we may remark that the same idea seems to have independently occurred to a foreign surgeon, for whom a claim of priority has been set up, but, as we believe, without any foundation. Another use to which we have seen the incandescent galvanic wire applied, is the illumination of dark cavities. The intense heat to which the platinum wire is exposed produces a dazzling white light, which can be thus usefully turned to advantage, especially in the exploration of the mouth and throat.

XVIII. *A Case of Strangulated Obturator or Thyroideal Hernia, successfully Relieved by Operation.* By Henry Obré.

This is a highly creditable and instructive case, related in a manner as excellent as its matter is good.

On the 21st of February, 1851, Mr. Obré met Mr. Gardener, of Lisson Grove, in consultation on a case of supposed internal strangulation. For three days, the patient, a stout female, 55 years of age, had suffered from all the symptoms of strangulated hernia, which were rapidly becoming aggravated, and threatening speedy dissolution. No swelling could be discovered in any of the usual situations of hernia; nor had the patient ever suffered from that affection. On carefully comparing the upper part of the right thigh with its opposite fellow, a *slight degree* of fulness was detected in Scarpa's triangle; and the whole limb looked fuller than natural. There was no distinct tumour, but on making firm pressure over the neighbourhood of the femoral artery, a little below the saphenous opening, "a distinct hardness could be felt (slight in its extent), giving an impression as if the sheath of the vessels were being pressed in."

In the belief that this tumour might be caused by constriction of the intestine deeply situated in the femoral canal, an incision was made in the situation of Scarpa's triangle, beginning about three inches below Poupart's ligament.

"The dissection was continued in the direction of the enlarged and hardened structure, until the cribriform fascia was opened, and the saphenous opening exposed, when some little disappointment was felt in not finding a portion of intestine confined there. The extremity of the index finger could now distinctly feel the hardened structure, deeply situated at the inner border of this opening; the dissection was resumed and continued with difficulty in so deep a part, and some embarrassment was caused by the saphenic vein, which passed through part of the wound, as did also branches of the anterior crural nerve. The fascia lata was divided, and the pectineus muscle exposed at the inner side of the wound, which it was found necessary to elongate, as it was impossible to continue the dissection at such a depth, unless the original incision were extended. The external fibres of this flat muscle were cleared from the surrounding structure, and divided transversely for about an inch and a half or two inches. I had only to separate with my finger some cellular tissue, when a portion of intestine, covered with its sac, and firmly held down by the other muscular structures that surrounded it, came into view. On being liberated, it suddenly ascended into the wound, being distended by flatus to the size of a pigeon's egg. Its true character was now discovered, and the finger, with some difficulty, from the depth at which it was situated, passed along the protruding intestine to the obturator opening, through which it had escaped. The narrow circumference of the foramen and the surrounding bones were examined both by Mr. Gardener and myself. The symptoms of strangulation having existed three days, it was considered prudent to open the sac, which contained a portion of the small intestine, blue and congested; and though the opening through which it had passed did *not tightly* inclose its neck, it was deemed prudent to divide its edge slightly, as some difficulty would have been found in returning the intestine without using pressure to empty it of its flatus. The extreme depth of the wound, added to the upward turn which the index finger was obliged to take in being used as a director to a blunt-pointed bistoury for the division of the stricture, was found the most difficult part of the operation, as well as to avoid various important vessels and nerves that surrounded the knife. Unfortunately, the saphenic vein not having been sufficiently held out of the way, was divided at the same time as the stricture; and from the extreme depth of the wound it was a matter of congratulation that no other serious mischief had taken place. The intestine having been returned as well as the sac, some little difficulty was found in securing the *upper* division of the vein, which was the only bleeding part. This being the only ligature requisite, the wound was closed by the necessary bandages, and the patient placed in bed.

No medicines were administered. As the bowels had acted three times in the course of the day, after the operation, an opiate was prescribed at bedtime." (pp. 236, 237.)

The patient subsequently rapidly got well.

This case Mr. Obré believes to be unique.

There are several instances on record, in which death has resulted from strangulated thyroideal hernia; but, if we except a doubtful case, reported by M. Arnaud, there is no example of an operation having been resorted to for its relief.

XIX. *Some Observations on the Pathology of those Affections of the Ear which produce Disease of the Brain.* By Joseph Toynbee, F.R.S., F.R.C.S. &c.

Mr. Toynbee still continues his laborious investigations into the Pathology and Treatment of Diseases of the Ear; and as we are not without hopes of ere long encountering his researches in a more complete and extended form, we shall for the present defer giving an account of the present paper, which treats only of a part, and that the pathological, not the remedial part, of a very important class of affections.

XX. *A Case of Obstruction of the Colon relieved by an Operation performed at the Groin.* By James Luke, F.R.C.S., Senior Surgeon to the London and St. Luke's Hospitals, &c.

A thin, temperate man, sixty years of age, applied to Mr. Luke, on the 16th December, 1850, complaining of constipation and a general feeling of illness. He was ordered to take ten grains of the compound rhubarb pill, and the next day some castor-oil, as the pills had operated very imperfectly. On the 18th there had been no relief to the bowels, but the abdomen had become tumid and uneasy, and the skin hot and the tongue foul. The stomach also rejected everything taken into it. A blister was applied to the pit of the stomach, and he took calomel and opium every four hours, as well as a brisk cathartic in the evening. By this and similar means, the more urgent symptoms were mitigated for a time; but the constipation still continued as obstinate as before. On the 21st, Dr. Munk attended in consultation; and agreed in thinking that there were strong grounds for supposing the existence of formidable intestinal obstruction, but that as yet it was premature to propose any operation. An elastic tube was passed into the rectum, to the extent of twelve inches, when its further progress was arrested; and warm water injected through it returned without bringing away any fæces.

On the 23rd, the symptoms were so much aggravated, that it was thought right to afford the sinking patient all the chances which surgical interference might give him. The chief points to determine were the seat of the obstruction, and the particular kind of operation to be performed. For the reasons which led Mr. Luke to the conclusion that the obstruction was situated about the sigmoid flexure of the colon, we must refer to his paper; confining ourselves to his description of the operation.

"As I thought it not prudent to assume that our conclusion respecting the seat of obstruction was certainly correct, I determined to adopt that operation which would at least give me some opportunity of extending my search, provided

I did not find the obstruction at the point where it was supposed to be, thinking that the increased probability thus afforded, of finding the obstruction, would be more than an adequate compensation for the little increased danger from peritoneal section. I therefore opened the abdominal parietes near the groin. Assisted by my colleague Mr. Wordsworth, Mr. Tomkins, and Dr. Munk, I made a nearly perpendicular incision, about four inches in length, a little to the outside of the course of the epigastric artery, the lower extremity of which incision terminated a little above Poupart's ligament. This part was rendered prominent by the distended intestine bulging forward, and was selected for the above reasons, as well as for the purpose of avoiding the epigastric artery. The abdominal muscles and fascias were divided in succession, and the peritoneum opened to the extent of about two inches. The colon, greatly distended, presented itself at the opening, with a considerable tendency to protrude; this, however, was obviated by the hand. When a finger was introduced, serous fluid, to the extent of several ounces, made its escape, and when the finger was passed along the surface of the intestine in a downward direction, it came in contact with a hard diseased mass, which appeared to encircle the intestine, and limit the extent of its distension. This was recognised to be a stricture. Having thus clearly ascertained the precise seat of obstruction, and its cause, I proceeded to open the intestine above the part obstructed. This was accomplished at the part which presented itself at the opening in the parietes, by means of a longitudinal incision through the tunics to the extent of about one inch. Through the aperture thus made, half a chamber-utensil full of fluid fæculent matter made its escape, after which the patient expressed himself much relieved. The finger was next introduced, through the intestinal aperture, towards the rectum, when it was ascertained that the colon had been rendered impervious by the stricture, about two inches from the aperture. Having thus satisfactorily accomplished the object of the operation, the wound was partially closed by two sutures, the lowest of which was passed through one of the appendices epiploicæ which lay conveniently for the purpose; this being done with the intention of securing a correspondence between the intestinal and parietal apertures, for the more ready discharge of fæculent matter through the wound. The surface was loosely covered with lint, and the patient replaced in bed. He was but little exhausted by the operation, and did not sustain a greater exposure of the peritoneal surface than is common in the ordinary operation for strangulated hernia. Twenty drops of tr. opii were ordered to be taken immediately, and brandy-and-water and beef-tea occasionally." (pp. 267, 268.)

The further progress of the case was almost uninterruptedly good. In a short time the fæces began to pass through the natural passage; thus showing that of whatever character the obstruction might have been, it was not organic or permanent, although its exact nature remained undetermined. At the date of the latest report—namely, the 20th of August, 1851—the patient is stated to be engaged in his ordinary occupation, that of a wine-cooper, which he pursues without inconvenience; as a truss effectually prevents the escape of fæces from the small fistula still remaining at the groin, and the greater part of them pass by the anus.

Mr. Luke concludes his paper with some observations on intestinal obstructions generally; and gives the preference in most cases to Littré's operation of opening the abdomen in the groin, over that advocated by Amussat in the loin.

XXI.—*On the Variations of the Sulphates and Phosphates excreted in Acute Chorea, Delirium Tremens, and Inflammation of the Brain.* By H Bence Jones, M.D., A.M., F.R.S. &c., Physician to St. George's Hospital.

This is another of Dr. Bence Jones's elaborate contributions to the chemistry of the urine; and gives the results of inquiries which have been prosecuted subsequently to his former communications to the Medico-Chirurgical Society and to the Royal Society, on the products of the oxidation of the muscular and nervous tissues. The determination of the amount of the sulphates and phosphates was made in 148 specimens of urine from 58 subjects; the diseases to which most attention was given being *acute chorea,* as the type of the class in which the muscular structures chiefly were affected; *delirium tremens,* as the type of functional disorders of the nervous centres; and *inflammation of the brain,* as the type of acute structural diseases of the nervous system. A selection of the most interesting cases of each class is given by Dr. Jones; we can only find room for the summary of results.

The phenomenon common to acute chorea and to delirium tremens is increased and unceasing muscular action, the result of which is an increase of the sulphates and of the urea in the urine; just as in health they would have increased if continued exercise had been taken. The phosphates were found to be diminished in acute chorea; but this diminution was not greater than could be accounted for by the complete abstinence from food. A still more marked diminution was observable in delirium tremens; and this, as remarked by Dr. Jones on a former occasion, is too great to be thus accounted for, and seems to indicate that the irregular action of the brain is connected with *deficient* oxidation.* Dr. Jones's continued investigations have confirmed him in the conclusion that in inflammation of the brain the phosphates are considerably increased; and they lead to the belief that the sulphates also are raised above the average amount. This is what might be expected, when it is borne in mind that the brain contains a considerable amount of albuminous as well as of fatty matters; and that the sulphur of the former will undergo oxidation, at the same time, and under the same circumstances, as the phosphorus of the latter.

XXII.—*Account of a Case in which a large Cyst containing Hydatids was developed at the Root of the Neck; death ensuing from Rupture of the left Subclavian Artery.* By James Dixon, Surgeon to the Royal London Ophthalmic Hospital.

An hydatid cyst may, of course, be developed in any situation, and give rise to symptoms which are interesting, principally, on account of their obscurity. Regarded as surgical puzzles, the value of such cases is comparatively small; and after the mystery is once revealed, depends in great measure upon the manner in which the symptoms have been appreciated

* May not this be the direct result of the impregnation of the blood or of the nervous matter with alcohol? The experiments of Prout, Vierordt, and others, tend to show that the normal amount of carbonic expired is diminished for a time after the ingestion of fermented liquors, and then rises considerably; whilst many other observations agree in stating a venous hue of arterial blood as one of the results of their continual employment in large quantity; both sets of facts concurring to prove that the presence of alcohol in the blood tends to obstruct the elimination, by the oxidizing process, of the products of the "waste" of the system.—REV.

by the practitioner to whom the case has occurred, and the fidelity with which they are transcribed and illustrated. It will excite no surprise in the reader's mind to learn that Mr. Dixon has made the most of the exceedingly interesting case here related; and we shall not attempt to shorten his impressive narrative, lest in so doing we weaken its effect, and reduce what is really a very good lesson in medical reasoning to the rank of a mere curiosity.

XXIII. *A Case of Aneurismal Dilatation of the Posterior Tibial Vein, communicating indirectly with the upper part of the Popliteal Artery.* By Edward Cock, Surgeon to Guy's Hospital.

Not only is this case probably unique in its character, but it derives additional interest from the circumstance that its real history extends over a period of eleven years, and that its result proves the bad effects which, even after so long a period has elapsed, may follow the violation of that sound rule in surgery, which requires that a wounded artery shall be searched for and tied at the seat of the injury, and not in its main trunk. For the purpose of shortening the narrative, we shall give the history of the events in the order in which they occurred.

In October, 1839, George Mortimer, a lad of eleven years old, received a wound in the popliteal space from a worn and sharpened dinner-knife. The external wound was small, and the flow of blood so slight, as to give the impression that no vessel of importance had been injured. A compress was applied, and the limb bandaged. For the time the bleeding ceased, but returned every day or two in small quantity. On the twelfth day the edges of the wound had not united, and on making moderate pressure with the finger, a small-sized coagulum slipped out, and a moderate flow of clear blood followed. "Now," says the surgeon, who was in attendance, in his letter to Mr. Cock, "I determined to take the bull by the horns, and accordingly tied the femoral artery, the ligature round which instantly stopped the hæmorrhage." That this was not taking the bull by the horns, the loss of the patient's limb, and very nearly of his life, eleven years afterwards, is a sufficient proof, even if the established rules of surgery did not proclaim the same thing.

We doubt if there is a hospital surgeon in London, who will not agree in condemning the course here followed of tying the femoral artery, instead of searching out the source of the hæmorrhage, and placing a ligature on the wounded vessel itself.

The immediate result of the operation was favourable, and for several years the patient experienced no ill effects from his accident. In October, 1850, however, he applied at Guy's Hospital for a painful and swollen state of the left leg, which had supervened on an attack of fever.

The principal enlargement was at the back of the leg, where deep-seated fluctuation was very distinct; but the circumstance that the femoral artery had been ligatured some years before, induced Mr. Cock to pause before making an incision into what he thought could be nothing else than an abscess. Examination disclosed nothing to modify this view; the pulsation in the femoral was natural, and so was that in the arteries of the foot, and there was not the slightest trace of pulsation or of murmur in the swelling itself.

On the 30th October, therefore, an incision was made into the swelling, and two or three ounces of dark grumous blood discharged, which had evidently been extravasated a long time, and had lost the power of coagulation.

The day but one afterwards, a copious discharge of pus occurred, mixed with half-decomposed coagula, and this continued to flow, improved however in character, for the next fortnight. Suddenly, on the 14th of November, the wound burst out bleeding, and before assistance arrived, the man lost three pints of arterial blood; and the sac of the abscess filled with blood, burst, and the fluid gradually effused itself through the popliteal space into the thigh. Under these circumstances, Mr. Cock, for obvious reasons, declining to place a ligature on the femoral or iliac artery, and the patient being in too debilitated a condition to bear the tedious operation of searching for the bleeding vessel in the calf, determined to amputate the limb above the knee.

This was done, and after a tedious confinement and a narrow escape from phlebitis, the patient at last got well, and is now filling the situation of gate-keeper at the Custom-house. A careful dissection of the limb disclosed the following remarkable condition:

The gastrocnemius was very thin, and beneath it existed a large diffused collection of semi-coagulated blood. On washing this away, an aneurismal sac, about the size of a duck's egg, was brought into view; and it was seen that this sac had given way by an ulcerated opening at the upper part near the entrance of a vessel.

For the minute description of the condition of the vessels, we must refer to the original paper; and must content ourselves with quoting the enumeration of the several facts connected with this case, which were brought to light by the dissection.

"1st. That the popliteal vein and artery had both been wounded by the original injury eleven years ago, and that a permanent communication had become established between the two vessels, maintained through the intervention of a small sac common to both.

"2ndly. That the popliteal vein above the seat of injury had become obliterated and was obstructed up to the line of amputation. That it probably regained its permeability at its junction with the saphæna minor vein, which vessel appeared to have returned the greater part of the blood from the leg.

"3rdly. That the popliteal vein below the seat of injury had become dilated and thickened, apparently from the impulse of the blood received into it from the artery, and that it terminated below in aneurismal dilatation of the posterior tibial vein, while all the venæ comites of the leg had become completely obstructed and obliterated.

"4thly. That at some subsequent period the walls of the venous aneurismal dilatation must have given way, allowing the extravasation of a certain amount of blood beneath the gastrocnemii muscles.

"5thly. That in consequence of or at any rate connected with this extravasation of blood, was the occurrence of an abscess, which finally, after the lapse of eleven years, again brought the patient under surgical treatment.

"Thus it will appear that the history of this case extended over a period of eleven years; the original wound and the final abscess constituting the commencement and termination." (pp. 333, 334.)

XXIV. *On a New Method of Treatment applicable to certain Cases of Epiphora.* By William Bowman, F.R.S., Professor of Physiology and of General and Morbid Anatomy in King's College, &c.

In this elegant communication, Mr. Bowman directs attention to the inconveniences resulting from displacements outwards of the puncta lacrymalia, and to closure of the puncta or canaliculi following mechanical injury or ulceration, and suggests an operation for their relief.

In the most aggravated examples of this displacement—namely, in ectropion—the stillicidium lacrymarum frequently subsides in a marked degree, in consequence of the diminished sensibility of the everted mucous surface of the lid, by exposure to the atmosphere; and no operation may be called for on this account.

Sometimes, however, a slight chronic inflammation of that part of the conjunctiva lying near the punctum, produces thickening and eversion of the punctum, or the same result is brought about by a sort of chronic eczema of the lower lid, which produces general, though moderate contraction. These conditions require attentive examination to detect them, but they produce, trifling as they seem, all the distress which is occasioned by constant lacrymation; and the reason is thus stated by Mr. Bowman. We recommend the argument to the next writer on natural theology, as being one of the most pleasing examples of design ever adduced in support of an All-wise Designer:

"It is a remarkable fact, that an extremely slight displacement outwards, will destroy the function of the lower punctum, and the following considerations are offered in explanation of it. The puncta are naturally so placed, as to be either altogether on the conjunctival aspect of the lid (as in some of the lower animals), or else (as in man) at the very margin at which the skin and conjunctiva blend. Now skin differs from mucous membrane, in the superficial layers of the cuticle being rendered greasy by the sebaceous secretion, so that they throw off water as greased paper does; whereas, the corresponding part of mucous membrane is moist, and water adheres to it. To apply this to the eyelids, and their relation to the passage of the lacrymal fluid:—the tears have to be directed towards the puncta, and to enter these orifices; and if the skin were not greasy up to the margin of the lids, the tears would be very apt, indeed would be certain, to ooze over the tarsal margin on to the cheek. The skin of the lids is, however, exceedingly delicate and thin, and deficient in sebaceous follicles. But to supply this want, there is a great development of sebaceous glands, placed so as to pour their secretion on the margin of the lid, close upon the line of junction of the skin with the mucous membrane—on that part where there must be a constant tendency for the moisture of the mucous membrane to soak through and wet the surface of the cuticle, and yet where it is so essential to the retention of the tears, that the skin should be greasy. So that I regard the Meibomian glands as existing, not for the purpose, as stated in anatomical works, of preventing the agglutination of the lids when closed (which would be more likely to happen here, without sebaceous matter, than it is to occur between the lips of the orifice of the urethra), but for the purpose of maintaining that greasy state of the surface of the cuticle at the margin of the lid, which prevents the tears from escaping over the cheek. And I may add, that the probable use of that sebaceous gland, called the caruncle, is to throw the tears into a little pool above it, where they may be taken up by the puncta; for even the lower punctum glides above the caruncle in the winking movements of the lids. Now it appears, that the punctum, though situated on the confines of skin and mucous membrane, partakes only of the character of the

latter. Its margin is always naturally moistened by the tears, and is not greasy. The mucous surface on its inner side, over which the tears approach it, is also moist and conjunctival. Now in the cases to which attention has been called, the punctum has its margin greasy and cuticular, as a consequence of its displacement and exposure, and the membrane on its inner side is similarly changed, so that the tears are prevented from coming up to it, and do not wet it, but collect in a drop at the caruncle. And if the punctum is pushed back into contact with the tears, its margin instantly throws off the moisture, and cannot be wetted by it. In some cases, a margin of $\frac{1}{60}$th of an inch of greasy membrane on the conjunctival side of the punctum, is sufficient to destroy its function." (pp. 339, 340.)

The remedy for these evils accidentally suggested itself to Mr. Bowman whilst examining the eye of a female who, many years before, had torn the lacrymal canal completely across, and in whom the orifice still remained open. It occurred to him that a similar division of the lacrymal canal might afford a new way into the lacrymal sac in some cases of epiphora; while experience showed him that such an orifice in the canaliculus would probably not contract, but remain permanently open. He accordingly put his idea to the test in a case of eversion of the punctum consequent on eczema of the lower lid; and having introduced a probe into the punctum, and then brought it out through a slight wound made in the conjunctival surface of the canal, he slit-up the canal for a short distance towards the punctum. This operation established a sort of artificial canal or drain, leading into the sac, which answered admirably so long as it remained open. Some difficulty was experienced in maintaining its patency; but on repeating the operation in other cases, Mr. Bowman has found it sufficient to slit-up the canal from the punctum, and to break down the newly-formed adhesions with a probe, for two or three times in succession after they have formed, after which this artificial entrance remains permanently open. He suggests, also, that the same operation may be resorted to in cases of obstructed puncta or canals, where the point of obstruction is sufficiently far from the sac to allow the canal to be slit-up through the conjunctiva, as it is essential that the canaliculus itself should furnish the channel.

Art. XIII.

The Principles and Practice of Obstetric Medicine and Surgery, in Reference to the Process of Parturition. With One Hundred and Twenty Illustrations on Steel and Wood. By Francis H. Ramsbotham, M.D., F.R.C.P., &c. &c. Third Edition, enlarged.—*London*, 1851. 8vo, pp. 726.

It is quite superfluous for us to dwell upon the merits of a treatise so well and favourably known to the profession as the one before us. Though we are far from admitting that extent of sale is in itself a valid criterion of the value of any literary or scientific production, yet we believe that, in the case of a systematic treatise like the present, it is a fairer test than in most other instances; for the profession is scarcely likely to have invested upwards of four thousand five hundred pounds in the purchase of as many copies of a somewhat ponderous volume, except in the belief that it was receiving an equivalent in value; and any deficiency in this would soon

have been discovered. Doubtless the marvellous cheapness of the volume, the number and beauty of its illustrations being considered, has had much to do with its extended sale; but we have a better opinion of the discrimination of our brethren, especially in practical matters, than to believe that these alone would have sufficed to win so substantial a testimony of its approbation.

It will be unnecessary for us, therefore, in announcing the appearance of a new edition of this work, to do more than briefly notice the principal novelties which it contains; and we wish that our remarks upon these could have been equally favourable with our general appreciation of Dr. Ramsbotham's merits as a practical Obstetrician. We trace, however, rather too strong a conservative element in his character, and too little disposition to allow merit to the reasoning or the practice of others, if it happens to be in opposition to his preconceived views. In illustration of our meaning, we refer to his comments upon the practice of detaching the placenta, in cases of placental presentation, which, though by no means one which we would commonly recommend, has been proved, we do not hesitate to say, to be by far the best that can be adopted in appropriate cases, and has saved many lives. Now his objections to this practice are rather theoretical than practical, and are founded upon a notion of the structure of the placenta, which we feel bound to characterize as entirely mistaken. This notion prevents him from subscribing to the idea put forth by Dr. Simpson, as the foundation of the practice, that the hæmorrhage, in cases of partially-separated placenta, proceeds from the placental rather than from the uterine surface; and he does not admit that the practice *can* be successful, because, according to his notions, it *ought not* to be successful. Thus he says (p. 405), "it appears to me . . . . . that leaving the child *in utero*, after the placental mass was withdrawn, to be expelled by the natural powers, would be to run the risk of a persistence of the hæmorrhage, and of the woman's sinking under any additional discharge that might ensue." Now the *fact* is, that in by far the greater number of such cases, the removal of the placenta has been found the most effectual method of inducing efficient contraction of the uterus, the closure of its bleeding orifices (for we by no means hold, or believe that Dr. Simpson holds, that *all* the hæmorrhage comes from the placenta), and the favourable termination of the labour. And even if the theory of placental hæmorrhage were quite wrong, the practice might on this account be the most useful that could be resorted to. Strange to say, however, Dr. Ramsbotham does not make the slightest reference to the large number of cases that have now been put on record by different obstetricians, attesting the value of the method; and he therefore gives his readers no opportunity of judging for themselves.

Dr. Ramsbotham tells us in his Preface, that in this edition he has "entered more fully into the connexion between the maternal and fœtal systems, through the intervention of the secundines;" and we naturally looked to this additional matter, as likely to contain a complete and accurate account of the structure of the placenta. In this, however, we have been completely disappointed. Dr. Ramsbotham appears to have taken no note whatever of the recent progress of this department of investigation, especially as regards the *development* of the placenta; and while he admits,

as he cannot help doing, the *closed* nature of the entire fœtal system of umbilical ramifications, denies that the blood passes into or out of the placental cells by vessels prolonged from those of the uterus, but avers that the cells of the maternal surface of the placenta are applied against the patulous orifices of the uterine sinuses. He grounds his assertions upon what we must take leave to consider a very coarse mode of dissection; and does not seem to have taken any pains to ascertain what is the nature of the "innumerable small, excessively tender, filamentous bands of communication between the two surfaces," which are to be observed when one is detached from the other. "Some of these," he continues, "*appeared to be* the fibrous bands of the easily lacerable decidua; whilst others, that were drawn out to a greater extent, *were no doubt* the minute arteries ramifying in that membrane." (p. 69.) Now we will inform Dr. Ramsbotham that we can fully confirm Dr. Reid's assertion, that some of these are *tubes* forming a communication between the uterine arteries and the interior of the placenta, the "curling arteries" of Hunter, whilst others are the prolonged fœtal tufts which dip down into the uterine sinuses; and that a complete *closed* connexion exists between the subdivided cavity of the placenta and the uterine sinuses, by the continuity of the inner membrane of the latter throughout the whole interior of the former. When he shall have occupied *weeks* in the dissection of a single attached placenta, under water, as we know that Dr. Reid did, and when he shall have watched the development of this organ with the same care and attention as that which has been bestowed upon it by Professor Goodsir, we shall be ready to attach some weight to his opinion; for the present he must excuse us if we credit the testimony of our own eyes, and give our confidence to men who have peculiarly distinguished themselves in anatomical research; confirmed as their statements have been, on all essential points, by every anatomist who has seriously applied himself to test their validity. We cannot help expressing our regret that teachers of Dr. Ramsbotham's eminence should take so little pains to examine into the present state of knowledge, on points as to which they cannot possess, from the very nature of their ordinary avocations, adequate opportunities of forming a judgment; and should commit the very common error of summoning up old authorities, against new observations that have been made in methods which the "wisdom of our ancestors" never dreamed of.

Another of Dr. Ramsbotham's principal additions consists in a discussion of the whole subject of anæsthetics, and of their administration in surgery as well as in midwifery. No fewer than thirty-four pages are thus occupied; and we cannot but think that so controversial a view of the subject as is here taken, might have found some more appropriate place of expression than the pages of a text-book. Dr. Ramsbotham is an unmitigated opponent of the use of anæsthetics for almost any purpose whatever in obstetric practice, and takes the following as the basis of his objections:—"Before anæsthetics can be introduced for the relief of the ordinary pains of childbirth, it must be proved incontestably that they are *invariably* safe both to the mother and her infant." (p. 183) In the ordinary operations of obstetric medicine, too, he would "reject the hazardous co-operation of anæsthetic vapours, as in the more common and natural cases." (p. 182.) And he mentions only one or two possible contingencies, to which he can

imagine them applicable. Most of his objections have been already replied to by anticipation in a previous article; and with regard to the one on which he evidently lays most stress, we would simply say, that if invariable safety is to be the rule of medical, surgical, or obstetric procedures, we know few indeed of the more potent methods of interference that could be practised under its sanction. We were curious to see how our author would carry out this rule in regard to ergot of rye, of whose use he has been long one of the most zealous advocates; and we found to our surprise that he not only dismisses the whole subject of its use within three pages of his text, but makes no mention whatever of the evil influence which it has been proved to exert over the life of the child—an influence which has induced many distinguished obstetricians to abandon it altogether, except for the sake of inducing uterine contraction after the birth of the child, or when the child is known to be already dead, or when some imminent peril to the mother justifies the employment of means that may be fatal to the offspring.* The subject, however, is not altogether passed by; for Dr. Ramsbotham returns to it in an Appendix, to which, however, no reference is made in the text as containing anything but a "History of the Ergot," and which, therefore, might easily escape the reader's notice. He *there* brings together a considerable body of evidence, that the administration of the ergot is hazardous to the child; and yet not one word of caution on this point is given in the text; nor, in spite of the positive danger which has been proved to attend its use, does our author banish this remedy, like anæsthetic agents, from his *armamentum obstetricum.*

Having thus discharged our consciences by commenting upon these important drawbacks from the value of Dr. Ramsbotham's treatise, it is pleasant to be able to conclude with a notice of the very valuable statistics which are appended to it. The total number of cases on which these are based, is now not less than 68,435; all of them having occurred at the Royal Maternity Charity between January 1820 and December 1850, under the superintendence of Dr. Ramsbotham, sen. and jun. For these, however, we must refer to the work itself; which will always sustain its character as that of a thoroughly practical man, who lets his readers know without reserve the opinions which he has drawn from a large experience; and which will, we trust, in a future edition, be brought up more fully to the present state of physiological science.

* See our second volume, p. 479.

PART SECOND.

Bibliographical Notices.

ART. I.—1. *The Life of the Hon. Henry Cavendish, including Abstracts of his more Important Scientific Papers, and a Critical Inquiry into the Claims of all the alleged Discoverers of the Composition of Water.* By GEORGE WILSON, M.D., F.R.S.E.—*London*, 1851. 8vo, pp. 478.

2. *Physiological Chemistry.* By Professor C. G. LEHMANN. Translated from the Second Edition by GEORGE E. DAY, M.D., F.R.S., F.R.C.P., and Professor of Medicine in the University of St. Andrews.—*London*, 1851. 8vo, pp. 455.

WE have on a former occasion made known to our readers the plan and objects of the Cavendish Society, which is doing for chemical science what the Sydenham Society accomplishes for medical literature. But we are induced more particularly to direct the attention of our readers to its objects and deeds, at the present time, in consequence of having had for some months before us two volumes of the last year's issue, which are of such peculiar interest, that we should not be discharging our duty to our readers, if we did not bring them specially beneath their notice. One of these is a translation, by Professor G. E. Day (whose competence for such a task was evidenced by his translation of Dr. Simon's 'Animal Chemistry' for the Sydenham Society), of the very important work of Professor Lehmann, of which as full an account as our limits permitted has recently appeared in our own pages. The progress of research in this department is so rapid, that Professor Lehmann's treatise must be regarded as having completely superseded that of Simon; and all who desire to possess a systematic work on Physiological Chemistry by a man who is thoroughly qualified, both by his physiological and chemical acquirements, by his own eminence as an experimentalist, and by the philosophic impartiality of his habits of thought, to afford a comprehensive and exact view of its present aspect, should lose no time in attaching themselves to the Society by which it is in course of publication.

Like the "Sydenham," the "Ray," and other publishing societies, the "Cavendish" has felt itself bound to do special honour to the man whose illustrious name it had assumed; and the task of bringing together the few memorials which have been preserved of his personal history, and (what is far more important) of following out a critical inquiry into his scientific labours and merits, has been undertaken by Dr. George Wilson; than whom it is probable that no more appropriate individual could have been selected, combining, as he does, high literary ability with thorough scientific com-

petency, and holding truth in so much honour as to allow no personal predilections to warp his judgment or bias his verdict. Our readers can scarcely be ignorant that the question of the real discoverer of the composition of water is one which has been frequently and keenly discussed, from the very date of the discovery down to the present time. Cavendish, Watt, and Lavoisier, have been brought forward as rival candidates for the honour; and men no less eminent in science and letters than Arago, Dumas, Brougham, Brewster, Jeffrey, Harcourt, Whewell, and Peacock, have enlisted themselves as champions of their respective causes. The spirit in which Dr. Wilson has entered upon his very difficult task will be best appreciated from the following extract from his Preface:

"It was open to me to write as a partizan, as an advocate, or as an historian. I have chosen the last character as the only befitting one. I do not pretend to bear witness to my own impartiality, of which others must be the judges, but I can at least testify to the spirit in which I have sought to write; and candid readers, I think, will acquit me of partizanship. The conclusions to which I have come in reference to Cavendish's priority and merits as a discoverer, and his integrity as a man, are such that I can rank myself amongst his most hearty admirers and defenders. Had I written, however, *only* as his advocate, I should have left much unnoticed which I have recorded. Thus I have been at pains to point out the defects of his theories, as well as their excellencies, and to indicate the merits of his rivals, as well as their faults. The reputation of Lavoisier, and of Watt, is as sacred a thing in my eyes as that of Cavendish; and I should be the first to regret if the tone of this work should seem at variance with the catholic spirit of esteem for all great philosophers, which is an essential element of vitality in associations like the Cavendish Society. Whilst thus, however, I have endeavoured to be impartial, and to make the biography a faithful sketch, not a eulogy, I have deemed it an essential part of my duty as a biographer to vindicate the moral character of Cavendish from even the shadow of suspicion. It has been impossible to do this, without censuring those who have called his good name in question. If in uttering censure I have forgotten what is due to great authorities in literature and in science, even when they are in error, I shall deserve and bow to reproof; but if I have only reluctantly fulfilled an imperative though invidious duty, and have justified my censures by showing that they are deserved, I shall hope to be vindicated at the hands of my readers." (pp. vii. viii.)

We believe that we not only express our own opinion, but give expression to the unanimous judgment of such as have carefully read this volume, that Dr. Wilson has fully acted up to the intentions which he has professed; and that although those who had previously formed their opinions may neither agree with him in his conclusions, nor approve of the mode in which he has arrived at them, it is scarcely possible for such as have kept themselves open to conviction, to avoid concurring in the former, and giving him credit for the highest motives in the adoption of the latter.

We had fully intended placing a short summary of this discussion before our readers; but a press of other important topics has hitherto prevented us, and we do not wish to delay any longer our notice of the work. We must therefore satisfy ourselves with presenting them with Dr. Wilson's portrait of the very singular character whom he delineates; a character which is interesting alike as a psychological study, and in its relations to the nature of the scientific inquiries to which Cavendish devoted himself. Of the general scope and nature of his pursuits, we are told—

"Few of our men of science have been so catholic in their tastes as Cavendish, so far at least as physics are concerned. He was an excellent mathematician, electrician, astronomer, meteorologist, and geologist, and a chemist equally learned and original. In the fullest sense of the term, indeed, he was a natural philosopher, and had he published during his lifetime all the researches which he completed, his reputation would have been much wider and more varied even than it was. He was exactly the opposite of a certain class of thinkers, whose fertility of invention, and skill or success in research, are far below their desire of distinction, and who are diligent in coining every thought, though it be but a farthing's worth, so as to put it into immediate circulation. Such men have nothing to reveal in private; the public are already in possession of all they know. Cavendish, on the other hand, dealt with his discoveries as with his great wealth, and allowed the larger part of them to lie unused in his repositories. His published papers, accordingly, give but an imperfect notion of the great extent of ground over which he travelled in the course of his investigations, and of the success with which he explored it." (p. 19.)

His personal character is strikingly portrayed in the following extract:

"The account I have given of him has necessarily assumed the character of a Mosaic, made up of fragments furnished by different hands. I have thus supplied each reader with the means of drawing a likeness for himself, and it only remains that I offer very briefly my own estimate of the character of the Philosopher. Morally it was a blank, and can be described only by a series of negations. He did not love; he did not hate; he did not hope; he did not fear; he did not worship as others do. He separated himself from his fellow men, and apparently from God. There was nothing earnest, enthusiastic, heroic, or chivalrous in his nature, and as little was there anything mean, grovelling, or ignoble. He was almost passionless. All that needed for its apprehension more than the pure intellect, or required the exercise of fancy, imagination, affection, or faith, was distasteful to Cavendish. An intellectual head thinking, a pair of wonderfully acute eyes observing, and a pair of very skilful hands experimenting or recording, are all that I realize in reading his memorials. His brain seems to have been but a calculating engine; his eyes inlets of vision, not fountains of tears; his hands instruments of manipulation, which never trembled with emotion, or were clasped together in adoration, thanksgiving, or despair; his heart only an anatomical organ, necessary for the circulation of the blood. Yet, if such a being, who reversed the maxim 'nihil humani me alienum puto,' cannot be loved, as little can he be abhorred or despised. He was, in spite of the atrophy or non-development of many of the faculties which are found in those in whom the 'elements are kindly mixed,' as truly a genius as the *mere* poets, painters, and musicians, with small intellects and hearts and large imaginations, to whom the world is so willing to bend the knee. He is more to be wondered at than blamed. Cavendish did not stand aloof from other men in a proud or supercilious spirit, refusing to count them his fellows. He felt himself separated from them by a great gulf, which neither they nor he could bridge over, and across which it was vain to stretch hands or exchange greetings. A sense of isolation from his brethren made him shrink from their society and avoid their presence, but he did so as one conscious of an infirmity, not boasting of an excellence. He was like a deaf mute sitting apart from a circle, whose looks and gestures show that they are uttering and listening to music and eloquence, in producing or welcoming which he can be no sharer. Wisely, therefore, he dwelt apart, and bidding the world farewell, took the self-imposed vows of a Scientific Anchorite, and, like the Monks of old, shut himself up within his cell. It was a kingdom sufficient for him, and from its narrow window he saw as much of the Universe as he cared to see. It had a throne also, and from it he dispensed royal gifts to his brethren. He was one of the unthanked benefactors of his race, who was patiently teaching and serving mankind, whilst they were shrinking from his coldness, or mocking his peculiarities. He could not sing for them a sweet song,

or create a 'thing of beauty' which should be 'a joy for ever,' or touch their hearts, or fire their spirits, or deepen their reverence or their fervour. He was not a poet, a priest, or a prophet, but only a cold, clear Intelligence, raying down pure white light, which brightened everything on which it fell, but warmed nothing—a Star of at least the second, if not of the first magnitude, in the Intellectual Firmament.

"His Theory of the Universe seems to have been, that it consisted *solely* of a multitude of objects which could be weighed, numbered, and measured; and the vocation to which he considered himself called was, to weigh, number, and measure as many of those objects as his allotted threescore years and ten would permit. This conviction biassed all his doings, alike his great scientific enterprises and the petty details of his daily life." (pp. 185, 186.)

We must take leave to question, however, whether Cavendish ever thought at all about the matter. It seems to us rather as if he weighed, numbered, and measured, just as Mozart wrote music, because it was his *nature* to do so, rather than with any conscious estimate or settled conviction of his peculiar vocation. Putting aside the first sentence in the following passage, which implies a greater amount of "ideality" than we think that Cavendish possessed, we think that the remainder shows a very just appreciation of his psychical nature.

"Whatever, accordingly, we may think of the ideal which Cavendish set before him, we must acknowledge that he acted up to it with undeviating consistency; and that he realized it to a far greater extent than most men realize the more lofty ideals which they set before them. The pursuit of truth was with him a necessity, not a passion. In all his researches he displayed the greatest caution, not from hesitation or timidity, but from his recognition of the difficulties which attend the investigation of nature; from his delight in reducing everything to numerical rule, and his hatred of error as a transgression of law. *Cavendo tutus* was the motto of his family, and seems ever to have been before him." (pp. 189, 190.)

Art. II.—*The Morbid Conditions of the Pulmonary Artery.* By Norman Chevers, M.D., Civil Assistant Surgeon, Chittagong, Bengal.—*London*, 1851. 8vo, pp. 137.

This work, reprinted from the 'Medical Gazette,' in which it appeared in the form of a series of papers, is a monograph on the diseases, congenital and acquired, of the pulmonary artery. It is by far the most complete treatise on this subject that we know; and is most creditable to its laborious and talented author. The facts are so numerous as to defy anything like analysis; and we are therefore compelled to limit ourselves to a mere notice of the work. The greater part of it is occupied with the details of malformations of the pulmonary artery, and necessarily includes many particulars of malformations of other parts of the heart; in fact, this portion is almost an account of congenital disorders of the heart, with special attention to the pulmonary artery. So far as we have been able to observe, it is very complete and accurate. Subsequently, acute and subacute inflammation, ulceration, aneurism, and dilatation, of the pulmonary artery are considered; and cyanosis, pulmonary apoplexy, &c., receive more or less full notice. This treatise is to be considered, especially as far as the malformations of the artery go, as a work of reference; and we are happy to find in the English language a summary so ample and complete.

Art. III.—*The Principles of Physiology applied to the Preservation of Health, and to the Improvement of Physical and Mental Education.* By Andrew Combe, M.D. Fourteenth Edition, revised and enlarged. Edited by James Coxe, M.D., F.R.C.P.E.—*Edinburgh,* 1852. Fcap. 8vo, pp. 345.

No popular treatise on hygiene has ever attained to anything like the general acceptance which that of Dr. Andrew Combe has so long and deservedly enjoyed; for the very simple reason that none has ever possessed those peculiar merits, which have at once drawn attention to it as a readable book, and have connected its teachings with what so intimately concerns "our own business and bosoms." It is, to speak concisely, in the union of philosophic habits of abstract thought with powers of acute observation, and in the possession of that strong practical common sense which carries principles most advantageously into practice, that Dr. Combe's *forte* peculiarly lay; and it is by these that all his productions have been characterized. We do not know that he ever developed an original idea; but he saw the connexions and relations of the ideas of others, and brought them to bear upon the daily life of mankind, in a manner that their original propounders perhaps never dreamed of.

As no better treatise on hygienic physiology has since appeared, we are glad to welcome a re-issue of Dr. Combe's excellent work, under the able editorship of his nephew, Dr. James Coxe, who has already shown his qualifications for such a task, in the last edition of Dr. Combe's 'Physiology of Digestion.' The necessity for a considerable amount of addition and alteration, however, to bring the work into harmony with the present position of the science (no improvement having been made in it since the year 1844), has evidently placed the editor in a position of considerable difficulty; since he has found it impossible to mark, as he would undoubtedly have wished to do, the relative shares which belong to the original author and to himself; so that in perusing the book we continually find references to very recent discoveries, without any mark of interpolation beyond the general announcement in the preface. Some of these references, we must take leave to say, appear a little out of place in such a treatise; and we cannot but fear lest, if future editions should be treated in the same manner, the original simplicity of Dr. Combe's design, and the singleness of purpose with which he carried it out, should be somewhat obscured by the very laudable desire of his successor to engraft upon it every scientific novelty that derives from its intrinsic interest a sufficient title to attention. Thus, for example, we are rather struck with the incongruity of the introduction of some of the most abstruse questions on the structure of the nervous system, in the midst of the simple outline of its best-determined doctrines, which was all that Dr. Combe thought necessary. We have been far more pleased, however, at perceiving the number of new illustrations, many of them drawn from our own pages, which have been derived from recent sanitary investigations, of the importance of the simple hygienic precepts which it was Dr. Combe's great object to enforce. Everything that is done in the way of popularizing such knowledge is of essential value; and the reader will only be repelled by the drier scientific details, which do not present any definite meaning to his mind, and which go to swell the bulk of a

book that has already far outgrown its original dimensions. We make these remarks in all good feeling to Dr. Coxe; and doubt not that they will be duly weighed by him in the preparation of his next edition. And we would further suggest, that instead of announcing it as "revised and enlarged," Dr. Coxe should substitute the phrase, "adapted to the present state of science," which will leave him more free to do what he will with the *matter*, provided that he maintain the author's original *plan*.

ART. IV.—*Lectures on the Diseases of Infancy and Childhood.* By CHARLES WEST, M.D., Fellow of the Royal College of Physicians, and Physician to the Hospital for Sick Children, &c. Second Edition. Enlarged.—*London*, 1852. 8vo, pp. 559.

IN our third volume (p. 406), we presented to our readers our first article on the Diseases of Children; and from that period until the present time we have endeavoured, as far as our limits would allow, to keep them on a level with the rapidly advancing literature of a subject, that forms so important a branch of the duties of a practical professional career, as does Pædiatrics. On the occasion alluded to, we selected for review chiefly the very able treatise of Dr. West, as a most fitting introduction to the department of medicine in question, and as a most agreeable duty for ourselves to perform. In the course of our labours, we expressed our opinion, and rather forcibly too, of the great necessity that existed for the establishment of a Hospital for Sick Children, and for the systematic clinical instruction of students in the pathology &c. of the diseases of early life; and, finally, we strongly recommended the lectures of our author, not merely as being a most able exposition of the subject to which he had devoted himself, but as forming a treatise quite unapproachable in excellence by any other in our language. The opinions thus expressed were deduced from no theoretical views, but were the truthful deductions of a rather extended practical acquaintance with the several points upon which we then gave our judgment. We are highly gratified, then, in being now able to state, that a Children's Hospital has just been opened, that we have reason to believe that every opportunity will be offered for clinical instruction (whether accepted or not by the students is a different question, upon which we have expressed an opinion elsewhere), and that Dr. West has presented us with a second edition of his valuable work. Respecting this, we are at present unable to say more, than that it must still maintain its pre-eminence, and that we have no doubt the additions which it has received will be fully appreciated by its purchasers.

"The whole work has been carefully revised: a few formulæ have been introduced, and a minute alphabetical index has been appended: while additions amounting altogether to fifty pages have been made, wherever I felt that more extended observation, or more careful reflection, had enabled me to supply some of those deficiencies which I am well aware are still far too numerous. The work now contains the results of 640 observations, and 199 post-mortem examinations, chiefly made among 16,276 children who came under my notice, during the ten years of my connexion with the Children's Infirmary in Lambeth." (Preface.)

We shall notice the most important of Dr. West's additions, in connexion with other recent works on Children's Diseases, in our next number.

Art. V. — *A Dictionary of Medical Science, containing a Concise Explanation of the various Subjects and Terms of Physiology, Pathology, Hygiene, Therapeutics, Pharmacology, Obstetrics, Medical Jurisprudence, &c.; with the French and other Synonymes, &c. &c.* By Robley Dunglison, M.D., Professor of the Institutes of Medicine &c. in Jefferson Medical College, Philadelphia. Eighth Edition, revised and greatly enlarged.—*Philadelphia*, 1851. 8vo, pp. 927.

On the appearance of the last edition of this valuable work, we directed the attention of our readers to its peculiar merits; and we need do little more than state, in reference to the present re-issue, that notwithstanding the large additions previously made to it, no fewer than four thousand terms, not to be found in the preceding edition, are contained in the volume before us. Whilst it is a wonderful monument of its author's erudition and industry, it is also a work of great practical utility, as we can testify from our own experience; for we keep it constantly within our reach, and make very frequent reference to it, nearly always finding in it the information we seek. That it is highly estimated in the land of its nativity, may be inferred from the rapid sale of its successive editions; and we had expected that it would have been introduced, by reprint, to the notice of the profession in this country. As we hear nothing, however, of any intention to reproduce the work, we may mention that we understand that it may be readily obtained from the agents of its American publishers, at a small advance upon its original cost.

Art. VI. — *The Stomach and its Difficulties.* By Sir James Eyre, M.D., &c., &c.—*London*, 1852. 12mo, pp. 152.

As a specimen of good-humoured garrulous egotism, this little book may afford some amusement to such as think it worth while to read it; but we certainly cannot recommend it as containing even an infinitesimal amount of real instruction. The "peptic precepts" which it is designed to enforce, are those with which every intelligent practitioner is already familiar; and the one panacea which our worthy knight recommends, with a zeal that shows that he at least has implicit confidence in his remedy (which, as our readers well know, goes so far to *make* it successful, by influencing the mind of the patient), is his much be-praised oxide of silver, which he describes in terms that remind us of the old alchemists, as "one atom of our second-best metal, with one atom of our most noble gas." Has he ever tried the oxide of *gold?* Surely, upon the principle here propounded, it ought to have at least twenty times the potency of silver.

The book may in fact be designated as an autobiographical sketch of Sir James Eyre, interwoven with the aforesaid peptic precepts, and with testimonies in favour of the virtues of the oxide of silver as the one remedy for *nearly* all the "difficulties" into which the stomach may be brought by "fast" living, or by injudicious management. We will not assert that the author has had the public, rather than the profession, in view in its composition; but we should certainly have made such a charge, if the book had proceeded from a young man struggling for notoriety, instead of from a gentleman whose position must forbid the idea of his being actuated by any unworthy motive. The following are the chief novelties of fact or opinion that we have been able to discover in it:

Sir James was a pupil of Abernethy, and was deputed by his fellow-students to present their "dear master" with a piece of plate.

Sir James was Mayor of Hereford in the year 1830; and was knighted, *without application*, on the accession of William IV.; *only one* other chief magistrate being so distinguished.

Sir James having "renounced nocturnal professional avocations," is now "the more qualified, by calm reflection and undisturbed consideration, to watch the ever-varying phases of that important viscus the *stomach*."

The stomach, in Sir James's opinion, "is, like the fire, invaluable—as a slave; but otherwise becomes a mischievous and dangerous, because powerful despot. It is either like the 'vernal airs' which move the teeming clouds that usher 'gentle spring,' mildly but munificently, by the soft stealing showers, refreshing and vivifying the earth; or, on the contrary, it resembles the rude and turbulent violence of elemental strife—disrobing and devastating, by its impetuous fury, all that arrests its violent course."

The greater part of the book is composed of materials of equal value with the preceding.

ART. VII.—*Thomson's Conspectus of the British Pharmacopœias.* Sixteenth Edition. Edited by EDMUND LLOYD BIRKETT, M.D. (Cantab.) F.R.C.P., &c.—*London*, 1852. 12mo, pp. 216.

OF a work so well known and so fully appreciated as Dr. Thomson's "Conspectus," it is quite unnecessary for us to say anything in recommendation; and we need not do more than state, therefore, that this new edition has been adapted by Dr. Birkett to the last editions of the London and Dublin Pharmacopœias, with every appearance of care and discrimination.

ART. VIII.—*The Descriptive and Surgical Anatomy of the Arteries, and Relative Anatomy of the Veins and Nerves of the Human Body.* By JOSEPH HENRY CORBETT, M.D., Professor of Anatomy and Physiology to the Apothecaries' Hall of Ireland.—*London*, 1852. 12mo, pp. 355.

WITHOUT any wish to depreciate this treatise, we must remark that we are at a loss to discover what there is in its plan or execution, which can give it a claim to supersede the various excellent treatises on this department of descriptive anatomy which are already within the student's reach.

ART. IX.—*The Druggist's General Receipt Book, comprising a copious Veterinary Formulary, and Table of Veterinary Materia Medica, numerous Recipes in Patent and Proprietary Medicines, Druggists' Nostrums, &c.* By HENRY BEASLEY. Second Edition, corrected and improved.—*London*, 1852. 24mo, pp. 460.

WE noticed this little volume with approbation on its first appearance, but little more than two years ago, as containing a great variety of information likely to be useful to the members of our profession who practise in country districts; and we are glad to find that the early call for a new edition has justified our recommendation.

PART THIRD.

Periscope.

ANATOMY, PHYSIOLOGY, AND ORGANIC CHEMISTRY.

On the Development of the Ductless Glands in the Chick. By Henry Gray, Demonstrator of Anatomy at St. George's Hospital.

In this very meritorious paper, the author has demonstrated the evolution of the Spleen, Supra-renal and Thyroid glands, and the tissues of which each is composed, in such a manner as to show the place that may be assigned to each in a classification of the glands.

The *Spleen* is shown to arise between the fourth and fifth days, in a fold of membrane which connects the intestinal canal to the spine (the "intestinal lamina"), as a small, whitish mass of blastema, perfectly distinct from both the stomach and pancreas. This fold serves to retain it and the pancreas in connexion with the intestine. This separation of the spleen from the pancreas is more distinct at an early period of its evolution than later, as the increased growth of both organs causes them to approximate more closely, but not more intimately with one another; hence probably the statement of Arnold, that the spleen *arises* from the pancreas. With the increase in the growth of the organ and the surrounding parts, it gradually attains the position that it occupies in the full-grown bird, in more immediate proximity with the stomach; hence probably the statement of Bischoff, that it *arises* from the stomach. Later, when its vessels are formed, the membrane in which it was developed is almost completely absorbed.

The author then considers the development of the tissues of the spleen, which clearly establishes, not only the glandular nature of the organ itself, but the great similarity it bears with the supra-renal and thyroid glands.—The external capsule and the trabecular tissue of the spleen are both developed between the eighth and ninth days, the former in the form of a thin membrane composed of nucleated fibres, the latter consisting of similar fibres, which intersect the organ at first sparingly, and afterwards in greater quantity. The development of the blood-vessels and the blood are next examined. The former are shown to arise in the organ independent of those which are exterior to it. The development of the blood-globules is shown to arise from the blastema of the organ at the earliest period of its evolution, and continue their formation until its connexion with the general vascular system is effected, at which period their development ceases. No destruction of the blood-globules could ever be observed. These observations disprove the two existing opinions of the use of the spleen, as the blood-discs are not formed there (excepting during its early development), as stated by Gerlach and Schäffner; nor are they destroyed there, as stated by Köllicker and Ecker.—The development of the pulp tissue is next examined. At an early period, this closely corresponds with the structure of the supra-renal and thyroid glands at the earliest stages of their evolution, consisting of nuclei, nucleated vesicles, and a fine granular plasma, the former constituting a very considerable portion of its structure. When the splenic vessels are formed, many of these nuclei are surrounded by a quantity of fine, dark granules arranged in a circular form, and these increase up to the time when the splenic vein is formed, when nearly the whole

mass is composed of nucleated vesicles, the nuclei of which gradually break up into a mass of granules which fill the cavities of the vesicles. The Malpighian vesicles are developed in the pulp by the aggregation of nuclei into circular masses, around which a fine membrane soon appears, in a manner precisely similar to those of the supra-renal and thyroid glands, with which they bear the closest analogy.

The author then traces out the development of the *Supra-renal glands*, and shows the close analogy that exists between them, the spleen, and thyroid, from the similarity which their structure presents at the earliest period of their evolution with those glands, and from the development of the several tissues following the same stages in all.—They are shown to arise on the seventh day as two separate masses of blastema, situated between the upper end of the Woolfian bodies, and the sides of the aorta, being totally independent (as concerns their development) of those bodies, or of each other. At this period, their minute structure bears a close resemblance to that of the spleen, consisting of the same elements as that gland, excepting in the existence of more numerous dark granules, which give to the organ, at a later period, an opaque and darkly granular texture. The gland tissue of the organ, in the form of large vesicles, makes its appearance on the eighth day, whereas in the spleen it did not exist until near to the close of incubation, an interesting fact in connexion with the function of the former gland, which is mainly exercised during fœtal life, whilst the spleen exerts its function mainly in adult life: hence the difference in the development of the tissues at different periods. The manner in which this tissue is developed is similar to that by which the gland tissue of the spleen was formed—viz., by an aggregation of nuclei into circular masses, around which a limitary membrane ultimately forms: these are first grouped together in a mass, without any subdivision into cortical and medullary portions. On the fourteenth day the first trace of this subdivision becomes manifest, by the vesicles being aggregated into masses which radiate from the circumference towards the centre of the gland, in some cases complete tubes being formed by the junction of the vesicles, as indicated by hemispherical bulgings along their walls. At a later period, the organs increase in size, they attain their usual position, and a more complete subdivision into cortical and medullary portions is now observed.

The author lastly traces out the development of the *Thyroid* glands, and shows the great similarity that exists between them, the spleen, and supra-renal glands, from the similar structure they present, and from the development of those structures occurring in a similar manner in each.—These glands are developed between the sixth and seventh days, as two separate masses of blastema, one at each side of the root of the neck, close to the separation of the carotid and subclavian vessels, and between the trachea and the branchial clefts, but quite independent, as far as regards their development, of either of those parts. Their minute structure at an early period closely corresponds with that of the spleen and supra-renal glands. Later, when the gland-tissue, of which the thyroid gland ultimately consists, is formed, it is developed in a manner precisely similar to the same tissues of the spleen and supra-renal glands—a fact which shows the analogy they bear to one another.

From these observations, the author concludes that a close analogy exists between the glands already described, so that the propriety of their classification under one group, as the "Ductless Glands," may be considered clearly proved. And although the spleen by many has been excluded from them, the author considers that its classification with them is correct, for the following reasons:—1st. From its evolution being similar with that of the supra-renal and thyroid glands; 2ndly, from its structure, which at an early period closely corresponds with them; and 3rdly, from the development of its tissues following the same law as that upon which the tissues of the allied glands are formed.—*Proceedings of the Royal Society*, Jan. 15, 1852.

[Every contribution to the anatomy and physiology of these perplexing structures is of value, as tending to throw some light upon the nature of their func-

tion; and Mr. Gray has most ably filled up a lacuna which had been left by the many excellent anatomists who have devoted their time and abilities to this perplexing and, as yet, profitless inquiry.]

On the Mode of Termination of the Nerves in the Skin of the Fingers.

By Dr. Rudolph Wagner.

This celebrated physiologist has recently been making the distribution of the nerves in the skin of the tactile extremities of the fingers his peculiar study; and has communicated the following results of his inquiries to the Royal Society of Göttingen.

What are usually called the tactile papillæ are of two kinds—namely, *vascular* papillæ, which only contain capillary loops; and *nervous* papillæ, which are placed between them. These last have a conical form; and each of them contains in its interior a peculiar corpuscle, also of conical form, which receives the finest of the nervous fibrils that enter the papilla. Each primitive nerve-fibre divides into a great number of smaller branches, to which these tactile corpuscles are attached; and thus each is connected with several corpuscles. It is further considered by Wagner, that each single fibre conducts the impressions made upon any of these branches to a certain spot in the nervous centres; and that thus but a single sensory impression is produced, whether the corpuscles supplied by any one fibre are touched separately, or all together.—*Gazette Médicale*, Mars 6.

On the Influence of a part of the Spinal Cord upon the Supra-Renal Capsules.

By M. Brown-Sequard.

In eight or ten guinea-pigs, in which the lateral half of the spinal cord had been divided in the dorsal region, from ten to fifteen months previously, M. Brown-Séquard has found a remarkable hypertrophy in the supra-renal capsules, which had acquired, in some cases, double, or even triple, their normal volume. On examining the supra-renal capsules of guinea-pigs on whom this operation had been performed only a few hours or days previously, M. Brown-Séquard has met with congestion or even effusion of blood in these organs. In reply to the objection, that this effect may take place as a mechanical result of the operation, he remarks, that the kidneys do not in these cases present any trace of congestion. Whichever side of the spinal cord be divided, both of the supra-renal capsules are thus affected; and a prick of the cord has sometimes been adequate to produce this curious result. Hence M. Brown Séquard concludes, that it is brought about in some way by nervous influence.—*Gazette Médicale*, Feb. 1.

On the Influence of the Sympathetic Nerves on Sensibility, and on Calorification.

By M. Cl. Bernard.

This industrious experimenter has recently communicated to the Société de Biologie two very remarkable results of his experiments on the sympathetic nerve, which we believe to be altogether new. It has long been known that section of the cerebro-spinal nerves tends to diminish the temperature of the parts which they supply; and in the case of the pneumogastrics, to lower the temperature of the body generally. But, according to M. Cl. Bernard, when the trunk which unites the sympathetic ganglia of the neck is cut through on one side, the temperature on that side of the face undergoes a remarkable *increase*, which is not only perceptible to the hand, but which shows itself in a thermometer introduced into the nostrils or the ears, to the amount of from 7° to 11° (Fahr.) When the superior cervical ganglion of the sympathetic is removed, the same effect is produced, but with yet greater intensity. This difference is maintained

for many months, and is not connected with the occurrence of inflammation, congestion, œdema, or any other pathological change in the part. The effect is not prevented by section of any of the other nerves of the face, whether sensory or motor.

A not less unexpected effect is produced by division of the sympathetic upon the *sensibility* of the parts supplied by it; for this, instead of being diminished, is greatly augmented. As the appreciation of this fact, by ordinary methods, is difficult, M. Bernard had recourse to the woorara poison, the effect of which is to produce a gradual destruction of sensibility over the whole body; and he found, that when the cervical ganglion had been removed, the whole of that side of the face retained its sensibility much longer than did any other part of the surface.—*Gazette Médicale*, Feb. 21.

Influence of Muscular Paralysis on the Modification of the Blood, and on the Duration of Irritability. By M. BROWN-SEQUARD.

IT has been remarked by M. Brown-Séquard, that after the section of the nerves of a limb, the transformation of arterial blood into venous is imperfectly effected; but that the change is perfect if the paralyzed limb be galvanized.

The same experimenter has shown, that when the muscles of one limb have been paralyzed by section of their nerves a few days previously, their irritability lasts much longer after death than that of the muscles of the other limb; and their cadaveric rigidity is much later in coming on.—*Gazette Médicale*, Fév. 1, et Mars 6.

Two Cases in which an Ovule, or its remains, was discovered after death, in the Fallopian tube of the unimpregnated human female, during the period of Menstruation. By H. LETHEBY, M.B.

AT the commencement of the paper the author points out that the arguments which have been adduced in support of the opinion, that the menstruation of the human female corresponds to the "heat" or "rut" of the lower Mammalia, and that an ovum is normally matured and thrown off at each menstruation, are entirely of an analogical character; and that although the ovaries of women who have died during the menstrual period have been frequently examined, and Graafian follicles found in a recently-ruptured state, yet the discovery of the liberated ovule had not, so far as he was aware, ever been detected. The importance of his cases rests upon three grounds—namely, 1st, the circumstances under which the women had died, which forbade the idea of recent sexual intercourse; 2ndly, the finding of recently-ruptured Graafian follicles; and 3rdly, the discovery of the ovule and its remains in the fluid matter of the Fallopian tubes.

In the first of the cases recorded, the woman died during a menstrual period. She had been an inmate of the London Hospital for twenty-four days before her death, where she was closely watched day and night by a nurse, in consequence of her having attempted self-destruction, by cutting her throat twenty-nine days before her death.

An examination of the body showed that the pelvic viscera were much congested; that the uterus was considerably enlarged; that the vagina contained a sero-sanguineous fluid; and that the hymen was unruptured. The ovaries were covered with stellate fissures, or cicatrices; and at one part of the left organ there was a purple spot having a ragged hole in its centre. By means of an incision into the gland through this spot, it was found that the opening led into a small cavity which was surrounded at its lower part by a dense tissue, infiltrated with dark coagulated blood. After macerating in spirit for a short time, it was noticed that the clot consisted of four parts, which the author described.

In other parts of the ovary several false *corpora lutea*, in different stages of decline, were found. The Fallopian tubes were highly congested, and the cavities of

the tubes were filled with a bloody mucus. The left one contained, at about one inch from its fimbriated end, a small vesicular body, which was, in the author's opinion, an ovule; for it consisted of nucleated cells and oil-globules. The fluid matters of the uterus and Fallopian tubes were made up of blood-discs, cylindrical epithelium, granular corpuscles, and a few spindle-shaped bodies.

The second case was that of a girl who had died in St. Luke's Hospital, where the supervision of the patient was quite as strict as that in the last case. In this instance the anatomical features were precisely like the preceding. The right Fallopian tube contained a globular body similar to that found in the left on the former occasion. This globular body, on being crushed between two pieces of glass and examined under the microscope, was found to consist externally of a mass of nucleated cells, the remains of the *tunica granulosa*, and of a transparent ring, enclosing an opake granular mass, and a highly pellucid spot. The author considered that this body was the liberated ovule, and the influence of chemical reagents served to support his opinion.

An examination of the *corpora lutea* found in both cases, showed that they consisted of large granular corpuscles and oil-globules.

Among the conclusions arrived at by the author were the following:

1. That ovules escape from the ovaries of women during the period of menstruation; and that their escape is a spontaneous act, taking place quite independently of sexual intercourse.

2. That immediately before, or else consentaneously with, the escape of an ovule, the whole substance of the Graafian follicle becomes charged with effused blood; and that a sort of fatty degeneration of the effused matter soon afterwards takes place.

3. That the mere presence of a yellow body containing a clot in the ovary, is not by any means a certain sign of recent impregnation.

4. That a sanguineous fluid is poured out over the whole mucous tract of the generative system during the catamenial period.—*Proceedings of the Royal Society, May* 1, 1851.

On the Fatty Matters of the Blood. By M. GOBLEY.

M. GOBLEY, believing that the fatty matters of the blood have not been sufficiently examined, judging from the contradictions prevailing among chemists upon the subject, has entered upon the investigation of their nature. As the proportion which this fluid contains is scarcely more than three or four parts in the thousand, M. Gobley has been obliged to employ large masses of it; and in order that the blood should be operated on as nearly in its fresh state as possible, he has had it caught, as it flowed from the vein, in a vessel containing rectified ether, replacing the latter as long as any fatty matter continued to be separated. The fat thus obtained was of a yellowish colour, and M. Gobley found that it contained cholesterine, cerebrine, and lecithine, with traces of oleine and margarine. He also sought for the margaric and oleic acids, whether free, or in combination with soda, as soaps; but he was never able to find them. He has, however, found that the putrefactive process, operating on the lecithine, gives rise to them with great rapidity, and he entertains no doubt that this has been their source when they have been discovered by prior observers. As to seroline, the peculiar body discovered by Boudet, his experiments lead him to believe that it is no immediate principle of the blood. He thus concludes his paper:—1. There are no fatty acids, whether free or combined, in the blood.—2. Seroline is a complex body, which cannot be admitted as an immediate principle.—3. The composition of the fatty matter of the blood is more simple than has been supposed. It is formed of oleine, margarine, cholesterine, lecithine, and cerebrine.—4. Cholesterine is the only crystallizable substance in the fat of the blood, and presents the same composition as the cholesterine of the yolk of egg, biliary calculi, and the brain. The phosphorous matter, or lecithine, is not crystallizable, and gives as products of

decomposition, oleic, margaric, and phosphoglyceric acids.—6. The cerebrine possesses the properties of that met with in the yolk of egg, the ova and the melt of the carp.—7. The fatty matters, under the influence of putrefaction, easily give rise to the production of oleic and margaric acids.—*Jour. de Chem. Méd.* 1851, p. 577.

PATHOLOGY AND PRACTICE OF MEDICINE.

On the Hæmorrhagic Diathesis. By Dr. LANGE.

DR. LANGE has recently contributed an interesting paper on this affection, containing the tabulated results of an examination of the history of 140 examples. It has hitherto been only met with in the northern hemisphere, occurring in America between 30° and 45° N. latitude, and in Europe between 45° and 60° N. latitude. Germany is among the countries most liable to it, and especially so along the course of the middle Rhine and the Maine. The coasts are far less liable than are inland regions. The inhabitants of mountainous districts are less liable than those of plains traversed by rivers. Vine countries are very liable.

The subjects of the disease do not attain great age; for among 140 examples only nine were aged more than fifty, the female sex seeming to possess less power of endurance than the male. The causes giving rise to the hæmorrhage have usually been insignificant, as, e. g., scratches, cuts, leech-bites, extraction of teeth. When the bleeding is *spontaneous*, that from the nose is most frequent and oftenest fatal. *Vomiting of blood* is much more rare; and it is remarkable, that only one of the fatal cases arose from *hæmoptysis*—the disease indeed seeming to have no affinity with tuberculosis. In one case vaccination gave rise to profuse bleeding, without, however, preventing the rising of the vesicle. The statement of Fordyce and others, that deep wounds bleed less freely than superficial, is not confirmed; but the healing of all kinds of wounds is very tedious, although gangrene seldom occurs.

The disposition to hæmorrhage sometimes first shows itself during the first weeks of life, but usually during the first or second year, the latest period in these tables being the eleventh year. The earlier the hæmorrhage exhibits itself, the earlier, as a general rule, does death occur. At a later period the inclination to hæmorrhage usually manifests itself as petechiæ and ecchymoses. Accessory diseases do not, as *à priori* it might have been expected they would, pursue a more dangerous course in these subjects, excepting when those of the respiratory organs excite epistaxis. The *duration of the attacks* is very different, this being perhaps, on an average, ten or twelve days—spontaneous bleeding usually continuing longer, but being better tolerated than traumatic. The effect of *season* is not determined; but the bleeding seems to occur oftenest in spring and autumn. The blood is thin, and deficient in coagulability. In the majority of instances, the intellectual powers of these patients are of a high order; and in most of them the colour of the eyes is blue, the complexion fair and delicate, and the hair light. Usually the constitution is strong, and the muscular system is often powerful and developed. In certain cases, the radial artery has been observed to be transparent in some spots, owing to the deficiency of the fibrous coat—confirming Rokitanski's view, that the disease consists in a remarkably delicate construction and vulnerability of the vessels, and a watery condition of the blood. The cholerico-sanguineous temperament prevails, and so-called rheumatic pains are very constantly observed. Spots of *ecchymosis*, often absent, may, when present, be either spontaneous or traumatic, the latter being usually much larger than the former. They change in colour as after an ordinary contusion. Sometimes the ecchymoses stand in a critical or antagonistic relation to other affections; thus, e. g., they may disappear on the advent of a paroxysm of gout, and reappear at its termination. In some hæmorrhagic families, they constitute a lower form of the disease in certain of

the members. Traumatic ecchymosis may be produced by strong muscular exertion, by falls, or even pressure. It usually disappears unaided, though requiring a longer period than the spontaneous; and if opened, it gives rise to dangerous hæmorrhage.

According to the tables, no difference seems to exist as to condition of life, or residence in town or country. The influence of hereditariness is only imperfectly indicated. The *mortality* from the disease seems to be greatest between 20 and 30, and then diminishes. The earliest age at which it has been known to cease spontaneously has been between 25 and 28. Commonly disease of the liver, hæmorrhoids, gout, or rheumatism, supervenes, and the influence of art is of little use. In palliating the affection, a certain time must always be allowed for the blood to flow, or congestion of internal organs is produced; and in such cases, where this precaution has been neglected, dry cupping of the extremities should be resorted to. Generally, the bleeding stops of itself; but only when exhaustion has occurred. Compression is of little use; and among the stypties sulphuric acid, muriated tincture of iron, and a spirituous solution of sal ammoniac, are the best. Cauteries and caustics usually fail.—*Schmidt's Jahrbuch*, vol. lxx. p.35

On the Application of Statistics to Therapeutical Inquiry.

By Professor WUNDERLICH.

IN an introductory lecture recently delivered at Leipsic, Professor Wunderlich expressed his decided opinion that medical scepticism and the progress of homœopathy are only to be effectually opposed by our founding our therapeutical procedures on an improved statistical basis. After recommending the accurate record of success and failure in private practice, and the formation of societies for the better comparison of results, he continues as follows:—

"I shall be told that Louis introduced statistics into medical inquiries, and few are the fruits they have borne. By them *everything* that was sought for has been proved; and they have only served to strengthen the false maxim, that it matters not by what mode a patient is treated, nearly as many dying and as many recovering by the one mode as the other. This absence of result arises from the faulty manner in which statistics have been employed. Everything has to undergo development, and medical statistics has had to pass through its stage of crudity. If we succeed in showing the development of which it is capable, it will be seen, that not only is the question of the relative worth of a mode of cure to be decided, but that we may expect from it, and from it alone, that it will furnish a solid foundation for those peculiarly delicate indications of which we stand in need for the management of individual cases.

"A fundamental maxim is first to be laid down—viz., that the statistical proof of any therapeutical procedure shall never be attempted unless the condition of the patient justifies it. Humanity enjoins us not to play with the welfare of man in the pursuit of any scientific aim; and fortunately the dictates of humanity are here in harmony with the requirements of science. Here I am in opposition to the idea of therapeutical statistics hitherto received. This requires, and has obtained, that a number of persons suffering from a given disease shall be treated after one method; and that another number, without selection, shall be treated by a second or third distinctly-defined therapeutical procedure; and that not an iota of such treatment shall be departed from, whatever may befall the patient—the decision upon the relative merits of the different plans being solely founded on the number of deaths. Not only is such a proceeding barbarous, but it is utterly useless for the decision of any question: and it is because such a proceeding has been adopted, that therapeutical statistics have made no progress. Never do cases that are brought forward for statistical trials exhibit such an agreement, that one and the same procedure is fitting for all. A great number of these cases will therefore be maltreated by each method; and we do not acquire a statistical statement of which method is most useful, but at best an account of which is most

dangerous in its inconsiderate employment. If on a statistical comparison of cases of typhus treated by laxatives, by bleeding, or without any medicament at all, we find the mortality very similar in either case, we can only conclude that all treatment of this disease which does not keep in view the peculiarities of the individual circumstances is alike unsuitable. If Dietl treated pneumonia in 1842-3 by venesection, in 1843-4 by antimony, and in the two next years only dietetically, and found, that while the mortality in the first periods was twenty per cent., it was only seven per cent. in the last—we may perhaps conclude (independently of other objections to the entire applicability of his statistics) that the lancet and antimony, in the absence of proper precaution, are more dangerous than when no treatment is pursued."

Contrary to Louis, therefore, who rejects the choice of all means founded on their *rational indication*, M. Wunderlich wishes that *only* such as are supposed to offer this should be employed—meaning by the term *rational indication*, not merely a theoretical deduction from hypothesis, but a conclusion from earlier experience in the use of the means in question. Thus, if we wish to statistically test the influence of venesection in pneumonia, we must not include in our calculation those persons to whom old age or other circumstances had shown that bleeding proves injurious—our object not being to exhibit the degree of *destructiveness* of the remedy, but the *degree of certainty with which it may be resorted to with utility;* and to ascertain the point at which such utility is to be expected. It is in connexion with this mistake, that entirely erroneous questions have been submitted to statistical solution. The question should not be, how does a method operate against a *thing* termed pneumonia, or typhoid? but, how does it influence men having infiltrated lung or ulcerated intestine, in whom, moreover, at the same time, different organs may comport themselves in manifold abnormal conditions? And the error of former statisticians is the greater in this, that the danger to life very frequently depends precisely on the condition of these other organs.

From the above considerations it results, that statisticians err when they only take into view the final results—death or recovery. Almost every case is so long a chain of occurrences, that its termination bears only a remote connexion to its commencement or its middle. Moreover, death or recovery may occur without the means previously employed having exerted the slightest effect in its production. It is thus the *proximate* consequence of the employed method, that is to be brought out by statistical agency, distinguishing when it is a mere accidental effect. The *final* results can only be ascertained by a careful comparison of appropriate cases.

The statistical observer, then, must not neglect the fundamental operation of all naturalists—analysis; and as long as statistics is confined to the consideration of the total effect in the mass, it will prove a gross and unuseable machine. Its object should be much rather the observation of particular changes; as, e. g., to what degree, on an average, the frequency of the pulse or the respiration is lowered, or the heat of skin diminished; how often general sensations are relieved and local pains diminished; or in what organ are such effects attainable with most certainty and rapidity. The questions are indeed innumerable, and these are only given as examples; but it is only when we have in this way statistically tested the influence of a remedy upon the particular phenomena, that we come into possession of a fundamental means of judging of its worth and influence.

It is also to be observed, though often overlooked by statisticians, that the problem in therapeutics consists not only in the accomplishment of recovery, but in the mitigation of suffering. All suffering is peril, and every mitigation of it, when other danger is not produced thereby, is a result. Therapeutical statistics has, therefore, to occupy itself with incurable as well as curable maladies.

So, likewise, are to be noted the cases in which a means, apparently indicated, fails in exerting the expected influence. A series of such negative results compared with others of a positive character, will in time furnish the explanation of the circumstances on which the efficacy of a remedy depends; and with our figures in hand, our indications and contra-indications will repose upon a very different basis from that which now usually supports them.

Instead of, as hitherto, making a determined *form* of *disease* the point to be examined, it would seem more fitting, as regards medical statistics, to make the *means* of *treatment* the object of our research. Of forms of disease, though they may bear the same name, we can rarely assemble a sufficient number of completely-agreeing cases; while, when we start from the means or mode of treatment employed, the accumulation of a large number of similar terms is far easier. We employ the mode of cure when it seems indicated, entirely unembarrassed by the name of the disease. We separate the cases in which it acts beneficially, from those in which it produces no effect, and others in which it acts mischievously. The instances are noted in which it induces this or that phenomenon, moderates, or excites; and we then examine what the occurring cases of disease exhibit in accordance with each of these series of phenomena. These diseases may not always bear the same names; for conditions are found which, with heterogeneous appellations, are yet comparable as therapeutical objects. In this way we attain delicate as well as the most certain indications possible; and the smallest statistical material may become useful and instructive.—*Schmidt's Jahrbuch*, band lxx. p. 106.

Case of Ague, affecting only half the Body, after Fracture of the Spine. By Dr. KNAPP.

DR. KNAPP relates a case of fracture of the first dorsal vertebra, which induced complete paralysis of sense and motion in the lower half of the body. While in this state of paraplegia, the man became the subject of ague; but the part of the body *above* the fracture alone suffered, the lower paralyzed portion retaining its normal temperature. The distinctive symptoms of the intermittent fever were strongly marked, and observed their regular succession, in all parts where the cerebro-spinal influence was maintained. The parts below the fracture were quite exempt from all febrile phenomena—neither cold nor heat, pallor nor rubor, nor sudor, being present.—*Charleston Med. Jour.* Nov. 1851.

On Spontaneous Small-pox. By Dr. PURPLE.

IN this paper, Dr. Purple relates four cases in which very strong evidence is furnished of the occurrence of small-pox, independently of approach to diseases of infection. In three of these cases, the attacks occurred nearly simultaneously, the patients being nearly thirty miles apart from each other, and no portion of the intervening districts suffering from the disease prior to its appearance in these individuals. Each case became then the focus of an epidemic, so that great attention was drawn to the circumstance; and much inquiry in vain instituted, in order to trace it back from them to prior cases. Dr. Purple quotes a published case, strongly corroborative of the possibility of this origin of the disease;* and from a full consideration of these and other cases, he deduces the following conclusions:—"1. That small-pox, as well as other contagious diseases, does occasionally present itself among us without specific contagion.—2. That the physical system is so constituted, that it is obedient to certain organic laws, and among these is the generation and development of any disease to which each particular animal is subject.—3. That there are seasons when atmospheric causes render the system more obnoxious to the invasion of these diseases, either by spontaneous development or increased susceptibility to contagious influences. This was particularly the case in 1831, when there was a wide-spread exanthematous tendency, and very many instances of spontaneous appearance.—4. That as the higher latitudes are more subject to small-pox than tropical regions, the coldest seasons of the year are the most favourable for the generation of this disease; and in many instances, where we have known a well-marked case of spontaneous appearance, it has occurred in the coldest

* See the British and Foreign Medico-Chirurgical Review, vol. v. p. 533.

weather, when the thermometer was nearly or quite at zero.—5. That when small-pox appears without contagion, it is usually severe, or at least it is apt to attack those who lack recuperative energy, or are ill-qualified to develope the disease in perfection, and conduct it to a favourable issue. Hence, independently of the treatment they receive, such cases are very liable to prove fatal.—6. That when it thus appears, owing to the epidemic or endemic cause that produced it, the whole population, including those who have been protected, are more liable to be affected by it, than when it has been received by contagion. Indeed, the exanthematic tendency renders the vaccine disease more severe than at other times.—7. That there is a constant and progressive change manifest in the human family in relation to susceptibility to small-pox. This is apparent in the fact, that the disease is more mild than it was some centuries ago, and, independently of treatment, the mortality in a given number of cases is much less than formerly. It is manifested also by the appearance of varioloid after vaccination, which seldom or never appeared in the early experiments with this preventive; and there is no doubt, but that it appears spontaneously more frequently than in the early recorded history of the disease.—8. That vaccination is the only safe prophylactic; and independently of any deterioration in the vaccine matter, the change above referred to renders a second vaccination more necessary than formerly.—9. The small-pox is not always attended by the minor exanthemata, and when they do appear together, it is as apt to precede as to succeed them, as was manifest in three of the above-mentioned cases."—*New York Journ. of Med.*, vol. vii. p. 192.

Case of Conversion of the Pancreas into Fat. By Dr. Verga.

This was observed in the body of a countrywoman, aged sixty-five, who died under an attack of delirium, occurring in religious monomania. The walls of the abdomen and the various viscera were covered with large quantities of fat. The liver was found to possess less consistence than a normal spleen, its large lobe being especially flabby, and yielding to pressure almost like a lung. Its colour was too light; and the gall bladder contained only a little pitchy bile. The spleen was small, flabby, but not fragile; and by slight pressure could be reduced to two lines in thickness. The pancreas was a mere roll of fat, in which, however, the Wirsungian and secondary ducts were visible, although no traces of acini could be perceived. The gland when handled was soft, and imbued the hands &c. with an unctuous yellow liquid; and when it was handled under water, drops of darkish oil ascended to the surface.

Dr. Verga relates this case as offering some objection to the reception of Bernard's views, which make the pancreas the sole emulsifying agent of the fatty matters of the aliment; and he asks how we are to explain by them the extraordinary accumulation of fat which was observed in this individual, in whom the organ destined to effect the absorption of fatty matter was itself the subject of fatty degeneration? He suggests that the pancreatic fluid does not so much serve to emulsify and ensure the absorption of fatty matters, as to render such fitted to undergo ulterior organic transformations. So that the fatty matters, which normally would be acted upon by the pancreatic fluid, are now, on the destruction of the secretory organ of that gland, promptly separated in the form of fat. Adiposity occurs either from an excessive ingestion of hydro-carbonaceous matters, or a defective elimination of the excess of these. The lungs and liver are the organs by which such elimination takes place. When the pulmonary oxidation of these principles is defective, the liver becomes reloaded with them; and when this latter organ is, from its diseased state (as in the present case), unfit for their elimination, they are deposited here and there, in all the tissues into which they can be received, and especially in those viscera, in which, if the secretory functions were normal and sufficient, they would be separated in some form from the blood, and eliminated.—*Annali Omedei*, vol. cxxxvi. p. 370.

SURGERY.

Treatment of Ununited Fracture by the Application of Tincture of Iodine. By Professor BLASIUS.

PROFESSOR BLASIUS communicated, in 1847, an account of the success he had obtained in the external application of iodine in pseudarthrosis; and in the present paper he furnishes three other cases. The first was a healthy soldier, aged 28, who had suffered a simple fracture of the tibia and fibula. The ends of the bones had continued moveable for six months, when the following tincture was ordered to be applied externally, night and morning:—℞ Iodin. ℈j, Iod. Pot. ʒ½, S.V.R. ʒj. In three weeks the callus was completely consolidated. In the second case, the fragments of a fractured femur (occurring in a soldier, aged 23) remained moveable after thirteen weeks; but became quite firm after three weeks' pencilling with the iodine. The third case, occurring in a boy 12 years of age, was equally remarkable. —*Med. Zeitung*, 1851, No. 39.

Remarkable Course of a Ball in a Gunshot Wound of the Thorax.

THE subject of this observation, while standing two or three paces off, was struck from behind on the left side of the thorax by a ball, which had rebounded at a sharp angle from a beam. He died in twenty minutes. The middle of the left shoulder blade and sixth rib had been grazed, as well as the posterior lobe of the left lung—the left branch of the pulmonary artery being also wounded. A large quantity of blood was effused into the left pleura. No ball could be found, until the pericardium and heart were opened—both being quite uninjured. It was now found quite flattened in the right ventricle. The coats of the pulmonary artery, immediately at the division from the trunk, were penetrated, and the ball had fallen by its own weight through the trunk of the pulmonary artery into the right ventricle.—*Schmidt's Jahrbuch*, vol. lxxii. p. 328.

Case of Congenital Membranous Obstruction of the Œsophagus. By Dr. PISTOCCHI.

THIS occurred in the person of an old woman, who from the earliest period had manifested great difficulty in swallowing solid bodies. These had to be reduced to small portions and moistened, or to be suspended by the aid of cookery; and even with these precautions she could take only very small mouthfuls, or swallowing was prevented, and œsophageal vomiting induced. At the *post mortem* the œsophagus was found somewhat dilated below the pharynx for six fingers' breadth, where was observed a completely circular valve, having an aperture in its centre of about a centimetre in diameter. Below the valve, the œsophagus resumed its usual dimensions. The valve seemed formed of the lining membrane folded on itself, and enclosing around the aperture a layer of firm, circular, tendinous fibres, which conferred on it great power of resistance and elasticity, so that any solid body even small enough to pass through the central aperture, met with great opposition.—*Bulletino delle Sc. Med.* vol. xix. p. 267.

On Foreign Bodies in the Air-Passages. By M. JOBERT.

THE following is a summary of the principal conclusions with which M. Jobert terminates a series of papers founded on clinical and experimental observation.

1. Foreign bodies tend especially to lodge in the right lung, owing to the direction and dimensions of the bronchus of that side.—2. They penetrate when the *cordæ vocales* are most widely separated, and a strong column of air rushes into the trachea, as occurs during the rapid inspirations and expirations in the

action of laughing.—3. They traverse the superior aperture of the larynx without raising the epiglottis, which is never closed down upon this, as has been stated.—4. The epiglottis is always raised by virtue of its own elasticity; and its chief office seems to be to direct the passage of certain articles of food, as along a gutter, during deglutition.—5. The bodies traverse the air-passages rapidly, by reason of the laws of gravity, the impulse of the column of air, and their own nature.—6. They are only temporarily arrested at any particular point, and may change their place, until they have excited the inflammatory process which enables them to hollow out a receptacle, in which they become lodged.—7. A peculiar sound is engendered by their presence; and the bronchial secretion is always increased, and may become sanguinolent.—8. A louder respiratory sound, and a more extended vesicular murmur, is heard on the *opposite* side, than on the side in which the body is placed.—9. Foreign bodies whose size exceeds four lines in all directions, cannot be expelled by the sole efforts of nature, which are only efficacious in the case of very small ones.—10. In dogs, on the other hand, in whom the glottis is on a level with the upper aperture of the larynx, the expulsion of foreign bodies easily takes place, by reason of the dilatability and dimensions of the aperture.—11. In the dead body, foreign bodies pass the glottis with difficulty, even when aided by the impulse derived from a considerable column of air.—12. In the living body, they have to overcome, not only this passive resistance, but the very active resistance of the constrictor muscles of the glottis.—13. It is only quite exceptionally that the operation of tracheotomy can be dispensed with; and it should be resorted to as early as possible, in order to prevent inflammation, local changes, and rapid or slow asphyxia.—14. It is a delicate operation, which should be performed by the successive division of all the tissues, and not by an incision comprising all or the greater part of the soft parts of the region at once. This is the best means of preventing hæmorrhage, the introduction of air into the veins, lesion of the thyroid body, &c.—15. The trachea should be as widely opened as possible, so as to facilitate the escape of the foreign body.—16. We can only be certain that the trachea has been opened, when the air escapes with its characteristic sound.—17. When the foreign body does not issue on the opening being made, we must wait awhile, and excite the sensibility of the trachea by the introduction of a blunt body, so as to cause cough and expulsive efforts.—18. The trachea must be more largely opened, when a foreign body of a nature to swell from moisture has been long retained.—19. Re-union may be obtained by the primary or secondary intention.—20. The union by primary intention may be obtained by simple compression, or by the interrupted suture, this only implicating the *dartroid lamella* that surrounds the trachea.—21. Agglutination may be produced by another procedure, which consists in traversing the walls of the trachea entirely, or in part, leaving the sutures hanging externally, these coming away from the fourth to the thirteenth day.—22. A plastic production serves as the means of union between the lips of the wound. 23. Cicatrization only takes place by means of an intermediate production, and not by the direct fusion of the lips of the trachea.—24. The suture comprising the thickness of the walls of the trachea, may excite inflammatory action both within and without the canal, and give rise to organized fistulæ and encysted abscesses.—25. The suture which only implicates the covering, or a portion of the thickness of the trachea, only induces a plastic inflammation, and is to be preferred.—*L'Union Médicale*, 1851. No. 68.

On Compression of the Carotid in Pains of the Trunk and Limbs.
By Dr. TURCK.

DR. TURCK, in former communications, has shown the relief which is obtainable in certain pains of the head from compression of the carotid arteries. While investigating this subject, he was induced to try the effects of such compression in painful affections of remote parts of the body. In a great number of such,

considerable relief has been obtained, as, e. g. in colic-like pains of the abdomen, pains of the chest in phthisis. In some of these, he attributes the effects to the coincident pressure made on the nervus vagus and the trisplanchnic. In the same way, pains which seem dependent on an affection of the spinal marrow are often relieved.—*Rév. Méd. Chir.* vol. x. p. 206.

On the Gradual Reduction of Herniæ long irreducible. By M. MALGAIGNE.

IN this article M. Malgaigne brings forward two new examples of the efficacy of his plan of reducing old and voluminous herniæ. This consists in subjecting the patient to a very low diet and purgation, applying ice or cold poultices to the tumour, and employing the taxis daily. One of these cases was an enormous inguinal enterocele, which had remained unreduced for several years, and now equalled in circumference the size of an ordinary hat. Complete reduction was obtained after continuing the above means for 17 days. The other was an inguinal entero-epiplocele, which had remained unreduced for 7 years, and was reduced completely in 6 days.—*Rev. Méd. Chir.* x. 179.

Case of Circular Arterial Distribution around the Neck of a Direct Inguinal Hernia. By Professor RIZZOLI.

THIS patient was operated upon for a large strangulated direct inguinal hernia on the left side. On passing his finger to the ring, in order to ascertain the locality of the obstruction, Professor Rizzoli found that, on whatever side he explored, it was met by a strong arterial pulsation. He only ventured to make several superficial scarifications, which were not deep enough to implicate the arterial branches, and gradually obtained sufficient dilatability to procure the return of the hernia. The intestine, indeed, from delay, was in a state of gangrene, and the patient died. At the *post mortem*, the origin of this circular arterial pulsation was found to be as follows:—The umbilical artery, rising from the external iliac, and continuing pervious, coursed up on the inner side of the ring, and sent two branches above the upper border of this. From the epigastric artery, which ran on the outer side, also sprang two branches. One of these coursed around the lower and inner side of the ring, so as nearly to surround it, and the other ran along its upper side parallel to the branch coming from the umbilical. An incision anywhere around the tissues of the neck of the sac would have given rise to fatal hæmorrhage, so large were these branches. A diagram is furnished with the paper, and the preparation is preserved in the Bologna Anatomical Museum.—*Bull. delle Scienze Mediche,* vol. xix. p. 143.

On the Employment of Chloroform during Operations on the Eye. By Professor JUNCKEN.

WHEN the patient is effectually brought under the influence of chloroform, the upper lid can always be easily raised, even when the palpebral opening is small and the eyes are deep sunken—for it hangs down like an inert veil. The muscles of the eye are also in complete relaxation—neither voluntary, involuntary, nor convulsive motions occurring; while the eye itself is motionless and turned up as in sleep. It can be brought down by drawing forcibly on the lower lid and conjunctiva. The pupil is motionless—neither contracting nor dilating, unless belladonna has been used, when it remains dilated. This relaxation of the eyelids and muscles much facilitates operations and diminishes irritation; but, on the other hand, it has the inconvenience of allowing the eye easily to change its place under the pressure of the instrument—a fact requiring attention, in order that means

may be taken, during the operation, of preventing this deviation. The employment of chloroform is indicated in the following cases :—

1. In persons whose eyes are irritable, and liable to congestions; and in general in all nervous persons. There are persons suffering from chronic inflammation of the palpebral borders, especially of the Meibomian gland, in whom the conjunctiva becomes readily injected, and very sensible to light, air, or mechanical irritation. On the least touch, the eyelids close spasmodically, the eye is retracted in the orbit, and the conjunctiva becomes entangled in front of the cornea. Unless carefully managed, the upper eyelid becomes everted, and all attempts at opening the eye increase the irritation, and cause the muscles to act only the more energetically. As a general rule, we may reckon upon finding consecutive accidents and inflammatory action violent in proportion to the difficulty there is in opening, and keeping open, the eyelids, and the efforts made by the patient to close them. Under such circumstances, the vitreous humour and iris are easily ejected during extraction; while on depression and reclination it is very difficult to keep the lens down, as it tends to remount before it is sufficiently removed from the field of the pupil. Anæsthetics remove all these difficulties.

2. It should be employed for all persons born blind, or who have become blind at a very early period. Such subjects have no control over the movements of their eyes, turning them sometimes during operations so as to render the aid of the ophthalmist indispensable.

3. It should be employed on all persons subject to nyctagmos, whether idiopathic or sympathetic. These persons have no control over the movements of the eye; and the nyctagmos becomes more and more developed, in proportion as the patient is subjected to the operation of strong emotion. Under the influence of anæsthesia, operations can be executed on such eyes as safely as in the dead body.

4. It should be employed for children, young persons, and all who have much fear of the operation, from whatever cause. The difficulty of operating upon the young is known to all, and that even for the mere extraction of a foreign body; and operations for cataract and artificial pupil could not be undertaken prior to a certain age (14 to 18), notwithstanding the great advantage which would result from the earlier establishment of vision. By the aid of chloroform we can operate upon children at any age.

5. It should be employed for the extraction of foreign bodies, when these are firmly lodged, and the eye very irritable.

6. It should be employed for all long and painful operations.

7. In the operation for artificial pupil the advantages are numerous. It is indispensable when there is great nyctagmos, or when the patient is unable to give the eye the necessary direction. Especially is it valuable in the operation of *decollement* of the iris, or, which is preferable, iridectomia; as it often happens that when the iris is seized with the forceps, a sudden movement is made in the opposite direction, which is not the case if chloroform is used. On the other hand, it must be admitted that it is not so easy to give the corneal incision the requisite extent, as the paralysis of the muscles renders the eye so mobile. Yet is the incision required to be large, since in the state of anæsthesia, the iris, not being pushed forward by the contraction of the muscles of the eye, does not present itself.

8. For the operation of cataract, the chloroform is only required under the conditions already stated; and the majority of operations *can* be performed without it. Still, even in ordinary cases, its employment is very useful, and unattended with inconvenience. It allows the lens to be much more easily depressed and maintained, as the muscles of the eye do not compress by their contractions, and the vitreous humour does not oppose any resistance.

Even when chloroform gives rise to vomiting, this does not give rise to the ascent of the crystalline so easily as vomiting from other causes does—as the muscles of the eye continue, for a certain time after its administration, still relaxed. Even when the patient recovers from the anæsthesia, he does not generally open his eyes, as the muscular relaxation continues for a considerable time longer.—*Bull. de Thérap.*, xl. p. 319.

MIDWIFERY, &c.

On the Employment of Tracheotomy in Croup. By M. Trousseau.

In the present series of papers M. Trousseau relates the cases in which he has most recently performed tracheotomy for croup. Adverting to his entire experience upon the subject, he states that he has performed this operation altogether 169 times (11 for chronic disease of the larynx, and 158 for croup); and that 43 of these cases, or a little more than a fourth, have recovered. Among his last 18 cases, however, there have been 8 recoveries, or nearly one-half. The results obtained at the *Hôpital des Enfans* have not been less satisfactory of late; for of 19 cases operated upon, between January and August, 1851, one-half have recovered, and M. Guersant has been as successful in his private practice. M. Trousseau believes that one reason of the greater success in later years is, that now the principles of treatment in these cases are better understood; the children are brought to the hospital in a less exhausted state, their powers not having been lowered by the application of leeches and blisters, heretofore so common. Still more importance, however, does he attach to the modifications he has made in the treatment after opening the trachea. Thus, he has discontinued the application of a strong solution of nitrate of silver to the trachea and bronchi, which he used formerly to insist upon. He now too employs a double canula, so that the inner one may be taken out and cleaned when necessary, without disturbing the other; and after the wound is dressed he covers all the parts over with a cravat, and thus avoids the difficult expectoration and desiccation of the mucus which occurred when they used to be left exposed.—*L' Union Médicale*, 1851, No. 100.

On the Acute Ascites of Children. By M. Trousseau.

M. Trousseau recently called the attention of his class to a form of acute ascites in children, which, according to his experience, is by no means rare. Only four days before this child was brought to the hospital, it was in complete health. After only very slight pain in the belly, this became swollen; and on admission, a large quantity of fluid, causing dyspnœa, was detected—no fever being present, and the digestive organs continuing in a normal condition. These cases are exactly analogous to those of acute hydrothorax dependent on pleurisy, as these do on peritonitis—a pleurisy, however, in which the effusion is so great and so rapid, that death ensues if paracentesis thoracis be not resorted to. There is the same difference between acute febrile peritonitis (almost always fatal), in which the inflammatory symptoms are violent and the effusion small, and the peritonitis with little fever and much effusion, as between acute febrile pleurisy and acute hydrothorax. In hydrothorax, however, we can remove the mechanical obstruction to respiration by paracentesis, which is a harmless operation; but in this form of ascites, though the prognosis is as favourable, our procedure must be different. In fact, the experience of M. Trousseau and his colleagues is entirely unfavourable to paracentesis abdominis in children—the operation almost always terminating fatally. Even in the adult, the repeated tappings which sometimes take place are usually performed on women, and are, in fact, operations upon ovarian cysts, and not upon the peritoneal cavity. In men, as in children, it is the peritoneal cavity itself that is opened, and the prognosis is much more unfavourable. In the child, when it is not complicated by tubercular disease, the treatment of this ascites is best accomplished by giving small doses of calomel, until the gums are slightly swollen, and keeping a hemlock cataplasm applied to the abdomen night and day. If resolution does not quickly occur, the surface of the abdomen should be painted with iodine.—*Gaz. des Hôp.*, 1851, No. 100.

On the Measurements of the Fœtal Head in Europe and America.
By Dr. HEWSON.

DR. HEWSON, struck with the larger dimensions assigned to the fœtal head by Meigs (in whose accuracy he has implicit confidence), as compared to other authorities, inquires whether this has arisen from any error on the part of the latter, or from an actual difference in the size of the head of the fœtus in America and Europe. He took the opportunity of a residence at the Dublin Hospital to measure carefully the heads of the 166 full-term children who were born there during March and April, with the result of confirming the difference above stated. The following table will exhibit these:—

	Bi-parietal diameter.	Occipito-frontal.	Occipito-mental.
Baudelocque . .	3·34 to 3·50	4·50	5·50
Velpeau . . .	3·50	4	5
Cazeaux . . .	3·50 to 3·75	4·25 to 4·50	5.25
Burton	3·50	4·30	5·60
Ashwell . . .	3·50	4·50	5·25
Murphy . . .	3·50	4·50	5
Churchill . . .	3·50 to 4	4 to 4·50	5
Hewson . . .	3·60	4·68	5·25
Meigs	3·88	4·83	5·50

The figures of Cazeaux and Churchill do not represent averages, but only the points between which they may be found.—*Phil. Med. Exam.*, vol. vii. p. 634.

On the Absence of the Fœtus from the Ovum in Early Abortion.
By Dr. MEYER.

DR. MEYER observes, that he is in the frequent habit of examining the masses discharged in early abortion. When these do not prove to be mere coagula, they present the following appearances. In form they resemble internal coatings of the uterus, the fundus and cervix being quite discernible. The external appearance of the mass is that of a coagulum of blood, with a more or less smooth surface; and this it is found to be on cutting into it, until we arrive at about its middle, when we come upon a cavity having smooth walls more or less collapsed. Upon nearer examination this cavity is found to be lined with two membranes, the chorion and amnion. An affixed funis is always found, and near its attachment the umbilical vesicle, and frequently the *ductus omphalo-mesariacus*. The free extremity of the funis has all the appearance of being torn. These various appearances fix the age of the fœtus at about two months: but no fœtus is to be found, or even the fragments of one. The conclusion usually come to, that no fœtus has been present, was regarded by Dr. Meyer as inadmissible, and he therefore instituted a more exact investigation into these cases. He accordingly found in all of them a rent, extending through both membranes, usually at the place which corresponded to the orifice of the uterus, and this rent led into a canal of greater or less length, amidst the external coagulum. So constantly is the funis directed towards this rent, that in one case, in which the placenta was implanted more towards the orifice of the uterus, and a large rent had occurred at the fundus of the ovum, the funis passed directly upwards. It is evident, then, that the fœtus escapes through the rupture of the membranes; and the following seems to be the proximate cause of its doing so. Abortions of this kind are complicated with considerable hæmorrhage; and the blood effused between the walls of the uterus and the ovum, whether in a fluid state or as a coagulum, when acted upon by the uterine contraction, compresses and bursts the ovum. The membranes collapse, and the funis becomes fixed in the position it assumes on the exit of the fœtus through the rupture. So small an object as the fœtus, becoming mingled with the coagula, is easily overlooked.—*Henle's Zeitschrift*, band x. p. 238.

On the Chlorosis of Pregnancy. By M. CAZEAUX.

M. CAZEAUX recently read at the Paris Medical Society a paper, the object of which was to show, "that hydræmia or serous polyæmia is the most frequent cause of the functional disturbances in advanced pregnancy usually attributed to plethora." The analysis of the blood of pregnant women exhibits a diminution of globules and an increase of water, differing indeed only from that of chlorosis by containing an increased quantity of fibrine. The quantity of fibrine is far less than in phlegmon, and the buff it gives rise to has been often observed in the chlorotic. The functional disturbances of pregnancy resemble those of chlorosis, many of these indeed being common to plethora and chlorosis. The effect of treatment confirms this view of their nature; for while here, as in chlorosis, depletion may prove a temporary and fallacious means of relieving serous plethora, it is from the employment of animal food and iron that real benefit is obtained; and this even in cases wherein local bleeding may be deemed advisable. M. Cazeaux does not, however, deny that true sanguineous plethora may be met with occasionally, and especially in the early months.

During the animated discussion which followed, M. Duparcque admitted that pregnancy may occasionally induce a condition analogous to chlorosis; but he referred to the marked power of venesection in arresting threatened abortion from active uterine congestion; and believes that the practice followed by our predecessors of bleeding at the middle of pregnancy, on account of the then active disposition to abortion, may often be advantageously imitated. A similar plethoric determination takes place at the seventh and ninth months; and when the mother does not suffer ill effects from this, it may produce cerebral apoplexy, or that state of general congestion termed asphyxia, in the infant—the plethora killing the child, though it spared the mother, when precautionary venesection had been neglected. Puerperal convulsions might often be prevented, if bleeding were instituted for the plethoric condition in which they so frequently originate. In judging of the presence of plethora, too much weight has been attached to the highly-coloured condition of the skin, especially that of the face and its adjoining mucous membranes, and to the projection of the veins. But it is very common to see persons who are constantly plethoric, and who are liable to phlegmasiæ, congestions, and hæmorrhages, exhibiting so colourless a condition of the tissues, that from their mere aspect, we might believe them subjects of chlorosis. Such persons bear losses of blood, which those of a higher colour, and apparently eminently sanguineous temperament, could not endure.

M. Jacquemier stated that he had examined the blood of about 200 women, in the eighth and ninth months of pregnancy, most of them being persons from the country. The so-called inflammatory crust was not met with so often as is usually supposed; but occurred much oftener in winter (when many of the women suffered from bronchitis and influenza) than in summer; it being met with at this latter period only once in six or even in nine cases. Most frequently when the buff did exist the clot was pretty large and softish, and the serum was not in excess; the hard, retracted clot, covered with a thick buff, and bathed in a large quantity of serum, as seen in inflammation and chlorosis, being rarely met with. According to his observations, the excess of fibrine, whether absolute, or relative to the diminution of globules, is not considerable enough to habitually give rise to the production of the inflammatory crust. The diminution of globules is infinitely greater in a chlorotic person than in a pregnant woman; and all the analogy that can be traced between the two conditions may be stated in the fact, that a considerable number of women, after the middle period of pregnancy, exhibit the commencement of anæmia. Clinical observation does not favour the view of the identity of the two conditions. Among many hundreds of women auscultated at the Maternité, during the last two months of pregnancy, M. Jacquemier only met with the carotid *souffle* in two or three.—*Rev. Medicale*, 1851, vol. i. p. 553; vol. ii. p. 51.

Cases of Fracture of the Fœtal Cranium, without External Violence. By Drs. FLÜGEL and SCHILLING.

DR. FLÜGEL relates two cases of this occurrence, the possibility of which it is important to bear in mind in discussions on infanticide. The first was a face presentation, which terminated in seven hours without aid, the latter pains having been both forcible and rapid. The child was still-born, although it had been ascertained to be alive just before delivery, immediately prior to which a loud crack was heard by the pupils. At the *post-mortem* examination, the pericranium was observed raised into a bloody swelling at three different points of the left parietal bone; and on opening these tumours, fractures from 11 lines to 1 inch and 4 lines in length were discovered. In the second case the presentation was natural, but the proposition to use the forceps on account of defective pain, so alarmed the mother as to bring on a succession of severe pains, which, together with her excessive exertions, soon terminated the labour. The child, which was large, died six hours after birth, in convulsions. Two fractures of the left parietal, 1½ and ¾ of an inch long, were found. The vessels of the brain were found highly congested, and its cavities contained bloody serum. Had these appearances been observed in the case of a suspected secret birth, the statement of their having been produced during the natural course of a labour rapidly terminated, would probably not have received credence.

Dr. Schilling was only called to his case after delivery, and was shown a dead child, having the parietal bones broken in several places and covered with blood. He supposed this to have resulted from external violence, but received the following statement from the persons present at the birth. It was a first labour, the pains being at first slight and the woman tossing about. When severe pains set in, its progress was still gradual, during the fifteen hours they continued. Just at its termination she was seized with dreadful spasmodic pains, and remained for awhile unconscious. The child soon followed after her recovery, but it was still-born.—*Casper's Wochenschrift*, 1851, Nos. 38 and 40.

Case of Early Viability.

As these instances are always interesting, we may cite the particulars of one recently communicated to M. Velpeau, with complete authentication. Madame D——, at 35, was not aware until she arose, 15th June, 1850, that her menstrual period had come on, she having had connexion that morning. The discharge ceased in the evening, and the suppression remained permanent without injury to health. After a time, signs of pregnancy were observed, and she went on very well until 9th October, when she was much shaken during a journey. Next day some reddish water was discharged, and continued to be so, at intervals, until the 27th December, when she was delivered of a very delicate infant, having a skin so intensely red, that the friction caused by washing was forbidden, lest it might cause bleeding. Its power of generating heat was very feeble, so that constant attention was required to this point; but it swallowed a little food, and on the tenth day took the breast feebly. It had not increased in volume much by the 15th March, the natural term of the pregnancy, though its weight was greater. Its development is now that of an ordinary child of its age. The child was, therefore, born 6 months and 10 days after conception, and the case is additionally interesting from the fact of the waters having commenced discharging at 3½ months, without abortion ensuing. The French law fixes 6 months or 180 days as the term of legal viability.—*Gaz. des Hôpitaux*, 1851, No. 74.

Case of Ossification of the Placenta. By Dr. GARRISON.

Dr. GARRISON relates the case of a lady whom he attended in her second labour, learning that her first had been followed by hæmorrhage and difficult extraction of the placenta. This labour soon terminated, and the contraction of the uterus was

good; but very soon such fearful hæmorrhage came on, as led Dr. Garrison to determine at once to remove the placenta. He found it so closely adherent, that even with the exertion of great force he had much difficulty in effecting his object —a crackling noise being produced while he was so employed, which was audible to the attendants. On examining the uterine surface of the placenta, it was found in a state of what the author terms "ossification;" though it is to be regretted that he did not submit the changed structure to minuter examination than by means of a pocket-glass. "It presented a kind of cellular arrangement, similar in appearance to what might be produced by sticking the whole surface full of the husks of wheat which had been broken in two, leaving the broken edge of the husk turned outwards. It was undoubtedly the separation of this bony matter, which gave rise to the crackling noise which had been heard during the process of delivery. I could reproduce the same sound in the placenta after it was expelled. It crackled in my fingers like frozen grass or ground. I had never on any previous occasion been compelled to use an equal degree of force in the delivery of the placenta, and I felt very doubtful as to what might be the issue of it. The utmost strength of my fingers was barely able to break up this bony union."

In four out of five other confinements, the same hæmorrhage and bony adhesions prevailed.—*Amer. Jour. Med. Sci.*, vol. xxiii. p. 134.

MATERIA MEDICA AND THERAPEUTICS.

On the Treatment of Pneumonia by the Inhalation of Chloroform. By Dr. Varrentrapp.

Although Dr. Varrentrapp is well aware that the 23 cases of which he furnishes the history in his paper, are too few in number to justify him in drawing any positive conclusions from them, he thinks that the results are sufficiently striking to justify his adding them to those already published by Wucherer and Baumgertner, which indeed he considers have not excited attention in proportion to their importance. His own cases differed from theirs, in his employing larger and more frequent doses of, and trusting exclusively to, the chloroform. The cases were composed of the working-classes who frequent the Frankfort hospital, the average age of the 23 persons (21 men and 2 women) being 31, the oldest 62, and the youngest 19. Upon the average, the treatment was commenced on the morning of the fifth day of the disease. In 10 the right, in 8 the left, and in 5 both lungs were affected. The inhalations for the whole number averaged 74 for $10\frac{1}{2}$ days; the smallest number being 27 in 5 days; the largest 162 in 15 days. Sixty drops of the chloroform were poured on tightly compressed cotton and inhaled for 10 or 15 minutes, from 8 to 12 times during the 24 hours. It seldom happened that the chloroform could not be borne on account of immediate narcotic or unpleasant symptoms; and any affections of the sensorium, vomiting, &c., that appeared at first were soon relieved by temporary suspension. Of the 23 cases, 1 was bled, and another cupped in mistake. An emetic was given in 1 case, and a purgative in 9. In 5 cases of pleuritic complication, blisters were applied, and in 2 calomel and digitalis were administered. Among the 23, 1 case was fatal, and 22 completely recovered.—*Henle & Pfeufer's Zeitschrift*, N.S. band i. p. 76.

On the Application of Collodion in Erysipelas. By M. Latour.

M. Latour, believing that Erysipelas is best treated by the application of an impermeable covering to the skin, has experimented with a great variety of substances, and pronounces collodion to be the best. Spread out on the surface, however, it sometimes so compresses the skin as to cause pain and irritation—phlyctenæ also forming where it cracks. The following combination, however,

remedies these inconveniences completely:—*Collodion*, 30 *grammes; Venice turpentine*, 15 *decigrammes; castor oil*, 5 *decigrammes:* to be mixed by shaking. It is easily detached by a linseed poultice. The following is also used with great success, rarely irritating the skin:—*Collodion*, 30 *parts; old castor oil*, 2 *parts.—Révue Médicale*, 1851, p. 462.

Injections of Salt in Intoxication. By M. Lalaux.

The difficulty with which intoxication is sometimes distinguished from comatose cerebral affections, renders valuable the possession of a simple means of at once aiding the diagnosis and dissipating the symptoms. This, M. Lalaux declares, exists in the administration of an injection of warm water containing two tablespoonfuls of salt. He explains the benefit derived by the partial evacuation of the poison in the copious stools that are promptly produced. The injection also sometimes induces vomiting, when mechanical irritation of the fauces has failed to do this.—*Gaz. des Hôp.*, 1851, No. 121.

On Iodine Clysters in the Treatment of Dysentery. By Dr. Eimer.

Dr. Eimer believes that the great point to which practitioners have to direct their attention, is the enormous amount of organic losses consequent on the continuance of this affection—so that, according to Œsterlen,* within three weeks, more than the entire blood-mass may pass away as albumen in the stools. As a means of cutting these discharges short, he strongly recommends iodine clysters; which, in recent cases, may at once arrest the progress of the disease, and in all diminish the number of stools, and normalize their condition, whatever the individual peculiarities of the case may be. From five to ten grains of iodine, and as much iod. pot., are administered in two or three ounces of water, from two to four times a-day—twice daily usually sufficing. If the rectum is too irritable to retain it, ten or fifteen drops of tr. opii are to be added, and a mucilaginous vehicle substituted for water. In spite of unfavourable conditions, so constantly successful did Dr. Eimer find this remedy during an epidemic, that he believes the disease will, as a general rule, be found curable by it, if it be resorted to before the organic changes in the intestine have advanced too far, exhaustion become too considerable, or important complications set up. In some slight cases it was employed alone. Generally, a simple oily emulsion was also administered, and sometimes acetate of lead and opium.—*Henle's Zeitschrift*, Band x. p. 238.

Belladonna as a Palliative in Epilepsy. By M. Fredericq.

Even when this malady is not curable, it is of great importance to diminish the number of fits, which become multiplied by habit, and render the disease less and less amenable to treatment. For this purpose, M. Fredericq has advantageously used belladonna; and he gives it in the following doses to several young epileptics at the Hospital of Courtroy, reputed incurable:—Ex. Bellad., gr. iii; aquæ, ʒ vi. A tablespoonful three times a day, and when premonitory symptoms are perceived.—*Bullét. de Thérap.* tom. 41, p. 373.

On the Employment of the Iodide of Sodium in the Treatment of Secondary Syphilis. By Dr. Daveri.

Notwithstanding the great success which has attended the employment of the iodide of potassium in the treatment of venereal disease, its disagreeable taste, and the gastric irritation it sometimes gives rise to, induced Dr. Daveri to try how far the iodide of sodium might be advantageously substituted for it. In the nineteen

* See British and Foreign Medico-Chirurgical Review, vol. v. p. 245.

cases of secondary syphilis affecting the bones and periosteum, in which he has employed it, he has found it equally beneficial, while it is far more palatable. It is also borne in larger doses, and these can be more rapidly increased; so that the duration of the treatment is abridged. Some cases which proved rebellious, or only slowly yielded to the iodide of potassium, have been rapidly cured by the soda preparation.—*Bulletino delle Sc. Med.* vol. xix. 269.

Case in which extraordinary quantities of Acetate of Morphia were administered. By Dr. CREDE.

IN a case of malignant disease of the abdomen, which in its diagnosis baffled the skill of many eminent practitioners, the pains were so intense as to lead to the patient taking such extraordinary quantities of acetate of morphia, as are probably quite unprecedented. An apothecary has supplied an account of the quantity which he furnished; but besides this, the remedy was several times obtained elsewhere, and various other forms of medicine containing opium were ordered. Commencing with 2 grains of acetate, in Feb. 1849, the quantity was gradually increased, so as to bring it to 14 grs. for May, 50 for June, 60 for Oct., and 136 for Dec.—making a total of 454 grains from Feb. to Dec. Commencing with 192 grains for Jan. 1850, this reached 648 in March (21 grains per diem), and 936 in May—thus averaging 30$\frac{1}{5}$ grains per diem; and the highest quantity ever taken in one day—namely, 52 grains—was taken this month. After this the quantity diminished again to 648 in June, 216 in Sept., and 288 in Dec.—making a total of 5175 grains (or 10 oz. 6 drachms, 15 grains, Apoth. weight) for 1850. During four months of 1851, the quantities taken continued much the same, amounting to 756 grains. The entire quantity taken during 25 months amounted to 6385 grains, or 1 lb. 1 oz. 2 drachms and 25 grains, Apoth. weight.—*Casper's Wochenschrift*, 1851, No. 45.

On the Changes producible in the Properties of Bodies by Pulverization. By M. DORVAULT.

To the present time, pharmacologists have always considered pulverization as a mere change of form in bodies—each particle of the divided body being regarded as a diminutive, without change of property, of the entire mass. While admitting that, in most cases, this is a mere expression of the fact, M. Dorvault believes that there is a greater number of substances than is suspected, in which this operation induces a modification of their chemical characters and medicinal properties. At present, he can only adduce two or three decided examples in justification of this opinion. Every one knows that sugar, on being powdered, loses a portion of its solubility and sweetening power. Is this referrible to an altered electrical condition of the sugar, as the phosphorescence which is developed during pulverization in the dark might lead us to suspect? Again, gum arabic, when powdered, possesses neither the same taste nor solubility as when entire; and pulverization so diminishes the solubility of arsenious acid, that while a kilogramme of water will dissolve forty grammes in the vitreous state, it will only dissolve fourteen of the powder. In the above examples, the modification is exhibited by diminution of solubility, but in other cases it may manifest itself in other directions.—*L'Union Médicale*, 1851, No. 150.

On the Local Action of Tartar-Emetic. By M. GUERIN.

M. GUERIN observes that, just as Rasori proved that the absence of vomiting, during the *internal* use of tartar-emetic, is no accident, but is due to the presence of certain determinate pathological conditions; so an examination of the subject during the last ten years has convinced him that the production of a pustular eruption is not a part of its operation, in the case of acute inflammatory action,

when applied *locally*. He has employed the ointment in hundreds of cases of acute *arthralgia*, with the best effect, and has found the benefit to be derived prior to the appearance of any such eruption, or mostly without its coming out at all. Indeed, there is usually something antagonistic to the production of the eruption during the acute stage; as it does not occur though the ointment may be applied for days or weeks. It may, however, be produced when the acute stage has passed, or in the vicinity, beyond the sphere of the inflammatory action. It is in the early stages that the remedy is indicated, when it produces a prompt diminution in the pain and swelling, and an abatement of frequency of pulse and of the other signs of fever. M. Guerin has also employed this means in simple and complicated pneumonia, in pleurisy, and in acute and chronic tuberculization, and has reaped excellent effects from it. He considers this plan of treating diseases, by obtaining the absorption of the antimony, as only another mode of attaining the same end that is achieved by Rasori's procedure.—*Gaz. Méd.* 1851. No. 44.

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