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Analytical and Critical Reviews.

I.—Laboratory Teaching.[1]

The importance of chemistry as an educational instrument, and as a valuable branch of knowledge, is becoming gradually recognised. Of course, for thirty years to come, we must not expect our legislators to do much towards helping forward the general diffusion of natural science. The present race of politicians, and, in fact, 999 in 1000 of our upper classes, had no opportunity during their school or college life of learning anything concerning the materials and forces of nature. Physicians, chemists, and natural philosophers, who know what powerful aid science can render to the advance of our country, must do more than they have yet done to impress their convictions upon influential and intelligent, though in this matter ignorant, persons. First of all, however, our methods of instruction must become more definite, and more worthy of the great branches of knowledge with which they are concerned. We take the present opportunity to offer some suggestions as to the way which shall be at once the soundest and the most effective of teaching practical chemistry.

Whatever may be the ultimate path in life of the schoolboy or the student, their first steps in chemistry should be identical.

[1] 1. *Qualitative Chemical Analysis.* By Dr. C. R. Fresenius. 7th edition. Edited by Arthur Vacher. Pp. viii, 264.

2. *An Introduction to Practical Chemistry, including Analysis.* By John E. Bowman. Edited by C. L. Bloxam. 5th edition. Pp. xx, 316.

3. *A Course of Practical Chemistry, arranged for the Use of Medical Students.* By W. Odling, F.R.S. 4th edition. Pp. xii, 235.

4. *Chemistry, General, Medical, and Pharmaceutical.* By John Attfield. Pp. viii, 624.

5. *Laboratory Teaching; or, Progressive Exercises in Practical Chemistry.* By C. L. Bloxam. Pp. xx, 227.

6. *Exercises in Practical Chemistry.* By A. G. V. Harcourt, F.R.S., and H. G. Madan, M.A. Series I. Qualitative Exercises. Pp. xv, 355.

Chemistry is often made not only uninteresting but unintelligible if the lecturer or teacher plunges, as he too often does, into the arcana of the science; the laws, for instance, of volume and of atomic weights. Let him commence first of all by mentioning and showing some material, some phenomenon, some experiment, with which even the most unobservant boy must be familiar. In the hands of a judicious teacher, a splint of wood, a marble, a lucifer match, a drop of ink, a pinch of gunpowder, will secure the attention of his auditors. The interior and hidden meanings and powers of well-known objects will be revealed, and the knowledge, though pleasantly gained, is not usually easily forgotten. But the lecturer has only accomplished half his work until he has made his pupils repeat, as soon after his lecture as possible, all the feasible and important experiments he has shown and explained. Thus a series of laboratory lessons is devised in which familiar things—the air that enters and that leaves the lungs, the water we drink, the soil of our fields, our metallic coinage, quicksilver, sulphur, and a hundred other common bodies—are made the subject of testing and experiment. Advantage should, of course, be taken, here and there, of any very suitable and striking result, to expound, by its means, the chemical laws which it may happen particularly to illustrate. When sulphur is the subject of experiment, the three molecular states of matter may be introduced; when zinc foil is burnt in a candle, the nature of oxidation and oxides may be unfolded; when water is decomposed, and its constituents afterwards reunited, combination by volume and the evolution of heat may be described. We urge, then, not only the use of common and familiar materials, and the greatest simplicity of arrangement, but the gradual development of the laws of the science. Too often an utterly different and, we presume to say, a totally wrong method of teaching chemistry is adopted; long and tedious tables of elements and atomic weights, with the laws of combination and other details, dry at first but interesting afterwards, are given out to be learnt, and the boys conclude that chemistry is a dull matter after all—only another sort of arithmetic; the brilliant lights and gay colours which they have seen associated with it being, as it were, the jam that covered the grey powder! And the common way of teaching chemistry goes wrong in another direction. Chemistry should be taught in the same way as, during the last 100 years, it has been itself developed or learnt. The familiar things which chemistry explains should be introduced, not so much as illustrations of chemical facts and laws, but as being the very starting-points from which those facts and laws have been evolved. Tannic acid, for example, should not introduce leather, but leather tannic acid; a looking-glass and a cake of vermilion will serve a double purpose if used as points of departure for a description of mercury, rather than as mere illustrations of the application of this metal to common uses. Thus the objects of everyday life, even our food, our clothes, and the contents of our rooms and our pockets, will remind us continually of some of the chief facts of an exact science, and make that science interesting. It has been said by a recent writer, who ought to have, but evidently has not, much real acquaintance with our present subject, that the last thing to be thought of is to make scientific teaching interesting. For two reasons chemistry *ought* to be made interesting to boys, for thus only will it be possible to get the attention of a fair proportion of any audience, and at the same time to secure and concentrate on the subject all the available mental powers of each individual.

Another characteristic of chemical teaching, if it is to be not only interesting, but sound and effective, may now be noticed. It is a characteristic which can be more largely developed at college than at school. It is often necessary, while teaching pure science, to give it a particular bias. One student may be devoting himself to agriculture, another to medicine, another to the fine arts; to each the central laws and facts of chemistry must be taught, but, as opportunity offers, these ought to be introduced and illustrated by examples pertaining to the several arts of agriculture, medicine, or painting. An agricultural student would have, for instance, the laws of diffusion illustrated to him by the processes which go on in the soil and in the plant; the medical student, on the other hand, would study the same laws as they work in the liquids and gases concerned in the functions of the human body. The agricultural student would devote particular attention to those compounds of nitrogen which are used as manure, while the medical student would make the acquaintance of those which are of importance in the treatment of disease. Both students follow the fortunes of nitrogen till it appears as ammonia, then the former combines it with sulphuric acid, the latter with citric; both will study analysis, one to detect and estimate phosphoric acid, the other arsenic. We lay particular stress upon this apparently simple mode of giving a special direction to chemical teaching, because we think that applied and pure science, if thus combined together or taught simultaneously, give each other mutual support. At the same time, it will be evident that medical students, for example, would be able to devote much more time to specially appropriate chemical studies, during their short summer laboratory course, if they had been previously well grounded, when at school, in the elements of the science, and had likewise had it presented to them in its practical aspects and in an attractive form. Thus the work of a year would no longer have to be compressed into three months, and instead of being, as in too many cases it is, a profitless drudgery, it would be begun with pleasure and finished with satisfaction.

It remains to inquire how far such a scheme of chemical manipulation as we have indicated can be carried out by the aid of the text-books and the other means at the disposal of the teacher. Of books we have no lack. But chemists who write on the practical part of their science are so much in the habit of supplementing their printed directions by oral additions and paraphrases, that no one manual can be said to answer wholly the end in view. Perhaps the most minute and complete directions for experimenting are those given in Messrs. Harcourt and Madan's 'Practical Chemistry,' of which the first part has lately appeared. The number of preliminary lessons in manipulation is rather limited, only about twenty-five; but the details given render them easily learned. They might, however, have been rendered more attractive had the subjects of some of the earliest lessons been drawn from the most familiar and even rough materials, so as to convince the student at the outset of the intimate connection between science and common life. For instance, chalk, salt, and sand, separately and together, provide an excellent lesson on solution, filtration, decantation, &c. &c. Strips of galvanized iron, silver threepenny pieces, bronze halfpennies, and many other common metallic substances, readily serve, under suitable chemical treatment, to teach the various characteristic differences of their several constituents. It is only just to the distinctive merit of Messrs. Harcourt and Madan's book to point out the remarkable precision and accuracy with which they have treated every part of their subject. We do not, however, agree with our authors in their recommendation to make all test solutions of known strength. A vast expenditure of time and trouble is thus caused, both in the preparation and employment of the liquids. Of course it is idle to make a standard solution, unless you measure it out on each occasion of use; and how are you to know what volume to take where, as in qualitative analysis, the very nature of the substance sought for is unknown?—or where, as in quantitative work, the amount of the substance known to be present is the very object of the analytical inquiry? The chief points calling for further remark in this manual are the excellent and exhaustive lists of "apparatus required" prefixed to each lesson, the compact descriptive lessons on the properties of the common radicles, the numerous hints as to home-made mechanical contrivances, and the precautions and directions which, under the head of "memoranda," open the volume. With one only of these directions we disagree. We are told, "In making a gas, the residue left in the generating vessel will often be of use, at any rate interesting as a specimen. It should not, as a rule, be thrown away." It is true that such residues may now and then be usefully kept, perhaps for further class-work; but even then they should be thrown into a common stock, not separately preserved. But just imagine a class of thirty pupils making antimonetted hydrogen, and storing in thirty bottles the resulting residues. It would require a clever chemist, indeed, to get anything "interesting" or "useful" out of such a mixture of the sulphates and tartrates of zinc, potassium, and antimony. He would probably mentally regard it as a confounded mess, and treat it as such.

Of some of the other books at the head of this review we have formerly expressed an opinion. Dr. Odling's most useful practical chemistry for medical students combines sound science with an effective plan of instruction, and introduces, with appropriate fulness, special examples of toxicological, physiological, and pathological importance. Bowman's book on 'Practical Chemistry' has long been a favorite with teachers; but as the last edition has been noticed in these columns before, we need only say that its editor, Mr. Bloxam, has improved upon its mode of treating chemical work in the laboratory. Mr. Bloxam is a master in the art of qualitative analysis, and in his new book, entitled 'Laboratory Teaching,' he has concentrated the results of his long and varied experience. The book, at first sight, startles us with an analytical table as its opening page. But the student need only look to page 3, and he will see how to begin the practice of chemistry; for the author presupposes no knowledge of the science on the part of the learner, yet his treatment of the subject differs in two ways from that ordinarily adopted. The ordinary course is chemical manipulation first, then the study of the chemical "reactions" of acids and bases, and lastly the identification of salts.

Mr. Bloxam, in order to make the plan less tedious, begins with the examination or analysis of simple salts, introducing the necessary reactions and the special manipulations requisite as they are wanted. This system has some merits, not the least of which is the insight which it gives the student of the particular forms or combinations under which acids and bases are most likely to present themselves. Thus will be avoided the absurd mistakes as to the nature of the substance under examination which students trained under the ordinary system often make. But it degrades the teaching of practical chemistry into an art of cram, an effect which the author himself, in fact, anticipates in his preface. The plan which we have recommended would obviate Mr. Bloxam's objections to the ordinary system. Let manipulation be taught by the use of common substances, and let the performance of each series of qualitative reactions be followed at once by their application to the analysis of solutions containing the particular substances which they include. Then the abstract character and the tediousness of the system of which Mr. Bloxam complains would be avoided. In parting with Mr. Bloxam's book, we have to note that it avoids all formulæ whatever, as well as everything that savours of theory, and that its nomenclature is a trifle old fashioned. We, however, value it very highly for its clear insight into the difficulties of the working student, and the ready help it offers to those who are obliged to work without a teacher. Nor must we omit to praise the numerous and excellent woodcuts, which not only adorn, but really explain, the text.

Dr. Attfield's 'Practical Chemistry' is a rich storehouse of information for the pharmaceutical as well as the medical student, and in the hands of a competent teacher will become a most efficient laboratory guide. The plates in it must also be mentioned as worthy of particular commendation.

The seventh edition of Fresenius's 'Qualitative Analysis' shows signs everywhere of extensive change. It has been greatly condensed, the notation and (though imperfectly) the nomenclature have been modernised, the complex cross-references have been abandoned, tabular analytical schemes have been introduced, and the materials of the volume have been in part rearranged. Some such alterations as these were required; but we cannot consider that the task has been performed with adequate skill or judgment. Many of the peculiar merits for which this work of the distinguished German analyst was noted have been obliterated in its last revision. In fact, the value of the sixth edition of this book has actually been enhanced by the appearance of the seventh.

We trust that what we have said as to the method of teaching chemistry in the laboratory, and as to the value of some of the treatises on this subject, may help to extend the effective pursuit of this important study. At the very outset of such work let us connect the shreds of knowledge which the student already possesses with the exacter and fuller revelations of science. Let us take the most feasible and characteristic of our lecture experiments as the basis of our laboratory practice and illustrations. Let us commence such laboratory practice by the exhibition of all the necessary apparatus and materials, and by the repetition of the experiment to be performed, accompanying, moreover, our lesson with oral explanations, and the use of diagrams of operations and of their results; and, finally, let us give a professional bias to our teaching wherever we can do so without damage to the clearness and soundness of our instruction. We hope that we have succeeded in convincing our readers that precept and example, here as elsewhere, must not be dissevered, and that no mode of chemical teaching can really be good which involves, like Mr. Tuckwell's unhappy suggestion, made before the British Association at Exeter last year, lectures without lessons the first year, and lessons without lectures the second.

II.—Obstetric Operations.[1]

THERE is probably no department of medicine which owes so much to the labour and ingenuity of British practitioners as obstetrics. The bare enumeration of the improvements in midwifery originated in this country, from the great invention of the Chamberlens down to Dr. Barnes's latest suggestion of using the écraseur in cases of dystocia, would form a list of which we may well be proud. While our countrymen have done so much to improve the practice of the art, it must be confessed that they have been somewhat behindhand in the systematic study of the various operative procedures necessary in the trying emergencies which so frequently arise.

It is true that all our manuals of obstetrics treat of operative midwifery at more or less length, and that one or two works of an elementary character have been specially devoted to the subject. But since the great work of Davis, which has now long been obsolete, no attempt has been made to discuss obstetric operations with the fulness of detail which their vast importance merits. The announcement of Dr. Barnes's lectures was, therefore, received with much interest. There are few men in England better suited for the task. From the commencement of his career this eminent physician has been remarkable for his unceasing assiduity in the cultivation of obstetric science, and above all for the essentially practical nature of his work; while as physician to the Royal Maternity Charity, a field which has already produced the admirable writings of Davis and the two Ramsbothams, he has enjoyed opportunities of gaining practical experience, without which his theoretical acquirements would have been of little value.

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[1] *Lectures on Obstetric Operations, including the Treatment of Hæmorrhage, and forming a Guide to the Management of Difficult Labour.* By ROBERT BARNES, M.D. Lond., F.R.C.P., Obstetric Physician to, and Lecturer on Midwifery and Diseases of Women and Children at, St. Thomas's Hospital, &c. &c. Pp. 526.

The chief charm, indeed, of the book before us is the impress every page bears of its being the work of one who has studied each statement he makes at the bedside.

We may differ from the author in many of his views, we may object somewhat to his mannerism, and we may not be able to conceal our conviction that he has often rendered but scanty justice to his fellow-labourers in the same field, but we cannot fail to recognise in his work the hand of a skilled and scientific physician, who has a thorough knowledge of his subject. And there is one point above all in which, as it seems to us, Dr. Barnes deserves the highest commendation. In spite of the advice we are constantly hearing, "jurare in verba nullius magistri," it is far too much the habit of medical writers to move in the old grooves, and to hesitate in the recommendation of a practice which is different from that usually taught by standard authorities. Our literature is full of illustrations of the way in which authors preach what they do not practise, and there are many who have not the courage to inculcate in their writings what they do in their actual work. From this fault Dr. Barnes is singularly free. We shall have occasion to point out how, on more subjects than one, he differs from the routine practice which author after author has taught, and it must be added that he is generally able to give a good and sufficient reason for the belief that is in him.

Dr. Barnes has only done himself justice in describing his work as "a fairly comprehensive treatise on a great department of medical practice." It is only a bare statement of fact to say that it is in itself by far the most complete guide in the emergencies of practice that has yet been published, and, as such, it well merits the careful study of every practitioner who wishes to qualify himself for the scientific pursuit of his profession.

In his first chapter Dr. Barnes takes a general view of the problem to be worked out in a normal labour. Here we have to consider the body to be expelled, the passages through which it has to pass, and the powers which effect its expulsion. Difficulty and delay may arise from defect in any one of these, and the treatment to be adopted must necessarily vary according to the cause.

Confining his attention, for the moment, to deficient or imperfect action in the expulsive power, Dr. Barnes briefly refers to an interesting point, which, so far as we know, has not been previously discussed in any English work, viz. "Can we, without resorting to oxytoxic medicines, arouse or impart a *vis à tergo*—can we apply direct mechanical force to push the fœtus out of the uterus, instead of dragging it?"

It is unquestionable that the accessory muscles employed in delivery often act feebly and imperfectly, and it has been shown by Von Ritgen and other German writers that defective action may be supplemented by firm pressure applied to the uterus during the pains, or, in their absence, at intervals to resemble them. In this way it is often very feasible to push the fœtus down into the pelvis; and although it may not be possible to complete delivery by this means alone, we undoubtedly may greatly accelerate it.

Since reading Von Ritgen's paper we have tried his plan in several cases, and can testify to its great occasional use in exciting uterine contractions, and in aiding the progess of a slow and powerless labour. It is a resource which it is well to bear in mind, since the method is little, if at all, known in this country. We well remember a case in which the pains had been lingering and feeble for many hours, and the head, which had barely passed through the brim, had been for some time quite stationary. A resort to the forceps was deemed essential, but, while they were being sent for, this plan was tried. The patient being laid on her back, the palms of the hands were spread out on each side of the uterus, and firm downward pressure in the axis of the brim was made with each pain. The effect was almost magical. With the first application of the pressure the head commenced to move and the action of the uterus was greatly increased, and in less than ten minutes the labour was over. So successful a result is, of course, exceptional, but we have no doubt that many a tedious labour might be shortened, and the use of instruments avoided, by a judicious exercise of pressure, which, carefully used, may be considered quite harmless. We presume that the "aid to parturition," which Dr. Protheroe Smith exhibited at Leeds, acted in a somewhat similar manner, only he committed the mistake of attempting to produce by a cumbrous and expensive instrument that which can be far more easily and efficiently done by the hand, that "master-instrument of all," as Dr. Barnes happily terms it.

The remainder of the chapter is devoted to a brief consideration of the obstetric bag and its contents. Like all practical accoucheurs, Dr. Barnes is opposed to the needless multiplication of instruments. Nothing is more absurd than the endless varieties of forceps which load the shelves of the instrument makers, leading one almost to imagine that it is impossible for a man to have any pretensions to eminence in obstetrics unless he has attached his name to a special pair of his own invention. One well-chosen pair of forceps is undoubtedly sufficient for all practical purposes, and we are pleased to find Dr. Barnes recommending Sir James Simpson's as one of the best types.

The next four chapters of the work are devoted to a very full and complete consideration of the forceps and their substitute, the lever, their power, mode of application, and the indications for their use. Dr. Barnes here lays special stress on the importance of using an instrument of sufficient power, showing very clearly how it is more conducive to the safety of the mother to have at our command a reserve of force, which we may use or not, as occasion may require, rather than be tempted by the feebleness of the instrument to violence of effort. This opinion, we think, all who have used Simpson's or a similar pair of forceps, and compared them with the short straight instrument so commonly employed, will cordially endorse.

After briefly considering the action of the forceps as a tractor, as a double lever, and compressor, Dr. Barnes passes on to a consideration of the action and uses of the lever. This instrument is evidently not a favorite with him, and he considers it to be, as a general rule, far inferior to the forceps, an opinion in which the majority of his readers will concur. It must not be forgotten, however, that the instrument is still held in high estimation by many able practitioners, and that for long it held its ground in the metropolis and elsewhere as a formidable competitor of the forceps. Dr. Barnes denies to the vectis any direct power as a tractor, maintaining that its apparent action in causing descent of the head is really due to the leverage exerted on the point over which it is placed. The opposite point being more or less fixed against the pelvis, descent of the point acted on naturally takes place. This view of its action is probably correct, but it seems to us that Dr. Barnes rather underrates the risk to the maternal soft parts which its use must often involve. It is quite true that, theoretically, the fulcrum ought to be the fingers of the left hand, on which the instrument should rest, and a careful operator, bearing the danger of bruising the maternal structures well in mind, may use it without danger. But the pelvic bones form so convenient a fulcrum, and one so near at hand, that this precaution will not always be remembered, and serious injury to the mother may be inflicted. Indeed, in many of the descriptions given of its application it is evident that it has been used in this way. Thus, in the latest edition of Cazeau's 'Midwifery,' annotated by Tarnier, we are told, "when the instrument is properly placed, the handle is to be raised, and, the arch of the pelvis serving as a fulcrum, it acts as a lever of the first kind." The risk of injury to the urethra and neighbouring structures must necessarily be very great when the lever is thus used.

It is probably as a rectifier of malpositions, as, for example, in brow presentations, when the head has not descended low into the pelvis, that the lever may be most scientifically and advantageously employed. Dr. Uvedale West, among others, has well shown how useful it may be in the difficulties which occasionally arise in such cases.

That eminently unscientific instrument the fillet is alluded to by Dr. Barnes, but scarcely with the reprobation which we should have expected. It is the more important to have a clear idea of the action of this instrument, since it appears still to be used by many practitioners, and it was lately advocated at a meeting of the London Obstetrical Society. No doubt, in cases in which the head has descended on to the perinæum, slight traction with the fillet may suffice to give it the start which, in such cases, is often all that is wanted. But in any other position not only must it be somewhat difficult to apply, but even when applied, if traction be made in the direction of the pelvic axes, the fillet being placed over the occiput, it must almost necessarily be pulled off. If we draw in any other direction we run the risk of changing the proper position of the head or of bruising the soft parts of the mother; while, if it be passed over the face, we are likely either to destroy the flexion of the chin on the sternum, or seriously to injure the child, as has happened more than once, even to the extent of amputating the head. All, indeed, that can be said in favour of the fillet is, that it may be improvised when the forceps are not at hand, but to recommend it as a substitute for them seems to us a most retrograde movement.

We regret that it is impossible, in the short space at our disposal, to enter any length into a discussion of the many points of interest which are raised in the chapters on the forceps. The whole of this portion of the book deserves the most careful study, and we know of no description of the mode of application and uses of this invaluable instrument more complete and trustworthy. We must, however, content ourselves with noticing one or two points in which Dr. Barnes's teaching differs from that contained in most of our standard authorities.

Although Dr. Barnes devotes some space to the description of the short forceps, and of the classical method of applying them over the ears of the child, it is evident that he considers the instrument to be in every way inferior to the long double-curved forceps, not only when the head is high in the pelvis, but even when it is resting on the perinæum. "The long forceps," he says, "possesses a more scientific adaptation to the pelvis throughout the whole canal than the short forceps. And if the long forceps is found in practice capable of taking the head through the pelvis from brim to outlet, it follows that, since the whole contains the part, the long forceps is qualified to take up the head at any point below the brim."

With this view we cordially agree. In some rare and exceptional cases, as, for example, when it is found essential to attempt the rotation of the head, as in certain occipito-posterior presentations which cannot otherwise be delivered, the short straight instrument may find a useful application. Practically, we believe that all who have much experience in the use of the long instrument prefer it to every other, and find it answer admirably in all positions of the head.

Although Dr. Barnes has made some concession to the ordinary method taught, and has described the application of the forceps in relation to the position of the head, he does not conceal his conviction that this plan leads to unnecessary difficulty, and that the forceps should, in the majority of cases, be passed in preference within the ilia, as in the high operation. We are tempted to extract his opinion on the subect.

"As the rule is to apply the short forceps over the ears, the introduction of the blades must be governed by the position of the head. You must first, then, determine the position of the head. So say most, if not all, our systematic writers. So many positions of the head, so many varying modes of applying the forceps! Now listen to the voice of experience—experience that so often sets at nought the refinements of theory, and clears out for herself a straight and simple path through the intricacies woven in the closet. Dr. Ramsbotham says, 'In employiug the short forceps, I lay it down as a rule that the blades should be passed over the ears; the head is more under command when embraced laterally, and there is the danger of injuring the soft parts during extraction. *But I confess that for many years I have been accustomed, however low the head may be, to introduce the blades between each ilium, because they usually pass up more easily in that direction.*' I think I am not wrong in believing that many others do the same thing; some not knowing it, and even imagining that they are following the ancient rule."

We are glad that Dr. Barnes has the courage to teach what Dr. Ramsbotham had only the sense to practise.

This view of the application of the forceps is not, however, peculiar to either of these obstetricians. It is the one commonly taught by the German writers, and we believe it to be far preferable to that generally inculcated. To a novice nothing is more confusing than an attempt to study the mode of applying the forceps from the descriptions in our standard works, and much of this confusion arises from the supposed necessity of describing as many different methods of applying the instrument as there are positions of the head. For ourselves, we find it by no means so easy always to feel the ear, which we are taught to look upon as the proper guide; and we believe Dr. Barnes to be perfectly right in supposing that many pass the instrument within the ilia without exactly knowing that they are doing so. Whether this be the case or no, we fail to see any reason why the head should be caught in the biparietal diameter, rather than over the brow and side of the occiput, as usually happens in the method now recommended.

Occipito-posterior positions, especially those in which the forehead is pushed down so as nearly to form a brow presentation, often give rise to great delay and difficulty. There is still much diversity of opinion as to the proper method of delivering with the forceps under these circumstances. The older authors, such as Denman, Hamilton, Ramsbotham, and Davis, recommend that the head should be extracted with the forehead to the pubes. Since the mechanism of delivery has been more carefully studied, and the natural rotation of the head backwards in spontaneous delivery has been fully recognised, it has seened to many the more scientific practice to attempt to produce artificially the turn which, in an unassisted case, usually takes place. Hence recent authors have generally taught that, in addition to extraction, we should give a quarter turn, so as to bring the face into the hollow of the sacrum. Thus, Sir James Simpson tells us, "I am strongly convinced that in artificial extraction of the head, in occipito-posterior positions, we should make the forehead pass backwards, and the occiput forwards, according to the rules we have seen nature following under the same conditions." It must be remembered, however, that turning the head forcibly with the forceps is a very different thing from the same rotation produced by the natural adaptation of the head to the pelvis. The difficulty, indeed, often arises from some interference with this natural adaptation, such as the want of due flexion of the chin on the sternum. It is not difficult, then, to understand how forcible twisting with the forceps, under these circumstances, might prove dangerous to the child. Indeed, Dr. Leishman has well shown in his excellent work that artificial rotation is only possible when the head is either free above the brim, or after it has descended quite to the floor of the pelvis. Recognising this, Dr. Barnes advises that no forcible attempt at rotation should be made, and points out, what we believe to be the case, that if the adaptation of the head to the pelvis is such as to favour rotation, the turn will take place of itself. It will be the duty of the accoucheur, therefore, to apply traction so as to wait upon nature, not to guide it. "Extraction, then, simply without troubling yourself about rotation, is all that is necessary. If nature prefer or insist upon rotation, your business is to consent. As the head advances the occiput may come forwards, and you will feel the handles of the forceps turn upon their axes." This seems to be at once a simpler and more scientific practice than any forcible attempt on the part of the accoucheur to usurp the place of nature, and insist upon turning the head "vi et armis." It has happened to us, in delivering with forceps under these conditions, to find the head turn, and to find it also come with the face forwards. If there is much resistance it will be time enough to try and effect the rotation artificially, and, in such cases, the straight forceps would probably be preferable. Since the difficulty often arises from want of proper flexion, this is, doubtless, a case in which the lever may be of great service by pulling down the occiput, at the same time that we aid the ascent of the forehead by upward pressure with the fingers. Dr. Uvedale West's admirable practical paper on this subject is well worthy of careful study by all who are interested in the question.

We are surprised to find no mention made of the risk to the child from delay in the second stage of labour, and of the importance of a much earlier use of the forceps on this account. This seems to us a serious omission, which we trust Dr. Barnes will supply in his next edition, as we know of no question of greater practical importance. The attention of the profession has been ably drawn to the subject by Dr. Hamilton, of Falkirk,[1] who by a much earlier use of instruments than is generally customary, has arrived at a fœtal mortality of about 1 in 317 cases, and has had, in his own practice, as many as 731 consecutive births without losing a single child. When we compare these results with a fœtal mortality of 1 in 20 or 30, which is about the average, taken from the reports of the Rotunda Hospital and other sources, it will be admitted that the question is one meriting our serious consideration. Dr. Hamilton uses the forceps on an average once in every eight cases, while the average in the Dublin School, under Collins, was 1 out of every 684. It is, probably, because this question can scarcely be argued without referring to figures and statistics, that Dr. Barnes has omitted to mention it, since statistics seem to be his "bête noire." The question, however, is one of primary importance; and if the extraordinary results which Dr. Hamilton is able to show are really due to his earlier resort to instrumental delivery, of which we entertain no doubt, then it behoves us to consider seriously whether the ordinary practice of delaying interference as long as possible is not mischievous in the extreme. Dr. Hamilton recommends us to use the forceps within a quarter of an hour after an ear can be felt, if no progress is being made. The more custom-

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[1] 'Edin. Med. Journ.,' vol. viii, 1862-3, p. 317.

ary rule is to wait two hours or more, and not to interfere until spontaneous delivery seems hopeless. All this time the uterus is contracting tightly round the body of the child, and the fœtal circulation is being seriously interfered with; we cannot wonder, therefore, that the proportion of still births should be so great. It may be argued that the risk to the mother would be greatly increased by the early resort to instrumental delivery; but when we require to interfere for this reason, the head is generally low in the pelvis, and the application of the instrument is easy. It seems to us that far more injury is likely to accrue to the mother from hours of fruitless labour and weary waiting, than from the comparatively simple operation then required. Were it not that we dare not refer to statistics in a review of Dr. Barnes's work, it would be easy to show that the maternal mortality is not a whit greater in the practice of those who use the forceps often than in that of those who employ them only as a *dernier ressort*, to be avoided as long as possible.

No less than eleven chapters are devoted to the consideration of turning, in which Dr. Barnes considers many sources of difficulty not generally included under this heading, such as difficult breech presentations, locked twins and double monstrosity, dorsal displacement of the arm, &c. The subject is evidently a favorite one with our author, and it is discussed with much detail and great clearness. The numerous original and excellent illustrations, which form a special feature of the work, render material assistance in the explanation of many points difficult to understand from a merely verbal description. Dr. Barnes's account of the way in which transverse presentation is produced, as well as of the means by which nature, when unassisted, terminates the labour by spontaneous version or evolution, is well worthy of careful study, and much more complete than any we have elsewhere met with. Space, however, will not admit of our doing more than merely refer our readers to it.

The bipolar principle in version, to the elaboration of which we are mainly indebted to Dr. Braxton Hicks's admirable book on "combined external and internal version," is carefully described and dwelt on at considerable length. This is unquestionably the greatest improvement in operative midwifery which has been made for many years. The possibility of altering the position of the child by external manipulation alone had been recognised and practised by Wigand, Esterlé, and others. It is, indeed, by no means a difficult thing to do, and we have ourselves successfully practised it in more than one instance, in which the existence of a transverse presentation had been recognised before the commencement of labour. The use of two hands in concert during the operation of turning had also been taught by Sir James Simpson and others; but they did not recognise the full extent to which the method could be carried, and it is beyond doubt to Dr. Hicks's excellent memoir that we owe the systematic description of the operation. We trust that Dr. Barnes's careful account of the services it is capable of rendering will popularise a practice which is probably not yet sufficiently understood by the generality of practitioners.

The difficulties which are apt to arise in breech cases have been but slightly described by systematic writers; yet our experience is the same as Dr. Barnes's, who says that a case in which the breech is impacted in the pelvis is amongst the most troublesome we may have to deal with. The practice he recommends is to break up the presenting part by bringing down a foot, which will enable us to control the farther progress of the labour. As long as the breech is pretty high in the pelvis, and the feet near the nates, this, no doubt, would be easy to do, and would afford the readiest solution of the difficulty. If, however, the breech has been pressed very low into the pelvis, so as to occupy its cavity, or if the legs are extended so that the feet are placed near the fundus, it would seem to be by no means easy to effect this alteration. Dr. Barnes admits the difficulty in the latter case, but recommends that the patient should be deeply chloroformed, and the hand introduced till one foot can be seized and brought down, and "no ordinary case of turning involves passing the arm so far." The insertion of the hand and arm past the breech in this manner is a serious operation, not to be lightly undertaken. We question whether, in a case of the sort, if a foot cannot be readily reached, the passing of a lac over the groin, by which considerable tractive power can be exercised, would not be preferable, and not so hazardous to the mother in the hands of practitioners of less operative skill than Dr. Barnes. A simple method of passing it is by means of a stout brass wire, bent into a loop, and guided over the groin after the manner of a blunt hook. Through this a piece of short tape or a skein of wool is passed, and on the wire being withdrawn the lac is carried over the groin.

In those difficult cases of turning in which the shoulder is pressed down into the pelvic brim, the liquor amnii drained off, and the uterus tightly contracted, the bipolar method of turning is no longer practicable, although even here considerable assistance may be derived from the hand outside co-operating with that passed into the uterus. There are two practical rules given by Dr. Barnes, which are not usually dwelt on, and which may prove of use. The first is as to position, and he recommends the position on the side in all dorso-anterior presentations, the left hand being introduced in preference; while in dorso-posterior presentations the patient should be placed on her back, either the right or left hand being used. The second is of great practical value, and has been specially dwelt on by Sir James Simpson. It is that, after introducing the hand, we should not be content to seize the first leg we meet, but choose the knee farthest from the presenting part. By pulling the *opposite* knee downwards the shoulder *must* be raised, while if both legs or the one on the same side are brought down this will not happen. This is opposed to the general description given of the operation, in which we are told to bring down both feet, and the reasons for it are very clearly shown in the diagrams in the book.

In cases in which the uterus is very tightly contracted round the fœtus, the child being necessarily dead, Dr. Barnes strongly advocates delivery by evisceration or decapitation, as safer to the mother and easier than forcible attempts at turning. This operation is happily seldom called for. For decapitation Dr. Barnes recommends Ramsbotham's cutting-hook, which will separate the head very effectually, but which is difficult to pass over the neck. A strong pair of scissors, or a stout single-wire écraseur, passed over the neck by means of a bent brass wire, alluded to above in speaking of breech presentations, seems to us a simpler and safer way of effecting this. The head would probably be easiest delivered by means of the cephalotribe.

Dr. Barnes devotes an entire chapter to that *quæstio vexata* in midwifery, turning in cases of contracted pelvis. We must, however, content ourselves with stating that he strongly advocates the operation in cases of minor contraction ranging from 3·25″ to 3·75,″ especially if the long forceps have failed, and entirely endorses Sir James Simpson's views as to the greater ease with which the head can be extracted with the base downwards. Although we do not doubt the value of turning as an alternative operation in such cases, and believe that by means of it many children, otherwise doomed to destruction, have been saved, we can hardly agree with Dr. Barnes that the operator is not in a worse position with regard to craniotomy if the attempt should not succeed. We have, indeed, more than once met with considerable difficulty in delivering by craniotomy in cases of the kind. We believe, however, that the cephalotribe would render great assistance in this emergency.

The succeeding chapter discusses those operations which involve the destruction and mutilation of the fœtus, including craniotomy, cephalotripsy, the use of Van Huevel's forceps-saw, and Dr. Barnes's own original suggestion for using a single-wire écraseur.

He gives a decided preference to the craniotomy forceps over the crotchet as a means of extraction after perforation, an opinion which most obstetricians of the present day will cordially endorse. The latter instrument we believe to be not only clumsy and comparatively inefficient, but much more likely to injure the maternal structures. In minor degrees of contraction, in which the forceps have been applied and failed, we have been in the habit of perforating without removing the forceps, a plan to which Dr. Barnes does not allude. The diminution in the size of the head thus effected is generally sufficient to admit of its extraction by the forceps with great ease.

In more advanced dystocia Dr. Barnes recommends the method lately advocated by Dr. Braxton Hicks, of removing as much as possible of the vault of the cranium, and then bringing down the face, either with a small hook or with the craniotomy forceps, so that the base of the skull shall be brought edgeways through the contracted pelvis. By this means he maintains that a full-sized head may be delivered with safety to the mother through a pelvis measuring even less than 2·00″ in the conjugate diameter.

The operation of cephalotripsy has lately attracted so much attention in this country that we are not surprised to find Dr. Barnes devoting a considerable space to its discussion, although he does not appear to have himself practised it to any very great extent. It has now been the recognised operation for so many years on the Continent, and is held in such high estimation by those who have practised it, that it is not very creditable to British accoucheurs to have been so tardy in testing its merits. The tide seems at last to have turned, and, within the last year or two, four or five varieties of cephalotribes have been introduced in Great Britain. The main endeavour in these has been to reduce the apparent bulk of the instrument and to render it more manageable. The size of the blades in all of these remains much the same as in Baudelocque's original instrument, and it is chiefly in the extra-vaginal portion that the improvement has been effected. We do not doubt that before long the cephalotribe will almost entirely supersede the craniotomy forceps and crotchet.

In discussing the powers of the cephalotribe, Dr. Barnes is of opinion that "the all-essential point is that it shall be able to compress, and even crush down, the base of the skull." In point of fact, however, in the majority of cases in which it is used, this is not done. Dr. Kidd, of Dublin, has clearly shown that, in the cases in which he has used it, the base of the skull was not destroyed, but tilted up, so as to pass edgeways through the contracted brim. The result is to produce much the same effect as in Dr. Hicks's plan of bringing down the face. It has also been shown, in a numerous series of experiments conducted on fœtuses, that the true base of the skull is not, as a rule, crushed by the instrument. If it is essential to break up the base, a more powerful instrument than those lately invented would be advisable; and Dr. Matthews Duncan may, perhaps, be right in thinking that we have gone too far in our endeavours at simplification, and in recommending an instrument differing but slightly from Baudelocque's original pattern.

Dr. Barnes's suggestion of using an écraseur to divide the head is ingenious and well worthy of consideration. The result would be much the same as that effected by Van Huevel's forceps-saw. Those who have used the latter instrument speak so enthusiastically in its favour that it is to be regretted that it has not been tried in this country. Dr. Barnes's operation has not, so far as we know, been practised even by himself, so that it is impossible to say how it may act. We fear, however, that it would prove very difficult to apply the loop of the wire accurately in the extreme cases of dystocia in which Dr. Barnes thinks it will prove serviceable.

In his chapter on the Cæsarean section Dr. Barnes shows himself to be the most uncompromising opponent of that operation. He maintains that even in the most extreme contraction, when the conjugate diameter measures no more than 1·50″, craniotomy offers a better prospect of recovery to the mother. The question is one most difficult to decide. We fear, however, that in cases of such extreme distortion the chances to the mother, after dragging a mutilated fœtus through so small an aperture, would be infinitesimal indeed, and that there would not be much to choose between the two operations as regards the ultimate result.

In his rhetorical objections to the Cæsarean section Dr. Barnes has scarcely done justice to the published facts regarding it. He says there may possibly be ten, or perhaps twenty, successful cases on record. Since, however, he quotes Pihan-Dufeillay's paper, he must be aware that that gentleman has collected no less than fifty successful cases between the years 1845 and 1852 alone. The reference to each of these is carefully given, and we can see no reason to doubt their authenticity. Dr. Winckler, of Gummersbach, has also reported fifteen cases operated on by himself or in his presence, which are not included in Pihan-Duffeillay's tables, seven of which recovered. We have no wish to advocate the Cæsarean section, and trust we may never be called upon to perform it, but we feel bound, in spite of Dr. Barnes's objection to statistics, to point out the incontestable fact that in Germany, where the operation is much oftener resorted to, recoveries are very far from uncommon, and that there are several women now alive on whom it has been performed more than once.

We are surprised to find that Dr. Barnes makes but little mention of gastrotomy after rupture of the uterus. We have a strong conviction that in cases in which the child has escaped into the abdominal cavity, gastrotomy gives a better chance to the mother than the usual plan of dragging the fœtus forcibly back through the laceration, and extracting per vaginam. Two interesting cases have been lately reported in this country, while in America the most encouraging results have followed the operation. When we reflect that after rupture there is always much blood and other fluids extravasated into the abdominal cavity, which must necessarily either decompose or act as foreign bodies, while after gastrotomy these may be removed, we can see a reason why the latter alternative seems to afford the mother a better chance of recovery. We in England, the country of ovariotomy, have come to look upon a simple abdominal incision as by no means the necessarily fatal thing it was formerly considered to be, and this operation clearly stands on a very different footing from the Cæsarean section.

The induction of premature labour, next to the invention of the forceps, is the greatest triumph of British midwifery, and has been more practised in this country than in any other. A simple and easily applied method of inducing uterine contraction is still a desideratum, since all of those in use are more or less uncertain, and leave something to be desired. Dr. Barnes makes a good division of the subject into the agents which are *provocative* of labour and those which are *accelerative*, and we believe there is much truth in his statement that the latter have been unduly overlooked, and that by confining our endeavours to the induction of uterine contraction only, leaving the labour to take care of itself, many infantile lives have been sacrificed. We fancy, however, that the majority of practitioners will scarcely be able to endorse his opinion "that it is just as feasible to make an appointment at any distance from home to carry out at one sitting the induction of labour as it is to cut for the stone." It is to be devoutly hoped that the directions he gives may conduce to this very desirable result.

One of the favorite plans in this country has been the use of the water douche, either the vaginal douche, as recommended by Kiwisch, or the intra-uterine douche. An instrument for the latter, by Lazarevitch, of Kharkoff, was exhibited at the conversazione of the London Obstetrical Society, and attracted considerable attention from its ingenuity. It has been, and we believe is still used, by several eminent metropolitan practitioners.

Dr. Barnes condemns *in toto* every proceeding of the kind, and we believe with good reason. We have heard of so many unfortunate results, that we confess we consider the method to be a most unsafe one.

Dr. Barnes regards separation of the membranes as one of the best means of inducing uterine contractions, and prefers for this purpose the passing of an elastic bougie for six or seven inches between the membranes and the uterine walls, where it is to be left for several hours. This is the plan practised by Braun, of Vienna, who uses catgut bougies, and we believe it to be one of the best that can be employed. The passing of a bougie so far, however, requires a little care and manipulative skill.

The plan of dilating the cervix, whether by sponge tents or caoutchouc bags, with the latter of which Dr. Barnes's name is specially connected, he regards as unscientific, "and not based on a rational view of the physiological or clinical history of the process." He considers that the dilators should be discarded as *provocatives* of labour, and that they should be chiefly used as *acceleratives* after uterine contractions have been induced by other means. This will undoubtedly surprise many who have been in the habit of regarding the so-called "Barnes's method" of inducing labour as essentially depending on cervical dilatation, and in his earlier writings on the subject no mention is made of the division into provocatives and acceleratives. The plan now recommended may be looked upon as the result of matured experience, and, as such, will command the attention of all who are studying the subject.

We regret, however, that Dr. Barnes makes no further mention of the inventor of the method than is contained in the following sentence:—"The subject attracted the attention of Dr. Keiller in Edinburgh, early in 1859, and in the March of that year he, Dr. Graham Weir assisting, accelerated a labour, which had been provoked by other means, by introducing within the os the simple caoutchouc bag and gently distending it."

This sentence clearly shows that, from the first, Dr. Keiller assigned to the dilators their proper function as accelerators, and not as provocatives, which it seems to have taken Dr. Barnes some years to discover; but one would scarcely gather from it that Dr. Keiller had not only been the first to introduce the plan of cervical dilatation by caoutchouc bags, but that he repeatedly demonstrated their use to many practitioners in Edinburgh, and employed them in numerous cases before Dr. Barnes's original paper was published. This is the more to be regretted since, in the 'Edinburgh Medical Journal' for March, 1863, Dr. Keiller has written a paper especially to prove his priority of invention. The following quotation from the proof sheets of a forthcoming volume of 'Transactions of the Edinburgh Obstetrical Society' will show the opinion of the profession in that city on the subject:—" Sir James Simpson and other members expressed their conviction that the priority in this matter was quite evident, and if any name ought to be attached to this special mode of practice it was unquestionably that of Dr. Keiller, and not of Dr. Barnes."[1]

These observations are rendered necessary by the fact that the invention of the caoutchouc dilators is almost universally attributed to Dr. Barnes, both in this country and abroad. Thus, in Tarnier's edition of Cazeaux he is spoken of as the inventor; in Joulin's 'Midwifery' the operation is described as the " dilatation de Barnes," and the bags themselves are universally known as Barnes's bags, and are so talked of all through Dr. Barnes's book, the fact being that they differ from Keiller's chiefly in being fiddle-shaped instead of pyriform.

While we have deemed it necessary to make these observations in justice to a distinguished accoucheur, whose modesty has prevented him from pressing his own claims, we hasten to add that the profession is unquestionably greatly indebted to Dr. Barnes for extending the uses of the dilators, and making them much more widely known than they, perhaps, otherwise would have been.

We lack space to follow Dr. Barnes in his consideration of the cases in which the induction of premature labour may be practised. We would only specially refer to his recommendation of combining this operation with craniotomy in cases of extreme deformity, thus rendering the latter more easy, or possibly obviating the necessity of resorting to the Cæsarean section. This is a subject of great importance, and well worthy of attention.

The four remaining chapters are devoted to a very full consideration of the varieties of hæmorrhage occurring in connection with pregnancy and delivery. This is a subject not usually treated of under the head of obstetric operations, yet it is of such vast practical importance, gives rise to such serious emergencies, and so frequently calls for active interference on the part of the practitioner, that Dr. Barnes has done very wisely in devoting to it much careful consideration.

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[1] As accuracy is of importance in a question of this kind, we append the titles and dates of the papers referred to:—" On a new mode of Induction of Premature Labour, with new mode of Dilating Os Uteri, &c., by means of a Dilating India-rubber Pessary." By Dr. Keiller, Edin. Obst. Soc., March 9th, 1859. " Case of Induction of Premature Labour by Keiller's Dilator." By Dr. Pattison, ibid., May 11th, 1859. " Cases of Induction of Premature Labour by Keiller's Dilator." By Dr. Keiller, ibid., May, 11th, 1859. Dr. Keiller's paper claiming priority of invention was published in the 'Edin. Med. Journ.' for March, 1863.

The first lecture is devoted to hæmorrhage in connection with abortion. This is a subject very inadequately considered in most of our standard works, and yet some of the most trying cases met with in practice come under this heading. We are glad to find Dr. Barnes insisting so strongly on the importance of removing as much as possible of the uterine contents in cases in which part of the ovum has been thrown off, for which purpose he recommends, when necessary, the dilation of the cervix by means of the laminaria tents. There can be no question of the truth of his assertion that no woman can be considered safe from the risk of hæmorrhage, perhaps still more of septicæmia, as long as portions of the ovum are retained in utero. The whole of this chapter is well worthy of careful study, and it forms a more complete guide to the treatment of these troublesome cases than any writing we know of.

The succeeding chapter contains a résumé of Dr. Barnes's well-known views on the management of placenta prævia, originally published in his Lettsomian lectures. Although containing much interesting matter, well worthy of discussion, the space at our disposal will not admit of its consideration. Post-partum hæmorrhage, one of the most dreaded accidents of labour, forms the subject of the concluding chapter of the work. Dr. Barnes's views on this subject have been brought under discussion at the Obstetrical Society of London, and met with general approval. We are inclined to agree with some of the speakers at that meeting in thinking that Dr. Barnes has not sufficiently dwelt on the paramount importance of prevention, and in no instance is the truth of the maxim as to its superiority over cure better illustrated than in this. Everything, indeed, depends on the management of the third stage of labour; and when one hears of a practitioner who is constantly meeting with cases of post-partum hæmorrhage, it may be confidently assumed that there is something defective in his practice at this stage of delivery. Dr. Barnes scarcely does justice to Dr. Credé's views in this respect, when he accuses him of teaching "what has long been a familiar practice in this country." We know of no English work in which the strong firm pressure on the uterus, which is the essence of Credé's method, is inculcated. At most we are told to follow the contracting uterus by the hand, as it expels the child, then to wait five, ten, or fifteen minutes, until the uterus contracts, and possibly to aid the contraction from time to time by gentle friction. Now, Credé teaches that very strong compression should be made on the uterus from the instant the child is expelled, so as to squeeze the placenta out, and cause it to be extruded from the vagina, without touching the cord, and that the hand should not be removed from the uterus until the placenta has been thrown off. This is the plan we have followed for several years with the best possible results. It is a fact, which must be more than accidental, that since employing it we have not met with a single case in which there was excessive hæmorrhage, although several of our patients have been women who had flooded terribly in former labours. Nor is the prevention of hæmorrhage the only advantage, since firm contraction prevents the formation of clots in utero, and hence prevents after pains, and also, as we believe, is the best means of diminishing the risk of septic absorption. It requires some knowledge of the plan to succeed in effecting the spontaneous extrusion of the placenta, since, at first, one is apt not to use compression with the steadiness and vigour which are necessary. A most useful adjunct, after the expulsion of the placenta, is the ergot of rye, which here finds its best application. We know of several eminent practitioners who now always make a point of administering a full dose after delivery, with the view of keeping up steady uterine contraction.

The peculiarity of Dr. Barnes's teaching on the subject of post-partum hæmorrhage is his advocacy of astringent injections in severe cases, so as to coagulate the blood in the gaping mouths of the vessels. For this purpose he uses a solution of the perchloride of iron. It is to be remarked that Dr. Barnes does not recommend this in the minor degrees of hæmorrhage, in which the contractility is not entirely lost, but in those severe and intractable cases in which all the usual means of inducing contraction have been tried and failed, and in which the uterus remains persistently lax and flabby. It is unquestionable that in such cases the remedy is of the utmost value, and all the speakers in the debate at the Obstetrical Society already alluded to bore testimony to its efficacy. When, in spite of every remedy, the terrible drain is going on, it is an unspeakable comfort to have at our command an agent so certain in its action and so readily employed. That there are certain theoretical risks in connection with it Dr. Barnes himself admits, but we know of no case in which any bad consequences have been known to attend its use. Dr. Barnes strongly advocates transfusion as a last resource. There are so many cases on record in which it has apparently snatched patients from the very gates of death that it certainly should never be overlooked as a resource in apparently hopeless cases. The difficulty is with regard to the apparatus. Most of them are cumbrous and expensive, and, what is worse, they are rarely at hand when wanted. Perhaps Aveling's little apparatus would answer well for carrying about, but Graily Hewitt's seems, on the whole, one of the best.

We must here close our cursory and necessarily imperfect review of Dr. Barnes's work. We have, perforce, been obliged to omit the discussion of many points of interest, for which we recommend our readers to consult the book itself. As a safe and reliable guide in the difficulties of practice we know of no book published of late years to equal it, and we must end by congratulating Dr. Barnes on the production of a work which of necessity must place him in the first rank of British obstetricians.

III.—Recent Advances in Cutaneous Pathology.[1]

Whatever legitimate reason there may be for separating other departments of medicine from the study of the profession as a whole, there would seem to be none in the case of cutaneous diseases. Their investigation does not demand skill in the use of instruments like the ophthalmoscope or the sphygmograph; the application of the microscope in diagnosis is as easy with the skin as with the urine; and the treatment of these affections requires none of the dexterity requisite in operative surgery or in midwifery. Moreover dermatology can only be rightly studied as a branch of general pathology, on which its advance is dependent, and to which in its turn it renders the most important aid.

It is, indeed, instructive to remark how closely exact knowledge of the morbid conditions of the skin has followed that of the other organs of the body. For a long time after the dawn of pathology in the seventeenth century, diseases of the skin were still studied under the influence of the arbitrary metaphysical dogmas of which we are even now reminded by such phrases as *similia similibus* (or *contraria contrariis*) *curantur* and *vis medicatrix naturæ*. In the works of the elder dermatologists we do not meet with observations of the actual appearances seen, but with theories of their dependence on assumed humours, diatheses, temperaments, and acrimonies of the blood.

It was the distinguishing merit of Willan that he subjected affections of the skin to that exact anatomical study which in the hands of Morgagni, Hunter, and Laennec, gave so great an impulse to general pathology. Clear and accurate in detail, and often felicitous in definition, Willan's 'Delineations of Cutaneous Diseases' laid a sure foundation for the objective study of this

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[1] *Lehrbuch der Hautkrankheiten.* Von Dr. Isidor Neumann, Docent an der k. k. Universität in Wien. Mit. 49 Holzschnitten. Wien., 1869, Braumüller Pp. 368.

branch of medicine. It is remarkable that after the continuation of this work by Bateman so little was done by English dermatologists until quite recent times. Biett, however, who had crossed the Channel during the war with Napoleon in order to study under Willan, introduced his master's system into France, adding to it important contributions of his own; and the doctrines of the English school continued to be taught by Biett's disciples, among whom Cazenave and Schedel are well known by their writings. The premature attempt of Alibert to construct a so-called natural system of dermatology failed for lack of the knowledge necessary for such a task, though his name became celebrated by the popularity of the lectures he delivered in the hospital of St. Louis, and by the beauty of his atlas of plates.

The next great advance in cutaneous pathology was made in Germany, and was the result of the impulse given to all medical studies by the general application of the microscope to pathological research. Just as the exact diagnosis of disease introduced by Laennec was accompanied by the rise of the anatomical school of Willan, so the elaborate investigations in morbid anatomy carried out by Rokitansky led to a corresponding progress in dermatology. In fact, it was avowedly as a disciple of the Viennese pathologist that Hebra constructed the system of cutaneous medicine which he has made so well known. The most important service, however, directly rendered to dermatology by the microscope up to a very recent date has been the discovery that many cutaneous diseases depend upon the presence of a vegetable parasite. In 1839 Schönlein detected the achorion in Willan's porrigo lupinosa, now known as favus or tinea favosa. Subsequently Eichstedt demonstrated the microsporon furfurans in pityriasis (vel tinea) versicolor, and Malmsten the trichophyton in true ringworm or tinea tonsurans, the porrigo scutulata of Willan. Meissner, Gruby, Bazin, and Köbner have completed our present knowledge of this group of disease by detecting the parasitic character of some cases of disease of the nails, of sycosis, alopecia and eczema. Among the additions made by German writers to our knowledge of diseases of the skin Von Bärensprung's papers on herpes zoster, and congenital syphilis deserve especial mention; but to no pathologist are we so much indebted as to Virchow, of whom it may be truly said '*nihil tetigit quod non ornavit.*'

As an example of the close connection between general pathology and the exact knowledge of cutaneous diseases we desire, in the present article, to call attention to the most recent researches of the Vienna school of dermatology, a good summary of which is offered by the work of Dr. Neumann, placed at the head of this article. Hitherto the application of the microscope to dermatology has been of somewhat the same kind as that which led to the detection of the various morbid constituents of the urine, and of the cells, fibres and other elements which make up new growths. But now that we are gradually acquiring a knowledge of the manner in which morbid tissues are built up, we are no longer satisfied with examining their *débris*, and tumours are classified according to their thin sections instead of their scrapings.

The difficulties of making a similar step in the investigation of cutaneous diseases are obviously great, but they have been in great measure overcome by the skill and perseverance of Professor Biesiadecki, Dr. Neumann himself, and some other members of the Vienna school, among whom should be mentioned Drs. Warren and Haight of the United States.

The chief interest then of the work under review lies in the account given of the minute anatomy of the skin in disease; and a number of excellent woodcuts illustrate the microscopic appearances. These would, however, be still more useful if the magnifying power employed in each case were noted. The general pathology and therapeutics are little more than an intelligent abridgment of what is taught by Professor Hebra, and fairly represents his views on those subjects which have not yet been treated in his text-book. The type is clear and bold, like that of Rokitansky's Pathological Anatomy, and, as in that work, there is an unfortunate absence of all headings to the pages, or any other direction to the contents but the short table at the beginning. The list of errata is also much too long, and includes many more than mere printer's blunders. Moreover there is no proper distinction to mark the beginning of a new subject; so that, though far better printed, the book is, for its size, almost as difficult to refer to rapidly as the German edition of Hebra's text-book itself. Notwithstanding these defects of form there is so much valuable information as amply to reward the reader, and the style differs from that of the work just mentioned in being remarkably succinct.

The author very properly begins his work by a careful description of the healthy structure of the skin and its appendages. Further on is an interesting account of the changes which take place in the skin during old age. Pigment is increased; the cutis is thin, the cuticle dry, rough, and fissured. The sebaceous follicles are dilated and prominent, but scarcely secrete at all; the sudoriparous glands are shrunken, and appear also to have lost their functions. The papillæ are also diminished in size, and their vessels and nerves atrophied (figs. 38, 39).

Dr. Neumann's enumeration of the "external forms" assumed by cutaneous diseases, corresponding with the primary divisions of Willan's system, is identical with that of Professor Hebra, except that among the "secondary forms of efflorescence," the *schuppengrind* (Crusta lamellosa) is replaced by "collections of pigment." Prof. Hardy adds to the "lésions élémentaires" usually admitted three others—"taches hématiques" (purpura), products of sebaceous secretion, and parasitic animals and plants.

The short sections upon the general diagnosis, etiology, and therapeutics of cutaneous diseases, do not add anything of importance to the elaborate second chapter of Hebra's work on these subjects. The system of classification there adopted is also followed, "for want of a better," but is preceded by a list of those of Plenk, Willan, Alibert, Wilson, Bazin, Hardy, and others.[1]

Our readers will be thankful that Dr. Neumann does not propose any new classification of his own; and, in fact, the orders, genera, and species he adopts are so simple, that the rest of the book looks more like a treatise on any other department of medicine, and less like one on mineralogy or botany than is often the case.

The chapters on hyperæmia, anæmia, and abnormalities of the cutaneous glands, closely follow those on the same subjects in Hebra's text-book.[2] We need only mention the record of a unique case of sudoriparous adenoma, recorded by Dr. Lotzbeck, in which the tubules were arranged in pyramids like those of the kidney.

Farther on in the book (p. 134) Dr. Neumann repeats Prof. Hebra's account of *sudamina* and *miliaria*.[3] The former name has been improperly applied in Germany to an eczematous eruption produced by the irritation of excessive sweats, and is familiar enough to pedestrians. True sudamina (Miliaria crystallina) are the pearly vesicles which may appear in the course of continued fevers, acute rheumatism, and other pyrexiæ. Their anatomy has been lately investigated by Dr. Haight, of New York,[4] who finds the fluid to be placed in a simple cavity between the layers of the horny cuticle. As a sudoriparous gland always opens into the vesicle, there can be little doubt of the truth of Von Bärensprung's hypothesis, that the fluid is retained sweat. So that a true miliary vesicle is anatomically

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[1] These tables may be advantageously compared with the list published by Dr. Tilbury Fox (Hardwicke, 1864).

[2] 'Sydenham Society's Translation,' vol. i, p. 80 and p. 383.

[3] For a much fuller and clear description of the diseases of the sebaceous glands, we may refer our readers to Prof. Rindfleisch, 'Pathol. Gewebelehre,' p. 261, *et seqq.*

[4] 'Ueber Blasenbildung bei einigen Hautkrankheiten,' p. 4, fig. 3.

precisely analogous to a comedo arising from obstruction of a sebaceous follicle.

In the next section, on the *contagious exanthemata,* beside an interesting account of a case of smallpox supervening on a syphilitic eruption, we find the following statement of the result of experiments on the transmission of syphilis by vaccination:

"If vaccination is performed with a mixture of equal parts of pus from a [soft] chancre and vaccine lymph, the vesicle is observed to sink on the eighth day, and on its removal an ulcer is seen, with the characters of a soft chancre. . . . . If matter taken from a syphilitic patient be used to vaccinate a healthy person, a genuine indurated sore, instead of the ordinary vesicle, will be the result, *should any blood have been mixed with the lymph;* but if the lymph is clear and free from blood, the result will in most cases be a pustule which shows none of the characteristics of syphilis." (p. 86.)

A woodcut in this chapter illustrates the minute anatomy of a variolous pustule, and the following account of it from Auspitz and Basch is subjoined:

"On the second day of the eruption the epidermis is raised, apparently by the rete Malpighii having become thicker from an actual increase of size in its several cells. The nuclei are enlarged, the vessels of the cutis dilated even beyond its papillary layer, and on their walls are found numerous small round cells. Similar corpuscles are seen in the stroma of the papillæ, which are otherwise unaltered, nor are the cutaneous glands in any way affected.

"When a *vesicle* has taken the place of this papule, a layer of elongated cells is observed beneath the epidermis, and these pass without interruption into the obviously swollen cells of the rete Malpighii. Under this layer is a network enclosing serum, which is situated nearer the epidermis than the corium, and forms the proper vesicle. This network consists of bands of apparently fibrous structure, which plainly result from the spindle-shaped cells of the Malpighian layer being flattened out and pressed together. This network contains the pus-corpuscles, sometimes as many as ten of them being found in one of the lacunæ. Beneath it are round cells, intermediate in character between those of the trabeculæ of the network and those of the rete Malpighii. They reach down between the papillæ of the cutis, which are shorter and thicker than normal, and hence contrast with those around the vescicle, which are remarkably elongated.

"When the *pustule* is fully formed, the network spreads towards the corium, and thus becomes deeper, while its lacunæ are filled with round [pus] cells. The vessels of the papillæ beneath are surrounded by a mass of compressed cells, which plainly pass into connective-tissue corpuscles as one traces them to the circumference of the pustule. The puriform contents are covered in by a capsule, formed of two layers of non-nucleated epidermic scales. Beside ordinary pus-corpuscles, there are found in the network above described a number of nuclear bodies with granular contents, and undissolved by acetic acid."

It will be observed that this description confirms the statements of Fuchs, Bateman, Rayer and Gustav Simon, as to a sacculated structure of the smallpox pustule: an opinion controverted by Von Bärensprung, and also by Prof. Hebra (see vol. i, pp. 335-8 of the Sydenham Society's translation). Professor Rindfleisch, of Bonn,[1] distinguishes the anatomical structure of a smallpox pustule from that of one of eczema, ecthyma, &c. In the latter, the process begins in the papillary layer of the cutis as a serous catarrh, which afterwards becomes purulent: in the former it begins as a parenchymatous inflammation of the rete mucosum itself, and the pus-cells infiltrate the whole epidermis. The dissepiments are confined to the upper part of the pustule, and are formed by the horny structure alone. The umbilicus depends on the presence of a hair follicle or sweat gland. The cicatrix is the result of secondary implication of the cutis in suppurative, not diphtheritic, inflammation.

The next of Dr. Neumann's classes, following Hebra's arrangement, is that of "*acute non-contagious inflammatory processes*," and contains a number of affections, of which little can be said in common. They are Erythema, Pellagra, Roseola, Urticaria, Combustio, Congelatio, Erysipelas, Furunculus, Anthrax, *Bouton d'Aleppo*, Maliasmus or glanders, Pustula necrogenica, Pustula maligna, Herpes (including Herpes zoster), and Miliaria. In all of these, the presence of "inflammation" is proved by its four signs; but so it is in eczema, scabies, and many other cutaneous diseases. Common dermatitis produced by local irritants should be treated of first in a treatise on diseases of the skin, and under it would be naturally included "erythema intertrigo," combustio, congelatio, and the effects of croton oil and of the bites of the various insects which infest the skin. The diagnosis of the cause of these traumatic inflammations is sometimes of considerable importance, though scarcely touched on in this or other works on dermatology.

Erysipelas is a form of inflammation of the skin, with its own clearly marked characters, and should rather be studied in connection with the acute exanthemata, on the one hand, and pyæmia on the other, than with such totally different conditions as nettlerash and carbuncle. The precise relation of traumatic and idiopathic erysipelas is certainly not fully made out. Bill-

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[1] 'Lehrbuch der Pathologischen Gewebelehre,' pp. 234–239. Leipzig, Engelmann, 1867-9.

roth's theory, that the former condition essentially depends on absorption by the lymphatics, seems very probable. It would then need to be expressed by a word that should bear the same relation to the old name, lymphangeitis, as "ichorrhæmia" does to "phlebitis." Anatomically, erysipelas consists in an infiltration of the corium and subcutaneous tissues with "inflammation-corpuscles," whereby their constituent structures are separated and swollen, while the epidermis and rete Malpighii are scarcely at all affected (fig. 13, and Biesiadecki, Sitzungsbericht d. k. k. Akad. der Wiss. 1867).

There appears no reason for regarding furunculus and anthrax as differing except in degree. They are both local suppurations seated in the deeper layers of the cutis, or in the subcutaneous fascia, and are further distinguished by the constancy with which actual necrosis of tissue is observed.

Glanders, pustula necrogenica, malignant pustule, are the results of direct absorption of animal poisons, and if they exhibit some features of resemblance to traumatic erysipelas, have as little as possible in common with the effects of a burn, with simple herpes or with sudamina. Moreover, how can either erysipelas or glanders be placed among non-contagious diseases?

Zona, again, is an affection with such clearly-marked characters that it cannot with advantage be classed with any of those united in the chapter under review. Its associations, as Mr. Hutchinson has pointed out in his well-known and admirable paper on the subject in the 'London Hospital Reports,' are, on the one hand, with neuroses, on the other with exanthemata; but it is perhaps as much a disease *sui generis* as any with which we are acquainted.

If this and the parasitic H. circinatus, or ringworm of the trunk, be separated from the other so-called forms of herpes, the remaining local varieties form a tolerably well-marked group of symptoms which it is quite unnecessary further to subdivide: for few dermatologists are prepared to follow Bärensprung in regarding them all as forms of zona.[1] In M. Hardy's so-called natural arrangements, H. præputialis is regarded, without reason assigned, as an eczema; H. labialis is placed with sudamina and typhus rash as a symptomatic eruption; and zona forms one of a heterogeneous class (maladies cutanées accidentelles) which includes not only erythema and urticaria, but prurigo, strophulus, ecthyma, pemphigus, and acne.

Erythema nodosum is a curious affection, with well-defined features of its own; but the remaining species of erythema are

[1] See the January number of this 'Review' in 1862, and Hebra's criticisms, op. cit., vol. i, pp. 366-7.

best included, as they have been by Hebra, under E. multiforme, and even then can only be defined as simple inflammation of the skin, not dependent on a traumatic origin, and not tending to form vesicles or pustules. The affection classed by Hardy as erythème copahique is in some respects allied to urticaria. The writer just named omits roseola as a separate disease, and it is scarcely more than mentioned by Neumann. If the name is worth retaining at all, it might be convenient to restrict it to any "rose-rash" without exudation or secondary products; as seen, for instance, in some cases of cholera, in typhoid fever before the appearance of its characteristic eruption, and as a very evanescent eruption in various disorders of children.

The most important contribution to our knowledge of the minute anatomy of the diseases enumerated above has been made by Biesiadecki, who has given a careful description of the tissue changes in zona. The normal papillæ of the skin are increased in size by serous infiltration, and by multiplication of minute, round exudation-corpuscles. Their connective-tissue-cells appear swollen, and the fusiform cells (whose presence in the normal rete Malpighii was first pointed out by Biesiadecki himself) increase in number, and spread through the papillary layer of cutis and the mucous layer of epidermis. This is the papillary stage. Then these cells increase in number so as to form a network throughout the papules, in the meshes of which exudation corpuscles appear and rapidly multiply, so as to push aside the normal epithelial cells. The papillary vessels beneath are meanwhile becoming dilated, and the contents of the network gradually assume the characters of pus. Whether this pus is developed directly from the cells of the rete mucosum, or only from the fusiform cells (which are, no doubt, modified connective-tissue corpuscles), is an important question in its bearing on general pathology. Dr. Neumann believes that the epithelial cells themselves take part in the pus-formation by division and multiplication of nuclei. Another important fact in the anatomy of zona is, that the neurilemma of the affected nerves, which observations of Danielssen and Von Bärensprung had before shown to be sometimes "inflamed" in appearance, is found to be filled with small, round, nucleated cells; and this condition is illustrated by a drawing (fig. 14), from a preparation made by Dr. Haight.

In the paper from which this drawing is copied, Dr. Haight describes and figures a partial division of the vesicle or pustule of zona into loculi, formed by fusiform nucleated cells. The serum is enclosed in this network, between the horny layer of cuticle and the rete mucosum. In the papillary layer of the corium are round granular cells, like white blood-corpuscles in size, and as the vesicle changes to a pustule, these increase in number and fill the network above." The same observer finds the minute anatomy of the bullæ of erysipelas to be precisely the same, except that no collections of these round "exudation corpuscles" are found around the nerves.

The next chapter of Dr. Neumann's work is occupied by an account of the diseases classed together by Hebra as "chronic exudative processes"—Psoriasis, Lichen ruber and L. scrophulosorum;[1] Eczema, Prurigo, and Scabies; Acne disseminata, Sycosis and Acne rosacea; Impetigo and Ecthyma; and Pemphigus. Of these affections, acne and sycosis are far better studied as simple inflammations of the sebaceous glands and hair-follicles respectively, produced by local causes of irritation. Acne rosacea, again, should be entirely separated from true acne, with which it has nothing in common but their occasional coincidence, as well as from the rest, which it does not even resemble in appearance. It is nothing but hypertrophy of the cutis and subcutaneous tissues, with enlargement of their blood-vessels, as a consequence of chronic congestion of the nose and other parts; and is analogous to the œdema durum of continued venous congestion, the hypertrophy seen in a long-prolapsed rectum or uterus, and, again, to the effects of more intermittent blood-stasis observed in chilblains. Impetigo and ecthyma are justly regarded by Hebra and his disciples merely as convenient terms to denote small or large pustules. Impetigo is the pustular form of eczema so common on the face and scalp of children, and though its peculiarities, due to its seat and subjects, may make it convenient to give it a distinct clinical name, as "porrigo," it passes into typical eczema by every shade of combination and transition, and agrees with it in its most essential characters. Most cases of so-called ecthyma, on the other hand, are either of traumatic or syphilitic origin, or else due to scabies. The remaining cases which are met with among children in dispensary practice appear to be more allied to furunculi than any other affection.[2]

Our author defines *scabies* to be "an artificial eczema,[3] produced partly by the irritation of the acarus, partly by the

[1] Dr. Neumann does not mention the pityriasis rubra described in Hebra's 17th chapter.

[2] Ecthyma is classed among furuncular affections by Mr. Erasmus Wilson.

[3] The same meaning is expressed by M. Hardy's statements, that in every hundred cases of scabies there are ninety-nine of prurigo; in every ten cases one of a vesicular eruption; and that in every twenty cases of ecthyma nineteen are due to the presence of the acarus. More rarely, he says, we observe the development of lichen eczéma or impetigo, but "it is not the acarus," he continues, "which has produced these *dartres*, it has simply acted as an irritant in leading to the manifestation of a pre-existing diathesis" ('Leçons sur les Maladies de la Peau,' 2me partie, p. 196).

patient's nails." That the characteristic appearances of itch are due to these two causes, and especially to the latter, is no doubt true, as has been fully shown by Hebra in his exhaustive chapter on this subject; but we should prefer to call scabies a form of dermatitis, for it has none of the distinctive marks of eczema, and differs from it so entirely in its natural history, prognosis, and treatment, that a practically useful arrangement should separate the two diseases as widely as possible. Dr. Neumann gives a good summary of Hebra's account of scabies, with some excellent woodcuts of the acarus, male, female, and young, with its six feet, besides one of an entire burrow, containing its inhabitant, her eggs, and her droppings. He also tells us (what is scarcely necessary) that it is no use looking for marks of scratching in the cases of lunatics kept in strait-jackets.

The various methods of treatment are also enumerated, including the two-hours' cure carried out at St. Louis, and justly boasted of by Professor Hardy. In fact, any one who has seen how quickly and efficiently itch may be cured, both in Paris and Vienna, must regret the waste of time, when a patient receives once a week a modicum of sulphur ointment, which he is told to rub well in. The proper use of baths, and the efficient application of ointments and other local remedies by suitable attendants, is one of the particulars in which the ordinary treatment at hospitals in this country falls lamentably short of that almost universally adopted abroad.

The remaining diseases classed as chronic inflammations of the skin—psoriasis, lichen, eczema, prurigo, and pemphigus—form undoubtedly a very natural group. They correspond, in fact, very nearly with the "dartres," as defined by most French dermatologists, or the arthritic group of M. Bazin. So far as pityriasis is a substantive disease, after abstracting cases of tinea versicolor and of seborrhœa sicca, it will come into the same group, and is so admitted both by Hebra and Hardy. Many cases of so-called lichen are, no doubt, abortive eczema, but there remain others (apart, of course, from papular forms of syphilis), which both these writers admit as distinct from, though allied to, eczema. It has been known and taught for many years past, both at St. Louis and Vienna, that almost all the cases of prurigo senilis depend on the presence of pediculi, though English physicians appear till lately to have been insufficiently aware of the fact. Indeed, M. Hardy refers to these cases very suitably, next to those of scabies, as examples of a "maladie parasitaire," and remarks, "l'absence de poux est fort rare dans une eruption prurigineuse."[1] The cases of

[1] Op. cit., 2me partie, pp. 86 and 189.

pruritus, or simple cutaneous irritation without any papular or other eruption, ought also, as Hebra observes, to be separated from true prurigo, and placed among neuroses. Pemphigus and prurigo are not admitted among the dartres by Hardy, but they certainly share in the chief characteristics of this group. Beside the chronic course, the great irritation, the frequent hereditary transmission, the proneness to recurrence, and the absence of deep or permanent injury to the tissues of the skin—which make these diseases a natural group, by whatever name we may call them—they also agree, as Mr. Hutchinson has pointed out, in their behaviour with reagents, especially arsenic. Strangely enough, the good effect of this drug in pemphigus, which few English observers doubt, is denied by Hebra, Hardy, and Neumann.

As in other parts of our author's work, the most important addition made to Hebra's account of these diseases is in the department of minute anatomy. The condition of the skin, as seen in fine sections made perpendicular to its surface, in different stages of *eczema*, has been fully investigated by Biesiadecki.[1] In the early stages of the process it resembles that observed by the same author in cases of zona (v. supra); the papillæ are enlarged, and filled with serum and exudation cells, while the normal connective-tissue corpuscles of the cutis are swollen, and the same fusiform cells are seen in the rete Malpighii. When the formation of cells in the papillæ becomes very rapid, and fluid is also rapidly effused into the network of fusiform corpuscles and epidermic cells which has formed the eczematous papule, a vesicle is produced. The fusiform cells take the office of ordinary corpuscles of areolar tissue, in carrying the liquid exudation from the inflamed papilla beneath to the epidermis, where it collects under the horny layer of cuticle (E. vesiculosum et impetiginodes). When this last falls off the same channels convey the continually secreted eczematous fluid from the papillary layer of the cutis to the rete Malpighii, now laid bare, and so keep up the characteristic moisture of eczema madidans (E. rubrum of Willan and Hardy). As the disease becomes chronic, the increase in size of the papillæ becomes more marked than ever, and their solid fibrous structure is hypertrophied, until at last they become visible to the naked eye. The whole cutis is thickened, and in long-standing cases bands of dense fibrous tissue stretch down among the fat cells of the subcutaneous fascia (figs. 21 and 22).[2] The cutaneous glands only share in the changes of the surrounding structures. Dr. Neumann's

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[1] Sitzungsb. d. k. k. Akad., 1867. (Quoted by Neumann.)
[2] Compare the figure given by Rindfleisch (op. cit., fig. 100).

own observations were made on the ear of a rabbit, in which artificial eczema was produced by rubbing in croton oil, in the manner so fully described by Hebra (vol. i, pp. 87, 88, 'Syd. Soc. Tr.'). The first effect was "a rhythmical contraction of the blood-vessels, which became alternately empty and congested; afterwards they remained widely dilated, and at last circulation ceased; the ear lost its transparency, was hot and swollen, and after a few hours became covered with numerous blebs. At the end of forty-eight hours the rabbit was killed, and the skin of the ear was found infiltrated with serum and cells."

We have here a good illustration of the important light thrown upon general pathology by investigations in cutaneous medicine. Pagenstecker, in a paper presented to the Imperial Academy of Sciences on the 23rd April last year, states that the fusiform cells of the epidermis, discovered by Biesiadecki, are increased in number whenever the formation of cuticle becomes excessive, in fact in most chronic as well as acute inflammations of the skin. He believes, moreover, that they pass by autogenic movement from the papillary layer to the rete mucosum; and this quite agrees with Biesiadecki's own observation, that they may often be seen half in the corium and half in the Malpighian stratum. Lastly, Pagenstecker thinks the conclusion is forced on us that, since these "migratory cells" (Wanderzellen) certainly do not form pus, since they appear neither to be thrown off by reaching the surface of the skin, nor to perish in the stratum mucosum, their destination can only be actual transformation into epidermic cells. If this theory should become generally accepted, it would probably relieve our text books of that undemonstrated structure of skin and mucous membranes which is known in Vienna as "die von den Engländern angegebene basement-membrane."

The pathological question remains, whence come the small round cells, which all the observers we have quoted find in eczema, zona, and other "exudative" diseases, first collected round the vessels of the papilla, then filling its network, and lastly, infiltrating the rete mucosum, and together with the effused serum forming the liquid contents of a vesicle or pustule. It is obvious that the Hunterian doctrine of coagulable lymph would be quite inadequate to account for these appearances. Nor are they readily explained by the modified "exudation theory," which was formed in consequence of Schleiden's discoveries, and which, even now, is rather in a state of suspended animation than of death. No clear fluid is seen to become filled with molecules, from which nuclei and cells are subsequently formed by spontaneous generation. So far the facts seem in accordance with Virchow's dogma *omnis cellula e cellulâ.* But it is impossible to help seeing the explanation which may be derived from the remarkable observations of Cohnheim (abundantly confirmed both in Germany and England) on the passage of white blood corpuscles through the walls of their vessels under irritation. These "exudation cells," first found so remarkably arranged around the vessels of the papilla, may as probably be corpuscles which have just made their transit as the offspring of connective-tissue cells, or the result of spontaneous molecular aggregation. It is true that there remains the admirable series of observations by which the great master of modern pathology showed how inflammatory processes are carried on in non-vascular tissues, and it is probable that we shall never have to unlearn the great fact that the primary seat of "inflammation," and most other morbid processes, is in the tissues, and not in the fluid which feeds them.[1] But in the present state of our knowledge it is not an improbable supposition that the "exudation" cells of vesicles and pustules have, like the serous fluid in which they float, passed into the rete mucosum from the subjacent vessels of the cutis, instead of being generated where we find them by the epithelial cells themselves, or by the fusiform corpuscles of Biesiadecki.

Dr. Neumann follows Hebra in recognising a papular, a pustular, a weeping and a squamous form of eczema, as well as the vesicular one described by Willan. That almost every case of eczema will, at some period or other of its progress, develop vesicles, is probable, but there can be little question that our notions of the nature of the disease and of its treatment have been much improved by the wider definition of its characters.

The infiltration of the skin, the peculiar character of the secretion (except in abortive papular forms, or in the squamous stage of involution), and the gradual thickening of the papillæ and cutis, while the epidermis remains thin—these are sufficient signs of physical diagnosis from the other "dartres," especially when we add the marked predilection of eczema in all its forms for certain regions of the body, which are as remarkably avoided in most cases of psoriasis and lichen, its nearest allies. Hebra's forms of eczema adopted by Dr. Neumann, coincide almost precisely with the *variéties suivant l'aspect* of Professor Hardy. Thus *Eczema vesiculosum* of the German corresponds to *Eczéma*

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[1] Von Recklinghausen's observations on the cornea when kept alive separate from the body confirm, if confirmation were needed, the views which have been made so familiar by Virchow's 'Lectures on the Cellular Pathology,' and for which we were in this country prepared by the original labours of Goodsir and Redfern.

simplex and *E. rubrum* (in part) of the French author, *E. papulosum* to eczéma lichénoïde, *E. impetiginosum v. crustosum* to eczéma impetigo, *E. rubrum v. madidans* to eczéma fendillé (*E. rimosum*) pretty nearly, and *E. squamosum* to the "complication" described by Hardy as a "pityriasis, qui survient ordinairement à la fin de la maladie," of which he says, "en raison le leur concomitance si fréquente, nous nons sommes demandés si ces deux affections n'étaient pas un seul et même maladie, à une période differente de son évolution, et nous croyons cette opinion parfaitement soutenable."[1]

The characteristic marks of *psoriasis* are so clear that modern dermatologists have done little more than separate from it the scaly form of eczema (pityriasis diffusa), and the squamous varieties of syphilitic eruptions. At the same time the useless name "lepra" is now quite given up on the continent, or applied to true leprosy. The morbid anatomy of the disease remained an almost complete blank until Wertheim published his researches on skin excised from living patients. He found the papillæ of the cutis enlarged from twelve to fifteen times their natural size, and believed their vessels to be also dilated. Dr. Neumann has followed up the investigation, taking his specimens, as he was careful to tell us, from private patients, whose consent he had previously obtained. Sections of the excised pieces of skin, generally tinted with carmine, showed the whole cuticle, including the Malpighian layer, greatly hypertrophied, and also the corium filled with abundant caudate cells, arranged chiefly, but not exclusively, around the blood-vessels of the papillæ (figs. 16, 17). The early condition of the skin in psoriasis then, would not differ (as the author remarks) from that already described in the latter stages of eczema, but the "inflammation" of the papillary layer of the cutis, which in the one case leads to a fluid "exudation" in the rete Malpighii, produces in the other a rapid formation of epithelial cells in the same stratum. In psoriasis, as in eczema, the cutaneous glands appear to be but little affected. The hypothesis advanced by M. Cazenave that the sudoriparous glands are primarily diseased in the former malady is, therefore, not only improbable, but demonstrably incorrect: and the various scaly affections dependent on sebaceous secretions, as lupus erythematosus and pityriasis capitis are as distinct from psoriasis in their pathology as in their course. The amount of oily matter found by chemical analysis in the scales of psoriasis is not more than is accounted for by the accumulated normal secretion of the sebaceous follicles. Their peculiar white and shining appearance

[1] Op. cit., 1re partie, p. 69.

seems to be due to the fact of air penetrating between the epidermic cells of which they are composed.

Willan's genus *lichen* included several affections which agree only in the presence of papules. It is one of Hebra's merits to have defined the characters of true lichen (*L. exudativus ruber*) as distinct from the papular form of eczema (l'eczème lichenoides). His account of its anatomy states that the root-sheathes of the hair are dilated and funnel shaped, while the papillæ are enlarged and their vessels dilated. The late Dr. Hillier found the hairs themselves fibrous and brittle. Dr. Neumann gives a very full account of the result of his examination of sections hardened in gum.[1] Beside general thickening of the cutis and epidermis, with dilatation of the papillary vessels, much as described above in the case of chronic eczema, the root-sheaths of the hairs are surrounded by a greatly increased number of nucleated cells, the hairs themselves split up from the bulb, their follicles are dilated, and the unstricted fibres connected with them are greatly hypertrophied. This last character has been also noticed in long-standing cases of eczema, in elephantiasis Arabum, and other chronic affections.

The disease Hebra calls *Lichen scrophulosorum* is not generally recognised in this country. From the observation of Dr. Moriz Kohn ('Sitzungsb. d. k. k. Akad. d. Wissensch.,' October, 1868) it would seem to be essentially an affection of the sebaceous glands, and therefore not distantly related to the pityriasis of the head and trunk common in phthisical and other cachectic subjects, the *Seborrhœa sicca* of Casenave, described by MM. Dumoulin and Hardy under the name *Scrofulide cornée*. At the same time it must be stated that Professor Hebra himself denies any connection between lichen scrofulosorum and pityriasis scrophulosorum, and his description[2] differs in several respects from that of Professor Hardy.[3] The acknowledged variety of both affections, if they really are independent, makes the comparison between them difficult, but they seem at least to agree in etiology and minute anatomy.

True idiopathic *prurigo*, as distinguished from pruritus without papules, and the papular eruption from the effects of lice

[1] This mode of preparation, though common in Vienna, does not appear to be much known in this country. It is particularly applicable to the investigation of the lung. The tissue to be examined is first washed and soaked in a concentrated solution of gum arabic. It is then taken out and enclosed in a paper box or trough (or simply twisted up in a fold of paper), large enough to contain about an equal bulk of the same solution. The whole is suspended in alcohol until the water has been drawn out and a mass remains behind of the consistence of soap, admirably adapted for cutting fine sections, which only need to be soaked in water and then tinted and mounted as usual.

[2] Op. cit., vol. ii, pp. 53, 54.

[3] Op. cit., 1re partie, pp. 145, 146.

(so-called prurigo pedicularis), certainly belongs to the same group as the foregoing diseases, which it resembles in its chronic course, its proneness to relapse, and the irritation which accompanies it, yet it is not included among the *dartres* by most French dermatologists. The drawings Dr. Neumann gives of its minute anatomy (figs. 28 and 29) show the same fibrous thickening of the papillæ and other structures of the skin as is found in other chronic inflammations of the mucous stratum of epidermis and papillary layer of cutis. So far, support is given to Von Bärensprung's hypothesis that the sensation of itching has its seat in the papillæ, and is, therefore, absent when they are destroyed, as in ulcers before they heal. We find at all events that this peculiar sensation does not affect the deeper structures, and is more or less entirely absent in exanthems, of which syphilis is a striking example.

We have already discussed the remaining members of Hebra's heterogeneous group of chronic exudative diseases, and the only one remaining which appears naturally connected with psoriasis and its allies is *pemphigus*. Until lately neither chemical nor microscopical investigation had thrown any light on this somewhat rare but well-marked and interesting malady. It has, however, been now established by the researches of Dr. Haight, that the bullæ of pemphigus differ from the vesicles of variola, zona, &c., above described. The fluid is not situated between the mucous and horny stratum of the epidermis, but between the layers of the latter, for the floor, as well as the roof of the bulla, is formed of flat cells, which show no nuclei and do not absorb carmine. Moreover, the cavity is simply filled with serum, and shows no locular network of spindle-shaped cells, as found in the above-mentioned diseases, and also in the bullæ of erysipelas and in those produced by burns. In one case of chronic pemphigus quoted, Dr. Hertz found extensive lardaceous disease of the viscera.

In the extensive and ill-defined class of "hypertrophies" Dr. Neumann places the whole series of *syphilitic affections* of the skin. The publication of Professor Hebra's great work has not reached to this point in his system; but the account given of this group of maladies in the present volume coincides almost exactly with his lectures, as heard by the writer of this review in 1864. There is prefixed a brief but careful summary of the various doctrines on syphilis, which have found learned exponents at Vienna in Sigmund, Zeissl and Auspitz. The conclusion arrived at by the author is that "the chancre can no longer be considered a disease independent of syphilis. The pus of the soft chancre contains a concentrated poison, which produces an acute morbid process, and thus prevents general infection by [local] destruction, while the secretion of the hard chancre sets up a slowly working contamination ('intoxication') of the blood. Nevertheless, it remains true that the soft chancre is, as a rule, a local affection, that inoculation from it upon healthy subjects only produces fresh chancres, and that it is rarely, at least, followed by syphilis." So that one may almost call Dr. Neumann a unicist in theory, a dualist in practice.

Lupus is classed by Dr. Neumann among hypertrophies. Professor Hebra, while admitting its occurrence in scrofulous patients, maintains most strongly that it is not confined to them any more than to the subjects of hereditary syphilis. He looks on it as a new growth of fibrous nodules in the cutis, which most often affects those whose health is from any cause already impaired. Dr. Neumann, both from his own researches into the minute anatomy of lupus, and from those of Wedl, Auspitz, and other independent observers, fully confirms this opinion. The constant condition found is increase of cells in the corium (eine die Lederhaut durchsetzende Zelleninfiltration) with swelling of the rete mucosum and enlargement of the papillæ. In fact, each nodule of lupus shows essentially the same characters as those of long-standing eczema and other chronic inflammations of the skin, but the hypertrophy of fibrous tissue is much greater and more circumscribed; it extends deeper, and is followed by the characteristic ulcerations and cicatrices of this terrible disease. We must not omit to mention that Professor Rindfleisch, of Bonn,[1] considers the seat of lupus not to be primarily the corium, but the sebaceous and sudoriparous glands, the process beginning in their fibrous capsules. Accordingly he names the disease an adenoma, and no one can doubt that the morbid product of which he gives a microscopic section in fig. 110 may be fairly so called. But the uniform testimony of all other observers, including Virchow, Wedl, Auspitz, and Dr. Neumann himself, is against the cutaneous glands taking any part in true lupus; in fact, they are generally destroyed. Virchow classes this affection among those produced by the new formation of granulation-tissue in the cutis, and agrees with Hebra in disbelieving its dependence on either scrofula or hereditary syphilis.[2]

There is, however, a malady named by M. Cazenave *Lupus erythematosus*, which has but little to do with true lupus, either in anatomy or pathology, and is certainly a sebaceous disease. Its occurrence on the nose and cheeks, its colour, and the small

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[1] Op. cit., p. 269.

[2] 'Die Krankhaften Geschwülste,' bd. ii, s. 487, 491-4.

scales which cover the patch, were described by Professor Hebra in 1845, six years before M. Cazenave's paper appeared, and he then recognised its true character, and proposed for it the name *seborrhœa congestiva.* It is the same malady styled by Veiel *erythema lupinosum*, by Biett *erythème centrifuge*, and by Hardy, *scrofulide érythémateuse.* The determination of its true character as a chronic inflammation of the sebaceous follicles was made by Dr. Neumann himself, and he gives a full account of its microscopical appearances, with a good illustrative figure. The result of an independent investigation, by Dr. Geddings, of New York,[1] communicated in a paper read before the Imperial Academy of Sciences in Vienna, fully confirms the sebaceous character of the malady and the two beautiful plates accompanying this paper leave no doubt of its anatomy.

The treatment of lupus recommended by Dr. Neumann is that long carried out by Professor Hebra, and consists chiefly in the destruction of the nodules by local caustics of various strength. For detailed account of these we must refer our readers to pp. 222—227 of the work under review. We may remark that the French authors, while insisting on the constitutional nature of the disease, make free use of the same method of treatment, and on the other hand cod-liver oil is largely used in the Vienna hospitals when otherwise indicated.

The disease known as *elephantiasis Arabum* or pachyderma resembles lupus in being a chronic inflammation of the corium and subjacent tissue, but differs from it anatomically in the new growth consisting of simply more or less œdematous superficial fascia, without any granulation-tissue (Rindfleisch). The seat of the hypertrophy is in the deeper layers of the cutis, not in the papillæ, which again distinguishes it microscopically from the chronic forms of eczema and other superficial inflammations. It is, therefore, a benignant or "homologous" structure, and its clinical history corresponds with its anatomy. The œdema, so characteristic of this disease, is not caused by mere serum, but by a fluid rich in fibrine, so that Virchow describes pachyderma as a diffuse fibroma of the cutis, combined with *lymphatic* œdema.[2] It appears almost always to begin with attacks of erythema or erysipelas, and these usually recur again and again, leaving fresh increase of fibrous hypertrophy every time.

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[1] Sitzb., 12 März, 1868.

[2] Op. cit., vol. i, p. 301. In Bateman's 'Practical Synopsis of Cutaneous Diseases' (1824) elephantiasis Arabum is carefully distinguished from true leprosy, and is correctly defined as "an enormously tumid condition of the leg, arising from a repeated effusion and collection of a *lymphatic* and *gelatinous* matter in the cellular membrane under the skin, in consequence of inflammation of the ymphatic glands and vessels: the skin itself is much thickened," &c. (p. 314).

The microscopic anatomy of *ichthyosis* clearly shows that the true congenital form of this disease does not depend on collections of dried-up sebum, but on excessive development of the horny layer of the cuticle, combined with considerable thickening of the whole skin. At the same time there is increased secretion from the sebaceous glands, which accounts for the greasy character of the scales, for their frequently dirty colour, and for the masses adhering so strongly to each other and to the skin.[1] Dr. Neumann gives two woodcuts of microscopic sections of the skin in cases of this disease (figs. 34 and 35), and quotes the following analysis by Schlossberger.

An alcoholic extract of scrapings from the skin deposited, on evaporation, crystals of stearin, and of (apparently) hippuric acid; while earthy phosphates, oil-globules, and cholesterine were left behind. A further treatment of the scales with ether showed a quantity of solid and liquid fat, as well as more cholesterine. After incineration, there remained chlorides of potass and soda, with phosphates of lime and magnesia, and a certain amount of iron. The following account is taken from Professor Rindfleisch:[2]

"On removing and breaking through a crust of ichthyosis, vertical streaks, or even actual fibrillæ, may be observed on the fresh surface. When it has been macerated in a weak alkaline solution, and carefully torn to pieces with needles, it may sometimes be entirely divided into prismatic rods, which might be equally well described as short and thick fibres. Each of them consists of a number of horny laminæ, arranged concentrically round an axis like the rings of a tree."

Occasionally a hair may occupy the centre of one of these prisms; but this appears to be accidental, for their peculiar character seems to depend on the arrangement of the subjacent papillæ alone.[3]

The pathological anatomy of *leprosy* (lepra vera, elephantiasis græcorum) has been fully investigated by Virchow. Dr. Neumann and Professor Rindfleisch only confirm the description given in the second volume of 'Die Krankhaften Geschwülste,' (pp. 509 *et seqq.*). The substance of the leprous nodules may be regarded as the type of granulation-tissue, and shows all the

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[1] The similar adherence of the scales of psoriasis is due, according to Rindfleisch, to their never having assumed the complete characters of the superficial, horny cells of the cuticle.

[2] Op. cit., p. 242.

[3] The same arrangement of horny laminæ round an axis corresponding with that of a papilla was observed by Dr. Hilton Fagge, and is well shown in the first figure in his paper on "Ichthyosis and other Cutaneous Affections," in the 'Guy's Reports' for 1869-70.

transitional cell-forms characteristic of this new growth; but it is probably unique in its long duration before undergoing the destructive ulceration which appears always to ensue at last. The anæsthetic form of the disease is also proved to depend on a corresponding new formation in the sheaths of the affected nerves (*perineuritis chronica leprosa*), and in this important fact the independent observations of Boeck and Danielssen, and of Dr. Carter of Bombay, agree with those of Virchow.[1]

Dr. Neumann devotes a short section to the description of a few cases observed by himself and Buhl, in which waxy or "amyloid" degeneration of the skin, beginning apparently in the vessels of the papillæ, was combined with considerable increase of tissue in the form of papules and warts, and ended in ulceration.

In a fatal case of *scleriasis*, complicated with Bright's disease, there was found increase of the white connective and elastic fibres of the corium, together with excess of pigment, chiefly around the blood-vessels. These vessels themselves, the rete mucosum, and the cutaneous glands were unaltered. Cases quoted from Arning and Förster agree in absence of any change in the epidermis and capillary layer of the cutis, the hair-follicles, or glands, while excess of yellow elastic fibres was present in both cases, but in the latter these were not so abundant as the white connective tissue. No further light is thrown by Dr. Neumann on the general pathology of this affection or its relation to other cutaneous maladies. We may, however, refer our readers to the critical account of keloid, scleriasis, and their allies by Dr. Fagge, which appeared in the 'Guy's Hospital Reports' for 1867.[2]

The disease described as *cheloid* by Alibert appears not to differ anatomically whether it occurs spontaneously or upon a cicatrix —the genuine and spurious varieties. Dr. Neumann figures a microscopic section of a nodule of cheloid (fig. 40) with a description of the appearances presented. The epidermis and papillary

[1] In an interesting record of the post-mortem appearances in a case of leprosy of the skin and throat, Dr. Moxon states that he found no thickening of the nerves, but an apparently adenoid sheath around the veins of the arms. He also lays stress on the extreme thinness of the integument where not invaded by the disease. In this case death ensued from tubercular phthisis, with extensive lardaceous degeneration of the viscera. See 'Guy's Hosp. Rep.,' 1868.

[2] See also the supplementary remarks by the same writer in the present number of the 'Reports' (vol. xv, pp. 297–305). In a lecture on cheloid, delivered in January, 1864, Prof. Hebra stated that in his experience he has found the disease disappear spontaneously in the course of years. As he defines it to be "an idiopathic scar," it is not impossible that the cases he refers to may have resembled Addison's rather than Alibert's cheloid.

layer are, at least, primarily unaffected; the deeper layer of the cutis is thickened by a multitude of fibres running parallel with the surface, which begin in bundles of spindle-shaped cells first observed surrounding the external coat (*tunica adventitia*) of the arterioles of the skin. The cutaneous glands are at first unchanged, but afterwards become pressed upon so as gradually to atrophy and disappear. This account is fully confirmed by the independent researches of Dr. J. C. Warren, of Boston,[1] who found the same cellular growth in the adventitia of the arteries, spreading to the deeper layers of the corium in the form of dense horizontal fibres running parallel to the long axis of the cheloid nodule. The epidermis and papillary layer are unaffected, as are the cutaneous glands, until they gradually disappear in the later stages of the disease. Dr. Warren concludes that there is no difference anatomically between true and false cheloid, that both consist of a "fibro-plastic" tumour of the corium, and that the "malignity" or tendency to recur after removal, which is so marked a feature of the disease, simply depends on the fact that the primary affection of the blood-vessels can be traced some distance beyond the circumference of the tumour, and can therefore reproduce a fresh nodule after the original one has been apparently completely extirpated. The chief anatomical difference between cheloid and an ordinary cicatrix appears to be that the fibres of connective tissue of which the latter is composed form a felted network like that of normal cutis, with no definite parallel arrangement. Virchow[2] regards the most typical forms of cheloid as sarcomata (a group which, it will be remembered, corresponds in great part with Lebert's fibro-plastic tumours), and distinguishes from these the fibrous growths described by Addison as true keloid, and others which he believes to be of cancerous or syphilitic origin. Rindfleisch agrees in regarding cheloid as a sarcoma, but places its seat in the papillary layer of the cutis, a supposition disproved, as we have seen, by microscopic investigations, which were wanting when he formed this opinion.

The *vegetable parasites* of the skin have been so well studied by Prof. Bazin and other foreign observers, and by the late Dr. Hillier, Mr. Berkeley, and Dr. Tilbury Fox in this country, that parasitic diseases may now be considered as among those best understood. As to their ætiology, Dr. Neumann admits that, though favus chiefly occurs in ill-nourished and sickly patients, yet pityriasis versicolor and herpes tonsurans attack

[1] Reprinted from 54th vol. of the 'Transactions of the Imperial Academy of Sciences,' Vienna, 1868.

[2] Op. cit., ii band, s. 242 *et seqq.*

those who are strong and healthy. He believes the single predisposing cause in these cases to be want of cleanliness.

The parasitic origin of the curious disease described by Hebra as *eczema marginatum*, may now be considered as established. This opinion was first propounded by Köbner, and has since been confirmed by Dr. Anderson and other observers. Dr. Neumann assents to it, though he points out that in some cases the results of the primary irritation may remain after the parasite has disappeared. He has not only found in these cases the fungus of tinea (herpes) tonsurans, but also of T. (pityriasis) versicolor. The success of antiparasitic treatment has confirmed this view of the nature of this complaint.

The curious atrophic depressions on the nails after severe attacks of illness, to which the late M. Beau and Dr. Wilks have drawn attention, have not entirely escaped notice in Germany. Prof. Rindfleisch writes—" I have repeatedly observed that the depression and restoration of nutrition which accompanies subacute febrile affections, are exhibited in the nails by a groove with a slight elevation behind it" (op. cit., p. 245).

Dr. Neumann appends to his work an ample list of prescriptions, both internal and external, which have been devised by various physicians for the cure of diseases of the skin. The majority of them are used in Prof. Hebra's wards, and the prominence there given to local treatment, is at once seen by the large number of formulæ for external applications.

IV.—Sanitary Statistics of Sweden.[1]

It seems to be generally acknowledged at the present day that statistics of mortality, founded as they are on a more or less incomplete and imperfect system of registration, are to a large extent untrustworthy. But though this is the case, the advantages which have ever resulted from an approximate knowledge of the variations in the death-rate from season to

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[1] *Bidrag till Sveriges Officiela Statistik. Helso-och Sjukvården.* I. Ny följd, 6. Sundhets-Collegii Underdåniga Berättelse året 1866. Stockholm, 1869. Quarto. Pp. 258.

A Contribution to the Official Statistics of Sweden. State of Health and Disease. I. New Series, 6. The Humble Report of the College of Health for the year 1866. Stockholm, 1869.

season, and of the probable causes of the same, more than counterbalance the defects of the system as at present organised. How far these shortcomings may be obviated is a question that has yet to be answered. But even granting that our registration system may be perfected, we may well ask whether there is not a further step to be taken—whether it would not be politic to extend the principle of registration, which has been hitherto restricted to *mortality*, to *disease* also.

The necessity of obtaining accurate statistics of morbility is, we take it, patent to the most superficial observer. Viewed as a problem in political economy, there can be no doubt regarding the paramount importance of ascertaining the actual sanitary state of a population at any given time.

Death is one thing, disease is essentially another. An epidemic of influenza, characterised, perhaps, by a very low percentage of mortality, may, nevertheless, by its mere excessive prevalence, paralyse a community to a far greater degree than a very fatal, yet limited outbreak of cholera. Instances of this are numerous, and do not need to be specified here.

Again, under ordinary circumstances the prevalence, or otherwise, of an endemic disease stands in no direct relation to the published returns of mortality attributed to it. The argument in favour of registration of disease deducible from this fact applies *à fortiori* in the instance of an epidemic, where the ratio of cases to deaths is of a most varying and uncertain character.

Difficulties, no doubt, stand in the way of carrying out an effective system of disease registration; but they are not insurmountable; and the fact that already some governments have inaugurated and prosecuted such a system should give us the greater courage in essaying to follow their example.

The volume now before us is a timely arrival, as it affords an opportunity of not merely examining the organisation of a system whose end is the registration of disease, but also of witnessing some of the effects of its operation.

For some years back the College of Health at Stockholm has received monthly and annual reports of the sanitary state of the country, from a corps of officially appointed medical men, who are styled provincial and district physicians. To them is paid, in return for their services, a fixed yearly salary, these services being not merely the collecting and reporting of sanitary statistics, but also attendance on the sick poor of their several districts.

The monthly reports furnished by these physicians are published from time to time in the 'Hygiea,' while the annual returns are compiled and arranged by Hr. Wistrand whom we may call the Swedish Medical Registrar-General, and are finally presented to the King in the form of a "Blue-book," or volume of official reports and tables.

In the year 1866 the number of the government medical officers was largely increased, and their sphere of work extended to the most distant parts of the kingdom; so that we find the general report for that year founded on the returns sent in by 237 medical men, of whom 125 were provincial or extra-provincial physicians, 30 district physicians, 2 physicians to the mines, and 80 town physicians. With regard to these titles, it would appear that in *status* there was no distinction between the provincial and district physicians, that the latter officers were appointed in cases where the *province* was of too large dimensions to be placed under the care of one medical man.

In the reports are embodied minute and accurate descriptions of the topography of their several districts, of the habits and employment of the inhabitants, and of the endemic diseases prevalent amongst them.

Such, briefly, is the system of disease registration at present working in Sweden. How it works may best be seen from a consideration of the report now in our hands.

The work is divided into two parts, the first being taken up with the report proper, the second containing elaborate tables illustrative of the same. The former, which extends over 163 pages of this large quarto volume, is divided under the following heads:

1. Sanitary state of the civil population.
2. Conscription.
3. Sanitary state and service of the army and navy.
4. Institutions for trained nurses.
5. Vaccination.
6. Legal autopsies.
7. Apothecaries' establishments.
8. Veterinary and epizootic establishments.
9. Subsidies for scientific expeditions.
10. Labours of the College of Health.
11. Causes of death in the towns.

Of these headings the first is the most important, and to the foreign reader the most interesting. We will briefly consider some of its most striking features.

It is subdivided into the following sections:

I. Topography and endemic diseases.
II. Compendium of the Medical Officers' Report.
III. Medical Officers of Health.
IV. Civil hospitals.
V. Mineral waters, salt water baths, and hydropathic establishments.
VI. Sanitary state of the prisons.

Section i. is prefaced by a few remarks, which it may be well to transcribe at length.

"As a continuation of and a supplement to the topographical communications already published, the college has considered it fitting here to adopt the following reports on the physical peculiarities of the country, and other circumstances that influence the sanitary state, as well as on the diseases which are endemically met with among the population in the official districts, both in the provinces of the kingdom and also on the island of St. Bartholomew, touching which last-named the college has for the year now in question received for the first time an annual report, furnished by Your Majesty's Government and Garrison Physician there stationed since 1865."

Had space allowed us we would gladly have given one of the medical officer's reports at length. We must, however, be content with the following short extract from the closing passage in the report furnished by Dr. Ericson, one of the district physicians of the province of Stockholm. Speaking of the Botkyrka district, he says:

"Among *endemic diseases* may be mentioned chronic stomach-catarrh (*gastritis chronica*), or, as it is called by the people, 'pain in the chest,'—a disease of such common occurrence, that nearly half of all the cases of sickness are referred to it. The causes of this must be ascribed to the bad food and to the abuse of coffee. Anæmia (*chlorosis*) also is common among the youth. Further, inflammation of the lungs may be reckoned among the endemic diseases of this district. It has sometimes been complicated with pleuritis, and has occasionally passed into a chronic form. The causes of this disease appear to me to depend partly on the changeable circumstances of temperature in the locality, the already described damp, cold fog, the bad clothing, and draughty domiciles of the people."

Of all the reports the fullest and longest is that relating to the little island of St. Bartholomew, in the West Indies, the only foreign possession of Sweden. It is situated in lat. 17° 53′ N., and has an area of only six square miles. The population, in 1866, amounted to 2898, of whom 1709 were women and 1189 men. The town population numbered 908, and the remainder, 1990, lived in the country. The ratio of the inhabitants to the square mile was 483, or nearly equal to that of the most populous countries in Europe. The general health of this world in miniature would appear to be excellent, for the total number of deaths for the year 1866 was but 32, and amongst them were included one of a man aged 100, and two of women who had entered on a second century.

Of these 32 deaths 20 occurred amongst the country folk and 12 in the town. The births in the same year numbered 96, 71 having occurred in the country, and 25 in the town.

The above number of deaths represents an annual ratio of but little over 11 in every 1000 of the population, while the number of births gives an annual ratio of over 33 per 1000.

Section ii. bears the impress of Hr. Wistrand's careful revision. In it a history of the prevalence of various diseases during the year 1866 is given, in the first instance at length, under the different names, as typhus fever, cerebro-spinal meningitis, &c.; and the section closes with an elaborate *résumé* of all the leading facts, elicited from an attentive study of the materials contained in that history.

As the gist of the section is contained in the *résumé*, we may be pardoned if we make a rather full extract from it.

"A *general retrospect* of the reports furnished of the diseases prevalent in the kingdom during the year 1866 shows that the morbility was, on an average, much more considerable than during the year immediately preceding; and that this was chiefly due to the fact that *epidemic cholera*, after a lapse of six years, again appeared, as also that *scarlet fever*, and to some extent *meningitis*, were on the increase.

"At the same time, *typhus* and *gastric fevers* (to which may be added *intermittent fever*) were met with almost as frequently as during the preceding year. *Dysentery*, on the other hand, *diphtheria*, *smallpox*, and *hooping cough*, decreased, while scarcely a single case of *measles* occurred.

"From an *epidemiological* stand-point the leading characteristics of the year now in question appear to be the following:

"I. That *cholera*, which since 1859 had not prevailed in the kingdom as an epidemic, and from which in 1865 only isolated cases of disease and death had come under notice, these latter resembling 'epidemic' or 'Asiatic cholera,' suddenly appeared in summer, and assumed the proportions of a much more general and frequent epidemic than had prevailed in any of the latter five years of the preceding decennial period. It disappeared again, however, towards the close of the year.

"II. It is also worthy of note that, contemporaneously with the outbreak of cholera, *diarrhœa* and *endemic cholera* appeared much more generally and frequently than during the preceding year.

"III. *Scarlatina* likewise increased and spread excessively.

"IV. *Meningitis* was rather more prevalent than in the previous year.

"V. *Gastric*, *nervous*, *inflammatory*, and *catarrhal* fevers, also, were more extensively and frequently met with than in 1865.

"VI. *Smallpox*, on the other hand, which during the three preceding years had seemed to be gradually assuming larger proportions, so that in 1865 it was so prevalent as almost to deserve the name of an epidemic as serious as that of 1859, began to diminish somewhat.

"VII. *Diphtheria*, *croup*, *hooping-cough*, *ague*, and *mumps*, likewise showed an insignificant decrease in frequency and prevalence, while *dysentery* and *measles* were only very sparingly met with. *Infectious ophthalmia* (*conjunctivitis granulosa et trachomatosa*) has in the year under review increased to no inconsiderable extent in frequency.

"Among diseases which *fluctuate* with the changes of the season and weather, *pneumonia*, *catarrhal fever*, *meningitis*, *diphtheria*, *croup*, and *mumps*, have, among winter diseases, prevailed most largely during the *first* half of the year. *Typhus* and *gastric fevers*, as well as *hooping-cough*, preponderated in the *second* half. Among summer diseases, *ague* was seen much more frequently during the *first* half year, while *cholera*, almost exclusively, as also *cholerine*, *diarrhœa*, and *dysentery*, occurred during the *latter* half with considerably preponderating frequency.

"Of *intercurrent* diseases, more independent of season, weather, and local circumstances, *smallpox*, and *scarlet fever*, with the few cases of *measles* that occurred, have all prevailed with largely preponderating proportion in the *first* half."

A few *meteorological notes*, from observations made at the Stockholm Observatory, 59° 23′ N.L., may be of interest in connection with the cholera epidemic of the year 1866, especially as the outbreak of the disease closely coincided with the rapid rise of temperature in June, while it declined quickly in October, a month in which a remarkably striking fall of temperature took place. It is also worthy of note that the rainfall of the period from June to September inclusive was greatly in excess of the average.

1866.

Mean Temperature = 45·8°
" Barometer = 29·712 inches (corrected and reduced).
Rainfall = 21·40 inches.

Month.	Mean Temp.	Rainfall.
January	35·5°	·67
February	24·7°	1·05
March	29.6°	1·16
April	42·1°	·58
May	49·3°	1·95
June	69·4°	2·11
July	66·1°	4·89
August	66·4°	3·93
September	63·5°	1·80
October	46·7°	·10
November	30·0°	2·93
December	26·6°	·23
Mean	45·8°	21·40

1862.	Mean Temp.	= 42·1°	Rainfall	= 12·81 inches.
1863.	"	= 46·5°	"	= 14·27 "
1864.	"	= 42·1°	"	= 13·95 "
1865.	"	= 45·4°	"	= 12·89 "
1866.	"	= 45·8°	"	= 21·40 "

In section iii. are given particulars of the changes and improvements effected in connection with the state medical service, to which we have already alluded in passing. We would, however, remark that this enlargement of the service was most opportune, as it not only enabled the government to obtain far more complete information regarding the hygienic state of the country, but also provided medical aid during the cholera outbreak of 1866 in places hitherto without a physician.

Section iv. is illustrated by, perhaps, the most elaborate tables in the volume. From them valuable information regarding the hospital system of Sweden is to be gained. It would appear that there are 44 hospitals and houses of recovery in that country, which receive from government larger or smaller sums towards their support. These institutions contain 2414 beds, and in their wards 13,979 patients were treated in 1866, the yearly average for the quinquennial period 1861-1865, having been 13,226. Of the number treated 754 died. The mortality was, in the hospitals, 7·8 per cent., and in the houses of recovery ·8 per cent.

In addition to the government institutions there are 11 hospitals, which are independent of State support.

In their wards, which contain 1001 beds, there were treated, in 1866, 8513 patients. Of these, 7001 were discharged convalescent or relieved, and 764 died. The mortality amounted to 8·9 per cent.

The following facts relating to the lying-in hospitals (5 in number) are likewise given in Section iv.

Of 1460 patients under treatment during the year the large number of 1091 were unmarried, 358 being returned as married, and 11 as widows; 735 were primiparæ. Of 1410 who were delivered, 74 were removed to other institutions for the sick; 1283 recovered, and 53 died.

Sixty-seven cases of puerperal fever occurred, of which 43 terminated fatally; 12 were removed to other hospitals; and 12 recovered; 53 of the 67 cases were *unmarried* women. The mortality from the above disease was 64·2 per cent. of those affected.

Section v. contains a mine of information for such as are interested in the medicinal springs of Sweden.

With regard to the sanitary state in the prisons, the prevalence of an epidemic of scurvy is the most noteworthy point. It was most severe in Langholm and the citadel of Malmö. In these two places 376 cases altogether occurred, of which only 48 were treated in hospital.

The second division of the report refers to the medical examination of conscripts, and consequently may be passed over.

From the report of the garrison hospital at Stockholm, given in the third division, or "Sanitary State of the Army and Navy," we extract the following passage concerning pneumonia:

"Inflammations of the lungs have in general been of a rather mild type, so that of 140 cases admitted only 8 died; the remainder recovered. The largest number were treated in June and December (28), in November, 19. Among those specified the seat of the disease in the different lungs was as follows:

In the right lung in	70	cases,	of which	2 died.
In the left "	35	"	"	none died.
In both lungs	11	"	"	6 died.
Total specified cases	116	"	"	8 or 6·9 per cent. died.

As complications, pleuritis was met with in 21 cases, delirium tremens in 8, pericarditis in 2, ague and bronchitis each in 1. Temperature observations were taken in 21 cases, and of these in six only did the range exceed 40° Celsius (104° F.). The mean temperature was 39·7° (103·5° F.). The highest temperature was on the average recorded on the fourth day. Resolution in by far the greatest number of cases began on the fifth, sixth, or seventh day. The relation between temperature and pulse was extremely variable, yet for the most part an elevation of the temperature accompanied a high rate of pulse, the latter varying between 80 and 130 beats a minute."

The remaining divisions of this valuable report contain much matter to which, had space permitted us, we would gladly have alluded. Regarding one of them, a few words may be admissible. In reading the title of the ninth division we are reminded of the existence, in Sweden, of a most laudable system, whose end is the encouragement of members of the medical profession in the prosecution of their more advanced studies. Every year a sum is set apart by the College of Health, to be divided in varying amounts, as "subsidies for scientific foreign travels." Those who enjoy these subsidies proceed to the principal continental and English cities and places of learning, where they carry on their studies as debtors to the generosity and far-sightedness of the government of their country.

In return, they send to the College of Health full reports of their travels, with the observations which have been made, and the information which has been acquired.

Among the recipients of these funds, in 1866, were C. E. Berg, town physician at Christinehamn, who was given 1800 rix-dollars (£116) for the purpose of studying the practice of medicine and surgery abroad; Dr. C. M. Groth, assistant physician to the Provisorial Lying-in Hospital of Stockholm, who obtained a sum of 1200 rix-dollars (£70) for the purpose of studying midwifery, &c., in Germany and France; and G. W. Sjöstedt, professor of veterinary surgery at Stockholm, and N. E. Forssell, lecturer on veterinary science at Skara, received each a like sum, "to proceed to Holland, Belgium, and England, and places on the Continent, to obtain a complete knowledge of the cattle plague, and the remedies in use against this disease, with their greater or less success."

The two latter examples show how alive the Swedish College of Health is to the interests of the people, and to what is going on in other countries. We might learn a lesson from that body in both these respects. JOHN WILLIAM MOORE.

V.—Hospital Efficiency.[1]

(*Continued from* vol. xlv. *p.* 450.)

The Data of Selected Small Hospitals.—Dr. Duncan states that Dr. Evory Kennedy's selected small hospital returns are unreliable, because of the smallness of the number of cases; which remark also applies to Le Fort's collection of cases. According to Dr. Kennedy, in New Ross Hospital, with but thirty cases annually, the mortality is 1 in 185; in Waterford, 115 annually, 1 in 295; in Limerick, 367 annually, 1 in 367.[2] The returns from these selected small hospitals prove too much; as the mortalities

"recorded are smaller than those in the practices of the best private practitioners. Private practice, can, I believe, nowhere show

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[1] 1. *Sixth Report of the Medical Officer of the Privy Council,* 1863.

2. *Hospitalism and Zymotic Diseases.* By EVORY KENNEDY, M.D., &c. London, 1869.

3. *Circular Report by the Surgeon-General of the Army Medical Service, in the United States, on the Organisation of Military Hospitals* (quoted in the 'Lancet').

4. *Outline of Observations on Hospital Gangrene as it manifested itself in the Confederate Armies during the American Civil War,* 1861—65. By JOSEPH JONES, M.D. New Orleans, 1869, &c.

5. *Reports of the Dublin Obstetrical Society* ('Dublin Quarterly Journal of Medical Science,' 1869 and 1870.

6. *Edinburgh Medical Journal,* December, 1869, and January, 1870 (several papers), and previous reviews on *Hospital Construction,* on *Scandinavian Medical Literature,* &c., in the 'British and Foreign Medico-Chirurgical Review,' 1866, 1869, 1870.

7. *Various Papers in the British and Foreign Medico-Chirurgical Review, Dublin Quarterly Journal, Lancet, Medical Times and Gazette, and British Medical Journal.*

8. *Note Statistique des Grandes Opérations, &c.* Par M. A. BARBOSA, Prof. à l'Ecole Medico-Chirurgicale de Lisbonne, etc. Paris, 1868.

Statistical Notice of the Great Operations, &c. By M. A. BARBOSA, Prof. at the Medical and Surgical School, Lisbon, &c. Paris, 1868.

9. *L'Union Médicale* (*Contributions on the subject of Maternity Hospitals*). By MM. TARNIER, HERVIEUX, and GALLARD.

[2] 'Dublin Quarterly Journal of Medical Science,' May, 1869, p. 295.

such fine results as these little Irish hospitals. The Dublin practitioners, as a whole, have a mortality about 1 in 114; those of Edinburgh, as a whole, have a mortality, probably, nearly the same. There must be something misleading in Dr. Kennedy's selection. If it prove anything, it is that hospitals are safer for lying-in women than the cottages or dens of the poor, the houses of the better classes, or the mansions of the rich. In short, it is a selection that does not command the confidence of the intelligent obstetrician. There is plenty of evidence to prove that a mortality of 1 in 295, or 1 in 367, has never on a large scale been known in this world."

This able statistician proceeds to show the fallacy of small numbers; cases will at once occur to every practitioner's mind in proof of this. Dr. John A. Byrne, for instance, quoted Dr. Griffin's report of the Killarney Hospital, in which 3 deaths occurred in 139 cases, in a period of three years and four months, not one being from puerperal fever, though many cases of that disease occurred in the neighbouring district. The outbreak of puerperal fever[1] at Maidenhead is another of the many instances of severe and fatal outbreaks of puerperal fever occurring where no lying-in hospital existed.

Dr. Duncan quotes the years 1807 and 1808: when, of 5176 cases delivered in the great Dublin Lying-in Hospital, but 25 died, or 1 in 207:

"In conclusion, then, we find" (writes Dr. Duncan) "that the statistics of maternity hospitals afford no countenance to the notion that small hospitals are better or more salubrious than large. Indeed, they at least appear to show that the reverse is true. They, however, indisputably prove that smallness is no guarantee of success or salubrity." Quoting the following from an address, published in a daily Edinburgh newspaper,[2] by Sir James Simpson: "When such a simple operation as amputation of the forearm is performed upon a poor man in the country and in his own cottage home, only about 1 in 180 dies; but the statistics of our large and metropolitan hospitals disclose the stern and terrible truth that, if these men had been inmates of their great wards, thirty of them, or about 1 in 6, would have perished." He proceeds:

"All our cherished Edinburgh hospitals depend mainly for their support on voluntary contributions. The sick poor are tended, medical education is carried on. Can any further support be expected from that portion of the public who unfortunately believe that the above quotation is even an approximation to the truth? I have in this short paper tried to find out the truth as to large and small hospitals in a department of medicine with which I am some-

[1] 'Dublin Quarterly Journal,' &c., August, 1869, p. 354; and 'Mr. Simon's Report.'
[2] 'Scotsman,' Oct. 27, 1869, p. 7.

what familiar. My readers will then easily understand how sadly I deplore the appearance of such statements as I have quoted. I know nothing, that if only listened to, is fitted to be more injurious to the sick poor and to the medical profession."

"Hospitalism," as it is termed by those who oppose institutions springing from Christianity itself, is constantly charged with a mortality, admitted by all to be fluctuating, arising from erysipelas. It is, therefore, well worth noting Sir B. Brodie's thoughts on this subject:[1]

"Now, it has been my lot to live, during the whole of my professional career, where I had abundant opportunities of watching the origin, progress, and termination of this terrible malady, and the result is that I am led to believe that it is not really contagious. Exposure to cold and damp, and especially to the influence of these two causes acting in combination with each other, may be the immediate exciting cause; but, if I am not greatly mistaken, it may in nine cases out of ten be traced to a still higher source than this, namely, to a depressed and debilitated condition of the patient's constitution. The depressing effects of the cold north-eastern wind which in this country pervails, on an average, for nearly three months between winter and summer, are felt and acknowledged by all, and erysipelas is never more prevalent than it is just at this period of the year. We may in the same manner explain the frequent occurrence of it during a season of extreme cold in winter, or of intense heat in summer. Then you may observe that it occurs especially after operations in which the patient has lost an unusual quantity of blood, and in those who either before or after the operation have been kept on a very low system of diet."

That eminent authority points out that, in his judgment, the prudent and judicious administration of nourishment and stimulants tends most effectually towards the prevention of erysipelas.

Starvation diet, so to speak, is but too well known to many of those who afterwards become inmates of large town hospitals, and who serve to swell their bills of mortality; but this is obviously not owing to any fault of the hospital. Well, indeed, has a recent writer said a low mortality argues a small utility in any hospital.[2]

Mr. Callender[2] copies the returns of amputations in private country practice,[3] numbering 2098 cases, with 226 deaths, and observes—

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[1] The works of the late Sir B. Brodie, vol. iii, p. 430. London, 1865.

[2] *Comparison of the Death rates after Amputations in Country Private Practice, in Hospital Practice, and on Country Patients in a Town Hospital.* By GEORGE W. CALLENDER ('Saint Bartholomew's Hospital Reports,' vol. v. London, 1869).

[3] From the 'Edinburgh Medical Journal,' No. clxv, p. 829, March, 1869.

"Of the 374 returns from which these figures are obtained, 217 returns give a total of 779 amputations, an average of 3·5 to each return, *without a single death*, a large number consisting of a note of a single operation (62), which recovered, or of two successful cases (59). 157 returns give 1319 amputations, an average of 8·4 to each return. In the 1319 are found 226 deaths, or in the proportion of 1 in 5·8, or 17·1 per cent."

Of 779 amputations, embraced in 217 returns, in private country practice, the whole number recovered. Of 1319 amputations reported in 157 returns one proved fatal in 5·8. The number of non-returns, or of those returned in blank from the country, is not stated. The larger number of arm and forearm cases in the country and the low rate of mortality in these cases largely reduces the death-rate on the gross number.

As we read the special pleading adopted by some whose position and professional reputation are calculated to mislead the public generally, if not professional men, we are reminded of the instructions contained in a very practical and valuable work, not the less valuable because written by a shrewd and practical surgeon, a good while before the present time, which is so remarkble for want of confidence in that which has preceded it, often, as it would appear, merely because it is not new. The work to which we refer[1] mentions the necessity for health and strength on the part of the young recruit, to enable him to bear the onerous duty of an active soldier's life. The following statement certainly applies to not a few of the cases met with in town hospitals:

"There is a very objectionable description of recruits often met with in large cities, namely, young men whose health has suffered from debauchery of various kinds. Their peculiar appearance is commonly well marked—complexion wan and colourless, doughy, sodden look, tremulous lips and hands, clean teeth, breath and smell peculiar to spirit drinkers; often fulness of the belly and tendency to fatness; their manners and language of the better kind. This class is usually composed of footmen out of place, clerks, shopmen, broken tradesmen, profligate, irreclaimable sons of gentlemen, &c. &c. I know no species of recruits more unfit for service; they are seldom out of the guard-room or the hospital."

How many such are to be found swelling the mortality of our city hospitals; and yet the records of St. Bartholomew's state that "nine years (in sixteen) elapsed without a single death amongst seventy-eight amputations of the upper extremities; and the results obtained in this, which ought to be possible in any other hospital, are quite as good as those in

[1] 'Hints to Young Medical Officers,' &c. By H. Marshall, Surgeon to the Forces. Published by Burgess and Hill, London 1828.

private country practice." Moreover, the three double amputations that were performed at St. Bartholomew's, within the same period, viz. two of both legs and one of leg and arm, all recovered.

Mr. Callender observes—

"No one can look over this series of cases" [given in a tabular form in the report], "with its great vicissitudes from year to year, without seeing that some disturbing causes influence the results, quite irrespective of the health and condition of the hospital, which, indeed, may be measured by the returns for the year 1855 or 1862, with a mortality, after all amputations, of 4 and 10 per cent. respectively.

"And no one who has watched the practice of this, or of any other great hospital, doubts but that the chief of these causes are the varying severity of the diseases for which amputations are obligatory, and the character of the material which in each case comes to us for treatment.

"With reference to this material, one class of patients forms the following exceptional group:—Country patients in this hospital recover, as well as country patients in private country practice."

In provincial hospitals the rate of mortality varies much, being highest in those with from 6 to 20 beds, and in such as have from 150 to 270 beds, and lowest in those having from 20 to 100 beds. The mortality after all amputations, in towns having 50,000 inhabitants and upwards, is 27·2 per cent. and 18·4 per cent. in those between 20,000 and 50,000, and 18·5 per cent. where the population numbers from 18,000 to 20,000.

Two hospitals, in every respect comparable, with an aggregate of 248 amputations, return a death per-centage of 11 and 35·4 respectively. In another like instance the numbers are 16·2 and 33·3 respectively. In a third case comparison is made between an hospital in a large commercial town and one in a cathedral city, the mortality being in the one 42·8 per cent. and in the other 26·4. It would be most unjust to argue that any of these institutions were less useful than others, for we must remember how much more dangerous to life some injuries are than others. The former class comprises bad railway cases, and some of those received in machinery, gunshot wounds, &c.

"If we take" (writes Mr. Callender) "157 returns of 1·319 cases in private country practice, which returns give the experience of practitioners, *each of whom has operated on an average in eight cases,* and which are in marked contrast with the 217 returns of 779 cases, with *an average of only three cases to each return*, and in which not one single death is recorded—if we take, I repeat, the 157 returns, we have the following figures:

"*Mortality after all Amputations.*

Country hospitals . . .	1 in 5·7 died,	or 17·5	per cent.
" cases in London .	1 " 5·8	" 17·0	"
" private practice .	1 " 5·8	" 17·1	"

"No one can fail to notice how closely these experiences agree.

"Is this a mere coincidence?

"Is it not rather the common sense truth about this question of the death-rate after amputations performed on country people here and elsewhere?"

Dr. Matthews Duncan investigates the information derivable from the statistics of the Dublin Lying-in Hospital, in which more than 190,000 cases have occurred in 113 years. We can quote only one of his four tables, owing to want of space. The original article[1] will, however, amply repay perusal:

TABLE IV.—*Showing the Rate of Mortality per* 1000, *amongst the Inmates of the Dublin Lying-in-Hospital, arranged according to the numbers of Inmates.*

Women delivered.	Rate of Mortality per thousand.			Number of years or observations.	Mean age of Hospital.
	Lowest.	Average.	Highest.		
Less than 800 . .	9·5	14·0	18·9	19	8
800 and under 1300 .	7·7	21·3	39·9	19	59
1300 " 1800 .	7·8	13·9	22·5	24	57
1800 " 2300 .	7·2	11·8	17·4	27	78
2300 " 2800 .	6·6	14·5	19·1	14	61
2800 " 3300 .	11·9	12·8	13·9	7	61
3300 and upwards. .	12·5	12·5	12·5	2	61

"From the best data obtainable, therefore, we find that *the mortality of the Dublin Lying-in Hospital does not increase with the increased number of the inmates, does not rise with the aggregation.*

"*The mortality of the Dublin Lying-in Hospital is neither in the direct nor in the inverse ratio of the aggregation.*

"The figures, indeed, seem to favour the view that the hospital is a better and safer institution the greater the aggregation. Certainly a smaller proportional number die when there are many in it than when there are fewer. It is plain that we cannot look to aggregation as an important cause of mortality in the Dublin Hospital. This is a great practical result; for it sets inquiry into other directions to find out the hidden sources of increased mortality. Dr. Kennedy's proposition above quoted is not only not proved by the data he refers to—it is proved to be false. The opposite of his proposition is shown to be nearer the truth. With the fall of his

[1] 'Edinburgh Medical Journal,' January, 1870, "On Aggregation in the Dublin Lying-in-Hospital."

'Redan' proposition fall all his conclusions regarding puerperal fever and the advantages of small hospitals."

"Although the supposed paramount evil influence of aggregation in this great hospital is now disproved, much more requires to be done, with a view to discover and avert the causes of so-called metriâ, which is the chief source of variations in its death-rate."

The celebrated advocate[1] of cottage hospitals and of country practice reiterates his assertions, as far as "some comparisons" bear out the same, that the comparatively unhealthy residents in the rural districts bear amputation better than the comparatively unhealthy residents in the metropolitan and large provincial town districts; but Sir James Simpson does not notice the fact stated by Mr. Callender, namely, that the mortality, after all amputations in country hospitals, in country cases, in London hospitals, and in country private practice, is respectively 17·5, 17·0, and 17·1 per cent.

"A small hospital," observes Sir James Simpson, "if overcrowded with beds and patients, become as insalubrious as a large hospital under one roof. On the other hand, a large hospital would be generally made almost as salubrious as a small institution, provided few beds were left scattered over its wards, and these wards were well ventilated and often changed."

Now the meaning of these sentences does not seem at all clear, nor is it all to be gathered from the context. Every one will at once admit that an overcrowded small hospital will be very unhealthy indeed, but it does not at all follow, even on his showing, that a well-regulated large hospital must be equally unwholesome as the crowded small one.

The next sentence states "a large hospital would be generally made almost as salubrious as a small"—healthy, we presume—"institution, provided few beds were left scattered over its wards, and these wards were well ventilated and often changed." Now our observation and reading lead us to the belief that if everything in the way of construction—sewerage, freedom from damp, cleanliness, &c. — were equally well attended to in large and in small hospitals, the result would be that, with the same class of cases and of patients, and in the same atmosphere,[2] a larger proportional number of cases could be treated, with a successful issue, in a block of building containing say from 50 to 100 persons, than could be treated in the so-called cottage hospital, or in the hut or cottage of the poor in country districts, and at an expense greatly less in the former than in the latter.

[1] Sir J. Y. Simpson. This distinguished member of the profession has recently passed from amongst us. May, 1870.

[2] As urban or rural.

We are conversant with institutions, for the most part on the pavilion principle, in which greater numbers than 100 persons are constantly accommodated, and the distribution proposed has long been adopted.

We recommend to our readers' consideration the elaborate paper[1] of Dr. Stark on the vital statistics of Scotland, as it may assist them in forming a correct estimate of the probability of life under the disadvantages of operations in town and country. We have already shown, in our quotation from Mr. Marshall's and from Mr. Callender's writings, that it is manifestly unjust to reason and on the premises assumed by Sir James Simpson and those who follow his anti-hospital crusade. We will now quote Dr. Stark's paper, based on the statistics of Scotland, which he claims to be the most perfect in the three kingdoms. He divides Scotland into three great groups of districts—viz. the Insular, with a mean population of 161,308 for ten years; which latter period and condition applies to the Mainland Rural, having 1,758,089; and to the Town, with 1,125,541 residents. During the same period the per-centage of deaths of population is found to be, in the three districts respectively, 1·60 per cent., 1·78, and 2·71. Very much other interesting matter is contained in the paper, but we must be content with quoting Dr. Stark's concluding paragraph as well worthy of consideration, and as bearing upon the subject of the lesser vital power of large-town residents as compared with that of the country folk, and as accounting for much of the fatality attending operations and sickness in the two classes respectively;

"Here, then, is a wide field for the philanthropist; for could he bring down the mortality of our towns even to that of the mainland rural districts, he would save annually upwards of 13,000 lives to the population of Scotland; and every individual living in the large towns would, on an average, live ten and a half years longer than he has any chance of doing at present."

Professor Stadfeldt,[2] of the Copenhagen Lying-in Hospital, affords corroboration of the statement of Drs. M'Clintock, Duncan, and others, namely, that metria is by no means dependent on the aggregation of large numbers of parturient women. This evidence is also opposed to Dr. E. Kennedy's theory, that puerperal fever depends on a poison generated by parturient women, which is active in proportion to "their

[1] "Contribution to Vital Statistics." By James Stark, M.D., F.R.S.E. ('Edinburgh Medical Journal,' December, 1869).

[2] See review of his paper in this number of 'The British and Foreign Medico-Chirurgical Review,' by Dr. W. D. Moore.

number cohabiting in a given number of feet of atmospheric space."

The Copenhagen Hospital contains forty-four wards, is built in two storeys, and in the year ending 31st March, 1869, there had been but 484 admissions to the institution, and yet the mortality was 4·7 per cent.; an epidemic of puerperal fever in December and January had increased the mortality, which was in excess of the average; that of 1865-6 had been 1·7 per cent.; and for the years ending March 31st, 1867 and 1868, the number of fatal cases were respectively 2·8 and 2·3 per cent. The hospital is closed in the months of July and August, the yearly average of admissions being under 500, and yet the average annual mortality, for four years, was 2·8 per cent. As 484 cases were distributed through forty-four wards during ten months, aggregation could not have operated in the production either of metria or the high average rate of mortality, we must rather look to a condition existing in the Copenhagen Hospital, as well as in that of the great Dublin Hospital, and in almost all other like institutions, namely, the great number of unmarried females to whom such establishments afford an asylum; but amongst whom—whether delivered in a large, clean, well-ventilated and well-regulated establishment, or in smaller establishments, though equally well regulated—a fatal result too often follows, owing to mental depression inseparable from their condition.

The unmarried females delivered in the Copenhagen Hospital numbered 73 per cent. of the total number. This class of patients too often enter hospital in that early stage of depression from the combined influence of destitution and mental anxiety and shame, which proves the forerunner of puerperal fever, or of other serious illness.

This doubtless was the important point dwelt upon by Dr. Johnston and other physicians who took part in the Dublin discussion. It appears to us that the more conscientious (if we may so say) who have fallen from various causes from the path of virtue, seek the asylum from the world and from public gaze afforded by a large maternity, but which the so-called cottage hospital does not afford, any more than the miserable lodgings that females in their forlorn state can only hope to inhabit. We would ask those who are now eager for the destruction of time-honoured and valuable hospitals, would they seek to deprive such unfortunates of shelter? and whether this very refuge be not an important deterrent against a continuance in sin and misery, and possibly a preventive against the crime of infanticide, and even of suicide? While we say this, we declare it our conviction that those who have sought to prove the unhealthiness of large hospitals, whether for lying-in women or for surgical and medical cases have not established their position.

Some may say lying-in hospitals are not to be kept up for the class to whom we now allude, and that if they are, they hold out a premium to unchastity; this is not so; human nature has always been what it is, and needs training and kindly Christian sympathy rather than austerity and pharisaical hypocrisy. The greatest teacher that ever trod this earth taught this great doctrine, or, in the words of H. Kirke White, "There was a reason for these things" . . . . "And to those that sought it there was also a cure."

The important influence exercised by the mental condition of the lying-in woman is further exemplified by the returns of the Swedish lying-in hospitals during the year 1866. In that year 1420 deliveries took place; puerperal fever appeared in 67 cases, of these 43 terminated fatally. Of the 67 patients so attacked, no less than 53 were unmarried, and only 14 were married.

When a comparison is instituted between hospital cases and persons in "affluent" circumstances, this fact of the proneness of the unmarried, who so generally seek the shelter of the hospital, to puerperal fever, must be borne in mind before we can at all approach a correct estimate of the share that lying-in hospitals have in the alleged production of metria. We are at once reminded of Dr. Barnes' experience in London, viz. that it was amongst the rather well-to-do, more than among the very poor, that metria appeared. We must also bear in mind the statement of some of the leading physicians of Dublin "that puerperal fever existed in some epidemics outside the walls of the hospitals, but not in those institutions."

Dr. Jones has given a useful and practical sketch of what appeared to be the agency at work in the production of hospital gangrene among the troops of the Confederate armies; and has brought to his aid not only sound medical knowledge, but a familiarity with chemistry to bear on the elucidation of the subject.

He frequently noticed that fever, loss of appetite, constipation, enfeebled irritative action of the "circulatory apparatus," &c., preceded the gangrenous appearance of the wounds. Bad food, lack of vegetables, exposure to malaria, over-fatigue, &c., appeared almost to insure the sufferer, when wounded, an attack of hospital gangrene.

Both local and constitutional in its nature the disease may arise in those exposed to the exhalations from gangrenous wounds, through the pulmonary and cutaneous systems, without any abrasion of the surface. It may also be communicated through the atmosphere to wounded surfaces without any direct application of matter. Sometimes a certain period elapses between the exposure of wounds to the action of the cause of hospital gangrene before the disease appears. In very debilitated and cachectic subjects death may occur from poisoning of the system by foul emanations from the sick and wounded in crowded and ill-ventilated hospitals before the local disease has progressed to any extent. From the fact that the fibrin in the blood of hospital gangrene exceeds the healthy standard, though below that of the phlegmasiæ, and from other local and constitutional changes, Dr. Jones looks on hospital gangrene as a species of inflammation, but different from ordinary inflammation. He drew the following conclusions from a large number of observations upon individuals selected from over 15,000 cases of hospital gangrene.

1st. *The local manifestations of hospital gangrene are attended by febrile action in the general system.*

2nd. *The fever of hospital gangrene appears to be manifestly the result of the action of an organic poison.*

3rd. *The febrile state of hospital gangrene is characterised by increased chemical change in the blood and textures, increased development of heat above the standard of health, marked diurnal variations of temperature, great irritability and feebleness of the general circulation, and imperfect and feeble capillary circulation, as manifested by the marked difference of temperature between the trunk and extremities, and the inability of the extremities to maintain a definite temperature, and withstand the effects of external cold.*

The hygienic condition of the hospital in which the patients are treated exerts the most powerful influence for good or evil in this disease. Such derangements of the blood and tissues and in the physical and vital forces, as we find in scurvy, appear eminently to favour the spread of hospital gangrene.

Dr. Jones insists on the ill effects of over-fatigue, improper food, exposure to malaria and overcrowding, followed by wounds, mental depression, and confinement in filthy, ill ventilated, and overcrowded hospitals, in producing hospital gangrene, and also observes that "the service has lost far more men from the effects of gangrene, destroying the power and use of limbs, than from the actual number of deaths caused by this disease."

If gangrene extends to a considerable joint (as the hip, shoulder, knee, or elbow), Dr. Jones considers the case hopeless, unless the limb be promptly amputated.

To consider the treatment in detail would be foreign to the present subject. We would, however, observe that the diet should be nutritious, antiscorbutic, and stimulants suited to each case will almost always be required. The absolute necessity of removal of all cases to a pure atmosphere should never be lost sight of, as neither the internal nor constitutional treatment at which we have just glanced, nor any local treatment, will avail while the patient is retained in a tainted atmosphere.

We may now observe that one thing is undoubtedly proven, namely, that very great amendment has taken place in the sanitary state of hospitals, and every way also in their efficiency; amendment has also taken place in the dwellings of the artizan and labouring classes; but in this respect more, much more, yet remains to be effected; and to sanitarians who are so anxious—and who is not—to amend the sanitary state of medical and surgical cases in hospital, we would strongly commend the experience and observations of Sir B. Brodie, of Dr. Collins, of Mr. Marshall, of Drs. Stokes, Beatty, Hughes Bennett, Macleod, McClintock, M. Duncan, Stark, G. Johnston, Denham, G. Kidd, Sawyer, Byrne, C. F. Moore, Messrs. Holmes Coote, Teale, and many others among the British and Irish schools; and among Continental and American authors the evidence of such eminent men as Drs. Faye, Semelweiss, Braun, Spath, Dubois, Legroux, Hervieux, Gallard, Barbosa, Jones, and others, will also serve to enlighten all, that the most certain method of diminishing mortality of hospital cases is to improve the sanitary state of their inmates before entering hospital; and while we say this, we would by all means advocate continued attention to improvement in the sanitary condition of hospitals and the careful consideration of the so-called antiseptic treatment of wounds, injuries, stumps, &c.

The evidence afforded by eminent men on any point which is not at the moment a matter of controversy, appears doubly valuable, as being entirely free from any possible approach to bias in favour of any particular view; it is for this reason that we will now quote the evidence of Dr. M'Clintock[1] and Prof. Levy, recorded several years ago, on the subject of the causation of puerperal disease. The first-named observes:

"The extraordinary influence exerted by the *state of the mind* over the organic functions of the childbed woman, is one of the most striking and important features of the puerperal condition," &c. Again: "Amongst patients at the lying-in hospital, a seeming causeless quickness of pulse often suggested an inquiry into their social state or domestic circumstances, when we discovered for the

[1] 'Clinical Memoirs on Diseases of Women,' by A. H. McClintock, M.D., &c., 1863.

first time some hidden grief or corroding care which was the perpetual source of irritation," &c.

Prof. Levy observes that physicians occupied solely with married women, or those of the better circumstanced class, may not easily understand the importance of the psychical elements in the puerperal condition in general.

"But the case is different in lying-in hospitals, and particularly in large institutions, where, as in ours, the mass of the patients are unhappy single women or married women of the most depressed and needy class.[1] Here, mental sufferings have their special home; here they claim the physician's constant attention, for here they play so important a part that they may be justly considered as one of the elements most hostile to the state of health," &c.

Dr. Tarnier, in a recent communication to the Medical Society of the French Hospitals, attributes the mortality in hospital maternities to the mingling of the sick and healthy in the same wards; he insists on the necessity of isolated chambers for those lying-in. Moreover, he points out the careless state of dust in which the bedsteads of the maternity hospital are allowed to remain, notwithstanding the so-called cleansing. It surely is not just to attribute illness appearing under these conditions to purely hospital agency; for as Dr. Tarnier observes—"Dans l'ignorance où nous sommes de la nature des agents morbides qui éternisent les maladies puerpérales dans nos hôpitaux, on peut supposer que des globules de pus, que des molécules septiques échappent à un nettoyage incomplet et perpétuent la maladie."

This author gives ground plans for a series of maternity wards on the principle of isolation; he proposes that the walls, flooring, &c., should be impermeable to moisture, the latter being of asphalt, the chamber to be washed out by hose and jet when vacated by each patient. By this mode of construction and arrangement he expects twenty-four hours to suffice for cleansing and drying the apartment. He is evidently more sanguine of good from his proposed plan than we would be. The space allotted to us, and the more immediate purpose of this notice, do not permit our more fully entering into details of construc-

[1] "*Deaths in Lying-in Hospitals.*—It may be useful to call attention to a source of erroneous inference which the figures quoted from time to time in the Registrar-General's weekly return relative to the mortality in lying-in hospitals are not unlikely to occasion. The deaths in those institutions are given without the very important distinction whether they are of mothers, or of children, or of both together. We believe that the return includes children as well as mothers; consequently the facts published afford no real criterion of puerperal mortality in metropolitan lying-in hospitals. May we suggest to Dr. Farr the desirability of making this distinction in future returns?"—'The Lancet,' 7th May, 1870.

tion, &c. It is necessary, however, to observe that Dr. Tarnier very much attributes the spreading of puerperal fever to the effect upon parturient women of seeing others seized with the disease, and hearing their cries, &c. Dr. T. Gallard, of La Pitié, regards overcrowding, whether of lying-in women or of wounded persons, as specially injurious.

The best way to avoid puerperal accidents would be to give each woman a large well-warmed and well-ventilated chamber, remote from any infection, with one attendant for each woman and her child exclusively. By giving extension as much as possible to attendance at the homes of the women, Dr. Gallard and "the Commission" hoped to have realised this programme. But it was quickly found that however excellent this plan might be, it would not be attended with all the good that was hoped for, inasmuch as a great number of women had no homes, and therefore imperative necessity drove them into the hospitals, even though they might not be healthful.

Those unfortunates were found by the Commission to number about 8000 yearly, and they undoubtedly supply the greatest number of the fatal cases met with in the obstetric service. This author goes on to liken the case of such females in parturition, to the wounded soldiers of a vanquished army: while, on the other hand, those wounded who belong to a victorious host, resemble rather the case of happy mothers surrounded by their husbands and friends in their own homes. Sorrow and shame,[1] depression and destitution, but too surely produce injurious effects upon the wretched and the outcast.

Referring to M. Tarnier's statement that 545 more had died in hospital than should, if a like mortality had occurred there and in the cases of those attended at their own homes, M. Gallard observes, it would be delusion to suppose the two conditions alike, and we must undeceive ourselves if we think the fatality in hospital should or could be much lessened.

It may be somewhat diminished, because the number is composed of two elements: first, the mortality of the unmarried (filles-mères) exceeding that of those married; and secondly, whatever may be due to hospital influence. The latter element should be abolished, but the first is in a manner invariable and fatal. It is only by removing the hospital influence that we can exactly estimate the fatality from the abandoned state in which the unmarried mothers find themselves. It is not merely a predisposing influence for evil that is thus exercised, but also too frequently the condition mentioned determines the invasion

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[1] It may be stated here that pages 62–63 of this review were written before the discussion given in the 'L'Union' came under our notice.—REVIEWER.

of the gravest puerperal accidents. M. Gallard then draws a vivid picture of an unfeeling and cruel visit by relatives to an unfortunate woman after her delivery, and the rapidly fatal progress of puerperal fever resulting from the loss of the last ray of hope thereby, to which the poor creature had before clung.

He has seen benefit arise from isolation of the cases in hospital, and regards sympathy between those lying-in as more potent for evil than contagion.

M. Hervieux[1] considers the propositions of M. Tarnier in regard to maternities under a doctrinal as well as a practical aspect.

He considers M. Tarnier's idea, "that every assemblage of persons is unwholesome," is only true if the persons wholly or in part are in bad health, and instances the numbers that he and every one constantly meet, whether in teaching, in the work of daily life, in houses of worship, in assemblies for scientific or political purposes or for amusement, as showing that such are innocuous. The statement, moreover, that lying-in women are a cause of disease, so long as they continue free from illness, is untrue; nor are they capable of engendering disease, especially infectious disease, without importation of a contaminating principle from without.

We must not confound in practice the vitiation and poisoning of confined air, with a contamination arising from the puerperal state. Again, it requires a morbid state, and consequently sick lying-in females, to produce puerperal poison. Our readers may remember that in the discussion at the Dublin Obstetrical Society, the principle that healthy lying-in women are not capable of originating puerperal fever, was insisted on by Dr. Stokes and other authors, in opposition to the statements of Dr. E. Kennedy. The truth of the innocuousness of mere numbers when in the puerperal state, was also most clearly shown in the able papers by Dr. Matthews Duncan. Corroboration of this position has been afforded recently, as we are informed, in Dublin, where a few cases of puerperal fever occurred in private practice amongst the affluent, but not a single case occurred in the lying-in hospitals of that city.

M. Hervieux's statement regarding the Parisian hospitals completely bears out the experience of our own countrymen; namely, that it is not the number of lying-in women that increases the mortality of an hospital, but the admission of one or more sick to the wards.

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[1] 'L'Union Médicale,' 1870, "Les Maternités. Communications faites à la Sociéte Médicale des Hôpitaux, Janvier et Février, 1870."

The solitary confinement system advocated by M. Ternier, is open to many objections. If applied to the healthy lying-in woman it is unnecessary, argues M. Hervieux; reserve it for the sick, and do not inflict a punishment scarcely endurable by the most hardened criminal, upon the healthy woman in labour. If one apparently healthy falls sick, remove her at once to the model infirmary.

To crowd persons in labour together who are already sick, would be manifestly injudicious; but, as M. Hervieux states, to confine poor sick and healthy women, or either, in solitary cells, and still to maintain the same attendants, is useless, inasmuch as the latter remain the means of communicating any contagious disease from patient to patient; and while the latter is much more inefficiently attended, the labour entailed upon the nurses and wardmaids is greatly increased.

M. Barbosa, of the Saint Joseph Hospital, at Lisbon, quotes the reports of other European and American hospitals. He considers the Lisbon returns as very favorable, especially when contrasted with those of Paris.

Amputations for injuries in France, England, and Germany, are more serious in result than those undertaken in consequence of disease: in Lisbon the reverse has been observed.

Herniotomy has been less favorable in its results in Portugal than in London, but considerably more successful than in Paris.

Lithotomy also, though more fatal than in London, contrasts favorably in Lisbon with the same operation in Paris.

Though not admitting any favorable condition of race in Portugal as contrasted with the French nation, M. Barbosa considers that there are some points connected with climate which very possibly exert an influence favorable to the sick in Lisbon. Its remarkable mildness permits the windows and doors to be kept constantly open, without danger to invalids, even in the winter season, securing in this way a better state of atmosphere for the sick than would be possible in more northerly climates. Admitting the many benefits of the Lisbon climate we do not, however, subscribe to the other conclusion arrived at by this writer, namely, that the ventilation of hospitals in more northerly latitudes must necessarily be defective and insufficient.

Some hygienic conditions existing in the hospital at Lisbon (Saint Joseph) no doubt exert a favorable influence upon those operated on there, "notwithstanding original defects of construction,[1] the great number of patients (it contains 845 ordinary

[1] "The edifice, now used as an hospital, was originally built for a college by the

beds, not reckoning the private wards), in spite also of its overcrowded wards and too numerous stories, and also its position in the centre of the poor quarter, consisting of narrow and crowded streets."

With the disadvantages just named, St. Joseph's possesses certain real advantages: thus, its position on the top of one of the city hills (which form so striking a feature in the beautiful panorama of Lisbon—REVIEWER),—prevents its being inclosed and overlooked (*dominé*), the nearest houses being at a distance of many hundred yards on three sides, whereby ample ventilation and light are ensured. It is, moreover, partly surrounded by gardens and a small farm which belong to it. The construction of the edifice has also some advantages, having nearly all its wards well ventilated. The windows are numerous and large. Besides the gardens, there are interior courts; one of the latter is very large, and contains trees. Built on the extensive scale that characterises the structures of its former possessors (the monks), it is furnished with a superb entry. Ample stairs and corridors pierce the building in every direction; some of the wards on the ground floor and on the third story are not suitable, all the others in general are of such dimensions and so well off in respect of light and air, as to leave almost nothing to be desired. No part of the building is damp, nor is the least odour perceptible, as might be the case where so many sick are congregated together, except in one ward on the ground floor and in some on the third story; the latter being placed under the roof, are indeed in very bad condition, and already condemned. The greatest cleanliness is carried out in every part, the wards are ventilated thoroughly, and the floors washed each morning; all the walls and ceilings are scraped and lime-whitened every six months.

Among recent hygienic reforms may be mentioned the arrangements in the three large wards allotted to the clinical school for medical, surgical, and obstetric cases; in each the twenty-two windows open obliquely above by means of horizontal compartments (compartiments horizontaux), which allow of continual ventilation without permitting currents of air to fall upon the inmates.

These wards, besides having several lattice-work openings on the level of the floor and ventilating tubes in the ceiling, contain but thirty-four beds each, with a cubic capacity of 2346 metres, which gives sixty-nine cubic metres to each patient. A mortuary has also been recently added.

To these successive improvements M. Barbosa attributes the

Jesuits, and was only used for the sick after the expulsion of the Jesuits by the king, D. Joseph, in 1775."

almost entire disappearance in late years of diarrhœa, erysipelas, hospital gangrene, and puerperal fever, that formerly caused great loss of life among our sick, as well as among the surgical and midwifery cases.

The results obtained from amputations, no doubt, are likewise owing in a measure to improved methods of operating and of dressing the stumps, and it is worth while mentioning that compresses applied to stumps in the Lisbon hospital are steeped in a saturated solution of camphor in alcohol. Some details remind us of the late Mr. Liston's method of not closing the stump until all bloody exudation had stopped. We allude to these points as M. Barbosa states that during the last two years, with fourteen thigh amputations, there has been but one death.

Upon a suitable diet, and upon port wine especially, much of the successful issue also depends. In addition to the Lisbon experience M. Barbosa quotes the evidence of English, Russian, and other practice on this important point. In Lisbon operations performed in autumn, and next in order those accomplished in summer, yielded the best results; the spring was the least successful, and winter occupied a middle place. This does not wholly agree with experience elsewhere as observed by M. Barbosa. The diet appears judiciously liberal, and we would commend the concluding note of the author on this subject to certain authorities, at home, as not only the most humane and successful course to pursue, but also as the most really economical.

"*Les médecins ont la plus grande latitude dans la prescription des diètes, qu'ils peuvent, pour ainsi dire, varier a l'infini, sûrs toujours qu'elles seront ponctuellement servies à leurs malades.*"

Professor Virchow lays much stress upon the principle *that danger lies not in the size and extension of the hospital, but in the pollution of its air.*

The tendency of to-day is not, he observes, to build small hospitals, for even these may contain bad air, but to build *hospitals with good ventilation.*

How this is to be accomplished, whether through well-ventilated larger or smaller hospitals, either attached buildings or separately constructed blocks of houses—barracks or pavillions as we may denominate them—is a question the solution of which depends on locality, necessity, or technicality. The problem is not solved in the same way for all cases. In an airy and dry situation, with open surroundings and easy access of the winds, larger buildings may be allowed without risk, while even small barracks may give occasion for anxiety in a confined situation, and where the air is damp and stagnant. The great injury sure to occur in the adoption of the corridor system is dwelt upon by

Virchow, and, as he observes, the larger the building the greater the evil.

We have often observed how frequently the principle upon which some of our older hospitals were originally built, has been departed from in making additions to or so-called improvements in them; in some instances additional wards have been built up against that face of a building which was formerly pierced with doors and windows to ensure sufficient light and ventilation; in other cases vestibules and glass doors have been added, as if in dread lest the air, which the original architect well knew the importance of, should still have free access to the interior of the hospital.

Professor Virchow alludes to the danger sure to arise in case of any defect of superintendence or management when an artificial mode of ventilation is depended on.

Captain Galton's recommendations for the construction of hospitals may be sufficiently summed up for our present purpose as follows,—the selection of a good situation in the open country upon a porous and dry soil, a free circulation of air around the buildings, but sheltered from the north and east, simplicity of construction, ample ventilation and light, speedy removal of refuse, and great facility of cleansing. He would caution against building for a long futurity lest the wards become permeated with organic impurities, in which case a new structure would be required.

Mr. Jonathan Hutchinson deprecates the idea that ventilation will accomplish what separation of strictly contagious diseases from the general run of, say, surgical cases will effect.

The evils of the corridor mode of construction was insisted on by the late Sir J. Simpson, Dr. Rumsey, and Dr. Stewart.

Dr. Hughes Bennett desired that knowledge should be based on the sure foundation of unquestionable truth rather than upon vague and fallacious assumption. The cause of epidemics and endemics, it must be admitted, were as yet unknown, and constituted the most difficult investigations it was possible to enter upon; yet the most contradictory opinions regarding it were now brought forward in order to influence the structure of hospitals. The medical department of the Royal Infirmary of Edinburgh, of which he was a physician, was one of the best ever planned, and so far as he knew no epidemic had ever originated in it, though it was become so old that a new building must now replace it. If walls were capable of absorbing organic germs, so surely this old hospital's freedom from such must be regarded as contradictory evidence of anything of the kind, besides the most powerful microscopes now made have failed to find any such germs. He further dwelt upon the fact that Sir J. Simpson's statistics were very questionable as to accuracy, some of the authors of the figures trusting merely to memory; he also showed the inconsistency of comparing town and country patients, &c.

Dr. Macleod, of Glasgow, stated he had amputated at the ankle-joint in fifty cases with a single death, and drew attention to the recent comparatively equal success in the Glasgow Royal Infirmary, occurring at all events since the introduction of carbolic acid dressing. As to the treatment of the sick in huts or tents he liked it much, but one thing should be borne in mind, that it was constantly necessary to move the hut or tent as the ground under and around the same soon became offensively contaminated.

For ourselves we cannot help being reminded in recent discussions and papers on the so-called hospitalism, of the variety of evidence corroborating the experience of those tried medical and military men who took part in the long campaigns of the earlier part of the present century, and who would invariably much prefer a good house to quarter in for any length of time, and for the winter half of the year, to any tent or hut. In fact in all such the inmate, whether sick or well, is dependent upon the weather for any sort of comfort or health, for so surely as the soil becomes wet so surely does his condition become wretched and unsuitable to health.

At Leeds Mr. Wiltshire read a paper for Mr. Holmes Coote, giving results of many operations performed in Saint Bartholomew's Hospital in 1866; among others were eighteen cases of lithotomy without a single accident. Mr. Coote opposes the term " hospitalism " as tending to imply the production of something in the wards of an hospital that did not exist elsewhere; he had never seen anything in hospital practice that did not occur also in private cases, and Sir William Ferguson concurred in these views.

Dr. Cormack insists upon the absolute necessity of ensuring access of sun and wind to all hospitals, and mentions that the little Galignani Hospital at Paris has been injured by a building being lately erected having a dead wall fifty feet high, rising immediately above the hospital, and extending seventy feet at right angles beyond the hospital, in the direction south-south-east, so that the wards are deprived of all sun until 12.30 p.m., and from that time the heat reflected from this immense wall becomes intolerable during warm weather. The hospital is thus deprived of all air coming from the south-south-east; and when buildings are erected on the other side of the hospital, as may be the case any day, air will also be excluded from the west. It is very necessary to dwell thus upon this matter, as three highly distinguished French surgeons, who were consulted, failed to see the injury that had been inflicted upon the hospital by the structure just mentioned.

Concluding remarks.—We have endeavoured to present our readers with the opinions of the profession throughout the world on the subject of hospitals and their efficiency.

For ourselves, and we believe we express the opinion of the great majority of the profession also, we feel convinced that hospitals are eminently useful in relieving the sick and those requiring surgical care, and that maternity hospitals also have a useful and important share in relieving the wants of those who possess no homes, or such as are unsuited to their time of need. Serious charges have been preferred against all hospitals, but it is against maternity hospitals that the weight of energetic attack has been directed.

These accusations have been made with more or less appearance of reason in many instances, but on closer examination it has been found that the opponents of hospitals have in very many instances relied upon hearsay evidence of very doubtful value. Thousands of operations have been collated from private practice, very many of them unsupported by any written statement; thousands of operations more, of by no means so satisfactory an issue, might also have been added from the same source. The same observation applies to maternity and private midwifery practice. Some statements made in reference to the latter, but more especially in connection with certain selected small hospital returns are particularly valueless.

We have in a foot-note on a preceding page, drawn attention to a great source of fallacy quoted from a contemporary in the returns given by the Registrar-General from lying-in hospitals, namely, the fact that no distinction has been, heretofore, made between the mortality of mothers and that of infants; this should be remedied without any unnecessary delay.

In the same way an accurate registration of sickness, both in private practice and in public institutions, and a more perfect system of death registration, should be accomplished before even an approach to accuracy, in the relative estimation of mortality in and out of hospital, whether in reference to midwifery or other practice, could be obtained.

From an extended experience in city and in rural dispensary practice, in temperate and in tropical climates, in large public, and in private or small hospital practice, we have no hesitation in saying that hospital efficiency does not depend upon the smallness or more extended size of the hospital, but upon other considerations; to wit, its construction upon the pavillion principle, its freedom from defective sewerage, embracing proper and carefully constructed water-closet or other system, whereby all offensive matters shall be promptly and effectively removed, separation of the infectious from non-contagious cases, ample air space and light for every case, thorough cleanliness, not necessarily dependent upon daily washing of hospital flooring, which under some circumstances of climate, temperature, &c., may prove highly injurious.

These general conditions, coupled with a proper and well superintended system of nursing, and a sufficiently liberal scale of diet, form most important auxiliaries to medical treatment, and go far to ensure a successful issue to the same, just as the absence of these particulars almost absolutely renders nugatory the most skilful treatment.

VI.—Life of Faraday.[1]

A LIFE of Faraday was a book to be written. So important a position had he for a very considerable period occupied in the scientific world; so identified is his name with the greatest advancements of our age in chemical philosophy; so popularised, if we may use the expression, was he among adult and juvenile learners of the mysteries of that science, by his skill in unfolding those mysteries to the minds of the untrained in scientific investigation; so beloved was he as a man, and, lastly, so interesting was the history of his career and of his self-made greatness, that a record of his life and of his doings became a public desideratum. And happily the biography has fallen into the hands of one who not only knew him intimately as a friend, but who also understood him as a philosopher.

Dr. Bence Jones' modesty, as exhibited in his prefatory observations, is calculated to disarm the critic from all hostile intentions towards the memoir he has written. He felt that Faraday was too good a man for him to estimate rightly, and too great a philosopher for him to understand thoroughly. He "thought that his biographer should, if possible, be one who was his own mental counterpart;" and it was only when this model man failed to appear that "he," Dr. Jones, "at last made the attempt to join together his (Faraday's) own words, and to form them into a picture of his life, which may almost be looked upon as an autobiography."

This expression "to join together" is the key to the character of the work performed by Dr. Bence Jones. "From his letters,

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[1] *The Life and Letters of Faraday.* By Dr. Bence Jones, Secretary of the Royal Institution. In two volumes. London, 1870.

his laboratory note-books, his lecture-books, his Trinity House and other manuscripts, I have (he writes) arranged the materials for a memorial of Faraday in the simplest order, with the least connecting matter."

This plan of letting the subject of a memoir speak for himself, in letters and other documents of his own writing, has certainly the advantage of securing to the reader a greater amount of accuracy of detail and a clearer conception of the habits of mind of the individual concerned, than that other wherein a friend or acquaintance undertakes to represent him, and yet can do so only from his own standpoint, and from his own range of observation and inquiry. At the same time the former has its defect in the literary result—a defect rendered more obvious when the connecting matter is reduced, as it has been by Dr. Jones, to a minimum; for by it the narrative is broken and more or less disjointed; repetition of facts occur; foreign matter is introduced, and references are made to which the reader has no clue. Interest is not so well sustained, and volumes are increased in bulk without commensurate information or instruction. However, we must give the writer of the present biography credit for the judgment displayed in the selection made of the letters, and of the extracts from papers and journals. The materials placed before us bring well into view the character of Faraday and the progressive stages passed through by him in his evolution from an errand boy and bookseller's apprentice into a world-renowned chemical philosopher.

Born in 1791, of poor parents, and receiving "an education of the most ordinary description," Faraday exhibited, before going as errand-boy to Mr. Riebau, the bookseller, in 1804, his tendencies towards science by reading Marcet's "Conversations in Chemistry," and the electrical treatises in the 'Encyclopædia Britannica." A methodical but painstaking character and ambition with his method, were quickly displayed in the preparation of a note-book in 1809 with the title of the "Philosophical Miscellany," wherein he jotted down "notices, occurrences, events, &c., relating to the arts and sciences, collected from the public papers, reviews, magazines, and miscellaneous works, 'intended,' he says, 'to promote both amusement and instruction, and also to corroborate or invalidate those theories which are continually starting into the world of science'" (p. 13). The predilection of his mind towards chemistry, electricity, and galvanism, at this early age, is seen by the subjects noticed in this scientific *omnium gatherum*; and the same fact crops up in the earliest letters available to his biographer, written to his friend Mr. Abbott during his apprenticeship. These same letters show likewise how diligent he must have been in improving the very slender education his parents were able to afford him. Had these letters, remarks Dr. Jones, "been written by a highly educated gentleman, they would have been remarkable for the energy, correctness, and fluency of their style, and for the courtesy, kindness, candour, deference, and even humility of the thoughts they contain" (p. 15).

His ardour for knowledge, especially for chemistry and allied subjects, led him to seize all opportunities of acquiring books and attending lectures, and by the kindness of a friend he had the supreme felicity of hearing four of the last lectures of Sir Humphry Davy, given in the spring of 1812 at the Royal Institution.

Fired with enthusiasm for science he wrote Sir Joseph Banks, the President of the Royal Society, to signify his wish to be engaged in scientific occupation, but fortune this time did not smile on the aspirations of the poor apprentice. Nevertheless, nothing daunted by this disappointment, he shortly afterwards renewed his application to Sir H. Davy, accompanying it with a volume of notes of the lectures of the great chemist, taken by himself, and had the happiness of receiving a favorable reply; and in the course of another three months he was duly installed in the office of assistant in the laboratory of the Royal Institution. Thus was established a connection between Faraday and that institution that remained dissevered until death.

His active and minutely penetrating powers of observation and of reflection are evinced in almost every letter penned by him at this early stage of life. We have marked several paragraphs in illustration of this, but can cite only two; writing, when in his twenty-first year, to his friend Abbott, he says:—

"I was this morning called, by a trifling circumstance, to notice the peculiar motions of camphor on water. I should not have mentioned the simple circumstance but that I thought the effect was owing to electricity. . . . . I conceive, too, that a science may be illustrated by those minute actions and effects almost as much as by more evident and obvious phenomena" (p. 25).

How characteristic is this remark of the future great discoverer in the regions of electric and magnetic forces! So, again, is the following quotation:—

"I forgot to insert a query when at the proper place, though I think an investigation of it would be of importance to the science of chemistry, and, perhaps, electricity. Several of the metals, when rubbed, emit a peculiar smell, and more particularly tin. Now, smells are generally supposed to be caused by particles of the body that are given off; if so, then it introduces to our notice a very volatile property of those metals. But I suspect their electric states are concerned; and then we have an operation of that fluid that has seldom been noticed, and yet requires accounting for before the science can be completed" (p. 29).

Faraday's appointment at the Royal Institution was made in March, 1813; in the autumn of that year Sir H. Davy resolved to travel on the Continent, and to remain abroad until April, 1815, taking Lady Davy with him. Faraday was induced to travel with Davy, and act as his amanuensis and assistant in such philosophical work as he might attempt. Unfortunately, Lady Davy was not so clear in regard to the relation in which Faraday rightly stood towards her husband and herself, and so caused the amanuensis no small chagrin by treating him much after the fashion of a servant, and by the exhibition of an imperious temper. Several letters sent home bear witness to this, and speak regretfully of the agreement their writer had entered into to travel abroad with Davy. However, his troubles were much solaced by the advantages he possessed in the constant companionship of his eminent master, in the general information and interest derived from travelling, and in the introduction it gave him to very many distinguished foreign savans, whose friendship he retained in after life.

Dr. Bence Jones has quoted largely from the journal kept by Faraday during this continental trip, and if the extracts show the peripatetic philosopher, always alert to whatever relates to philosophy, they also exhibit an observant traveller having a keen sense of humour. Were this a suitable place for such non-medical matters, we should be pleased to show Faraday brimming over with fun at the expense of the boatload of French officials sent on board the English vessel to scrutinise the passengers and luggage about to be transferred to shore; or, again, at the efforts, the expedients, the toil, and the noise attendant on the landing of Davy's carriage; or in giving the description of the French postilion and his jack boots; or that of the queer pigs he encountered at Drieux.

The gaiety of the young philosopher at this period is further shown by the part he took in the carnival at Rome, and by his appearance on one occasion at a masked ball "in a domino," and on another in a nightgown and nightcap. In the very letter in which he has to complain of Lady Davy's temper, and the menial duties at times imposed upon him, and in which he refers to the possibility of throwing up his engagement with Sir Humphry Davy, and of returning home to resume his "old profession of bookseller," he tells his friend, dear B—, that he went to a masquerade ball between two and five in the morning and found it excellent (p. 186). So far could he unbend from his graver studies, and forget the domestic annoyances that grieved him.

On his return to England in the spring of 1815, Faraday was installed as assistant in the laboratory and mineralogical collection and superintendent of the apparatus, at the Royal Institution, at the very modest salary of thirty-five shillings a week, with apartments. His own feelings on attaining this position were marked not only by supreme contentment, but also by delight in being so placed that he might add to the stock of his scientific knowledge, and be associated with Sir Humphry Davy. To quote his own words in illustration, as extracted from a letter:—"The glorious opportunity I enjoy of improving in the knowledge of chemistry and the sciences with Sir H. Davy," 'with that innate humility,' (as his biographer continues,) 'which was increased by his religion, he said, "I have learned just enough to perceive my ignorance; the little knowledge I have gained makes me wish to know more" (p. 209).

Dr. Bence Jones proceeds with his biography by selecting from letters, journals, and lectures, such matter as illustrates the progressive education of Faraday as an original observer and thinker, arranging it under two divisions, representing "his earlier," and "his higher scientific education." For Faraday had now to take rank among those (described in his lecture entitled 'Observations on the Inertia of the Mind,' read at the City Philosophical Society, in July, 1818) who "attain to the distinguished honour of being first on the plain (of human life), and of taking the lead of their generation, of the age, and of the world" (p. 266); and the principle actuating him, as stated in the same lecture (p. 277), was that "Industry is the natural state of man, and the perfection of his nature is dependent on it; the progression which distinguishes him from everything else in the material world is maintained by it alone." After completing the history of his "higher scientific education" in the first volume, Dr. Bence Jones opens up the second volume with a notice of the first period of Faraday's experimental researches, and follows with one of those of his latter period. But between those two periods there intervenes one, designated by Dr. Jones as a state of rest, although such only in comparison with the state of intellectual activity preceding and following it. For the letters and other papers produced, and the lectures delivered during this resting stage, clearly enough show there was no cessation of industry, but that he still abided in what, in his own language, was his 'natural state.' Overwork was the cause of the condition that demanded some cessation from his wonted industry. This was shown by loss of memory, and by attacks of giddiness, and:—

"For a year he rested almost entirely; he gave no lectures, and he went for three months to Switzerland. After a year he began again to work for the Institution, and when he did go on with his researches, he returned to the liquefaction of gases. In different ways he showed much of his character during this period of rest. The journal he kept of his Swiss tour is an image of himself. It was written with excessive neatness, and it had the different mountain flowers which he gathered in his walks fixed in it, as few but Faraday himself could have fixed them (vol. ii, p. 126).

This period of rest is reckoned by his biographer as extending from early in 1841, until the close of 1844. The second period of Faraday's electrical work lasted ten years. In the beginning of 1845 he worked on the condensation of gases; and in the August of that year began to experiment on polarized light and electrolytes.

To follow up the account presented to us in these pages of the progressive advancement of Faraday as a student of science, and of the ever-expanding series of researches continually suggesting themselves to his fertile philosophic mind, would be to write an abstract of the history of chemical philosophy—a task rendered as needless by the publication of Prof. Tyndal's notice of Faraday as a discoverer, as it would be out of place in this periodical.

The last chapter in this bright eventful history, is that of the period that must befal all of us permitted to descend into the vale of years with the inevitable weakening of mind and body. Not that Faraday fell into premature decay, for his decline was gradual, and the less so in mental energy than in bodily vigour. In 1866

"His loss of power became more and more plain during the autumn and winter; all the actions of the body were carried on with difficulty; he was scarcely able to move; but his mind continually overflowed with the consciousness of the affectionate care of those dearest to him" (vol. ii, p. 480).

In the following year, August 25th, 1867, the last scene was ushered in, and Faraday "passed away from this life, quietly and peacefully," dying in his chair in his study.

These rude outline sketches from this ample biography would be glaringly incomplete without some reference to Faraday as a man of high moral rectitude and strong religious belief. He belonged to the small sect that separated itself from the Scottish Presbyterian Church in 1760, and became known as the Sandemanian sect. In this matter he followed the example and instructions of his parents and grandparents. He continued stedfast in it throughout life, and for several years, after being chosen as an elder of the church, preached regularly. He looked at the religious sentiments he possessed as the unmerited gift of heaven. He was (as he wrote in one of the earliest letters quoted, in 1812) well aware of his own nature as evil, and felt it strongly (vol. i, p. 48).

The religious phase of Faraday's life is but slightly brought into view by his biographer, who has mainly restricted himself to the portraiture of him as a chemist and philosopher. However, in his concluding pages Dr. Bence Jones has briefly summarised the moral and religious aspect of Faraday's life in the following well-written paragraphs:

"As a man, the beauty and the nobleness of his character was proved by very many great qualities. Among the first and greatest was his truthfulness. His noble nature showed itself in his search for truth. He loved truth beyond all other things; and no one ever did or will search for it with more energy than he did.

"His second great quality was his kindness (agapê). It was born in him, and by his careful culture grew up to be the rule of his life: kindness to every one, always—in thought, in word, and in deed.

"His third great quality was his energy. This was no strong effort for a short time, but a life-long lasting strife to seek and say that which he thought was true, and to do that which he thought was kind.

"Some will consider that his strong religious feeling was the prime cause of these great qualities; and there can be no doubt that one of his natural qualities was greatly strengthened by his religion. It produced what may be called his marvellous humility. . .

"To complete this picture, one word more must be said of his religion. His standard of duty was supernatural. It was not founded upon any intuitive ideas of right and wrong; nor was it fashioned upon any outward expediencies of time and place; but it was formed entirely on what he held to be the revelation of the will of God in the written word, and throughout all his life his faith led him to act up to the very letter of it."

VII.—Prostitution.[1]

NOTHING could have been better timed than the publication of the second edition of Mr. Acton's book, following, as it has done, a Parliamentary inquiry into the operation of the Contagious Diseases Act, and coinciding with a popular movement for its further extension, which has aroused against it passionate opposition. We use this last epithet advisedly, for although, as we said in a former article, we doubt the expediency of the pro-

[1] *Prostitution considered in its moral, social, and sanitary aspects in London and other large cities and garrison towns; with proposals for the control and prevention of its attendant evils.* By WILLIAM ACTON, M.R.C.S., &c. 2nd Edition. London, 1870. Pp. 302.

posed extension, we are bound to confess that the logic is on the side of the promoters of the scheme, rather than on that of its opponents. Anything more wild and preposterous than the assertions boldly put forth by the pamphleteers and orators in the camp of the opposition, more utterly untrue to fact and sobriety in argument, in the placards which covered the walls of the borough of Southwark during a recent election there, it has rarely been our misfortune to read. No good can ever come of such reckless appeals to popular feeling. The history of legislation in this country at least ought to have taught this lesson —that as education has spread among our population, so they have acquired with it the faculty of discerning the difference between argument and declamation; and that Parliament, while listening with deference to the former, when based upon a truthful and moderately-worded exposition of facts, invariably turns a deaf ear to the latter.

We repeat that Mr. Acton's book is *apropos* of the occasion. It is a strictly scientific treatise on a most difficult subject, and if we do not, and cannot, agree with all his inferences, we must bear witness, as we are bound to do, and most willingly do, to the *abandon* of honesty which every page of the volume manifests. Bent on declaring the whole truth, so far as he knows it, he appears utterly unconscious at times of its bearing against his favorite project; indeed, so little has he thought of withholding facts which tell against it, that an adversary might gather some of his most pertinent reasoning from the very statements of the author himself. It is rarely that we meet with so excellent a quality in a controversial writer.

But we do not intend to discuss at any length Mr. Acton's views upon the subject of the extension of the Contagious Diseases Act. We have already declared ourselves in relation to this. Nor shall we consider those few last chapters of his work which refer to the subjects of Seduction, Bastardy, and Infanticide. We alluded to these in our April number. All that we propose to ourselves now, is to extend to such of our readers as have not possessed themselves of Mr. Acton's work, the result of his researches into the condition of Prostitution here and abroad, and into the natural history of the prostitute herself, adding such remarks of our own as appear to us to be called for. Information such as Mr. Acton gives is absolutely essential to the due appreciation of the plans put forward for the amelioration of this unhappy class of persons.

We may first, then, consider how and why it is that prostitution exists, and how women came to take up this disreputable line of life. Prostitution is the method in which, in civilized countries, the female community supplies to the male community a need which the latter experiences, or believes that it experiences; by which the weaker, more modest and retiring sex meets a demand from the stronger, less patient, and aggressive sex. An immoral want, an unreal necessity, we at once, with our author, admit; but a want which practically has served to create a demand notwithstanding. We must take the world as we find it. And if the demand, great in its numerical expression on the part of the male sex, has been met by the establishment of a profession on the part of the female sex, by which a comparatively small number of persons suffice to satisfy the requirements of the case, the institution must be regarded as a tribute to the higher degree of restraint in which the female community holds its sexual instincts. Viewed in this light, prostitution has been regarded in some degree as one of the safeguards of female purity. The prostitute, plying her trade in the public streets, from whose contact the virtuous shrink with abhorrence, has been regarded as the victim which society is content to sacrifice for the purchase of domestic safety.

Against this view Mr. Acton protests. He does not think

"That any serious diminution of the number of prostitutes would be attended with an increase of clandestine immodesty. Such a consequence is not one that I think need be apprehended: the insinuation that virtuous women to be made to yield, require only to be assaulted, is a base and unworthy calumny; nor is it to be supposed that the man who will use a harlot, is prepared to insult and injure a modest woman. But intercourse with depraved women debases the mind, and gradually hardens the heart, and each act of gratification stimulates desire, and necessitates fresh indulgences; and when grown into a habit, not only breeds distaste for virtuous society, but causes the mind to form a degraded estimate of the sex, until all women seem mere objects of desire and vehicles of indulgence" (p. 166).

Of course prostitution is wrong, sadly, miserably wrong; disgraceful to civilization, disgraceful to a Christian community; but history tells us that the highest civilization has failed to obliterate the blot; and that Christianity, as a popular profession, has been content with branding the prostitute as an outcast, whilst its ministers have been inculcating the most noble doctrine of universal fraternity, and urging to the practice of an universal charity. If then we speak of prostitution as a necessity even in our Christian civilization, it is only in the sense that the Christianity on which our social relations are supposed to be based, has, in point of fact, little more to do with the morality of the masses, than may result from the sanction it gives to conventionalities with which it is in accord, and which probably have been engrafted by it upon our national manners. As a living spirit influencing the conscience and enforcing the obligations of morality, where passion is concerned, it can hardly be said to have any very general application. And even these moral conventionalities are only observed by a section of society, and are quite inefficient in holding back multitudes of men from immoral practices, who are by circumstances debarred from marriage, to whom, in our artificial state of society, marriage is difficult, or who are unwilling, though not pecuniarily disabled, to submit to its restraints and incur its obligations. A necessary want, in the sense that is impossible for men in either early or later manhood to exist in comfort while preserving continence, certainly there is none. The want arises from unbridled desire, and the demand which calls for the infamous supply is licentious. Yet we see no hope, no prospect of anything better. "The evil are ever mingled with the good," and will be to the end of time, if we may trust the highest authority upon the subject.

Having thus shown why prostitution exists, we have to inquire how the prostitution market is supplied,—how it is, and why it is that women become prostitutes. For a woman to become a prostitute, that is, to trade with her person indiscriminately for money, two classes of causes must be believed mostly to operate. In the first place there are certain moral sentiments more or less deeply rooted in the nature of most women, which must either be wanting, or enfeebled. Those defects, which M. Despine regards as the predisposing causes of the prostitution of females, he enumerates thus—absence or feebleness of the sentiment of modesty, of self-esteem, and want of providence or disregard of consequences. It is because most women possess the moral qualities referred to, that it is not possible for all women to fall into prostitution. A woman may have lived with a man as his mistress, and have been abandoned; yet the possession of these qualities will lead her to do anything rather than adopt the life of a prostitute. If driven to it by poverty, her position is intolerable, and she will quit the trade as soon as she can see a way to obtain a livelihood by manual labour. If these sentiments are weak, they may more readily be overthrown and stifled by the causes more immediately determining the adoption of prostitution. Unhappily, most of the women from whom the class of prostitutes is recruited, owe the feebleness of these sentiments to their early education, and the circumstances of their position in childhood and youth. Their moral sense, which would perhaps revolt against some other sins, sees no evil in the abandonment of their persons to the will of any individual of the opposite sex. With such it can scarcely be said that they have lost the sense of modesty and self-esteem —they never had any, or if they had, it was crushed out of them by their bringing up. Mr. Acton quotes from a letter of Mr. Mayhew the following picture; and if we repeat it here, it is because we believe that a like cause operates very largely in the manufacture of prostitutes in our large cities, as well as in country districts. A man and woman intermarry, and take a cottage probably with but two rooms. All goes on pretty well until the family increases; then more beds are crammed into the single sleeping apartment. As years pass on, some of the children may yet be in their infancy, but others of both sexes have crossed the line of puberty; still the whole family, perhaps with cousins in addition of both sexes, continue to occupy the same room, and frequently the same bed.

"In the illicit intercourse to which such a position frequently gives rise, it is not always that the tie of blood is respected. Certain it is that, where the relationship is even one degree removed from that of brother and sister, that tie is frequently overlooked. And when the circumstances do not lead to such horrible consequences, the mind, particularly of the female, is wholly divested of that sense of delicacy and shame which, so long as they are preserved, are the chief safeguards of her chastity. She therefore falls an early and easy prey to the temptations which beset her beyond the immediate circle of her family" (p. 182).

Mr. Acton also quotes from the 'Times' a letter by a brickmaker's daughter. She says—"We all slept in the same room. There were few privacies, few family secrets in our house. Father and mother both loved drink. In the household expenses, had accounts been kept, gin and beer would have been the heaviest items." After recounting the experiences of her infancy, she goes on thus:

"Now commences an important era in my life. I was a fine robust, healthy girl, thirteen years of age. I had larked with boys of my own age. I had huddled with them, boys and girls together, all night long, in our common haunts. I had seen much, and heard abundantly, of the mysteries of the sexes. To me such things had been matters of common sight and common talk. For some time I trembled and coquetted on the verge of a strong curiosity and a natural desire, and without a particle of affection, scarce a partiality, I lost—what? Not my virtue, for I never had any. That which is commonly, but untruly, called virtue, I gave away" (p. 184).

Of the determining causes which lead women to adopt the life of a prostitute, "seduction," in the proper meaning of the word, plays no important part; yet probably numbers fall victims to the arts of professional and mercenary seducers of either sex, who take advantage of a girl's position to lead her astray, or flatter her tastes and evil sentiments by promises of indulgence. The sequel of the autobiography from which we quoted above, indicates the manner in which a low class of women similarly brought up, are introduced by others to the profession:

"According to my own ideas at the time, I only extended my rightful enjoyments. Opportunity was not long wanting to put my newly-acquired knowledge to profitable use. In the commencement of my fifteenth year one of our be-ribboned visitors took me off, and introduced me to the great world; and thus commenced my career as what you better classes call a prostitute. I cannot say that I felt any other shame than the bashfulness of a noviciate introduced to strange society. Remarkable for good looks, and no less so for good temper, I gained money, dressed gaily, and soon agreeably astonished my parents and old neighbours by making a descent upon them."

What follows certainly seems to support the doctrine of Despine as to the deficiency of the moral sense (so far as it applies to sexual irregularities), and of the sentiment of modesty, which he regards as appertaining to the class of prostitutes,—for our own part, we would say to the class of irreclaimable prostitutes.

"Passing over the vicissitudes of my course, alternating between reckless gaiety and extreme destitution, I improved myself greatly; and at the age of eighteen was living partly under the protection of one who thought he discovered that I had talent, and some good qualities as well as beauty, who treated me more kindly and considerately than I had ever before been treated, and thus drew from me something like a feeling of regard, but not sufficiently strong to lift me to that sense of my position which the so-called virtuous and respectable members of society seem to entertain. Under the protection of this gentleman, and encouraged by him, I commenced the work of my education; that portion of education which is comprised in some knowledge of my own language and the ordinary accomplishments of my sex;—moral science, as I believe it is called, has always been an enigma to me, and is so to this day. I suppose it is because I am one of those who, as Rousseau says, are 'born to be prostitutes.' Common honesty I believe in rigidly. I have always paid my debts, and, though I say it, have always been charitable to my fellow creatures. I have not neglected my duty to my family. I supported my parents while they lived, and buried them decently when they died. I paid a celebrated lawyer heavily for defending unsuccessfully my eldest brother who had the folly to be caught in the commission of a robbery. I forgave him the offence against the law in the theft, and the offence against discretion in being caught. This cost me some effort, for I always abhorred stealing.

I apprenticed my younger brother to a good trade, and helped him into a little business. Drink frustrated my efforts in his behalf. Through the influence of a very influential gentleman, a very particular *friend* of mine, he is now a well conducted member of the police.

My sisters, whose early life was in all respects the counterpart of my own, I brought out and started in the world. The elder of the two is kept by a nobleman, the next by an officer in the army; the third has not yet come to years of discretion, and is having her fling before she settles down" (p. 184).

Here then is one way in which prostitutes are made. Promiscuous herding of boys and girls together, "seductions—if seduction it can be called—effected, with their own consent, by boys no older than themselves," and an introduction by established prostitutes to their own way of life, to a life which though checquered with reverses has been probably from their point of view on the whole successful. We can readily understand, however, that all are not thus successful, and that multitudes never emerge from the depth of misery in which they have been trained. Such would be the poor creatures who infest the waterside resorts of sailors, and of whom, as living like wild beasts in the neighbourhood of the Curragh Camp, we have read such shocking accounts. According to French writers, distress is one of the most fruitful of the determining causes of prostitution, especially among young girls who have been abandoned by the men who seduced them, cast off by their parents and friends, and have been unable to obtain such work as will provide them an honest livelihood. Sometimes it is the distress of her parents or of her offspring, and to relieve their necessities rather than her own, that a woman prostitutes herself. Mr. Acton says, "It is a shameful fact but no less true that the lowness of wages paid to workwomen in various trades is a fruitful source of prostitution; unable to obtain by their labour the means of procuring the bare necessaries of life, they gain by surrendering their bodies to evil uses food to sustain and clothes to cover them" (p. 180).

Some are more exposed to this temptation than others; Mr. Acton enumerates among these, actresses, milliners, shop girls, domestic servants, and women employed in factories and working in agricultural gangs. A certain number appear to have been driven from their homes by the inhuman conduct of their parents, perhaps of a step-mother, and being predisposed by natural deficiency in those sentiments which usually guard the chastity of women, resort to prostitution in place of a life of honest labour. With many, other causes operate, probably, however, more as assisting causes than alone; such are vanity and a love of admiration and of dress, a desire of being free from the control of parents or masters, an innate love of excitement and pleasure, idleness, and a love of drink. It is doubtful if a mere exaggeration of the sexual instinct has very much to do with a woman adopting this life.

It will be gathered incidentally from some of the foregoing observations that prostitutes as a class are not altogether lost to every good and proper feeling. Like other people they have their virtues as well as their vices. They have their habits and modes of thought, all of which demand careful and elaborate study by any person who presumes to undertake to plead their cause, to legislate on their behalf, or to promulgate schemes for their reclamation.

To begin with their virtues. It must not be asserted, as some assert, that these poor creatures are all entirely devoid of the sense of shame and of self-respect. It is not for us to dive into the recesses of the heart, or to pretend to read in outward behaviour the true feelings which it experiences. There are those who, driven to their course of life by the untoward circumstances in which they find themselves placed, and looking back to a time of innocence, not only enter upon it with repugnance, but would gladly return if they could to the path of virtue. Although in the exercise of their trade they are willing to submit to the embraces of a stranger, yet it has been found that when accidentally surprised in a hospital ward they will quickly and instinctively cover themselves up from the eyes of a visitor, and put their hands over their eyes when surgically examined in the presence of students, while they studiously avoid recognition by those with whom they were acquainted in their virtuous days. It appears also, from a statement made by Dr. Stuart at a recent meeting, that they particularly object to surgical examinations being made in the presence of persons of their own sex, probably because they are aware that to them especially they are objects of contempt; and fallen as they are they have sufficient self-esteem to guard themselves against this degradation. When we contrast the behaviour of the same women in hospital or in prison with that which they exhibit when free in the streets and surrounded by vicious associates, it is difficult to do away with the belief that drink, rivalry, and excitement lend aid to their exhibition of immodesty in language and gesture, to their abandonment of self-respect, and to the recklessness of their conduct. We have already alluded to the fact that maternal and filial love are often well developed among some of the class. Where a sense of rivalry is absent they can be kind and charitable to one another, as well as to any in distress and trouble. M. Despine relates two interesting instances of this, in addition to many recorded by other authors.

It is said that where prostitutes are enrolled they are not in the habit of denouncing one another to the police. When one of them becomes pregnant, and especially at the period of her confinement, her companions assist her and her infant. They commonly exhibit much affection for their offspring, and have been known to place restraint upon their conduct in the presence of their children. Strange to say, too, they often have a lover to whom they are devotedly attached, and this notwithstanding ill treatment that they may receive at his hands.

The prominent vices of the prostitute class are naturally those which belong to persons untrained in youth to conform to moral rules, together with those which have been already mentioned as assisting to lead a girl into prostitution, and those which a reckless mode of life itself renders habitual. Hence we find them addicted to lying and sudden though ephemeral outbreaks of fury, improvident and reckless, fond of dress and extravagant, fond of drink, restless and intolerant of control. Individual character, however, will vary as might be expected with age, education, and the grade of society from which a woman has sprung.

Mr. Acton divides the order of prostitutes into three classes, premising that the shades of prostitution are as numberless as those of society at large. These classes are the "kept woman," who has in truth, or pretends to have, but one paramour, with whom she in some cases resides; the common prostitute, who is at the service, with slight reservation, of the first comer, and attempts no other means of life; and the woman whose prostitution is a subsidiary calling, adopted in order probably to obtain for herself and others whom she is bound to support those necessaries which the miserable pittance often paid to workwomen as wages for incessant drudgery will not suffice to furnish. The most important of these three classes is the second. These are the pests of London and our large towns, the brazen-faced unfortunate offenders against decency who render our streets dangerous to susceptible youths after nightfall, and defile by their presence many otherwise harmless places of public amusement.

Mr. Acton is cautious in forming any estimate of the number of common prostitutes in London. A return made by Mr. Kittle, of the metropolitan police, makes it about 6515, of whom only eleven live in brothels and 2155 in private lodgings, while 4349 are returned as infesting low neighbourhoods. This return only mentions two places, brothels, strictly so called, where prostitutes are kept. There are 1756 houses known to the police where prostitutes lodge, and 229 coffee houses or other places where business is ostensibly carried on, but which are used as places of resort for prostitutes or "accommodation houses." The description of brothels called "dress houses," where the women are farmed by the brothel keeper, being little else than his finely dressed slaves, have nearly disappeared from London: nevertheless Mr. Acton says there still exist establishments in which the women live with their landlady, by whom they are provided with food, dress, and lodging, all which are charged for at an exorbitant price, the landlady usually contriving to keep them in her debt: they have, however, the right of receiving and retaining their own money, and the privilege of accepting or declining at their own discretion the attentions offered by visitors.

It is unnecessary to follow Mr. Acton further in his account of the houses and haunts of prostitutes, since it is by no means a savoury subject, while the features of prostitution in the provinces do not differ materially from those exhibited in the metropolis. The most important points which he insists upon in respect of the career of these women are that harlotry is but a temporary phase of a woman's existence, that it is an error to suppose that there is no possible advance, moral or physical, in the condition of the actual prostitute, and that another error is that the harlot's progress is short and rapid. He maintains on the contrary that "the downward progress and death of the prostitute in the absolute ranks of that occupation are exceptional, and that she succumbs at last not to that calling, nor to venereal disease, but in due time and to the various maladies common to respectable humanity." In proof of this he quotes the small number of deaths of females over fifteen years of age registered as from syphilis, the small fatality of that disease among diseased women in hospitals, and the experience of hospitals, penitentiaries, &c., to the effect that these women do not, as supposed, fall victims to the other diseases incident to an irregular course of life. He dwells upon the fact that the prostitute is usually a girl with a healthy frame, excellent constitution, and in the vigour of life, and thus naturally possessed of great powers of resistance to unfavorable influences. He quotes Parent-Duchâtelet "that, notwithstanding all these excesses and exposure to so many causes of disease, their health resists all attacks better than that of the ordinary run of women who have children and lead orderly lives. They have (as some one has remarked) iron bodies which enable them with impunity to meet trials such as would prove fatal to others." He adds, on his own account, "If we compare the prostitute at thirty-five with her sister who, perhaps, is the married mother of a family, and has been a toiling slave for years in the over-heated laboratories of fashion, we shall seldom find that the constitutional ravages often thought to be necessary consequences of prostitution exceed those attributable to the cares of a family and the heart-wearying struggles of virtuous labour" (p. 39). He believes that by far the larger number of these women return sooner or later to a more or less regular course of life, that "before she has carried on the trade four years she has fully comprehended her situation, its horrors and its difficulties, and is prepared to escape should opportunity present itself." He says that encumbrances rarely attend the prostitute who flies from the horrors of her position. Many of the better inclined become the wives of men in every grade of society, while others, who have been enabled to lay by variable sums of money, work their own reclamation as established milliners, small shopkeepers, and lodging-house keepers. As to the first of these modes of escape Mr. Acton has a great deal to say. He regards the action of wedlock in this way as both enormous and continual, and quotes in confirmation the recent report of the select committee of the House of Commons. Mr. Sloggett stated in his evidence that out of 1775 prostitutes in Devonport 250 were known to have married; and Mr. Parsons, speaking of Portsmouth, stated that many marry, as shown by his own statistics, and, although it is difficult of belief, many marry exceedingly well.

With a basis such as that which may be constructed out of the preceding observations we may now go on to inquire, first, how far a Government such as ours may be supposed to be concerned with the social derangements of which prostitution is the result, and with the physical evils of which it is the parent; and next, supposing it is concerned with them at all, in what way its work of amelioration might be carried out in accordance with the traditional policy of the nation to which we belong, as well as with its moral sentiments; for it is clear to us that, whatever steps it may be designed to take, they must not be repugnant to the latter, or be in marked contrast with the former.

Mr. Acton, with much industry and the assistance of the Foreign Office, has furnished us with an account of prostitution abroad, and the measures adopted for its control and regulation, such as is not to be found in any other work in our language. The chapter on this subject includes France, Belgium, Hamburg, Berlin, Vienna, and Italy. He well says:

"If, on full examination of the means adopted in other countries for grappling with this evil, and of the results obtained from them, we refuse to follow their example, we may at least profit by their experience" (p. 99).

We will use these results where we find it necessary in the consideration of the questions now before us.

We have already, in a former number, declared ourselves upon the question of the propriety of our Government interfering directly between the *diseased* prostitute and her client. We may inquire here how far it can be regarded as the duty of the Government, how far it would be prudent, to adopt measures for the suppression of prostitution, and in what way, supposing the duty conceded, this business should be set about.

Prostitution may, as a social derangement, be regarded in three points of view. 1st. It is immoral on the part of the women and immoral on the part of men who consort with prostitutes; and moreover, all irregular connexions of this sort serve to withdraw both the men and women from marriage, the only sexual relation which public policy can sanction. But simply on the ground of immorality it is no part of the duty of our Government to interfere with the liberty of individuals, although it is its duty to prevent crime by such interference, and to punish criminals. The only duty which society owes to morals is to promote the moral tone of the nation by such measures as are calculated directly or indirectly to attain this object. We learn from Mr. Acton that where governments have directly interfered to suppress prostitution, not only have their efforts failed of the anticipated result, but other and greater evils have followed. Such evils have followed, even where, prostitution being recognised, an amount of control has been exercised which reduces the liberty of the prostitute, and surrounds prostitution with restrictions and inconveniences. Thus, in Rome:

"Prostitution is in no way recognised by either Church or State, on the ground, I believe, that the Pontiff's secular and religious functions are one and indivisible; and that the admission, much more the toleration, by Christ's Vicar, of unhallowed connexions, is an utter impossibility . . . . . Its resorts are proscribed and so continually hunted from point to point, in compliance with no written law, by arbitary authority, that scarce a dozen houses can continue to lurk within the limits of the Roman police jurisdiction, and then only through the bribed connivance of the lower officials. But the reverse of this pleasing show of external propriety shows clandestine prostitution, with its inevitable concomitant, depravation of morals, and wide diffusion of intense disease, invading domesticity itself" (p. 149).

The state of things in Stockholm is thus described by Mr. Bayard Taylor, as quoted by Mr. Acton:

"It has been called the most licentious city in Europe, and I have no doubt with the most perfect justice . . . . Of the servant girls, shop girls, and seamstresses, it is very safe to say that scarcely ten out of a hundred are chaste; while, as rakish young Swedes have coolly informed me, many girls of respectable parentage, belonging to the middle class, are not much better . . . There are no houses of prostitution in Stockholm, and the city would be scandalized at the idea of allowing such a thing. A few years ago two were established, and the fact was no sooner known than a virtuous mob arose, and violently pulled them down" (p. 148).

Again:

"The regulation of the prostitution of Berlin, a city of 702,000 inhabitants, has long been the cause of contention between the severe puritanism of the religious public and the police administration of the place. It has been warmly argued by the former that, inasmuch as marriage is a desirable state, it can be fostered by uprooting the vices peculiar to celibacy, and, therefore, when this party has been in the ascendant, vigorous crusades have been carried out against prostitution. The town has repeatedly been purged of prostitutes since the Reformation, but has as often immediately fallen a prey to desertion of infants, adultery, abortion, and clandestine prostitution . . . It appeared to a Commission of Inquiry in 1717, when repression was in vogue, that clandestinity had attained such magnitude, that the bridewells were inadequate for the reception of the arrested women. The tolerated houses which had been previously shut up were, therefore, again opened" (p. 139).

Again, Mr. Acton writes—

"The flourishing city of Hamburg proper began as early as A.D. 1292 to provide in its municipal code for the toleration and control of fornication. The system at present in force was initiated by the town itself in 1807, improved upon under the French occupation in 1811, and finally settled in 1834. It is of great length, and minute, as might be expected, in the extreme; but though of great value as a check upon the most fruitful source of venereal disease, is, like its doubles in other cited instances, *a painfully weak experiment as regards public morality*" (p. 138).

We need only further recall to the recollection of our readers the very strict regulations in force in Paris and Brussels for the control of prostitution, for they have been so much talked about of late, that few can be supposed to be ignorant of them. Yet what is the result? putting the question of physical evil to the population aside. Mr. Acton writes thus about Brussels:

"Truth compels me to avow my opinion that, however much the virulence of syphilis may have abated, and the health of the Brussels garrison have improved within twenty years, there is no marked improvement in the general tone of morals there" (p. 136).

He attributes this, however, to the fact that the application of the regulations to the suburban communes is at the discretion of the local magistracy.

The French regulations are based upon the principle that anything is preferable to clandestine prostitution, and hence the tendency of the system is to drive all prostitutes into registered houses. The result has been not only to render the return of the prostitute, there domiciled, to the paths of virtue hopeless, so long as she retains her attractions, and to render her condition one of the most wretched serfdom, but even to promote clandestinity itself.

Even where a prostitute is not domiciled in a regular house, the very fact of her registration as a *fille à carte* places an impediment to the abandonment of her calling.

"The same policy which considers the registration of the prostitute indispensable to public order, dictates the exercise of considerable caution in liberating her from supervision. The formalities which attend what is termed the authorised "radiation" are numerous and strict. The mere profession of changed sentiments is treated with suspicion, and a probation of two or three months under private surveillance is insisted upon. The prayer is granted only on its being made clear that it results from something more than an *intention passagère*, or disgust at the inspection—that means of honest support are more than probably forthcoming, and that public order and salubrity will not be jeopardised by the reappearance of the petitioner as an *insoumise* upon the public streets" (p. 107).

Who can wonder, with all this, that Parisian prostitutes do their utmost to baffle the police? that clandestine prostitution is promoted? Thus Mr. Acton:

"The *clandestine prostitute*, notwithstanding all the precautions of the police to register every woman gaining her living by prostitution, is frequently to be met with in the streets of Paris. Work girls, servants, and girls working in shops who wish to increase their small earnings, and are not yet registered by the police, come under the head of clandestine prostitutes" (p. 112).

Some persons, such as the writer of the recent articles in the Westminster Review, would paint clandestine prostitution as it exists in Paris in yet more gloomy colours. It must be large in amount, since we are told that no less than thirty detectives are employed to watch and trace prostitutes. In 1854 the registered women in Paris amounted to 4260. In the same year, of the unregistered women captured, one in four were found diseased. In the same year, 1485 diseased women (one registered) were admitted into the Lourcine Hospital. Multiply this number by four and it will give some idea of the amount of clandestine prostitution in Paris; 5940 women as against the 4260 registered, notwithstanding all the efforts of a stringent system of control.

Let us make one more extract. Mr. Acton is describing an inspection of prostitutes at Paris, at which he assisted—

"I was informed that on the day after these inspections the houses are specially frequented by the public, in the belief that there is then less chance of contracting disease."

The inference to be drawn from these quotations appears to us to be this, that severe legal restrictions placed upon prostitution have only served to defeat their own object, *quoad* the moral improvement of the population and the repression of male and female incontinence, and that the failure has been most marked in instances where the restrictions have been most severe. We do not and cannot deny, in the face of facts to the contrary, that the physical condition of the women has been amended, and the spread of disease to soldiers and sailors has been reduced, when the arrest and treatment of diseased prostitutes have formed a material part of the measures adopted.

2. But while such a government as ours cannot, on principle, undertake the regulation of individual morality, and even if it could, must be deterred from doing so by the experience of other nations who have attempted it; while it is obvious that prostitution as a disease of society cannot be put down by the strong arm of the law, but that women and men must be left to associate themselves sexually as they please; we must add that, in thus associating themselves, they are bound to injure no one but themselves, to interfere with the comfort and rights of no others, and to refrain from anything which shall bring open scandal upon their neighbourhood or the nation to which they belong. And with these objects in view, it is distinctly the duty of any good government to act, but to act for public protection only—wisely and considerately. This cannot, of course, be done without placing some restrictions upon public prostitution, but the restrictions might probably be such as shall not produce a counterbalancing amount of evil.

In the first place we think that a restriction should be placed, and might safely be placed, upon the public exhibition of prostitution. At present, our streets after nightfall are a disgrace to us in the eyes of many foreigners, and are dangerous to the youth of our towns. They offer enticements to immorality which ought not to be permitted, and there can be no question that many, more especially young men, are led into mischief from this cause, who do not premeditatedly go in search of the opportunity. The demand for prostitutes is not a natural one, it is a forced demand—forced by the very existence of a superabundant supply. That it is so, seems to be shown by the very fact that women parade themselves in public places and solicit. The natural demand must be left to itself. If men premeditatedly set out to obtain satisfaction of what they consider or fancy they feel as a natural want, no government can wisely interfere with them, but it can take care that the want shall not be artificially created or stimulated by the ready presentation of the opportunity for gratification. They should at least be put to the trouble of going in search of it. With this end in view, women who are known to the police as common prostitutes, should be prohibited from the open exhibition of themselves in public places in such a manner as to attract attention, and from loitering in conspicuous places, about public-houses, dram shops, and refreshment houses; and when in the public streets from soliciting by word or indecent gesture. We do not think that any hardship would be inflicted even by prohibiting women, known to the police as prostitutes, from frequenting the streets after a fixed hour in the evening. Any act of solicitation or indecency, or any riotous or noisy behaviour, should subject such a woman to arrest, when, if found diseased, she might be detained in a special hospital until cured. The police should be expected to know all the houses in which common prostitutes lodge, and should be empowered to give information to any neighbour who may desire to know the character of a house, and who may complain of annoyance. In any such case the woman should be required to remove to some place where she shall not be a nuisance, or on failure of obedience, be liable to arrest and detention. The trade of a procuress and the intervention of any third person between the male population and the prostitute should be mercilessly suppressed. So also should accommodation houses, wherever they give occasion to complaints from neighbours or residents in a district, under whatever ostensible definition such houses may pass. A refreshment house, the common resort of prostitutes, should be classed with accommodation houses, whether used as such or not. Houses of refreshment exist for the benefit of the moral as well as the immoral part of society, and the former should not be deterred from using them by the certainty of being exposed to dangerous temptation if they enter. With respect to licensed houses and places of public amusement, it is tolerably clear that it would be both impolitic and impracticable to attempt the exclusion of loose women, but while in them they should be required to behave decently, and to abstain from solicitation by word or gesture: the practice, however, of giving sucn women free passes with the object of increasing the attractions of the place to the immoral part of the public should be strictly forbidden, and subject the proprietor to the loss of his license. Some of these places are notorious resorts of prostitutes, and being so, would naturally be avoided by the well-disposed, who can go elsewhere for the innocent amusements they seek. In addition to this, information should be diffused by authority among prostitutes as to their rights in respect of women with whom they lodge. Such women should have no right to recover from a common prostitute arrears of rent, or money advanced for dress or for necessaries requisite for the carrying on of her trade. Complaint made by any prostitute to the police of any impediment to her freedom by the person with whom she lodges, or the expression of a desire to quit her course of life, should entitle her to protection and admission to an authorised refuge. We firmly believe that many a woman continues in prostitution because the state has provided her with no means of escape, and because she is ignorant of the law which would deliver her from the hands of the harpies who grow rich upon her misfortune. To this extent we believe the law might safely go in placing restrictions upon open prostitution, and that they would not tend to produce those evil results which have followed regulations of a more strict and repressive character. Women who adopt the profession of a prostitute would still be known to the authorities, but would be hindered from enticing and entrapping the unwary or those who are helpless from too free indulgence in drink; while the scandal of our public thoroughfares would, at any rate, be much reduced, and immoral men would be compelled to go in search of what they require, by resorting to places where the women whom they seek are to be found.

In making these suggestions we would not be understood as denying that the physical evils resulting from prostitution are, under some circumstances, worthy of the consideration of the legislature, or that a modified contagious diseases Act might with advantage be used as an auxiliary means of bringing about a social improvement. Upon this branch of the subject, however, we have nothing more to say at present.

3. We will, then, pass on to consider in what way our Government might act for the amelioration of those social derangements of which prostitution is the result, as certain physical evils are the result of prostitution. And now we, as medical men, have a right to apply our practical maxim, "Sublatâ causâ, tollitur effectus."

If a woman deliberately rushes into prostitution with her eyes open, whether to escape from labour which is repugnant to her, or because the prospect of license and indulgence in dress and pleasure attracts her, there is no power on earth which can prevent it, and like any other wilful person, she must be allowed to take her own way, and to find out by experience the folly of her conduct. She is pretty certain to find it out in the long run, and there should be openings by which she may escape. The path of repentance and reformation is a rough one, and cannot be made smooth, but in consideration of the character of the class, its roughness may be lessened in charity to the offender, and in view of the excellence of the end which it is sought to attain. In this point of view we should object to nothing but that sort of petting into which many of our most excellent schemes for moral improvement of the lower classes have been seen to degenerate. What we hold to be the duty of the state is, to take care that no woman shall rush into prostitution without having her eyes open, without knowing that she is doing that which is repugnant to decency and good morals. We are not among those who believe that the germs of sexual modesty are absent from the breast of any young girl, unless, indeed, there be exhibited some other evidence of mental or moral incapacity. But these germs may lose vitality by neglect, or grow under cultivation to be the most reliable safeguard of female virtue. It is the duty of the state so to arrange that none grow up in the midst of its population without some amount of moral training —such an amount at least as can be given within the limits assigned to popular and national education. It would be something if girls were taught by the discipline of a school that the indiscriminate intermingling with the opposite sex, even in childhood, is a thing which is to be avoided, and by the influence and instructions of their teachers be imbued with some notions of the obligations of morality and religion; that idleness is the fruitful mother of moral evils, and that even in this life wickedness and virtue carry with them inevitably each its appropriate reward. Even if, after a moral education, a girl should be unfortunately seduced in an hour of misplaced confidence, there would be less chance of her abandoning herself to prostitution, or, if she did so, a better chance of an early reform.

In our recent article on "Baby-killing" we had occasion to refer to the views of Mr. Acton respecting the prevention of seduction, and the care of women pregnant out of wedlock as well as of their offspring. We cannot but think that some such scheme as he proposes would, if adopted, have the effect of preserving many women from falling into the pit which so commonly yawns before them when the period of their confinement has terminated. We have ourselves just alluded to the probable good results which might be anticipated from a compulsory education of the lowest classes at schools where they might obtain some amount of moral and religious training. But the efficacy of the latter may be more than counterbalanced by the evil example and influence at home; and hence the importance of preventing, so far as may be practicable, that miserable herding of the lower orders, which we have mentioned as one of the causes of precocious female demoralisation. This is a work which Parliament, with all its efforts of sanitary amelioration, has yet been unable to accomplish. The Common Lodging-house Acts have effected some improvement in this direction, and so far good has undoubtedly been done. A houseless girl, with twopence or threepence in her pocket, may now spend the night in a registered common lodging-house, if not without danger to her morals, yet without danger to her virtue. She is certain only to be placed with persons of her own sex; and the Sanitary Act of 1866 makes ample provision against unwholesome, and indirectly against immoral crowding in houses where the rooms are let separately to distinct families, by providing for their registration and regulation by the local authorities. By the local authorities! Yes; and it is just here that the Act has failed to be operative, for the local authorities, with a few exceptions, with one accord have declined to put their powers into action. Of this we shall have more to say on another occasion. We signalise the neglect now as a warning to future legislators to leave as little as possible to the option of persons who have a vested interest in the *status quo*. If Parliament really desires to check prostitution, let it place it out of the power of vestries and guardian boards to refuse compliance with the 35th section of the Sanitary Act, when a responsible health officer certifies that it ought to be put in force. More will be effected in this way than by any measure yet proposed for the regulation or reclamation of prostitutes.

Among Mr. Acton's suggestions there are two more which we must mention with approval. One is the provision of an altered and improved system of female training. He is not speaking of ordinary school training. What he recommends is a system of training for domestic work, urging that such household education should be incorporated to a much greater extent than at present with the discipline of union houses and schools. A further remedy may be found in a judicious system of emigration, which would relieve the labour market of that excess which has led to the reduction of wages of workwomen to the line which marks starvation point.

We cannot better conclude our remarks than by quoting from the feeling appeal which terminates Mr. Acton's volume.

"It is absolutely impossible to exaggerate the suffering entailed by a life of prostitution. Instead of the scorn so freely lavished on the poor lost daughters of shame and misery, I plead for a little pity—nay, for more than pity, I plead for justice. If unequal laws between man and woman compel to a shameful and a hated trade the helpless and shivering victim of seduction, whose fall, though it has soiled and stained, has not utterly polluted her, I charge those laws with cruelty, and I say further, that her blood is on the head of those who know the injustice of such laws yet will not help to alter them. If human beings are left to herd together with indecent indiscriminacy, because in this rich and luxurious city they can obtain no more fitting shelter; if they are allowed to grow up from childhood to youth and from youth to adult years amid scenes of depravity and sin, I ask, on whose shoulders does the blame really rest, whether on the victim's head, reared to a life of infamy, or on society's, that leaves them to a fate so awful? If in this wide world, teeming with abundant supplies for human want, to thousands of wretched creatures no choice is open between starvation and sin, may we not justly say that there is something utterly wrong in the system that permits such things to be? If the traffic in human flesh and female labour is not repressed by the arm of the law, may we not justly accuse the law of falling far short of its duty?" (p. 301).

VIII.—Cholera in India.[1]

EPIDEMIOLOGISTS have long felt that no substantial progress was likely to be made in the satisfactory knowledge of cholera until its natural history was far more diligently and systematically studied than it has hitherto been, and until the very same method of investigation was applied to epidemiology that has been needed in kindred branches of scientific inquiry. It was only by the accumulation of authentic and multiplied observations, continued at different times and in different places, along with the faithful record of facts and phenomena at the time of their occurrence, followed up by cautious inductive reasoning based upon such evidence, that so many valuable discoveries in meteorology, in terrestrial magnetism, and in the "physical geography of the sea," have been made within the last five and twenty years; and that various occult problems in nature have been more or less distinctly elucidated. And, in the present day, the still more obscure subjects of solar and stellar physics are beginning to yield some curious and interesting results to the same slow but sure process of patient research. Medical men have seldom been willing to pursue this plan in conducting their inquiries. A few meager observations

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[1] 1. *A Report on the Cholera of* 1866-68 *in the Bengal Presidency, and its relations to the Cholera of previous Epidemics.* By JAMES L. BRYDEN, M.D., Surgeon, Bengal Army; Statistical Officer attached to the Sanitary Commissioner with the Government of India. Folio, pp. 410. Calcutta, 1869.

2. *Fifth Annual Report of the Sanitary Commissioner with the Government of India*, 1868. Folio, pp. 126. Calcutta, 1869.

3. *Report on Measures adopted for Sanitary Improvements in India during* 1868, *and up to June*, 1869, *&c. &c.* Printed by order of the Secretary of State for India. Folio, pp. 215. (Blue Book.) London, 1869.

4. *A Treatise on Asiatic Cholera.* By C. MACNAMARA, Surgeon to the Calcutta Ophthalmic Hospital. 8vo, pp. 557. 1869.

loosely recorded, and at once made the basis of hasty conclusions, have too often marked the mode of their procedure. The consequences may be seen in the tardy and doubtful progress of their science, and in the uncertainty and fluctuations of opinion which have ever characterised its history. Nowhere is this radical defect more conspicuous than in respect of epidemiological researches, and in no one individual subject more than that of epidemic cholera. It is indeed humiliating to reflect how very little we can be said yet to know as to the laws of its development, spread, and movements, its duration, recurrence, and a multitude of other points in its physical history, notwithstanding the enormous literature about the disease during the last fifty years. At no time in this period has there ever been a lack of speculation and theory respecting it, but unhappily almost always there has been a minimum of authentic certified information. And certainly at no previous time was there ever a greater amount of mere hypothesis and conjecture, perhaps, than in the present day, and for the last few years, since the date of the latest European visitation. That visitation found the medical mind throughout Europe so strongly impressed with the belief that the principal or only mode of dissemination of the cholera was through human intercourse, that the Constantinople Conference made this the starting-point in all their deliberations; and the cardinal feature of their conclusions, it will be remembered, was that, as the disease is essentially propagable from man to man (communicable also by fomites or infected articles), the main preventive and prophylactic against its diffusion in future must be looked for in a universal system of rigorous quarantine, without which all other measures of defence will be fruitless.

In this country the views of the Conference met with but little favour, chiefly because, foreign physicians allege, their adoption would of course interfere with the freedom of commercial intercourse. The dominant opinion here, at least in the metropolis, was that the spreading of cholera was principally, or solely, by transmission from one human being to another, but that this transmission took place in a peculiar manner—viz. by the morbific poison or material cause of the disease being discharged in the alvine excreta of the sick, these excreta subsequently contaminating the drinking water of the locality, and the poison being received into the body through the medium of this cholera-tainted water. These doctrines were so generally accepted that not only were they widely adopted by the periodical press of the day, but they have been admitted as scientific verities into standard works on physiology and practical medicine recently published, as well as into lectures in our medical schools, and into official documents both in this country and in India. And yet, can they be regarded as anything more than ingenious speculations, awaiting confirmation, or otherwise, by careful experiment and close observation? One of the most thoughtful and experienced of our Indian physicians remarks, in reference to the various current opinions relative to the views and nature of the specific cause of the disease, that—

"It is sufficient to remember that they are merely hypotheses, more or less probable, which serve to suggest the direction of further inquiry, and to inculcate certain useful sanitary measures—as, scrupulous care in the disposal of excreta, and the use of disinfectants, care being taken at the same time to guard against the neglect, which special views tend to encourage, of those general sanitary measures of whose preventive power there can be no question. Above all, future investigations relative to these hypotheses, and others which have probably yet to appear, should be conducted by minds without bias, capable of grasping the entire subject, and not satisfied with moving within the limited circles of certain sets of opinions."[1]

The elaborate report of Dr. Bryden may be regarded as an earnest effort to subject the study of cholera, especially as it appears in the Bengal Presidency, to the canons of rigorous inductive inquiry, and all must admit the extraordinary industry and great ability displayed in the attempt, whatever difference of opinion there may be as to the soundness of his views on some points. His leading object is to illustrate the natural history of the pestilence as observed in Bengal, and to show that its geographical and chronological manifestations are obedient to the operation of natural agencies, and to certain laws in respect of season, weather, locality, duration of epidemic outbreaks, the intervals between the recurrence of such outbreaks, and other natural phenomena, all of which, he maintains, are discoverable by patient scrutiny. His data (contained in three copious appendices) are derived from the tables of the cholera admissions and deaths, during the forty-three years from 1826 to 1868, among our European and native armies distributed in the various stations over the country, and also those among the inmates of all the jails throughout the Presidency from 1833 to 1868. The two series of statistical tables represent an average population from year to year of upwards of 150,000, scattered over an immense area, and all under direct inspection and registration. Appendix No. 3 is especially instructive from its containing a series of shaded maps, exhibiting at a

[1] 'Sketch of Sanitary Progress in Bombay from 1830 to 1860.' By Dr. C. Morehead, in the Report of the Army Sanitary Commissioners. 1869.

glance the extent of the geographical diffusion of the disease over the face of the presidency each year from 1854 to 1868.

In chapter I he seeks to show that "the cholera of every year has a geography which is definite and can be demonstrated." With this object he gives a continuous narrative of the history of epidemic cholera in Bengal during the fifteen years from 1854 to 1869. Of this we shall now give a summary, premising that the reader should have the map of India continually before him if he would appreciate our author's views. One or two terms of frequent recurrence in his descriptions must first of all be explained. There is a region where cholera is believed to be indigenous, and of persistent existence from year to year. This is the "endemic area." Throughout the rest, or at least the greater portion of the rest of the presidency, the disease is believed to be only an occasional and temporary visitant or invader. This is his "epidemic area."

The endemic area may be roughly described as the region bounded on the east by the ninety-first or ninety-second degree of east longitude; on the west by about the eighty-fifth degree, passing a little to the west of Patna; on the north by the twenty-seventh parallel of latitude; and on the south by the coast of the Bay of Bengal, including the deltas of the Ganges and of the Mahanuddy rivers, or from about Chittagong round to Pooree. It forms a great basin, having the hill-country east of the Bhurmpooter river for its margin in that direction, and the Rajmahal and Cuttack hills for its western margin; while its northern limit is the Terai of the Himalayas, from Lower Assam on the east to the Terai of the Purneah district on the west. The climatology of this region, which is at least 300 miles in extent from east to west, and rather more from north to south, is peculiar; it is a region of perennial humidity, from the drainage of its own bounding hills, and being also the outlet of the enormous bodies of water brought down by the Ganges and Bhurmpooter. Its rainfall is double that in any other province of the Bengal Presidency, exposed as it is to the full strength of the southern monsoon. The ground moisture is always within a few feet of the surface; and vast tracts are annually under water for a considerable time, until the cessation of the rainy season and the subsidence of the swollen rivers.

"It is in the dry months that cholera is extricated as soon as the breeding grounds appear above water in October; and here it is it appears epidemic when, in February and March and April and May, the spring and summer of this (the endemic) region, the permanent moisture causes vegetation to sprout forth in luxuriant life."

That the region of Lower Bengal is notably unhealthy is shown by the fact of the death-rate among prisoners being double what it is in most of the jails in the North-West Provinces, and three times what it is in the Punjaub. The diseases by which this excessive mortality is mainly caused are cholera and alvine flux, anæmia and atrophy, dropsies, phthisis, and asthenic pneumonia.

Macnamara's endemic area differs somewhat from that of Bryden's. The former says:

"We shall probably not be far wide of the mark if we draw a line to the north-east through Saugor, Allahabad, and Gorruckpore to the foot of the Himalayas; throughout the whole of the plains to the east of this line, cholera is endemic, the intensity of the disease increasing as we approach the seaboard of the Bay of Bengal, the cities of Dacca and Calcutta being pre-eminently the stronghold of this terrible malady. Cholera is less frequently met with as we advance to the north-west and west from the above line, until the disease may with certainty be said not to be endemic in the Punjaub, Rajpootana, and Sind."

To the greater part of the rest of the presidency, within which cholera is assumed to have no perennial existence or power of indigenous development, the term of "epidemic area" is applied by Dr. Bryden. The pestilence, it is believed, is generally, if not invariably, brought to it, either directly from the endemic area, or from an adjacent locality where it has recently existed. The epidemic area is regarded by him as essentially an "invaded area."

The area of the North-West Provinces is subdivided into an "eastern" and a "western" division, for the purpose of more conveniently following the seasonal development, and the geographical course and distribution of the pestilence in different epidemic years, and of comparing these events in the corresponding epochs of successive epidemic visitations. The line of 80° of east longitude very nearly separates the one division from the other.

"In the eastern division are included the Gangetic provinces from Behar westwards, Oude, the eastern half of the Jumno-Gangetic Doab, and Bundelcund; in the western, Rohilcund, Meerut, Agra, Central India properly so called, and the Punjaub."

It is not to be imagined that anything like a distinct line of demarcation is sought to be traced between the endemic and the epidemic areas; the one is rather, as it were, shaded off into the other, so that it often seems impossible to determine where the one ceases and the other begins. Moreover, the disease is liable at times to be strongly epidemic within the endemic area, moving from one region or district to another, before it passes over into an adjoining part of the epidemic area. And now we may proceed to follow the general features of the course of the disease in the Bengal Presidency during the fourteen successive years described in detail by Dr. Bryden.

From the beginning of 1854 to June, 1856, not a single fatal case occurred among the European troops, nearly 16,000 strong, stationed in the North-West Provinces and in the Punjaub. Of the total 46 deaths from cholera among the European army in the entire presidency in 1854, 27 took place in Birmah and Pegu, and all the rest in the province of Lower Bengal.

In the native army which numbered 126,095 (not including men on furlough), 90 fatal cases in all occurred that year. The great bulk of these seem to have been at stations in the Gangetic provinces, from Cawnpore eastwards, or among troops on the march. The remarkable fact is, that the large force, amounting to 78,000 men, stationed in the Punjaub and in the Meerut, Rohilcund, and Agra districts, was nearly exempt throughout the whole year.

In 1855, the cholera history of the European army was nearly like that of the preceding year. The loss was somewhat greater; the seats of the deaths (64 in all) were much the same. And again, among the native army also the experience of 1854 was in a great measure repeated. The number of fatal cases was, however, nearly double that of the previous year; and there were now tokens, from the occurrence of single cases or small groups of cases in different localities, far remote from each other, of an approaching epidemic coming from the east. No fewer than 40 of the deaths occurred in boats on the Ganges below Cawnpore, within twenty-four hours or so after leaving that city.

The cholera history of the jail population in 1854 and 1855 quite agreed with that afforded by the troops. There were extremely few deaths among the prisoners in the western division in either year, whereas the jails in the eastern division very generally suffered both years. The total number of prisoners throughout the presidency in 1854 was 59,351; of this aggregate, 25,000 were in the western division. Only four deaths from cholera, out of a total mortality of 277, occurred in this division. In 1855 no deaths at all from cholera took place, although the number of deaths from the disease in the rest of the jails was no less than 500. Nothing could more forcibly show than this fact how free from epidemic influence the northwest provinces and the Punjaub were, while other provinces were more or less deeply suffering. Two thirds of the above 500 deaths occurred within the endemic area, and the remaining third in the eastern division of the epidemic area, or in the region lying eastward of Cawnpore.

In the latter half of 1855 cholera was epidemic throughout Bundelcund, and in the provinces to the South-East, extending to Orissa. The extreme westerly point in Rajpootana which the cholera reached that year was nearly the seventy-fourth degree of east longitude. To the south, "the Central Provinces were covered by the disease from sea to sea; and from this great sheet of cholera, occupying the southern epidemic highway, emanated the cholera of Arabia, Eastern Africa, and Europe" of that year.

It was in the following year, 1856, that the great diffusion of the morbific wave, invading from the east, took place. Nearly the entire surface of the Bengal Presidency was, so to speak, overflowed by it; the only region to which it did not reach being the extreme north-west, between Mooltan and Peshawur. "Within two months from about the end of May the whole of the exempted area of 1855, from Nagpore to the Jhelum river in the Punjaub, was covered universally, and desolated by the advancing epidemic." In March and April, the presence of cholera as a moving epidemic had been manifested in different parts of the eastern division on both sides of the Ganges; every jail in the districts along the valley of the river was then more or less affected. In April, a native regiment proceeding in boats down from Cawnpore to Allahabad was attacked, as had been the case with another regiment in the previous August, and lost 30 out of 70 men who were attacked. At the same time, the Goruckpore district of Oude suffered heavily; and three or four weeks later, the valley of Nepal, further to the north, was invaded. The Agra and adjacent districts were attacked towards the end of May; and the occurrence at this early period of single fatal cases at Delhi, Meerut, and other places in Rohilcund, were "the forerunners of the advancing epidemic." It was not till the beginning of July that the epidemic-movement further northward, north-westward, and westward, fairly commenced. In these different directions the advance of the disease seems to have occurred simultaneously. Such distant points as Deyrah at the foot of the Himalayas, north of Hurdwar, Mean-Meer near Lahore, and Ajmeer in Rajpootana, were invaded about nearly the same time, in the first week in August. Mooltan was reached before the end of the month; and Persia[1] began to suffer in September. Throughout September and October the whole of Northern India was infected.

[1] "Persia appears generally to be invaded in the same year in which cholera, as an epidemic, enters our north-west provinces. This was the case also in 1860 and 1867."

None of the jails escaped; and the mortality among the general population in the North-West Provinces was immense. Several native regiments sustained heavy losses passing down the Ganges in November and in December, a sure index of the extreme virulence of the cholera prevailing all along the course of the river. The universality of the epidemic diffusion is strikingly illustrated by the statistics of the disease in the native army and in the jail population for this year.

For 1857, the year of the outbreak of the mutiny in the native army, the registers are of course much less complete than usual. It would appear, however, that the general geographical diffusion of epidemic cholera in that year was nearly the same as in the previous one. The great sufferings of the armies in the field throughout the presidency, with the sole exception of the extreme north-west part of the Punjaub, may in part account for this. It was not till 1858 that the pestilence spread to Peshawur and Caubul. At that time it had ceased over the eastern, and the greater part of the western, division of the epidemic area.

The deadly outbreak which took place in the valley of Peshawur, for the first time for many years, occurred in October and November. With its subsidence, the invading epidemic of the last three years is believed by Dr. Bryden to have come to an end. A fresh morbific wave, issuing from the endemic area, had, however, set in previously to that date. Cholera had become epidemic in that region in 1858. In 1859 it was more decidedly "in motion," not only there, but beyond its limits, and over a considerable portion of the eastern division of the epidemic area. Without attempting to follow or to define its course, the remarkable fact must be noted, that the area invaded by the epidemic this year was almost exactly the same area which was invaded by that of 1855, and the exempted area in both years was nearly the same. So strikingly was this the case, that "the map showing the geographical distribution of epidemic cholera in 1855 may stand for the distribution of the disease in 1859."

The geography of the epidemic in 1860 is considered by Dr. Bryden to be specially instructive. In the early spring the southern provinces were covered universally by cholera, invading from the east; but it was not till midsummer that it became evident that, further to the north, a cholera wave had swept across Central India from east to west. The advance northward seems to have been cut short at about the twenty-eighth degree of latitude, along the line which formed the southern boundary of the famine tract of 1861. "The exempted cholera area of 1860 was the famine tract of 1861, almost to a mile; the area, over which cholera was epidemic in 1860, escaped famine in 1861." The failure of the monsoon over the famine area of 1861 is believed to have caused the repulse, or non-development, of the cholera invading from the east and the south. Scinde, which had not been visited by cholera for many years, was invaded this year. Nepal also seems to have been more affected than it had been for several years previously. It was not, however, until 1861 that this epidemic extended to northern and upper India, and thence on to Caubul. The limits of this extension were nearly the very same as in the epidemic of 1856; and the districts which escaped in the one year escaped in the other. The rapidity with which the epidemic diffusion took place is shown by the circumstance that the whole of the wide area occupied had been brought under its influence within a period of about three weeks. It was in August that the terrible outburst occurred at Mean-Meer, which was the subject of a special Government report.

"We are to look," says our author, "for the same geography in the cholera of 1862 as in the cholera of 1858, since the statistics of both years represent an epidemic cholera in its fourth year in the eastern division of the epidemic area, and in its third year in the western division." The disease that year was almost entirely extinct over the former division; whereas it was universally diffused over the whole of the latter, from Agra to Jhansi, on to the extreme western or north-western frontier, where it appeared in April, and persisted till the beginning of November. After this period the epidemic, which is supposed to have commenced within the endemic area in 1859, became finally extinct over Northern India.

1863 was, as respects the geographical distribution of cholera, very nearly a repetition of the years 1855 and 1859. For the third time within nine years, a cholera-wave, coming from the east and south, covered the whole of the eastern division of the epidemic area. Agra was the most westerly station reached by it. All the north and west provinces remained free, while over the eastern and southern provinces "a universal sheet of cholera was spread from Nagpore to the Himalayas," the very area which had been exempt during the previous year.

The chief interest of the history of 1864 lies in the fact that, while all Rajpootana and Upper India continued to be free, the disease diffused itself from the seat of its prevalence in Bundelcund, right across the peninsula to the Indus and the western coast, involving the central provinces, Bhopal, Malwa, Guzerat, Berar, and Khandeish. The area occupied this year was thus very much the same as that occupied in 1860.

The principal feature of the geographical distribution in 1865 was, besides its persistence in the southern provinces of the peninsula, from the endemic area right across to the coast of the Bombay Presidency—the most dreadful epidemic on record in these provinces—its extension in a north-west direction towards the Punjaub, and as far as Umballa and the mountain stations of Kussowlie and Dugshai. "The same cholera of May, 1865, was simultaneously cutting off the Mhow artillery detachment before the Simrole Ghaut in Khandeish, was striking our stations on the Simla hills and the mountains of Abyssinia, and was decimating the pilgrims of Western Arabia."

In the first half of 1866 epidemic cholera appears to have been quiescent, if not extinct, throughout both the western and eastern divisions of the presidency. It was only in July that

"The aura of a fresh invasion swept up the valley of the Ganges, and across Bundelcund to its most westerly limits. From this, the stations on the Ganges, and these alone, suffered. . . . This is now the fourth time since 1855 that we have traced cholera rising in the east from the sea level, appearing on the hills which close in the endemic area, and covering as in a mist the plateau of Hazareebagh and Chota Nagpore, with an offshoot up the valley of the Ganges simultaneous with the advance of the body of the epidemic into the Behar provinces."

Dr. Bryden points out the remarkable resemblance in the geographical career of the pestilence this year with that of the famous one in 1817, so admirably described by Jameson. In 1866 it extended as far north as the Ferozopore district near the Sutlej and the country between Meerut and the foot of the Himalayas. The disease spread also far to the west into Rajpootana. It was the knowledge of these facts that led him to predict that outbreak at Hurdwar and the neighbourhood in the following spring, which forms the principal event in the history of the pestilence in 1867. His letter to the Sanitary Commissioner of the Government on the occasion furnishes a good example of philosophical anticipation in matters of geographical epidemiology. "The invading cholera of November, 1866," he remarks, "was, I think, spread over the whole area of Rohilcund, even up to the hills; and if this was the case, we are bound to expect its reappearance from all parallel history. The cholera of the years 1783, 1852, 1857, and 1862, teaches the same lesson throughout, that a cholera of this distribution will reappear in April." And so it proved; for it was in that very month that the earliest evidences of the coming outbreak among the huge mass of pilgrims and others assembled at Hurdwar appeared, as the reader will find from the last number but one of this Journal.[1] The great extension of the epidemic

[1] Of no Indian outbreak have we so many official reports as of this one, and never were more diverse opinions expressed respecting a remarkable epidemic;

in a north-west direction over the whole of Upper India, and beyond the boundaries of Hindostan, towards Caubul, Cashmere, and Persia, while the entire area of the southern and central provinces of Bengal remained quite exempt, was remarkable. The exemption is attributed by our author to the failure of the south-west monsoon over that region.

In 1868, the area of epidemic diffusion forms a striking contrast to that of the previous year. Over Upper India and the Punjaub the disease was greatly diminished or nearly extinct, appearing only in some districts along the base of the mountains. The whole region, too, of Central India west of the Jumna, and extending from the Sutlej on the north to nearly the line of the Nerbudda on the south, was exempt, the only coloured spots on the illustrative map, marking the presence of cholera, being the towns of Meerut, Delhi, Gwalior, Cawnpore, and Lucknow. There had been a great drought and prevalence of hot westerly winds over the region which escaped. Still further south, and right across the peninsula from Calcutta to the Bombay coast, is depicted on the map a broad belt, indicating the course of the epidemic along what our author calls its great "southern highway." During the early spring, there had been not only an unusual amount of cholera in the endemic area, but distinct indications of its becoming epidemic, and being in

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Inspector-General Murray declares that "its history showed that the visitation radiated with the pilgrims from one focus in all directions from 300 to 700 miles, advancing in strict conformity to their rate of travelling, and being accelerated by the railway to Mooltan." Dr. Cunningham, the Government Sanitary Commissioner, took the same view, and Macnamara shares his opinion. Bryden holds that "the geographical distribution of the disease would not have been different had no Hurdwar gathering taken place;" and the War-office Sanitary Commissioners take exception to the evidence adduced to prove the transmission by pilgrims from station to station, as being "too incomplete to enable any deductions to be made from it." Inspector-General Beatson remarks, "Notwithstanding the very clear indications of the existence of cholera all along the Himalayan Terai, prior to the appearance of the disease at Hurdwar, Dr. Innes observes, and I think very justly, that it would be nevertheless absurd to deny that the flood of tainted pilgrims returning from Hurdwar was not also an important element in its propagation." If we come down to particulars, the like discrepancy of opinion prevails, in respect, for example, of the agency of drinking water in disseminating the malady. Dr. Cunningham favours the idea that the choleraic poison in the excreta of the early cases may have found its way into the Ganges, and thus infected the water used by the pilgrims; he seems to rest his opinion on the belief entertained by many in this country as to the chief cause of the outbreak in the east end of London in 1866. On the other hand, Dr. Sutherland, the statistical officer to the Inspector-General, says that "the drinking water, which has been stated to have been the cause of many epidemics of cholera in certain countries of Europe, cannot be said to apply to the disease as it appeared in this country in 1867." Dr. Parkes hesitates in his decision; for after declaring his belief that the Hurdwar outbreak "was a case of cholera water-poisoning on a gigantic scale," he expresses a doubt as to its real origin—"Whichever explanation of that outbreak is chosen—water poison or aerial nuisance—the fact remains that the discharges (alvine) were not rendered innocuous by earth."

movement. Dr. Bryden is of opinion that a fresh efflux of cholera miasm had streamed out from the endemic region, and become directed in a westerly and southerly course, and, probably on that very account, had been almost entirely diverted from Upper India. "The history of 1868 recalls the history of the invading cholera of 1830 and of 1854, epidemics which reached Europe along the southern highway."

Although we must wait for another twelvemonth for an official account of the disease during 1869, we have learned enough of its history to know that it was of very wide and disastrous diffusion, "extending well nigh over the whole of British India."[1] Dr. Bryden, writing in April, alludes to its then wide spread in Eastern Bengal, and also to its prevalence in the Madras and Bombay Presidencies, and closes his narrative of this part of his subject with this remarkable passage:

> "The great cholera wave in progress in the Central Provinces, and which is epidemic also over Guzerat, is the exact counterpart of the cholera of 1864, following the invasion of 1863; and we shall wait to see whether or not it is destined, at the close of this year, or in the spring of 1870, to transgress the boundaries of Hindostan, and to make its appearance in Arabia, Syria, or in Eastern Africa. The occupation of the northern highway has occurred this spring (1869); the foreruners have been thrown forward into Northern India as far as to Jullundur, Lahore, and Mooltan; and even those who know the phenomena of invasion only from personal and local experience, recognise that the invasion of the northern provinces is imminent."

The latter prediction proved, unhappily, too true. At Peshawur, among other places, there was a disastrous outbreak in September; the mortality rose from 90 to 100 deaths daily, and the surrounding villages suffered more or less severely. At Umritzir and neighbourhood the disease was very fatal, and it extended to some of the hill stations, and also along the Thibet road beyond Simla. Subsequently, it prevailed with virulence at Jellalabad in Caubul. Prior, however, to these occurrences it had appeared at Herat, and at several places in Persia, not only in the interior at Ispahan, Shiraz, &c., but also at Busheer on the Persian Gulf.

And no less noteworthy has been the fulfilment of Dr. Bryden's forecaste with respect to the advance of the epidemic along its southern line of course. The recent prevalence of the pestilence at Zanzibar and other places on the east African coast is quite in accord with his anticipations. Its coexistence on the western side of the African continent is a fact not to be overlooked

[1] 'Indian Medical Gazette' for October, 1869.

by the epidemiologist. Whether, too, the appearance of the disease in some of the southern provinces of European Russia, in the early part of the present year, will ever be shown to have any traceable connection with its prevalence in eastern Asia, is yet a mystery. For the present, we have no intelligence that it has manifested itself in any region or district between Persia and the Black Sea. But as most governments are either so indifferent or so unwilling that the whole truth about such matters be made public, too much reliance must not be placed in the mere absence of authentic information about the topographical manifestations of a disease which excites not only alarm in a kingdom, but the dread of serious consequences to freedom of intercourse. Bryden and Macnamara are of opinion that all the past European visitations have been connected with antecedent specific epidemics in India, although, from the want of sufficient authentic data, the successive steps or stages of their migratory movements from one region or country to another cannot be distinctly made out. The suggestion is one that should be steadily kept in mind in the history of future visitations.

From such a consecutive narrative as the preceding of cholera for a series of years, we see at once that its epidemic diffusion varies greatly in different seasons, quite irrespectively of the freedom or extent of the intercourse between the several affected regions or districts. It is impossible to connect the fluctuations with the mere varying conditions of human gatherings, or with the greater or less facilities of intercommunication of the sick and healthy, however much these circumstances may serve to promote the spread of the disease. There must be, at the same time, other agencies at work to account for the remarkable differences in this respect from year to year.

That in many instances outbreaks of cholera occur suddenly, and often nearly about the same time, in several localities far apart from each other, is clearly shown by Indian experience. There is, in the present day, an unwillingness to admit the proposition that it ever advances or spreads with greater rapidity than may be explained by the transit of persons from one place to another. But this opinion is certainly not in accord with what is often seen in India. The advances of a moving epidemic sometimes suggest the idea that they take place *per saltum*, or by hidden transference from one place to another at a distance, leaving the intermediate region intact, at least for a time.

"In all cases," says Bryden, "epidemic cholera advances in a series of rapid strides, alternated with intervals in which there is no apparent progress. Moving as it does in obedience to meteorological agencies, the natural barrier cannot be overstepped, and the epidemic may cover in one week (during certain seasons) the same geographical area as in a year, provided the conditions for immediate diffusion be at hand, viz., the vehicle and the directing agency."

The complete duration of individual epidemics as watched in Bengal, reckoning from the date of their upspringing within the endemic area to their complete cessation or extinction throughout the whole presidency, is believed to be generally from three to five years. When an epidemic dies out, room is afforded, so to speak, for the diffusion of another "body of cholera miasm." At times, an epidemic appears to become "reinforced" by a fresh wave of cholerific material streaming up from the endemic area, and its continuance may then seem to be prolonged beyond its ordinary or normal period, whereas, in truth, there have been during the period two epidemic invasions.

That a parallelism may often be traced between different epidemics—in respect of the season of invasion, the geographical area occupied, and other phenomena attending their course—is a point strongly insisted upon by Bryden. One epidemic has been clearly proved to be a nearly exact repetition of a former one, as in the case of the visitations in 1856 and 1861; and "the cholera of 1826 presented an exact counterpart to that of 1817." Such parallelism is something more than a fortuitous coincidence of similar phenomena; it serves to inculcate the truth that "it is in subordination to natural agencies alone that the same phenomena of relation to space and to season occur with recurring epidemics." Generally speaking, the grand parallels of any epidemic are clear; but sometimes the minor details are complex and difficult to refer back to similar events in the history of the past. This point Bryden seeks to illustrate in respect not only of the fifteen years from 1854 to 1869, but also of the thirty-seven years previously, from 1817 downwards, by analysing the evidence, imperfect as it is, which exists in the records of the Bengal Medical Board, but which has never been published, nor turned to any account until quite recently by himself and Dr. Macnamara.

"It is," says he, "in these parallels (of different epidemics) that the history of the past may be read by the light of the history of the cholera of the present time; and it is the recorded parallels which, when a system shall have been established, will be employed for the anticipation of the occurrences of our epidemic years before they actually happen."

Another point which he seeks to establish is that a marked resemblance, in various respects, may be traced between the natural history of cholera and that of malarious fevers, premising that the natural history of the latter (may we not rather say, of both?) has yet to be written. The genetic miasm of each disease is specifically distinct, but the miasms are homologous; and the ascertained attributes or properties of one may serve to throw some light upon what is less clear in the behaviour of the other. In our present very imperfect knowledge of the natural history of either disease, we must of course avoid hasty conclusions as to presumed analogies. But of this one thing we are pretty sure that, as with cholera so with marsh fever, independently of the disease being endemic or perennially existing in certain districts throughout India, there is in some years a widespread epidemic diffusion over extensive regions of the Bengal Presidency, and lasting for months at a time. In 1850, 1856, and 1860, the chief seat of this epidemic prevalence of malarious fevers was the "western division; whereas in 1859 and 1866 it was the "eastern division." Many most instructive details respecting these epidemics are given by Dr. Bryden, which the Indian medical officer will do well to study. Want of space precludes our giving any of the particulars at present. Morehead also alludes to "the occurrence of adynamic intermittent fever epidemically, attended by great mortality, and of which there have been several accounts recorded within the thirty years of this sketch." And he remarks—

"That there is no infectious property in malarious remittent fever as usually seen in India, is undoubted; but Fordyce, Clark, Lind, and others believed that malarious fevers were liable to become infectious under the favouring circumstances of crowding and filth, and possibly this was true of the adynamic remittent fever which has been now considered."

Sporadic cases occasionally occur of intense malarial fever being attended with all the characteristic symptoms of malignant cholera. Instances of this sort are mentioned by Bryden among the troops at Peshawur, Mean-Meer, Agra, and Malwa. When fatal, they were sometimes registered as deaths from cholera. The appellation of "choleroid fever" has occasionally been given to them. Other writers have described the disease as a form of "sweating sickness," analogous to the sweating sickness in Europe. Dr. Murray, the present Inspector-General of the Bengal Medical Department, thus writes of the disease as seen at Malwa in 1839:

"It resembled cholera in having copious watery stools and vomiting, occasional cramps, with feeble action of the heart, and collapse. It resembled remittent fever in being periodical, generally commencing with headache, and in having a slight hot stage, which was followed by excessively profuse perspiration of a peculiar odour. The water ran off the bodies of the men, soaking their clothes and bedding."

An outbreak of this fever took place in 1856 at Agra, when nine cases, all rapidly fatal, "with all the symptoms of the collapse of cholera," occurred in the 3rd regiment. Bryden throws out the suggestion that such cases "afford grounds for further investigation of the epidemic in the south of France in 1821, described by Rayer, as possibly the representative in Europe of our great cholera epidemic of 1817-20."

We shall now briefly advert to the chief physical agencies which appear mainly to influence the development of cholera in Bengal, and to affect its migratory movements from time to time over its entire extent.

That the weather and seasons have much to do with the development, activity, and the diffusion of cholera, has been recognised by all experienced men in that country. The subject was pointedly dwelt upon in the official report on the great epidemic of 1817-18; and it is a proof, among others, of the accuracy of that excellent document that its information is still referred to as strictly applicable to what continues to be observed at the present time. Alternations of heat and cold, combined with rain, and a very humid state of the atmosphere, then appeared to be the most frequent and unmistakeable favouring causes. In almost every instance, an epidemic outbreak was preceded by a long course of unusually moist and sultry weather; and its subsequent periods of increase and decline were always modified by changes of the weather. Great stress was also laid by Jameson upon the marked influence of an easterly wind predisposing to the disease in Lower Bengal. The fact was undeniable, but its rationale was left undetermined; the wind might be, he said, either the vehicle of the poisonous morbific matter, or it might act merely "from its superior moisture in the light of a strong exciting cause, eliciting the disorder in places where the virus had previously existed, although it were not yet brought into action." The records of the Bengal Medical Board for forty years subsequently furnish abundant evidence to the same effect, and afford, says Macnamara,

"Strong presumptive proof of the influence of rain in favouring, if not generating, cholera in India. In every single instance of an outbreak of epidemic cholera of any magnitude, if the state of the weather is attended to, it is asserted that the meteorological phenomena were similar to those described in the report of 1820."

The cholera season, or rather seasons, differ much in different parts of India. The cholera season in Upper India is not the cholera season in Lower and Eastern Bengal. The progressive advance of an epidemic occurs in one region of the presidency in March and April, in another in June, and in a third in October, November, and December. It is by attention to this fact, together with a knowledge of previous epidemics, and due regard also to the meteorology of the region from year to year, that a forecaste or prediction of coming events in particular districts or areas may often be reasonably made, as has been successfully done by Dr. Bryden on more than one occasion. He holds, as we have seen, that there is much greater uniformity, amounting sometimes nearly to a parallelism, in the career of successive epidemic invasions, and also in the resuscitation of a latent or dormant cholera in a locality, than has been generally imagined. It is to be remembered that the seasons in India follow each other with a regularity quite unknown in temperate climates. This circumstance alone must greatly aid the observer in studying the relations of weather and seasons to the history of epidemic disease. Our author maintains that a humid atmosphere is the chief vehicle in transporting epidemic cholera from one province to another. In his opinion the south-west monsoon, heavily charged as it is with moisture, plays a most important part in conveying the morbific material from the endemic area over the epidemic area, in the period from June to September. He illustrates by a diagram the course of the moist wind which streams from the sea coast over the interior, and which so frequently appears to promote the advance of an epidemic in a north-west direction from Lower Bengal along the line of the Gangetic provinces, and thence on to Upper India and the Punjaub. Macnamara admits the fact, but he explains it otherwise. He thinks that the disease is conveyed up the Ganges from Calcutta, Dacca, and other emporiums of trade, by the large fleets of native boats which leave Lower Bengal at the setting in of the south-west monsoon for the upper provinces, and do not return down the river till September. This explanation will be deemed by most as anything but satisfactory; it seems to have been forced upon him by the requirements of his doctrine about the spread of the disease being mainly due to human intercourse.

There seems to be a general concurrence of opinion that great droughts and the prevalence of hot, dry winds are antagonistic to the development and epidemic spread of the disease. Macnamara says that "in districts absolutely free from moisture, cholera can only be generated to a very limited extent;" and Bryden cites some striking examples of its repression and arrest in districts where the monsoon influences were absent, and the usual periodic rains had failed. The exempted area of 1860 was, as has been already stated, the famine tract of 1861, "almost to a mile;" and the subsequent appearance of the disease in the exempted area was coincident with the earliest rains. So close is the connection between epidemic diffusion of cholera and atmospheric humidity in Bengal, that Bryden declares that, within the circuit of the presidency,

> "Epidemic cholera is never distributed in a dry atmosphere, and that, therefore, cholera moves only at those seasons in which the atmosphere is humid; that the retention of cholera in any tract is proportioned in time to the natural degree of humidity which it possesses; and that repression of a cholera which we know to exist also occurs in a degree proportionate to the prevalence of influences which determine a dry atmosphere."

The common assertion that epidemic cholera often advances against the current of prevailing winds, is declared to be a fallacy, and contrary to fact as regards India. "The prevailing wind is the agency which directs the course of an advancing epidemic, and determines its limitations in geographical distribution." That sporadic cases or groups of cases may occur to the windward of an infected place, is not denied; but such an occurrence does not of necessity imply that these isolated manifestations are part of an epidemic invasion; they may be due to the resuscitation or "revitalisation" of the cholera poison which existed in the locality during the previous year, and which but awaited the recurrence of favouring meteorological and other physical agencies to have its activity renewed. In the somewhat obscure language of our author,

> "The phenomena brought forward in support of it (the above assertion) are, generally speaking, those of a cholera reproduced over various and possibly widely-separated portions of the same natural province, months or years subsequent to the original invasion; or they may be related to the cholera of two natural and distinct provinces, each invaded from a separate source and in a different direction, in which cholera may be in existence at the same time."

It is time that we should now notice the views of the authors, whose works we have been considering, on some important points of practical hygiene relating to epidemic cholera.

Experience in India is certainly on the whole opposed to the doctrine that human intercourse is the only, or even a principal, cause of the spread of the disease over any large area of country. Often it refuses, so to speak, to overstep the boundary of a province, notwithstanding the continued passing and repassing of persons and things from circumjacent infected localities. Bryden admits that, at times, the importation of individual cases into unaffected localities is followed by the occurrence of fresh cases there; and he of course recognises the fact that streams of sickly pilgrims, prisoner-gangs, &c., may be the source of infection in a district, and, moreover, that towns and cantonments from which such streams have been excluded, have frequently escaped, whilst others where these precautions were not used suffered. But he much doubts whether imported cases have ever proved to be, as has been often asserted, the foci from which has arisen an epidemic outbreak weeks or perhaps months after the occurrence. The mere circumstance of a disease being occasionally transmissible from a sick to a healthy person is not necessarily the whole truth respecting its usual mode of dissemination. What Copland has said about influenza, Bryden adopts in respect of epidemic cholera:

"It has been often observed that communication with those already attacked appeared to favour the development of the complaint in the healthy; for when one individual came with the disease from a distance, the inhabitants of the house in which he arrived were usually the first attacked. But it must be conceded that this infection was a very subordinate cause to that upon which the epidemic principally depended, and that it was merely a concurrent and contingent circumstance in the diffusion of the malady."

In marked contrast to this view, Macnamara holds that

"The fundamental principle upon which our preventive measures must rest is an acknowledgment of the fact that cholera extends itself by the instrumentality of persons affected by it, or through articles of clothing, or anything which has been exposed to the choleraic *dejecta*, and to which the organic infecting matter can have attached itself."

He cites with approval the conclusions of the Constantinople Conference on this subject, and he gives a copy of the cholera map in their report, in which is represented by arrow lines, with all the precision of a traveller's route, the course and progress of the pestilence from its starting-point in Egypt over the continent of Europe, and across to the New World, as if all the data had been ascertained by accurate examination, and might be accepted with confidence. Inspector-General Murray is quoted by him as sharing the same opinion.

Bryden is far from being satisfied with the soundness of many of the doctrines of the Constantinople Commissioners, and contrasts them with the results of the careful investigations of the late accomplished Dr. Baly:

"It seems to me that the modern deviations from the verdict of the College of Physicians of London on the cholera of 1848 delivered through Baly's report, have been in the wrong direction, and that the conclusions of this classical memoir will be held in esteem in all time to come, when the natural history of epidemic cholera shall have been placed on a sounder footing than at present."

Consistently with his opinions as to the part which human intercourse plays in the dissemination of cholera, Bryden holds that the segregation and isolation of the sick may at times be decidedly useful, as a prophylactic measure; at the same time, he emphatically declares that often the most perfect quarantine possible will fail in keeping out the disease.

"In all cases where we have evidence that cholera is being carried about a province by human intercourse, lives will be saved by this means which would be lost through its neglect. Quarantine, however, has never been efficient to prevent the introduction of the disease into any country; and to trust to quarantine measures as the root of a system by which the epidemic cholera of India shall be met and restricted, is to lean on a broken reed. That man knows but half the truth who, judging from some local experience, concludes that the measure will ever prove an international defence."

The exemption of a neighbouring district or country has often occurred when nothing whatever was done to interrupt the free communication with infected places close at hand. Numerous instances of this might be quoted: take one:—In 1867, cholera existed for three months in the city of Jumnoo, in the Punjaub, and with great fatality at the beginning of the outbreak. Sealkote, only twenty-four miles distant, escaped; in consequence, the authorities believed, of a strict quarantine that was kept up by the establishment of a double line of sentries. "So rigid was the cordon maintained, that I believe" (writes the Commissioner at Sealkote) "not a soul passed from the Jumnoo territory into this district, except by the Zufferwal road, and then under passes from myself." In 1862 also, cholera had been extremely virulent at Jumnoo: on that occasion Sealkote and other neighbouring villages were filled with people flying from the danger; yet, writes the Commissioner at that date, "they do not seem to have brought the disease with them, as only one isolated case of cholera occurred in the district. A good month elapsed after the dying out of the cholera in Jumnoo when, about the end of July, the season of the periodical rains, the disease, brought from a totally different direction, became epidemic." Dr. Macnamara evidently expects much more from quarantine measures than Bryden in preventing the progress of cholera on a large scale; he does not hesitate to declare that Great Britain "has it in her power, as ruling over the endemic area of the disease, to prevent her native subjects from extending this terrible pestilence beyond the confines of British India." Morehead is more cautious and less hopeful.

"Another reflection," he says, "is suggested by Indian legislation on cholera. Enactments which obstruct the landing of tainted or suspected pilgrims arriving by sea, and enforce a system of quarantine; which recognise, and no doubt justly, the infection of villages by tainted pilgrims from tainted shrines passing through them; and which at the same time enjoin the movement of troops from infected localities, and while suffering from the disease cause them to move to and fro in the surrounding districts, may fairly be regarded as chargeable with some degree of inconsistency. It may be that advancing knowledge will prove the wisdom and necessity of a quarantine; but past and present knowledge already prove that it ought to be, in extent, organisation, and humanity, a system very different from anything that has hitherto existed in European or other countries."

The excretal theory of the rise and spread of cholera, which has been so much in vogue of recent years, is viewed very differently by our authors. Bryden does not accept the doctrine that the cholerific germs exist mainly in the alvine excretions of the sick, and that the multiplication and the propagation of the disease are best explained by the assumed dissemination of its germinal or material cause in this way.

"I have anxiously sought for evidence of the highly poisonous character of cholera evacuations, and I think I have done so with an unprejudiced mind. I do not go so far as to say that the evidence is against the presence of cholera germs in the evacuations. On the contrary, I think it highly probable that latrines are occasionally infected, especially hospital latrines."

He believes too that *fomites* are, at times, capable of retaining the morbific poison for a limited time. In marked contrast to Bryden's views, Macnamara adopts the excretal doctrine as an axiom, while he allows that the results of his own experiments with cholera *dejecta* on white mice do not accord with those of Thiersch and Sanderson, on which the hypothesis, according to many, mainly rests for proof. Nevertheless, he is so convinced that the *materies morbi* exists in the alvine evacuations of the sick,[1] that he proceeds to point out in detail how their poisonous nature may be most effectually counteracted. "Any substance which will render the alvine flux acid, destroys its specific powers, at any rate for the time being." And so highly does he think of the sulphate of iron as an antidote, that he maintains

"Not from conjecture, but observed facts, that cholera dejecta treated with it are harmless, and may be swallowed with impunity,

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[1] Dr. Murray, whose official report on the treatment of cholera is noticed in our present number, not only holds the same opinion, but also that "the evidence is strong that the poison is contained in the secretions of the liver, kidneys, skin, and lungs." He does not, however, mention where the evidence is to be found.

and probably digested and assimilated, as any other equal quantity of iron and organic matter would be."

It is by water alone that the poison is disseminated; "an epidemic outburst of the disease can only occur through the drinking water of the place becoming contaminated with cholera matter. If we can only preserve the drinking water from contamination, it is out of the question that cholera should become epidemic in either country or town." The case at Mean-Meer in 1861, which he regards as affording so strong evidence of the direct action of cholera-tainted water, and is narrated at p. 196, will not be regarded by most as nearly so conclusive as he seems to think it; the details are obviously incomplete, and are uncertified. Bryden is far from being a convert to the water-propagation theory, and Morehead points out the extremely inconclusive evidence on which it is based: "To regard it as an established truth is," he remarks, "to warp the entire field of ætiological inquiry, and sanitary practice in this disease." Pettenkofer's views are not accepted by any of the writers. Indian observation has hitherto given no countenance to the speculations of the Munich professor.

With respect to the important question of removing troops from their cantonments and camping them out on the first appearance of cholera among them, much yet remains to be known before we can speak decidedly as to the propriety of the measure on all occasions. The results have proved several times anything but satisfactory; at one time, cholera continuing its ravages, and at another, diseases due to exposure causing a heavy loss of life which possibly might have been avoided.

"Those who would point to the diminished mortality from 1863 to 1866," Bryden remarks, "as indicative of the efficacy of movement into camp as a remedy against invading cholera, have not given due weight to the fact that, as far as regards the stations most liable to the attack of invading cholera, the years from 1863 to 1866 constituted virtually, if not theoretically, a true interval of exemption between the epidemic of 1860-62 and that of 1866-67. And on the reinvasion of 1867, the results of movement into camp disappointed the anticipation which the proposers of the method had formed, so far as to demand from the commander-in-chief an explanation of why it is that so little success has attended a step from which so much good was looked for."

The whole of the circumstances which are to guide the medical officer in recommending the adoption of the step, and all the precautions necessary to be taken in carrying it into effect when deemed advisable, require to be much more clearly determined than experience hitherto has enabled us to do. Sanitary purity is, of course, of high importance everywhere, and so is the supply of good water;

but these measures are not sufficient, and many other hygienic considerations have to be attentively weighed. There is one point in connection with this subject that deserves special notice, viz. that the attack and death-rates from cholera are ordinarily much less among our native than among the European troops. A deadly epidemic outbreak is a rare event in a native cantonment; usually, the outbreaks are generally but the shadow of what too frequently occurs in British regiments in the same district. It is not to be supposed that native cantonments escape being affected when an epidemic is abroad; only, fewer persons are attacked, and the disease is ordinarily much less deadly. The risk of attack has been estimated as ten to one, and the chance of death as thirteen to one, in favour of the native soldier. The most fatal outbreaks among native troops have occurred on the march, or when the men have been crowded together in country boats, in moving from one station to another. On these occasions, they have sometimes suffered nearly as badly as Europeans. These exceptional occurrences are significant. As to the cause or causes of the marked difference usually between the two classes of troops, medical officers are by no means agreed. The sanitary conditions of a native cantonment are not superior to those of the British troops. The sepoy's hut is small, badly ventilated, and not raised above the ground; his water supply is from the nearest tank, and his duties at night (when most attacks occur) are more onerous, especially during an epidemic outbreak. Probably, the greater subdivision and dispersion of the men in their separate dwellings has not a little to do with their lesser liability to attack under such circumstances; while the difference of temperament, diet, and moral and social peculiarities may modify the virulence of the poison when received into the system. Whatever be the principal cause of the difference in question, there is sound practical wisdom in the following remarks cited by Bryden, and which, on the whole, are much more applicable to the European than to the native soldier:

"Let any medical man cast up in his memory the fatal cases of cholera which have occurred to him among all classes of patients, and he will have it at once impressed upon him how many fell victims to a temporary lowering of their vital powers. In one case, it was the depression following the use of stimulants to excess that was the exciting cause; in another, the fatigue of a journey, or of a shooting excursion; in a third, exposure to wet and cold; in a fourth, confinement and watching by a sick bed; in a fifth, mental depression from grief or fear of the disease itself; in a sixth (and this is perhaps more frequent than any other cause), temporary weakness caused by slight diarrhœa, the result of indiscretion in diet."

It has been sometimes necessary to replace European orderlies by native soldiers on a hospital on the occasion of a disastrous outbreak, in consequence of the former suffering severely. This was the case at Mean-Meer in 1861. Mental depression seemed to have much to do with the occurrence; for not a single one of the natives was attacked, and the medical officers too remained exempt. As a general rule, the sick should not be attended in hospital by their comrades; strangers should always be selected for the duty, especially in times of alarm. A mental shock is often the immediate antecedent of an attack of cholera.

On the general question of the liability of attendants in hospitals to be attacked, the experience of Indian hospitals, and of the great Calcutta hospitals in particular, during the last fifty years, seems to lead to the conclusion that very little risk of infection from the sick is incurred by such attendance. "It is the rarity of the occurrence, and not its frequency, that forces itself upon our notice." Hospital infection, it should always be remembered, is sometimes merely indicative of the infection of a locality, and nothing more; and the sickening of one or more of the attendants does not necessarily prove the communication of the disease from man to man. Experience in India appears to give but little support to the popular idea that washerwomen are much more liable to be attacked than other persons in the same condition of life. Macnamara takes it for granted, but without alluding to the actual results of experience around him in Calcutta.

The preceding pages will have shown how extremely uncertain and imperfect our real knowledge of epidemic cholera still remains, in spite of all that has been done during the last half century. That some change, therefore, in our method of investigation is needed can be disputed by no one. The main desideratum is obviously a more ample and more accurate record of authentic facts without regard to any theory, systematically and continuously pursued. The scheme for this purpose prepared by the War Office Sanitary Commission (to which allusion has been made in a previous number) promises, we think, to be most useful. If efficiently worked out by the medical officers of the two public services, it must lead to many excellent results, not only in respect of cholera but of epidemiology generally. When it is seen how much has been done by the resolute energy of one of their number in opening up a rich field of scientific research, they will be stimulated to enter upon it with zeal to assist in its cultivation. At the present time we seem to be on the threshold of some interesting discoveries. The exact and diligent study of the geographical and chronological relations of an epidemic disease like cholera is essential to our knowledge of its habitudes, and it augurs well that these very subjects have of late been attracting much more attention than formerly. The patient inquiries, over a number of years, of such men as Bryden and Inspector-General Lawson have recently done much good in this direction. It might be easy to point out objections to some of their propositions, and to take exception to several of their conclusions; but enough will remain unchallenged in their labours to excite the admiration of all who have sympathy with earnest workers after truth. Macnamara's volume may be read with profit in regard to its historical details, for it incorporates much valuable original matter, derived from the MS. Proceedings of the Bengal Medical Board, touching the career of cholera in India since 1820. From Dr. Cunningham's report, which only want of space prevents our noticing at greater length, we learn that his next report, that for 1870, will embrace much new matter and ampler details about the health of the troops and prison population throughout the presidency. He anticipates, moreover, that, ere long, reliable information will be at his command respecting the diseases prevalent among the general civil inhabitants of the country. This indeed will be a great gain in the interests alike of science and of civilisation and humanity. Dr. Cunningham pays a graceful compliment to the high value of the services of Dr. Bryden, who is now his colleague in the sanitary department with the Government of India.

IX.—Hospital Statistical Reports.[1]

These are statistical works, at least, in their first conception, however overmastered by the tendency to sink into clinical report. The first on the list is instructive and even engaging to those who love compression in detail, and prefer unsystematised results to more dogmatic teaching. It is open, however, to the objection that the attention is far more arrested by particular

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[1] 1. *St. Thomas's Hospital Statistical Report. A Report of Cases treated in St. Thomas's Hospital from* A.D. 1861 *to* A.D. 1865. London, 1869, pp. 156.

2. *Bericht der k. k. Krankenanstalt Rudolph-Stiftung im Wien Jahre* 1868.

Report on the Hospital Rudolph-Stiftung in Vienna for the year 1868. Vienna, 1869, pp. 503.

3. *Rapport sur la Statistisque des Hopitaux S. José, S. Lazaro et Desterro de Lisbonne, pour l'année* 1865, *suivant le plan du Dr. F. da Costa Alvarenga.* Traduit du Portugais par Dr. Lucien Papillard. Lisbon, 1869, pp. 188.

4. *Congrès Médical International de Paris. Note Statistique des grandes Operations faites à l'Hôpital N. et R. de Saint Joseph pendant les douze derniéres années.* Par M. A. Barbosa, &c. &c. Paris, 1868, pp. 38.

cases recorded, than attracted to general results, and as a statistical summary it must rank very low indeed. When we would draw from it any valuable conclusion, we find ourselves foiled every way; nothing is done to render us assistance, and the endeavour to help ourselves becomes intolerably wearisome and disappointing. There is little or no uniformity in the work, which may be regarded simply as a running commentary on the routine service of the hospital, as carried on from year to year. This so-called statistical report is, indeed, little else than five year-books of some thirty pages each, with small endeavour at collation of their contents, held together within a single wrapper. In the preface it is thus accounted for:—"The chief difficulty of compilation has been the very different amounts of labour devoted to the work, and the variable method adopted by successive registrars." This default can have no other cause than the low appreciation of the value of statistical results which now governs the action of hospital authorities, and which might be fairly put to shame by a comparison of statistical work performed abroad, with all of the like this country has to show, apart from individual inquiry. We are far from asserting that the work before us is not worth the trouble and expense bestowed on it; such is not at all our idea, still less would we detract from the merit of the gentlemen who have laboured at this somewhat ungrateful task. The tabulated results of Mr. Croft for the years 1861, 2, 3, appear to us, as far as they go, very good. It will never be singly, but out of a mass, and by consent, that statistical labour will repay the great expense of labour that is required for it. What each liberally contributes to the general stock comes back tardily and inappreciably as general information. The vast field which London affords for the cultivation of medical statistics, is marred by segregation and exclusiveness in single centres, each section addressing itself to a little public and patrons of its own. Add to this that the size of our hospital institutions cannot compare with those of the Continent, and they are only important collectively. Now that the College of Physicians, under Government sanction, have put forth their nosology, a basis will certainly be acknowledged for a more general agreement, to which it will be well to adhere; for, however defective a plan may be, it will certainly be better than none, and nothing is more calculated to lead to a salutary reform in nosology than experience in the tabulation of hospital work. More seriousness and responsibility in registration can only come from bestowing precedence and remuneration on the hospital registrar; and it will be well if, in these days of pluralism, he can confine himself for successive years to this important duty without a personal sacrifice; the registrar, in many points of difficulty, would be a most invaluable referee :—and by comparisons drawn from foreign data as well as from his home experience, he might counsel and appraise, as the actuary of an insurance office, in many a crisis and divergence of ideas. As it is, we commit to volunteer effort the performance of a scheme that allows not the smallest break, nor any casual *laches.* Let every metropolitan hospital have its defects and its advantages pointed out, its characteristics nicely defined; nay, more, let every ward receive its good or evil report, and have its weak points or excellencies, placed on record. No less than this has been accomplished in other countries. In England, as yet, however, medical statistics are not much relied upon; this is the more singular when we see the avidity with which statistics from this side of the water are quoted by our foreign colleagues; in fact, they are really more valued and more accredited abroad than at home. St. Thomas' Hospital, as is well known, has been latterly under reconstruction; transported to another site, it is now being rebuilt on an ampler scale. On January 1st, 1862, the number of patients in hospital was 452; the admissions during the year were 2330, some 1500 less than the year previous: the rate of mortality per cent. was on the medical side 11·38; on the surgical, untold. The bareness of generalisation displayed contrasts most unfavorably with that which is seen in the second publication on our list. The Austrians, indeed, have always been famous for office work, and are accused of pushing it too far; but here it is quite in place and well found. We have presented to us a work of 400 pages for the information of the minister of the interior, but forming also an instructive volume for the medical library. At the end of the year 1867, there remained under treatment in the *Rudolph-Stiftung,* 423 men, 285 women = 708. In 1868 were admitted 4921 men, 2605 women = 7526. Hence there came under treatment 5344 men, 2890 women = 8234. This number we divide as follows: the cured are 3670 men, 1092 women = 5562; the partially relieved or alleviated, 719 men, 339 women = 1058; not cured, 120 men, 90 women = 210; died, 438 men, 286 women = 724 deaths; at the end of the year remained, 397 men, 283 women. Of the 210 unrelieved, 110 passed into local refuges of different communes.

The rate of mortality was generally 9·5, without distinction of sex. In the men 8·8, in the women 10·9 per cent. With exclusion of the tuberculous, 6·6 without distinction of sex, *i. e.* 5·7 in the men, 8·2 in the women. In comparison with the former year, the numbers under treatment were less by 521, viz. 322 men, 189 women. The same diminution was the case as regards the number discharged by 520, of whom 309 men, 211 women. The number of deaths had increased by 27, of which 3 were men, 24 women, so that the death rate had increased 0·5 for the men, 1·4 for the women, and 0·9 without distinction of sex. A perfect and elaborate table follows, showing the variations for every month in the year, both of the prevalence of disease as denoted by the number in hospital and the mortality, with an average struck, and telling also the proportionate relation in the sexes. The maximum of patients in hospital was for the men, in the month of March, 472; for the women, 333, in the month of January. The minimum was for the men, in August, 275; for the women, in July, 226. Thus the difference between the highest and lowest health conditions was 197 in the men, 107 in the women.

In relation to the greatest number of admissions, the months rank in the following descending series, without distinction of sex:—January, March, February, July, May, August, April, November, December, June, October, September. For the men, March, January, November, February, December, April, May, July, August, October, June, September. For the women, January, March, July, August, June, May, April, February, December, November, October, September. Then follows a table of the per-centage of cures, arranged according to the months, with or without tuberculosis; with average, and with and without distinction of sex. The most favorable per-centage of cure (without tuberculosis) was 84·7 for the men in January, 82·8 for the women in August; 82·4 without distinction of sex in January. In respect to the per-centage of cure, the months succeed in the following descending series:—January, September, March, December, April, May, October, November, August, February, July, June. Next we have a table of the percentage of deaths, according to the months, with the same exactness as before; it shows a greater mortality among the women than the men, both with and without tuberculosis. In the month of July this excess even amounted to 7·4 per cent. The share which tuberculosis takes in the general mortality was in the following descending series:—May, 5·3; April, 4·2; November, 4·2; March, 4; February, 3·4; June, July, August, 3·1; January, October, 2·6; September, December, 2·4 per cent.

In the death-rate per cent. without tuberculosis the months follow in this descending series:—for the men, April, March, October, July, January, February, May, September, August, July, December, November. With the women—July, January, April, November, May, March, June, October, February, September, August, December. Without distinction of sex—April, March, October, January, June, July, May, February, November, September, August, December.

The following is a good table:

Variations in the Admissions of Important Diseases.

Diseases.	In the Month—											
	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.
Rheumatism . .	19	25	27	19	20	23	34	14	19	19	37	29
Typhus . . .	33	50	76	39	28	19	19	16	13	13	5	6
Periodical fever .	9	2	1	8	9	12	24	22	44	31	20	13
Tuberculosis . .	43	56	56	44	63	36	32	48	37	34	32	36
Catarrh of the lungs	63	30	37	29	30	11	8	11	10	10	14	24
Inflammation of the lungs . . .	16	14	21	23	14	5	3	12	6	10	15	12
Catarrh of the organs of digestion . .	32	23	22	26	25	21	31	16	17	23	12	24
Smallpox . .	21	10	18	31	21	15	15	21	11	14	20	34

A special nosology is printed at the end of the work. The surgical part is very weak, and it is without meteorological data. We are pleased at seeing abdominal typhus separated from the exanthematous in the clinical and nosological part, and in the same degree perplexed at finding them confused in another part; but if we suffered ourselves to linger over the work, we should find much in the clinical portion worthy of attention.

Alvarenga's book, which is third upon our list, is no new thing, and his reputation is well established as a leading advocate of statistical science, as applied on a large scale to medicine. It may fairly be recommended to those who would follow out this mode of inquiry, and, although of no recent date, the English student will find a good deal in it he may lay to profit. There he will meet with both rule and precept, and it is not unlikely that his conception of the requirements for able registration will be considerably raised.

The next work teaches by example. The hospital S. José at Lisbon affords a fine field for statistics; it is truly a noble institution, containing 845 beds, without enumerating the smaller rooms. Originally built by the Jesuits as a seminary for their order, they adopted such a scale of construction as makes it quite an exception to the general rule that no class of building intended originally for another purpose, is fairly convertible into a hospital for the sick. It is situated on a hill, with plenty of air and light, and the mildness of the climate admits of the windows being open during a great portion of the year, at least in daytime, and to a very large extent. It has plenty of internal space, a superb entrance, wide staircase and corridors. There is a planted inner court and a farm attached to it, and though in a crowded part of the city, the hospital is nowhere hemmed in with buildings; it is also very cleanly, with an excellent administration; the rooms are nowhere damp, and only in certain parts are there any bad smells. Of late, indeed, puerperal fever has been a total stranger to the obstetric wards. There is little wonder, then, that the statistics of surgical practice which Sr. Barbosa has adduced, are more encouraging than those of foreign hospitals generally; and, indeed, in many respects they compare very advantageously with the best results attained in England. This is very well shown in the amputations of the thigh performed by him during the last two years; of these he has only lost one case out of fourteen. Rather singularly, with him secondary amputations of the thigh have proved less successful than primary; this peculiarity may depend in part on climate, but it also seems owing to the less intensity of the shock compared to what is found in this country, where machinery is the common cause of accident. The circular amputations are preferred by Barbosa, retracting the skin without detaching it, and making a conical section of the muscles; this he greatly prefers to the method formerly in use, the *manchette de Brunninghausen,* which always appeared to him very faulty, from the large suppurating surface to which it gave occasion, and the vast circular furrow in which pus collected, infecting the wound. Great care was always taken after amputation to staunch the blood, and to apply firm bandages to the stump. He draws particular attention to a speciality of Lisbon practice, derived from old Portuguese surgery, but adopted of late from Coimbra; this consists in the application of compresses to the wound, soaked in a saturated solution of camphor in alcohol. This suggestion is quite independent of the use of alcohol to recent wounds, as introduced by Nelaton. Barbosa supports his patient after operations with wine as well as animal food, nor is he an advocate of low diet at any time.

In a total of 243 amputations of all sorts performed by Sr. Barbosa, the loss by the 'major' was 36·33 per cent., by the 'minor' 3·37 per cent. In 62 of the thigh the mortality was 46·8 per cent.; of 50 cases of the leg the mortality was 33 per cent. Of the arm, 15 cases, mortality 33 per cent.; forearm, 24 cases, mortality 20·8 per cent. The difference between the mortality from amputation of superior and inferior extremities:—for the major operations of the former, 25 per cent., of the latter 40·35 per cent.; minor amputations of the former 1·37, of the latter 12·5. The mortality after amputation from 61 to 70 years of age was found to be small, in accordance with the results of

Malgaigne, being not more than 9·09 per cent; in other respects mortality after amputation increased with age.

We should not omit notice of some more notable operations performed by Barbosa, who is in the vigour of life, and undoubtedly the most eminent surgeon of Lisbon; these are three disarticulations of the knee, all three of which recovered; two at the hip, fatal; three cases of resection of the superior maxilla, with one death; a successful case of ligature of the subclavian, for aneurism of the axillary artery; two cases of ligature of the common carotid for aneurism, one successful the other fatal; nine cases of ligature of the femoral, seven successful, two deaths from gangrene. We have only to compare these results with others to recognise their favorable character. The season of greatest mortality after amputations was the spring, 32·2 per cent.; next, winter, 25·42 per cent.; the summer, 21·33 per cent.; and lastly, autumn, 18 per cent. The greatest mortality was in the month of May, 42·86; the least in October, 5·56. The ancients for their operations preferred the autumn and spring, and this prejudice or opinion was maintained by Dupuytren and Velpeau; but it has been completely invalidated by the statistics of Malgaigne, of Fenwick, and John Walt, which seem further supported by those of Barbosa.

X.—Flint's and Liégeois' Physiology.[1]

THE works before us have so little in common that, with one exception, we must necessarily, in the following pages, consider them separately.

We have already in these pages expressed our opinion of the former volumes of Dr. Flint's 'Physiology,' and are happy to find that the present part is, if anything, superior to its predecessors.

It is occupied with an exposition of the various subjects of secretion, excretion, ductless glands, nutrition, animal heat, movements, voice, and speech; and, as the author remarks in the preface, the only great divisions that now remain for him to treat of are those of the functions of the nervous system, and of

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[1] 1. *The Physiology of Man.* Designed to represent the existing state of Physiological Science as applied to the functions of the human body. By AUSTIN FLINT, Jun., M.D., Professor of Physiology and Microscopy in the Bellevue Hospital Medical College, &c. &c. New York. 1870.

2. *Traité de Physiologie appliquée à la Médecine et à la Chirurgie.* Par TH. LIÉGEOIS, Professeur Agrégé à la Faculté de Médecine, &c. &c. Avec 101 figures. Paris. 1869.

A Treatise on Physiology, with its application to Medicine and Surgery. By TH. LIÉGEOIS.

the processes of generation and development; and he goes on to say, that

"Some of the subjects taken up in this volume have an especial interest to him, from the fact that he has investigated them by original experiments, and has succeeded in developing new facts of a certain degree of value; but it has been his endeavour not to give to these questions undue prominence, to the prejudice of other subjects of equal importance to the physiological student. The most prominent points developed by original investigation in the present volume are, the discovery of an excretory function of the liver, that had never before been described, and the mechanism of glycogenesis, a question that seems now to be definitely settled, notwithstanding the apparently opposite results obtained by different experimenters."

The first chapter, on Secretion, opens with a general view of the relations the secretions bear to nutrition, and a comparison of the secretions with the excretions, the distinction between the two being that the secretions are formed by special organs, and have important functions to perform, which do not involve their discharge from the organism, a typical representative being found in the digestive fluids; the excretions, on the other hand, contain certain characteristic principles which are formed in the tissues, as one of the results of the constant nutritive changes going on in all organized living structures, and which have no function to perform in the animal economy, but are simply separated from the blood to be discharged from the body.

Whilst the functional importance of secretions, and the absence of such value in the excretions, is generally recognised in physiological works, the distinction between the two in point of origin is not often so clearly worked out. But there are some fluids, as the bile, which perform important functions as secretions, and which, nevertheless, contain certain excrementitious matters. In these instances, Dr. Flint remarks, it is only the excrementitious matters that are discharged from the organism; and in a subsequent paragraph he says that, with regard to the origin of the principles peculiar to the true secretions, it is impossible to entertain any other view than that they are produced in the epithelial structures of the glands; and the old idea that they exist ready formed in the blood, though adopted by some physiologists (Milne-Edwards), cannot be maintained.

In speaking of the serous membranes, we are somewhat surprised to meet with the following passage:—

"The normal quantity of pericardial fluid in the human subject is generally estimated at from one to two fluid drachms. Colin found that the pericardial sac of the horse contained from two and a half to three and a half fluid ounces, the cavity being exposed immediately after the death of the animal from hæmorrhage. The quantity of fluid in the peritoneal cavity in horses killed in this way was from ten to thirty-four fluid ounces; the quantity of fluid in the pleural cavity of the same animal was from three and a half to seven fluid ounces. These estimates are simply approximative, but they give an idea of the normal quantity of liquid which may reasonably be supposed to exist in the serous cavities of the human subject. Judging from the weight of a man of ordinary size, as compared with that of the horse, it may be stated in general terms that the pericardial sac contains from two and a half to three and a half fluid drachms; the peritoneal cavity from one to four fluid ounces; and the pleural sac from three and a half to seven fluid drachms."

Now we very much doubt the accuracy of these deductions. We do not, indeed, doubt the results obtained by so careful and trustworthy an experimenter as M. Colin, but we cannot think that the quantities of fluid found in the several serous sacs after death by hæmorrhage, represents at all correctly the quantity present in health during life. We willingly acknowledge that the increased tenuity and dilution of the blood flowing from a vein in an animal dying from hæmorrhage, is essentially due to the absorption of watery fluid or lymph from the meshes of the connective tissue generally, and that therefore, at first sight, it would rather appear that death by hæmorrhage would cause a diminution by absorption of the fluid (if any) present during life in the serous sacs; but it must be remembered that the watery blood in the dependent parts of a dying animal, when it is more or less at rest, would readily transude though the vessels, and that this transudation would be promoted by the shrinking which all the organs contained in such serous sacs would undergo in death by hæmorrhage; for by such shrinking a tendency to a vacuum would be occasioned that would be resisted for a time, and within certain limits, by the discharge of watery fluid from the vessels. At all events, we can only say, in various experiments we have made on large dogs, we have never seen any quantity of liquid in either the pericardium or peritoneum; and though we have not made any to determine this point in particular, we scarcely think we can have overlooked so prominent a phenomenon. We are the more desirous to call attention to these, as we think, erroneous statements and deductions, on the ground that, if accepted, they would have an important bearing on pathology.

In the description of the secretion of mucus, we do not meet with any account of the cup, chalice or goblet cells so fully investigated by Eimer and others, and which many think produce the mucus, but are merely told that, according to Robin, mucus proper is produced by the epithelial cells of that portion of the membrane situated on the surface, between the openings of the so-called mucous follicles or glands. The discussion on this point we think was deserving of mention.

The third chapter treats of the mammary secretion, and gives, on the whole, a very full and satisfactory account of this important process of lactation; though perhaps scarcely enough stress is laid on the intimate connection that exists between uterine activity, however induced, and the development of the mammæ; nor is any attempt made to explain that relation. Yet there seems good reason for believing that it is of the nature of a reflex action, and that the excitation of the uterine nerve-fibres, whether occasioned by the presence of a child, or of a polypus in, or other disease of the uterus, leads to dilatation of the mammary vessels, and the consequent structural development and functional activity of the gland.

During the last year there have been one or two cases in which an interesting medico-legal question has arisen as to whether it is possible for a mother, taking strychnia medicinally, to poison her child through the milk, without herself experiencing any injurious results; and we turned to see whether, though not, perhaps, a purely physiological question, Dr. Flint had any remarks on the subject. He only says, however, "that many medicinal articles administered to the mother pass unchanged into the mammary secretion, and therapeutists have sometimes attempted to produce the peculiar effects of certain remedies in this way on the child." He then goes on to say that the glands appear to be capable of exerting a certain elective power, and that the articles which pass readily into the milk are, amongst others, the salts of soda, sesquioxide of iron, and the preparations of iodine.

On turning to the work of Liégeois, in which the subject of lactation alone constitutes common ground with those contained in the present volume of Dr. Flint's work, we find that although strychnia is not alluded to, the list of substances ascertained to pass into the milk, and the authorities, are very well given, and we subjoin them.

Antimony (Lewald).
Arsenic (Hertwig and Labourdette).
Subnitrate of bismuth (Chevalier O. Henri and Lewald).
Borax (Harnier).
Iron (Rombeau and Roseleur).
Iodine and Iodides (Lewald, Péligot).
Lead (Lewald).
Chloride of sodium (Péligot).
Oxide of zinc (Chevalier O. Henri, Lewald, Harnier).
Sulphate of quinine (Londerer).
Mercury (Lewald, Labourdette, Bouyer).

Respecting the presence of the last-mentioned substance there seems to be considerable doubt, as many eminent observers have failed in detecting its presence, even when large doses of corrosive sublimate and other salts of the metal have been administered. It would appear, therefore, to be a perfect waste of time to treat infants suffering from syphilis in this roundabout way, notwithstanding the high authorities by whom it has been recommended. Besides the above, the existence of various odorous, sapid, colouring, and purgative principles have been satisfactorily demonstrated to be eliminated from the system by and with the milk.

The chapter on the minute anatomy of the skin, in Dr. Flint's work, is chiefly compiled from the descriptions given by Sappey and Kölliker. He refers to the remarkable cases on record of sudden blanching of the hair, and adduces several cases in which this was undoubtedly observed by competent observers. One of these we do not remember to have seen noticed, and is as follows. It occurred under the care of Dr. Landois, and was seen also by Dr. Lohmer.

"The patient was a compositor, thirty-four years of age, with light hair and blue eyes, and was admitted into the hospital apparently suffering from an acute attack of delirium tremens. A marked peculiarity in the disease was excessive terror when any person approached the patient. He slept for twelve hours on the night of the 11th of July, after taking thirty drops of laudanum. Up to this time nothing unusual had been observed in regard to the hair. On the morning of July 12th, it was evident to the medical attendants, and all who saw the patient, that the hair of the head and beard had become grey. This fact was also remarked by the friends who visited the patient, and he himself called for a mirror, and remarked the change with intense astonishment. The patient continued in the hospital until September 7th, when he was discharged, the hair remaining grey."

An interesting point connected with this case is that the hairs were submitted to careful microscopical examination, and those that were white were found to contain a great number of air globules in the medulla and in the cortical substance, but the pigment was everywhere preserved.

Chapters v, vi, and vii, give a very full account of the structure of the kidneys, and of the chemical composition and physiological relations of the urine, of which we can here only give a short abstract. In regard to the structure Dr. Flint accepts the views of Henle as modified by Schweigger-Seidel, Gross, and others, showing that the tubuli uriniferi, before terminating in the cortical portion of the kidney, form long loops which ascend the pyramids to a variable height, and then bend back to end in the capsules of Malpighi, the primary tube and the looped portion being of very different diameter, and the latter being lined by an epithelium which presents marked points of difference from that of the former, a difference that led Henle, in his original memoir, to regard them as totally distinct systems of vessels. Dr. Flint holds the views of Dr. Isaacs in respect to the epithelial layers contained in the Malpighian body, recognising, therefore, two varieties, differing in size, form, and situation, one variety lining the capsule and the other covering the vascular tufts; the former are small, transparent, ovoid, nucleated, and finely granular, whilst the latter are larger and more opaque.

The section on the mechanism of the formation and discharge of urine, containing a summary of the views now generally entertained in relation to the origin of the excrementitious constituents and the results of experiments on the effects of ablation of the kidneys, of ligaturing the ureters, and upon the influence of variations of the blood pressure, nervous system, etc., seems to be well drawn up and to give the student a fair and accurate digest of the present state of physiological knowledge on those points.

Chapters viii, ix, and x, are occupied successively with the physiological anatomy of the liver, the excretory function of the liver, and the production of sugar in the liver. In the description of the minute anatomy of the liver that given by Kiernan is tolerably closely adhered to, except in relation to the ultimate ramifications of the bile ducts, respecting which so much has been observed and written during the last ten years. He considers the following to be probably the true relations of these parts:

"In the substance of the lobules is an exceedingly fine and regular network of vessels, of uniform size, about $\frac{1}{10000}$ of an inch in diameter, which surrounds the liver cells, each cell lying in a space bounded by inosculatory branches of these canals. This plexus is entirely independent of the blood-vessels, and it seems to enclose in its meshes each individual cell, extending from the periphery of the lobule, where it is in communication with the inter-lobular bile ducts, to the intralobular vein in the centre. The vessels probably have excessively thin homogeneous walls, though the existence of a membrane has not been positively demonstrated, and are without any epithelial lining, being much smaller indeed than any epithelial cells with which we are acquainted. This arrangement, as far as is known, has no analogue in any other secreting organ."

Dr. Dalton states that a peculiarly favorable opportunity for observing the bile duct in the lobules was presented in the livers of animals that died of the so-called "Texas cattle disease." This was taken advantage of by Dr. R. C. Stiles, who was able to verify in the most satisfactory manner the facts which have been lately established by the German anatomists, and in some instances the presence of what appeared to be detached fragments of these little canals constitutes an argument in favour of the view that they are lined by a membrane of excessive tenuity.

Dr. Flint entertains peculiar views of the function of the bile, and advances the proposition that it contains two classes of constituents, one of which, consisting of secreted material, is reabsorbed, whilst the other is an excretory material and is discharged in a modified form in the fæces. The former class consists essentially of the glycocholate and taurocholate of soda, the latter of cholesterine. In human bile very little of the glycocholate is present, and there seems to be little doubt that the taurocholate is generated in the cells of the liver at the expense of some of the constituents of the blood, since in experiments made by Müller, Kunde, Lehmann, and Moleschott on frogs, in which the liver was removed and the animal survived for some days, it was found to be impossible to show that the proportion of biliary salts which had accumulated in the blood bore any relation to the length of time during which life had been prolonged. Dr. Flint, however, does not hazard any conjecture either respecting the material from which the taurocholic acid is produced, nor the substances into which it is decomposed during its transit through the intestine. In regard to the cholesterine, to which Dr. Flint has paid particular attention, its proportion in the bile is generally stated to amount to from 1·60 to 2·66 per thousand, but in a single examination that he made of human bile the proportion was 0·618 of a part per thousand. It constitutes the only excrementitious principle as yet discovered in the bile, and in his opinion bears the same relation to this fluid that urea does to urine. It is a true excreted substance pre-existing in the blood, which probably derives it from the substance of the brain and nerves, in which it exists in large quantities. It might, however, be said that it was deposited in the nervous system from the blood. With a view of determining this question Dr. Flint instituted an analysis of the blood of the carotid artery, *i.e.* of blood going to the nervous centres; of the blood of the jugular vein, *i.e.* blood returning from the nervous centres; and, lastly, of the blood of the femoral vein, *i.e.* of blood returning from parts well supplied with nervous tissue. The results of the analysis of these three kinds of blood showed that the per-centage of increase of cholesterine in the blood from the jugular over the arterial blood was no less than 3·488, whilst the per-centage of increase in the blood from the femoral vein was even more, amounting to 4·134. The absolute quantities found in the carotid, jugular, and femoral blood respectively being 0·774, 0·801, and 0·806. These results seemed to show that cholesterine is produced in the nervous centres and cords, and is taken up by the blood as it traverses them. But if this be true, and if cholesterine be one of the products of the disassimilation of nervous tissue, its formation would be proportionate in activity to the nutrition of the nerves, and anything which interfered to any great extent with their nutrition would diminish the quantity of cholesterine produced. In cases of paralysis a diminution of the nutritive forces in the parts affected exists, and especially of the nervous system. Were the above theory of the origin of cholesterine tenable a marked difference in the quantity of that substance might be expected to occur between the venous blood coming from the paralysed parts and the blood from other parts of the body. This inference was also fully borne out by experiment; for in the analysis of the blood in three hemiplegic patients not a single crystal of cholesterine was found in any of the specimens of blood from the paralysed side, while about the normal quantity was found in the blood from the sound side. In yet another series of experiments comparative analyses were made to determine the proportion of cholesterine contained respectively in hepatic and portal venous blood, with a view of ascertaining whether there was a diminution in the former indicating that the elimination of this substance from the blood was effected at the liver; and, again, the results were consonant with the theory suggested. These experiments, therefore, we think, constitute a very satisfactory chain of evidence in favour of the theory maintained by the author that cholesterine is one of the products of the disintegration of nervous tissue, which taken up by the blood is eliminated from that fluid by the excretory action of the liver, and discharged with the bile into the alimentary canal, from whence it passes off with the fæces.

The important subject of the glycogenic function of the liver is next considered. The section commences with a short historical *résumé*, followed by a description of the various processes now commonly adopted for the determination of sugar, and the results of the application of those processes to the blood of the portal and hepatic veins. In regard to the much-debated question whether the liver contains sugar normally during life, he observes, speaking of his own experiments, that although sugar was always found in the blood of the hepatic veins he frequently, after having acquired great dexterity in manipulation, failed to obtain a distinct reaction in the extract of the liver, and it seemed, indeed, that the more accurately and rapidly the operation was performed, the more difficult was it to detect sugar in the hepatic substance. He then gives the details of four experiments on dogs, in which the general result obtained was that there was no sugar in the portal blood; there was no sugar in the extract of the liver when the removal of the organ or part of the organ was effected with great celerity, but that there was a marked reaction in the extract of the blood from the hepatic veins, the precipitate rendering the whole solution bright, yellow, and entirely opaque. Dr. Flint goes on to say:

"It is difficult to imagine how any observer, so well known and accurate as Dr. Pavy, could assert positively, as the result of personal examination, that the liver does not contain sugar when examined immediately after its removal from the living body when Bernard and so many others have demonstrated its presence in this organ in large quantity."

We do not see why Dr. Flint should hold that "it is difficult to imagine" that Dr. Pavy should take up a certain view when he had demonstrated the facts on which that view was founded to his own satisfaction, whilst they had afterwards been corroborated by MacDonnell, Meissner, Schiff, and to crown all by Dr. Flint's own investigations mentioned above; and we by no means regard Dr. Flint's argument as perfectly unassailable when he states,—

"It does not seem possible to deny the *sugar*-producing function of the liver in view of the conclusive experimental proof of the constant presence of glucose in the blood of the hepatic veins."

We add the ultimate conclusions at which, from a review of all the facts, Dr. Flint has arrived:

"1. A substance exists in the healthy liver which is capable of being converted into sugar, and inasmuch as this is formed into sugar during life, the sugar being washed away by the blood passing through the liver, it is perfectly proper to call it glycogenic or sugar-forming matter.

"2. The liver has a glycogenic function which consists in the constant formation out of the glycogenic matter, this being carried away by the blood of the hepatic veins which always contains sugar in a certain proportion. This production of sugar takes place in the carnivora, as well as in those animals that take sugar and starch as food, and it is essentially independent of the kind of food taken.

"3. During life the liver contains only the glycogenic matter and no sugar, because the great mass of blood which is constantly passing through this organ washes out the sugar as fast as it is formed; but after death, or when the circulation is interfered with, the transformation of glycogenic matter into sugar goes on; the sugar is not removed under these conditions, and can then be detected in the substance of the liver."

The further consideration of the subject includes the process of glycogenesis in the fœtus, the influence of food, nervous system, &c., all of which are fully discussed.

The eleventh chapter is occupied with the ductless glands, and the twelfth with nutrition; the latter poor in detail and illustration when compared with Paget's masterly lectures on the same subject.

The fifteenth and sixteenth chapters treat of movements, and of the active and passive organs by which movements are effected, including muscle, bone, elastic tissue, &c. This appears to us to be the least satisfactory portion of the work. The histological account of the several tissues, in particular, is very fragmentary and imperfect. In the description of muscle, for example, it is stated that "a primitive muscular fasciculus (*i.e.* a fibre) runs the entire length of the muscle, and is enclosed in its own sheath without branching or inosculation." In respect to the first statement, it is well known, and we believe it was first stated by Rollett, that although in the short muscles of the hand and foot the fibres are coequal in length with the muscle, yet in all the longer muscles of the limbs, and body generally, many of the fibres end in the substance of the muscle by pointed extremities. Again: Dr. Flint continues: "The sheath" (or sarcolemma) "contains the true muscular substance only, and is not penetrated by blood-vessels, nerves, or lymphatics." Now, although it be true that Dr. Beale still persists in denying that the sarcolemma is penetrated by the finer branches of the nerves, yet such a host of other observers are opposed to him, that the evidence in favour of such penetration is perfectly overwhelming; and in proof of this statement we would refer to the article on "Nerve and Muscular Fibre," by Kühne, in the 'Handbuch' of histology now in course of publication by Stricker.

Once more: Dr. Flint refers with apparent approval to the observations of Rouget on the structure of muscle, stating that "although *all* of his observations, particularly those with regard to the spiral form of the fibrillæ, have not been confirmed, there can be hardly any doubt that their structure is uniform, the appearance of alternate dark and light segments being due to differences in thickness." Surely Dr. Flint must have temporarily forgotten the evidence derived from polarized light, which seems to us so conclusive against this view. We regret we are unable to speak in any higher terms of commendation of the section of the physiological properties of muscle, in which the development of heat during action, and the absolute power of muscle, are wholly omitted; whilst rigor mortis, the electrical relations of muscle, and the chemical changes induced by activity, are only mentioned in the most cursory way. However, as Homer sometimes nods, so it is certain that Dr. Flint discusses the subjects of these chapters in a style much below the general average of the work. Some of them may perhaps be considered in the next part.

The last chapter includes the consideration of voice and speech. Dr. Flint quotes the interesting autoscopic observations of M. Garcia and Mrs. Seiler, and illustrates the effects of variations in the force of the expirations on the arrangement of the parts of the larynx, and the notes executed. Speaking of the differences in the purity of the tones in different singers, he observes that they are undoubtedly chiefly due to the unswerving accuracy with which some put the vocal cords upon the stretch; whilst in those in whom the tones are of inferior quality, the action of the muscles is more or less vacillating, and the tension is frequently incorrect.

"The fact that some celebrated singers can make their voice heard above the combined sounds from a large chorus and orchestra, is not due entirely to the intensity of the sound, but in a great measure to the absolute mathematical equality of the sonorous vibrations, and the comparative absence of discordant waves. Musicians who have heard the voice of the celebrated *basso* Lablache, all bear testimony to the remarkable quality of his voice, which could be heard at times above a powerful chorus and orchestra. A grand illustration of this occurred at the musical festival at Boston, in 1869. In some of the solos by Mme. Parepa-Rosa, accompanied by a chorus of nearly twelve thousand, with an orchestra of more than a thousand, and largely composed of brass instruments, we distinctly heard the pure and just notes of this remarkable soprano, standing alone, as it were, against the entire choral and instrumental force, and this in an immense building, containing an audience of forty thousand persons. The absolute accuracy of the tone was undoubtedly an important element in its remarkably penetrating quality."

Dr. Flint recognises three vocal registers. 1. The chest; 2. The falsetto; 3. The head register. The falsetto register, he agrees with Mrs. Seiler, being produced by the vibrations of the fine thin edges of the cords only, a greater width vibrating in the production of the chest voice, and he holds that the difference of opinion among laryngoscopists with regard to the mechanism of the falsetto, is probably in great part due to the fact that when these tones are produced the isthmus of the fauces is so powerfully contracted that it becomes exceedingly difficult to study the action of the vocal cords.

The most important theory of the mechanism of the head voice has been also proposed by Mrs. Seiler. After long and patient effort she was able to expose the glottis during the production of these tones, when it was found that the vocal cords were firmly approximated posteriorly, leaving an oval opening with vibrating edges, involving only one half or one third of the vocal ligaments. This orifice contracted progressively with the higher tones. This peculiar division of the vocal elements is due, according to Mrs. Seiler, to the action of a muscular bundle, called the internal thyro-arytenoid upon little cartilages, the cuneiform, extending forward from the arytenoid cartilage in the substance of the vocal ligaments, as far as the middle of the glottis. This voice is highly cultivated, especially in tenors and in the best female singers.

We have thus completed our analysis of Dr. Flint's third volume, and have, we believe, indicated all the principal points of interest it contains. It only remains to speak of the illustrations and the type. As regards the former the less that is said the better; the drawings are few, curiously selected, and for the most part coarse. The whole number is eighteen. That of bone is imperfect as a diagram, not exhibiting the lamellar structure of this tissue. Muscle is only illustrated by two woodcuts, one after Sappey, and one abominable one after Aeby. The three displaying the structure of the liver are good. Those on the skin and hair are bad. The ductless glands are not illustrated at all. We must not, however, take leave of Dr. Flint with a malediction on our lips. In spite of some shortcomings, which are excusable enough in so large a work, the preparation for which must have involved a very considerable amount of reading, and of that weighing of evidence which is so largely required in a subject like physiology, Dr. Flint's book will at once take a high position amongst those of its class. It is eminently impartial; it is written in good English, and by a man who has spared no pains to make himself thoroughly master of his subject. As regards the type, printing, and paper, we can only add they do credit to the well known and enterprising firm of Messrs. Appleton.

The work of M. Liégeois promises to be a large as well as complete epitome of physiology. The present part, which runs to 434 pages, includes only the prolegomena and the functions of reproduction, and is to be followed by five others.

The introduction contains a short historical account of the various doctrines that have been evolved from the inner consciousness of learned men in past ages to explain the phenomena of life, and which, after obtaining general acceptance, have successively made way for others not less inadequate to form the basis of a rational theory. Such were the archæism of Paracelsus and Von Helmont, the animism of Stahl, and the iatro-chemical doctrine of Borelli and others.

This chapter, and the next on organisation, we think too long. The world is travelling too fast nowadays, and there is too much positive knowledge to be obtained, for the student, and still less for the busy practitioner, to spend much time in poring over the musty and erroneous theories of the past. The description of what M. Liégeois terms "the media," light, air, heat, water, &c., seems to us, though also somewhat lengthy, to be remarkably well drawn up, and contains a good many facts collected from various writers that we have not elsewhere seen.

M. Liégeois devotes a short section to the rival doctrines of panspermism and heterogeny, and concludes by saying that the theory of spontaneous generation only belongs to the history of the subject: a somewhat bold assertion, considering the evidence that has recently accumulated, and which, in our opinion at least, leaves the whole matter open to careful investigation. He commences the subject of reproduction by an account of the "spermatic function," in which the male organs and their products are remarkably well described and illustrated. He alludes to the interesting fact noted by Mantegazza, that amongst 78 subjects he expressly examined, varying in age from sixteen to sixty, he found 11 in whom there were no spermatozoids, which were in point of age thus distributed: 5 from sixteen to twenty years of age, 3 from twenty to forty, and 3 from forty to sixty; an important result, as showing that the absence of spermatozoa, the indubitable sign and cause of sterility in the male, may occur from some purely functional derangement of the organ, which it is impossible to suspect without the aid of the microscope. He also refers to a case where the presence of a hydrocele appeared to prevent the development of spermatozoa. The man was operated on, when the spermatic fluid was found to contain spermatozoa, but the hydrocele re-collecting, they once more disappeared, and again returned after a second operation. He appears to agree with Kölliker in regard to their evolution, that is he thinks they proceed from the nuclei of primary cells in the tubuli seminiferi, and makes no mention of the observations of Lavalette St. George, who holds that the entire cell is transformed into the spermatozoid.

In regard to the phenomena of erection, which are minutely discussed, he gives the following theory:

"This act comprehends two periods, one in which the penis is simply turgid without being rigid; and a second, in which it is both turgid and rigid. The first appears to us to be due to a paralysed condition of the arteries and areolæ of the organ; the second, of the contraction of the muscles placed on the canals returning venous blood."

"We notice nothing calling for particular remark in the account of the early development of the egg, except that the description and drawings illustrating the first segmentation are much too schematic in character, as Kölliker clearly shows in his valuable lectures on development. M. Liégeois, however, introduces a very interesting statement, founded on the observations of M. Robin, of the production of the so-called polar globules, which are almost constantly found, and which are scarcely alluded to in English works. They are shown to result from a process of gemmation taking place in the liquid part of the vitellus. A transparent point is first seen at the surface of the vitellus, due to the disappearance of the granulations; this soon forms a hemi-spherical projection; then a cone, a cylinder, a pear, and is finally pinched off and remains free, as a globule, without proper wall, composed of an amorphous matter. Two or three may be successively produced in the same way, and these often reunite. Coincidently with their separation, the embryonic vesicle makes its appearance. In the description of the amnion which follows, M. Liégeois calls attention to the presence of muscular fibres in its substance. The contractility of this membrane was established by von Baer and Remak, and has been more recently studied by Vulpian. The movements which are capable of being excited in it are slow, but they can elevate or depress the embryo, or move it from side to side. In the chicken these movements can be observed from about the sixth day of incubation. What is their object? is it to effect a respiratory process?

We wish that we had space at our disposal to give some references to the development of the different organs, which are given at considerable length, and in general very well. We have been struck, in looking through the pages of this work, at the singular dearth of English names. Our friends across the Channel exhibit almost as complete an ignorance of our scientific literature as of our political institutions. The grand work of Milne-Edwards is, in fact, the only one in which we are worthily represented. Is it that so few French can read our language, or is it that the high price of our books precludes their reaching the hands of foreign savans? Be it as it may, it is certain the works of our best writers are very rarely quoted, either in France or Germany. We must not omit to state, in complimenting M. Liégeois on the manner he has discharged the task he has imposed on himself, that his foreign bibliographic references are full and accurate, and that his illustrations are both numerous and first-rate in their style of execution. The second part of M. Liégeois' work, embracing muscular tissue and movements, has just come to hand, and seems to treat very fully and satisfactorily of these subjects.

XI.—Medical Thermometry.[1]

(*Continued from* vol. xlv, *p.* 441.)

In passing on to review the teachings of animal thermometry, we shall find that in this, as in every other like investigation, the first thing to be done is, to make ourselves thoroughly familiar with the standards of health before we attempt to make an estimate of the fluctuations of disease. The importance of this has been well recognised by a few observers, by Dr. Davy especially; but as yet we lack records of sufficient variety, accuracy, and frequency, to settle all points. Dr. Davy's observations, like those of many of the earlier inquirers, are not altogether trustworthy; in many places, indeed, he is evidently in error. Dr. William Ogle's paper, in the first volume of 'St. George's Reports,' is excellent; and other essays have been published by Jürgensen, in the third volume of the 'D. Archiv. f. Klin. Medicin;' by Drs. Ringer and Stewart, in the 'Proceedings of the Royal Society' for February 11th, 1869; and by Mr. Garrod, in the same for May 13th, 1869.

Many experiments have been made upon the temperatures of those lower animals which have a constant warmth, but these are scarcely available for determining human phenomena. One fact is very clearly known, namely, that in a healthy man the limits of fluctuation, under various conditions, are very narrow, and upon this certainty medical thermometry becomes possible. Men differ a little, however, in this respect, as may be seen by comparing their observations, and by testing them personally. I find, for instance, that the limits of my own fluctuations are closer than those of Dr. Ogle.

It is worthy of remark that bodily weight, as measured in the same full-grown adult from year to year, is very stable. This, like heat, depends upon the regulation of waste and supply, and is independent, as Parkes has shown,[2] of external temperature. The constancy of the bodily temperature is a consequence of the remarkable regulation of evolution and loss of its heat, and is the expression of their difference. It is one example among many of the wonderful equilibration of function in the higher animals. The daily fluctuations follow a definite course, and their limits vary with age. The average maximum of temperature in persons under twenty-five is given by Ringer and Stewart as 99·1, of those over forty as 98·8. It should have been added that temperature rises again in old age, the second

[1] *Das Verhalten der Eigenwärme in Krankheiten.* Von Dr. C. A. Wunderlich, Professor der Klinik an der Universität Leipzig, &c. &c. Leipzig. 1868.

[2] 'Proc. Roy. Soc.,' June 20th, 1867.

childhood. The highest range of daily temperature is maintained between 9 a.m. and 6 p.m. After this time the temperature falls slowly and continuously if no alcohol be taken, but if alcohol be taken with the evening meal the fall is more sudden (Ogle). Otherwise food seems to have little influence upon normal temperatures, except, perhaps, in infants.[1] Sex, race, latitudes, seasons, weather, habits of life, and idiosyncrasies, go for very little in influencing the temperatures of healthy persons, so far as we can tell at present. Nor, again, should such changes as menstruation, pregnancy, and childbed have much influence if normally performed. This is a very important point; it seems that in the latter months of pregnancy there is a slight rise, compensated by a slight fall after delivery, when even in women who subsequently go wrong the temperature is generally normal.[2] The effects of cold and hot applications and the effects of alcohol ought to be ascertained beyond a doubt, but at present our information is rather contradictory. In testing the effects of alcohol we have this difficulty, which intrudes itself into many of these experiments; that is, we can scarcely say in a given case whether the influence seen in the change of temperature is really a normal fluctuation or one indicating the initiation of a morbid state.[3] It seems settled, however, that wine and brandy have at first an influence which depresses temperature. This depression, however, in my own case, is generally followed by a rise, the duration of the fall depending very much upon the dose.

Many elaborate investigations have been made into the comparative temperatures of different parts of the body. That the temperature of the inner body is higher than that of the outer admits of no doubt, and that of the closed cavities is from half a degree to a degree and a half (C.) higher than that of the axilla. This we should expect, for whether heat be produced chiefly in the blood-vessels, as Mayer believes, or chiefly in the organs themselves—and in certain organs especially, as is generally thought—it is still true that production is within, and that loss is on the surface, the blood acting as an equaliser of the whole. The heat set free within by numerous chemical processes—by the re-combinations of aliment in the blood, and by the oxidation of the tissues themselves—must be very great, and is spent on

[1] The ingestion of food after a long fast may, according to Jürgensen (loc. cit.), raise the temperature half a degree centigrade.

[2] I have just received a letter from Dr. Wiltshire, who has given this subject his special attention. Dr. Wiltshire has never found the temperature of pregnant women above 98° F.; on the contrary, in one or two he found it as low as 96° F.

[3] Take, for instance, Dr. Richardson's admirable observations on the influence of removing a limb.

the surface, partly in radiation, partly in conduction, partly in evaporation, partly in motion.

That the regulation between production and loss should be so complete is but another way of saying that a highly complex organism exists; its existence consists in this balance between function and medium, and complexity is but another word for power of rapid adaptation; but the blood being the chief medium of equalisation, the balance of temperature must depend greatly upon its distribution. In discussing the effects of hot and cold applications upon the whole heat of the body, we must, therefore, take into careful consideration the variations in distribution of the blood. When the superficial vessels are full, loss must be greater; when they are empty, it must be less. Mr. Garrod (loc. cit.) gives a very ingenious instance of this in the cold of one side of the body, caused by the exposure of the other side to a fire. As he points out, vascular tension is lessened, the vessels of the far side dilate with those of the warmed side, and radiation from them increases. Again, while the even distribution of the blood must be an important factor in modifying the temperature of particular parts, so the means of heat dissipation must be considered likewise, for the absence or excess of evaporation, for instance, in any part will modify the temperature as much as a difference in the blood distribution. These considerations explain how hard it is to ascertain exactly the effects of exercise and of warm and cold applications. If, for instance, we take the effects of exercise, we find that a part of the force set free by chemical change is diverted from heat production into mechanical movement; heat loss is also greater, for the blood circulates rapidly over the surface, and is thus more rapidly cooled, and so evaporation from the skin is also more rapid. On the other hand, oxidation is much increased, and the very rapidity of the circulation which, on the one hand, cools the blood, on the other carries more blood-corpuscles into the lungs.

These opposing changes seem so to meet together that exercise produces but little variation in the standard temperature of the healthy man. Brain-work has no effect at all upon temperature, as I have often found in myself; in tropical climates, however, Davy says that brain-work raises the temperature considerably, even so much as 4—5° (F.); but this statement must surely be received with hesitation.

I now pass on to the effects of artificial cold and warmth, a subject of very great importance at this time, when cold applications are put in use for the diminution of fever. Without committing ourselves to the belief that the effects of such applications are the same in health and in disease, yet it is of importance for us, first of all, to know the demeanour of the normal organism under such conditions. The effects of changes in thermal conditions are by no means so simple as they seem, and the more we learn about them the harder does it appear to arrive at any distinct conclusion. In the first place, it is very difficult to isolate the quality of temperature in such applications from other qualities. In cold and warm baths, there is, not only the question of temperature, but also of the water itself; in the atmosphere we have greater difficulties still to contend with—namely, the collateral effects of pressure, of moisture, and of currents and electricity; while the comparative effects of water and atmosphere are again to be determined. Moreover, if we assure ourselves, as we may do, that there is a difference between the effects of cold and warmth of the body, yet when we come to describe the effects of either, in its various degrees, we are perplexed. The effects, for instance, of cold alone are very complex, and vary from one moment to another. Thus the first effect of cold is to reduce bodily temperature, as that of higher degrees is to increase it; but these momentary results are soon balanced by modifications of heat development within; and in the case of cold, the reaction may carry up the temperature beyond the starting point. Warmth, on the contrary, which reduces the cooling process, may, by the reduction of heat development, be followed by a positive depression of bodily temperature. We know, for example, that a cold bath often warms us, while a warm one cools us to a very perceptible extent; and it must be remembered that these reactions may be more violent in morbid than in normal states.

The duration and degree of the artificial conditions are again important elements in the inquiry; applications, moreover, to various parts of the body and to the whole body have different results. Warmth, if less than bloodheat would cool the body quickly, by dilating the superficial capillaries, and allowing rapid conduction, while, at the same time, there is further heat abstraction in the consequent increase of more than one secretion. Cold, on the other hand, suppresses secretion, prevents the rapid conduction of bloodheat by closing the superficial capillaries, and drives the mass of blood inwards upon the heat-producing organs. Hence the reaction, after a short application of cold, is often very great, and is, probably, greater in fever than in health.

The very careful investigations of Leibermeister into the effects of cold baths are well known: the general conclusion from them, and from those of Kernig[1], is that on moderate use

[1] 'Exper. Beiträge zur Kenntniss der Wärmeregulirung beim Menschen,' Dorpat 1864. See also articles by Weissflog, 'Arch. f. klin. Med.,' ii, 570, and iii, 460.

of cold baths great abstraction of heat corresponds to great heat production, and slight abstraction to slight increase of production. Liebermeister's observations were taken in the well-closed axilla. I have myself made repeated observations on the cold bath, and find that deep long plunges into cold water have no effect in lowering my own temperature. I have tried this during cold weather in baths and rivers, never with a depressing effect, but sometimes with slight resulting elevations on reaction. Applications to exposed parts are more effective. Brown-Séquard found[1] that a hand immersed in cold water lost very much heat, and was long in recovering itself, while the temperature of the whole body was either unchanged or a little elevated. Flowing water, probably, abstracts heat more rapidly than still water. I do not find that cold-sitz baths of ordinary duration affect my own general temperature at all, but in some persons such baths lower the whole temperature even 1·5° C., which depression is followed by a rise to, perhaps, 0·5° beyond the normal. Warm-sitz baths, if of higher temperature than the blood, seem to cause a slight general rise, which, after the bath, is followed by a rapid recovery of equilibrium.

Binz[2] makes the interesting and useful observation that ice applications upon the abdomen cause a decided fall in a thermometer placed under the abdominal wall, the rectum temperature remaining unchanged. The influence of drinks needs further research, in the interest of clinical medicine. I have several times noticed that the favorite prescription of ice for suction is followed by depression of temperature and lowered resistance. In one case of heart disease, with nausea and sickness, the free use of ice was followed by a depression from 37° (C.) to 36·4°, and the general condition of the patient was also lowered.

Such are some of the conclusions to which we are led by investigation of the temperature of healthy persons; many points remain as yet very uncertain, and many statements need revision and extension; still we have gained something like a firm foundation for the consideration of the causes of fluctuation of temperature in disease.

In passing from the observations of thermometric changes in health to the like changes in disease, we are met by an old question, but by one which the thermometer is helping us to answer. What is disease? Modern pathology teaches us by many instances that disease does not consist in evil spirits or essences, in humours of the blood, nor in affections of the solids,

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[1] 'Journal de Physiol.,' i, 497.

[2] 'Beob. zur innern Klinik,' 1865, pp, 150—164, quoted by Wunderlich.

but in a loss of equilibrium. The body is an equilibrium mobile, and, like all such systems of forces, it tends, when disturbed, to regain equilibrium. The object of modern pathology is to learn in what ways equilibrium may be disturbed, and of modern medicine to learn how to help the system to right itself. In this inquiry medical thermometry takes a very high, if not the first place. We see by thermometrical records that there is no border line between health and disease; that the same influence affecting a stable individual and an unstable one may set up little disturbance in the former, but may shake the latter so evidently that his state will be called one of disease. Disease is therefore, an evident disturbance of equilibrium, and whether such disturbances in their degrees be called disease or not depends a good deal upon the observer. A certain amount of external disturbance, which amount varies for different individuals, is necessary for the actions and reactions of health; less than this fails to keep the equilibrium mobile in play, more than this throws it off its centre. The thermometer is in a large number of cases the most delicate test we have for variations of functional tension; in those cases, at least, where the loss of balance affects the regularity of nutrition, it gives us a new sense in enabling us to investigate the manner in which disturbance begins and the manner in which it rights itself. Hitherto we have supposed that the external influences we have examined were no more than compatible with normal movements; cold and hot baths, alcohol, exercise, and the like were supposed to act upon healthy organisms, and to exert no more pressure than such organisms could meet. Let us now suppose that these influences are slightly intensified so that their pressure is a little greater than the resistance of the organism, we shall then observe the first trembling of the balance, the first irregularity in the combination of functions, the first stage, that is, in the institution of disease.

The delicacy of the regulating actions of the living organism has been shown by a very telling experiment carried out under the direction of Donders, by two of his pupils. The sympathetic nerve in the neck of a rabbit is cut on both sides with the well-known effect of paralysing the vessels of the ear, and allowing the ears to be charged with an excess of blood. The consequence of this is that cooling goes on from the surfaces of the two ears with abnormal rapidity, and the general temperature of the animal is thereby lowered.[1]

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[1] Compare the well-known experiment of killing an animal by varnishing its skin, death being due to the increased radiation of heat from dilated superficial vessels, so that the animal is rapidly chilled down.

The organism thus affected, and deprived of its normal means of regulating the radiation from the ears by and through the sympathetic nerves, meets it by an increased tissue waste, and the animal recovers its normal temperature at the cost of a definite loss of weight. But the new position of equilibrium is less stable than the normal one, and consequently the animal has less power of rapid adaptation to external changes. Changes, therefore, which would have affected its temperatures momentarily and secretly, when the sympathetic was intact, are now met less promptly, and the thermometer shows their influence in larger curves of deviation. Here we find ourselves carried by insensible gradations from health into disease, the normal balance of the system is deranged, and impressions from without are met with less resistance. In health thermometrical variations are limited and constant, in disease they are inconstant and excessive, because the system cannot adapt itself to incident forces with its former readiness. The study of these marches, this no-man's-land which lies between that which on one side is certainly health, and on the other as certainly disease—is full of interest and of practical value. The variations of the normal curve of health set up by drugs are most important and suggestive in therapeutics, and the variations of the curve caused by excessive external impressions are equally important and suggestive on the side of ætiology. Many impressions, again, which cause no direct disturbance of the healthy curve, set up considerable disturbance indirectly. Prolonged and severe exercise causes in myself no change whatever in temperature, unless it be unduly prolonged, when a rise takes place, not immediately, but on the evening following the exertion. So it was with me again, after exposure to a severe heat. Happening one day to be on the premises of Messrs. Fairbairn and Company, I discovered a large oven for heating sand. I had no means of telling its temperature, but I was assured by bystanders, who knew less than I did about the heat regulations of the human body, that no one could remain in it. I fancy the temperature was not more than 300° Fahr. at most, and after the first half minute of immersion I found the air not intolerable. I staid in it for three minutes without moving my temperature a tenth either way, but five hours after, in the evening, my temperature rose, and I was able to detect in myself a mild sub-remittent fever-curve for the four following days. I had "caught no cold," for on leaving the oven I put on a thick over-coat, and returned home in a closed carriage. These secondary fevers following severe strains upon the regulation, but strains no more than were met at the time, are very curious, and scarcely explicable, except on the supposition of an excessive drain upon the nervous reservoirs. This brings us face to face with one of the chief physiological questions of the day. How far is the nervous system directly concerned in the development of heat? That the nervous system, by means of its vasomotor filaments, exercises a constant control over the contents of the arterioles, and thus over the radiation of heat from the skin, and over its formation inside the body, is well known and sufficiently intelligible. But is there anything more than this? Have the nervous centres the power of giving off energy directly as heat, instead of giving it off as movement, or, to put it otherwise, can the nerves, in connection with the intimate parts of tissues, exercise actively or passively such a dissolving or relaxing influence upon the tissues, as to increase heat in proportion to an excessive waste? Sir Benjamin Brodie's strong opinion that the source of animal heat was to be found in the nervous system is well known, and although his views and his modes of expressing them are not such as to fall in with more recent ways of thinking, yet we may admit that the convictions of so able an observer are not without significance. Dr. Parkes, in his definition of fever, directly refers the heat to increased tissue change, and the increased tissue change to the action of the nervous system. It certainly seems clear, as nutrition goes on in animals which have little or no nervous system, that there must be in the world nutrition and oscillations of nutrition which are not dependent upon that system. On the other hand, it may be, and it probably is, a condition of farther complexity, that particular parts should yield up their autonomy to some central regulating power, or combination for mutual advantage would be impossible.

When we meet with excessive temperatures in disease, we ask ourselves two questions; first, is the excessive temperature in the particular case an evidence of lessened molecular stability throughout the body; or, on the other hand, is it the evidence only of an abnormal distribution of blood? For example, in the flushed cheek which follows an injury to the cervical sympathetic we have only to do with an increased conduction consequent upon an increased exposure of blood on the surface.

But in ague, or in hectic chills, we find a rising thermometer coincident with a recession of blood from the surface, so that in this case we have to seek a different explanation. Or if we apply a thermometer to an inflamed knee-joint, we may be at a loss to know how much of the increase of temperature to attribute to afflux of blood, and how much, on the other hand, to an abnormal rate of tissue combustion.

Again, in cases of hæmorrhage into the mesocephalon, we commonly find increased disengagement of heat on the palsied side for many days or weeks, while the axilla of the other side appears rather, as we should expect, to be cooled down a little below the normal standard. A like result may be obtained by division of the lumbo-sacral plexus or of the brachial plexus, which is followed by increase of temperature in the respective limbs.

Or, as both Bernard and Schiff have proved, by section of one lateral half of the spinal cord in the dorsal region, we may raise the temperature of the leg on the same side, while that of the opposite limb is lowered. Schiff explains this latter phenomenon by supposing that section of one half of the cord sets up irritation in the other half. All these changes seem to be due only to variations in the distribution of the blood, and not at all to excessive tissue combustion; the effect of section of a nerve upon parts beyond it must be paralytic, and as this would directly cause lessened tissue activity, we explain the increased heat by remembering that this lessened tissue activity affects, among other parts, the contractile tissues of the arterioles, so that a dilatation of these ensues, with corresponding influx of blood. Irritation, on the other hand, if moderate, by causing contraction of the arterioles, diminishes blood supply, and so by loss of radiation makes sensible temperature lower. In sleep, therefore, when the great centres are at rest, and the arterioles thus at liberty, radiation increases, while the opposite states of mania or delirium, when the centres are discharging, and the arterioles therefore contracted, radiation is much diminished, and prolonged external cold is borne with impunity.[1] These instances of inequalities of distribution with ineffectual "regulation," fail, however, to explain those rises of temperature which exceed the common standard of the innermost parts. Rises of temperature which exceed 38° (C.) can scarcely be explained on the supposition of mere "vasomotor" palsies, but must be due to increased liberation of heat from the tissues. In hemiplegia, indeed, I have never found an increase of temperature to persist at a level of more than 1° (C.) above the other side, and as this can be accounted for by a paralysis of the arterioles, if we are sure that such a paralysis exists in their muscles, as well as in the voluntary muscles,[2] we need not assume that there is any excessive tissue combustion; indeed it has been shown by several independent observers that vascular palsy is, if anything, unfavorable to active combustion. Fever set up in animals in which paralysis of a region of vessels has been induced, has less effect upon the thermometer in such a region than in corresponding

[1] The dry harsh skin of the maniac is well known.

Bernard, indeed, believes that in hyperæmia of the ear, after division of the cervical sympathetic, the increased heat is due in some measure to increased tissue change. But he seems to me to base this belief upon insufficient grounds.

regions elsewhere. I cannot but think, therefore, that Dr. Handfield Jones,[1] and other writers who hold his views, are attributing too much to the vasomotor nerves and centres when they look to them as the means of any elevation of temperature above 38° (C.). I have had the advantage of some discussion on this subject with Dr. Jones, and I find that he is quite disposed to regard certain possible vasomotor centres in the encephalon as sufficient for the liberation of such high degrees of heat as we see in sunstroke and in acute rheumatism, a liberation which not unfrequently reaches 41·5° or 42°. But such temperatures as these seem to me to be far beyond any known results of the section of vasal nerve branches.[2]

Another hypothesis, which is, perhaps, an idol of the chemical cave, is, that although mere tides of blood are not sufficient to account for any large fluctuations in the heat-curve, nevertheless an increased tide of arterial blood must be followed by an increased oxidation of tissue, and thus by a great increase of temperature. This phase so often repeated, and evidently regarded as the most transparent of explanations, seems to me to be without meaning. Let us look at it more closely. If an increased tide of fresh blood passing into dilated channels is a cause of increased oxidation of tissue, it cannot be that the tissue remains as before. New activities can only be commensurate with new molecular aggregations, and we must expect to see one of two things, either that the tissues grow, or that they are disintegrated.[3] Now in abnormal tissue growth we are accustomed to see not only increased blood-tide but also increased work, and we regard the blood supply not as a cause but as a consequence. Tissues, like horses, may be supplied with fluids but cannot be made to drink; and in any case we should expect, from such a process as this, to find tension increased and heat made latent rather than manifest. The alternative notion is that the increased supply of oxygen may disintegrate the tissues and thus evolve heat. This is not only contrary to what we know of the action of oxygen upon normal living tissues, but it is also unseen in the tissues of the rabbit's ear and other parts when flooded with blood. All experimenters are agreed that no breaking down occurs in such parts unless a wound be inflicted. In all probability, therefore, the activity of the tissues is just as much or as little affected by the over supply of blood as the

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[1] For an interesting treatment of these inquiries, see Professor Rolleston, 'Quart. Journ. of Science,' April, 1870, and Dr. Handfield Jones' excellent volume on Nervous Diseases, lately republished.

[2] A most interesting case on the converse side of the question was lately read by Dr. Greenhow, at the Clinical Society. In this case the temperature fell as low as 84° F., coincidently with atrophy of the encephalon.

[3] Mere isomeric changes can scarcely be accompanied by much heat liberation.

appetite of the whole man is affected by the serving of four legs of mutton upon his table in place of one.

Another argument against the sufficiency of vascular palsy to increase the disengagement of heat may, I think, be derived from the daily experience of all of us, who constantly meet with extreme instances of vascular palsy, apparently of central origin, in neuralgic and like conditions, but without the concurrence of that important rise in axillary temperatures which we see in inflammation.

Observation seems to lead to some such solution as this of the problem of heat fluctuations, and of the allied problem of the trophic influence of nerves, namely, that each part, in taking its place as a member of a system, for this benefit gives up its own independence and enters into a fellowship with the remaining parts; as each part then becomes more and more special the more must it depend upon the supplementary functions of other parts, and upon the integrity of its relations with those other parts. Such a combination becomes possible by means of the nervous system, the centres of which act as banks to meet and to equalise demands. In such a composite body no part is sufficient for itself, but by entering into a community of parts it gains this, that in case of extraordinary demand it is backed up by the rest of the system. Hence we have a chronometric arrangement of functions, one organ or set of organs retiring into rest while another or others are in activity.[1] This periodicity and its manifestations in the nervous system are familiar enough. Cut a part away from communication with the rest, so as to throw it upon its own unaided resources, and it fails, as we see in numerous instances of local deterioration dependent upon section of a nerve trunk. I cannot now, of course, carry out these views of the Equilibration of Function in detail, but I have said enough to show how the nervous system in a complex animal gains an almost absolute command over the activities of all and sundry of its parts, and so becomes the equaliser of tension and a reservoir of force, like the fly-wheel of an engine. To say this is to say that the nervous system commands the liberation of heat. A part which of itself would be unable to resist a certain external influence[2] is enabled to do so with the additional force indirectly given to it by the rest of the body through the mediation of the nerves and the ganglia, or centres of tension. This prompt support we call the regulating action of the organism.

[1] To put my meaning otherwise, all organs have a rhythm of activity and rest. During activity waste predominates, and tension is lessened in them and in the associated nerve centres. During rest repair predominates, and tension is increased in the organs and in the centres.

[2] We do find, in fact, that with increase of complexity we have increased power of adaptation, and in low animals the power of resisting external changes is therefore comparatively small.

Let the external influence be excessive, that is to say, let it be an injury, and although all other organs fall into abeyance during the abnormal demand at the point affected, though we cease for the time to think, walk, digest, or make our ordinary secretions, nevertheless tension is soon lessened and molecular vibration increased throughout the system, and we see a liberation of energy as heat. Such, as it appears to me, is the part played by the nervous system in heat production, a part which is acquired rather than essential, but which in complex animals has gained pre-eminence. In considering these views we must also bear in mind the following propositions, to which I hope the reader will assent, namely, that the potency of a nerve depends only upon its central connections, and that the quality of a nerve depends only upon its peripheral connections. The old terms "motor nerve" and "sensory nerve" are open to this objection, that they seem to signify some difference of endowment in the nerves themselves, whereas the difference is only one of distribution; and it is much to be regretted that more terms of the same deceptive kind are creeping into use, such as the terms "trophic nerve," "inhibitory nerve," and the like. Every nerve is probably trophic, for it is the medium by which molecular balance is preserved between the ganglia or force reservoirs and the tissue-elements, to which it is distributed. Its effects upon a cell are of the same order as those of external impressions upon the system; there may be deficient stimulus, or there may be healthy balance of action and reaction, or, again, there may be irritation with molecular disintegration. In this latter case an abnormal disengagement of heat is to be found.

The interesting thermo-electric observations of Schiff in course of publication in the 'Archives de Physiologie Normale et Pathologique,' if hereafter established, will prove how constantly an impression upon the terminations of afferent nerves is followed by disengagement of heat in the centres, and it is probable that an abnormal disturbance of the centres would be seen in abnormal disengagement of heat at the periphery. If we can conceive any cause acting upon the nerve-centres so as to destroy their tension, the resulting liberation of energy may as well be converted into heat as into motion. Intermittent fever, for example, as a periodic discharge of tension with disengagement of heat, may be a true parallel to epilepsy, which is a periodic manifestation of a discharge of tension in the form of motion. It is a curious and interesting fact that recent observations lead us to the medulla oblongata as the centre of gravity, so to speak, in both cases. This centre, which seems to be the knot of life in more senses than one, is the point about which the manifold parts of the organism are centred. Here the nerves coming down from the head and special senses meet the great strands coming up from the trunk and limbs, and here they both are put into connection with the lungs, heart, and stomach. About this point their functions seem to revolve, and it is remarkable that as Schroeder van der Kolk, like Dr. Nothnagel and many recent writers, found a centre of convulsion in the upper medulla, so likewise the observations of Professor Heidenhain lead him to the medulla as the centre of pyrexial disturbance.[1] The passage of blood at different temperatures through the medulla probably exercises a modifying influence over the root of the vagus, and thus over the action of the heart; but this supposition has not explaining power enough to solve the phenomenon of pyrexia.[2] It would seem rather that changes in the medulla, radiating in several or in all directions throughout the body, influence in some way the tension of the tissues by way of the nerves; in states of its activity increasing their tension and concealing heat, in states of its paresis losing hold upon them and liberating heat. Thus any expenditure of force at any point is first met by the derivation of force from all other parts, which accordingly subside for a corresponding period. When the expense, however, becomes greater than can be met even by this coalition, the tension of the nervous reservoirs is lessened, the tension of all tissues throughout the body is likewise lessened, and they disintegrate with liberation of energy as heat.

In the case of specific fevers, we seem rather to have an influence acting directly upon the medulla in the first place, and lowering its tension. This is followed by slackened tissue tension throughout the body with disengagement of heat, or, as often in children, with a discharge of tension in the form of motion or muscular convulsion.

Postscript.—To return to a point which I omitted when discussing the technical details of thermometry, I may say a word concerning the trouble and expense which are caused by the disappearance of the index in pocket instruments. To prevent this, the natural resource seemed to be to narrow the neck of the instrument, and this was done about two years ago by Mr. Reynolds in several instruments. The disadvantages of this device, however, more than compensated the imperfect protection it gives. Fortunately, another plan has been now adopted by Mr. Reynolds, by which the index, if lost, can easily be restored by anyone without interfering with the accuracy of the indications. The plan is a very simple one, and consists in the retention of Prof. Phillips' chamber at the upper end of the instrument.

T. Clifford Allbutt.

[1] '*Vide* Tageblatt der Versammlung Deutscher Naturforscher in Innsbruck,' 1869. Published by Wagner, Innsbruck.

[2] *Vide* Dr. Rutherford's admirable article, 'Journal of Anatomy,' 1868-9, p. 402.

Bibliographical Record.

Cantoni on Heterogeny.[1]

THESE experiments, the results of which are entirely opposed to those of M. Pasteur, deserve attention, if only from the names of those engaged in making them, viz., Mantegazza, Oehl, Balsami, Maggi, Zoja, and de Giovanni, distinguished workers in the field of physiological experiment in Pavia. We find an improvement of the experiment of Fremy, and the result is comparatively more convincing; though the process, it must be confessed, is long and complicated. More interesting to us are those experiments which impugn the much quoted results of Spalanzani. Cantoni found that where the liquid was rich in organic matter, and the quantity of air in the sealed vessel greater than that of the liquid contained in it, prolongation of the boiling process made little difference as to the subsequent development of vibrios. Subjecting organic liquids in a Papin's digester even to a heat of 110° did not prevent their ultimate appearance. A mixture of yellow of egg raised to 117° always gave the same result when the temperature of the air was between 25° and 27°. Subjected to a higher temperature, it is true, the vibrios failed to appear, but the organic parts of the liquid then showed such a state of division, and so extensive a Brownian motion, as to prove a disorganised material. If the vibrios be derived from germs, they ought to perish at about the same temperature in various organic solutions, but no such confirmation of the germ theory was found in the experiments of Cantoni; and the lower the temperature of the air in respect to the above-named 25° as a

[1] 1. *Ricerche sull' Eterogenia.* Di GIOVANNI CANTONI, Professore di Fisica nell' Universita di Pavia. Tolto dalla 'Gazzetta Medica Italiana-Lombardia.' Serie V, tomo I, Anno 1868. 2. *Su l'Eterogenia.* Communicazione del Prof. GIOVANNI CANTONI, Membro effettivo del R. Instituto Lombardo. Letta nell' adunanza, del 16 Aprile, 1868. 3. *Ancora su la Produzione degli Infusori in Palloni suggellati ermeticamente e scal dati oltre i* 100° C. Idem nell' adunanza 25 Novembre, 1869.

1. *Researches in Heterogeny.* By GIOVANNI CANTONI, Professor of Physics in the University of Pavia. Taken from the 'Gazzetta Medicà Italiana-Lombardia.' 5th series, vol. I, 1868. 2. *On Heterogeny.* Communicated by Professor GIOVANNI CANTONI, Member of the Royal Lombard Institute. Read at the sitting, 16th April, 1868. Milan, pp. 4. 3. *A further Note on the Production of Infusoria in Globes hermetically sealed and subjected to the action of heat above* 100° C. Read at the sitting of the 25th November, 1869. Milan, pp. 5.

standard, the more easily, according to his experiments, the existence of the vibrios, by the proof of their non-recurrence, was terminated by the action of heat. For example, when the air was below 15°, the action of 100° or 105° sufficed to destroy them; when below 20°, they failed to come in yellow of egg after a heat of 108° or 110° had been applied. The germs of vibrios must indeed be subject to the same law of destruction as the vibrios themselves. In a solution full of these organisms kept for five minutes at a heat of 80°, after seven days had passed by there was no reproduction of them, so entirely were they destroyed.

The following are the conclusions Cantoni thinks himself warranted in affirming:

"1. The temperature at which the production of vibrios ceases in an organic solution varies with the quantity of the organic materials diffused in it; with the quantity of air included in the globes along with the solution; and, more remarkably still, with the temperature of the air in which these globes are kept after their exposure to the operation of heat.

"2. The germs of the infusoria which happen to be found in the organic solutions, perish indubitably during the act of ebullition.

"3. On raising the heat gradually above 100°, there occurs in the above solutions an ever-advancing disintegration of the organic materials until they are completely broken down.

"4. Still these dissociated elements can aggregate afresh, and with the more facility, as the temperature (above 15°) at which the solution is kept ranges higher, and the richer it is in organic material; either of these conditions being favorable to the intestine motions which serve for the re-aggregation of such elements. On the other hand, the less advanced this disintegration of the elements, *i. e.*, the less raised shall have been the heat applied to the solution, the better will such elements yield to reconstruction even with an atmosphere of lower temperature."

Cantoni makes some observations on the experiments of Hughes Bennett, which seem to show that he was at a disadvantage in operating in colder months, and also with too weak organic solutions; this poorness of organic material is much insisted on by Cantoni as the chief reason of the negative results of Spalanzani and Pasteur. We must then regard it as established by these experiments at Pavia, that vibrios will be obtained, in so great number as to form a coarse proligerous pellicle on the surface of the liquid, by using solutions rich in organic material, which may have been heated, not only to 100°, but to 105°, to 110°, or even to 117°, in globes hermetically sealed as used by Spalanzani, and enclosed in a Papin's digester. In subsequent experiments, July, 1869, with solutions of Liebig's broth in like manner subjected to heat of 110° to 118°, and maintained in communication with the air at from 27° to 29° for some days subsequently, such solutions gave no indication whatever of vibrios. The result had been altogether different with solutions of natural flesh and with yelk of egg. It would seem, then, that with different liquids the primary organic forms (analogous among themselves) do not issue directly from a preformed germ, but are constituted by peculiar laws of aggregation, and that they originate in one or many cells with definite materials as to quantity and quality, but with surrounding media and physical conditions of a different and variable character, and which are also in more or less degree favorable to the above organic constructions, and hence their evolution may be possible or not at different temperatures. The liquid in question was proved to be fully capable of sustaining vibrios; but even when a small portion was taken and poured into a glass with a moveable cover or bell, it remained six days without the evolution of infusoria, a fact scarcely reconcileable with the conclusions of Pasteur. In solutions of Liebig's broth not similarly treated by heat, infusoria were copiously generated. Under a morphological point of view, such solution of Liebig's broth was found more scantily provided with granules, and these were also of smaller dimensions than in the other liquids; vibrios which were produced from such solutions were also shorter and more slender. Professors Maggi and Balsami have interested themselves greatly in the morphological branch of the inquiry. Further experiments by which under the air-pump, eggs deprived of their shell and external membranes were impregnated with strong ammoniacal solutions, are very interesting from the low protein forms of a quasi-vegetable character, here called mellenic, which showed themselves invariably in four days' time: under such circumstances, the granules of the yelk were swelled in size, and the surrounding liquid was in some sort saponified by the ammonia. The high character which Pavia maintains as a school of experimental physiology made it imperative on us to notice these investigations, which appear indeed to be in advance of those performed by Bennet and Child. For details of the processes we must refer to the Italian text.

Koster's Anatomical Researches.[1]

W. KOSTER is well known as a most successful and original labourer in the field of anatomical science. We accordingly hail with pleasure the publication of some of his most recent researches in this department, in the *reports* of the Amsterdam Academy of Sciences for the present year.

His first contribution is on "The Artery of the inferior right Branchial Arch of the Embryo, recognisable in the Bronchial Ar-

[1] *Verslagen en Mededeelingen der Koninklijke Akademie van Wetenschappen.* Afdeeling, 'Natuurkunde,' 2de Reeks, Deel IV. Amsterdam, 1870.

Reports and Communications of the Royal Academy of Sciences at Amsterdam. Natural History Section, Second Series, Part IV.

teries." Writers on the subject of the branchial arches have hitherto held that in man the inferior (fifth) arch of the *right* side entirely disappears. W. Koster was first led to doubt this view from the examination of a case where a large right bronchial artery rose directly from the posterior aspect of the arch of the aorta. This vessel was clearly the representative of the embryonic fifth branchial arch. The author considers his opinion borne out by a case observed by Breschet, and alluded to by Dr. Turner in his exhaustive article on the "Irregularities of the large Blood-vessels," which appeared in the 30th volume of this Journal, pp. 173—189, and 461—482. Koster shows that this new theory facilitates the comparison of the development of the vascular system in the higher and lower vertebrata, while it also explains various anomalies observed in connection with the bronchial and other arteries, on which anomalies cardiac and pulmonary affections often depend.

A second communication is a case of *division of the inferior part of the omohyoid muscle.* There was nothing abnormal in the mode of origin of the muscle, but after running undivided for some distance, the muscle split into two parts, of which the anterior ran forward and joined the sterno-hyoid fibres. Koster recognises in this arrangement a trace of the embryonic value of the muscles between the scapula, sternum, and hyoid bone as intercostal muscles.

A third communication is on *an unusual course of the phrenic nerve of the right side.* Along with the supra-clavicular filaments of the cervical plexus a nerve of considerable size was found to run downwards and obliquely outwards until just above the clavicle, when it dipped behind the omohyoid muscle. On dissecting out its further course, it was demonstrated to be the phrenic nerve, having passed with a sharp curve inwards *in front of* the subclavian vein and internal mammary artery, so as to regain its normal situation, close to the superior vena cava. Of the ordinary phrenic nerve, in front of the scalenus anticus muscle, no trace was found.

Hartshorne's Conspectus of Medical Sciences.[1]

HERE is a perfect godsend for the student whose ambition soars to "getting through." The whole circle of the medical sciences complete in one volume, small octavo, so replete with the necessary information that the dullest brain crammed with it may be reckoned on as affording security to its possessor of passing triumphantly any examination in which such book lore may pass for professional knowledge. A well prepared book, indeed, considered as a digest,

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[1] *A Conspectus of the Medical Sciences: comprising Manuals of Anatomy, Physiology, Chemistry, Materia Medica, Practice of Medicine, Surgery, and Obstetrics.* For the use of Students. By HENRY HARTSHORNE, A.M., M.D., &c. With 310 illustrations. Pp. 1002.

but one liable to be abused and misused, and referable to the category of "crams" which were so popular when the College and Hall had to be "done" with as little expenditure of study as possible, but which the generally improved examination tests of late years have proved inadequate to their purpose, and, consequently, deprived of their value.

It is presumable, however, that such compendiums are still in request in the United States, where medical licencing institutions are sown broadcast throughout the country, where no general medical policy prevails, and each university or college is a rival to some other, and a competitor with it for students and graduates.

It would be a heavy and unsatisfactory task to examine critically the contents of such a treatise as this "Conspectus," but we have so far looked into it as to be able to say it is concisely written and its descriptions, so far as they go, accurate. The descriptions, however, are in our opinion only shadowy outlines of the knowledge the candidate for a diploma should possess. Certainly, the individual who, for supposition sake, might have crammed his brain with all the facts contained in this volume, would have occasion to look with almost contempt on ordinary mortals, not so "up" in particulars, however famous professionally; nevertheless, we should esteem him a wretchedly defective anatomist, quite confounded by an examination in practical anatomy, and a miserable pathologist unable to make a bedside diagnosis or to recognise a morbid tissue change. We trust that the sort of knowledge here conveyed does not carry students through examinations for medical degrees in America. And yet Dr. Hartshorne, in his preface, seems almost to imply that more complete knowledge is not to be expected from students. He thus writes:

"Experience shows that the thorough perusal of *extended* text-books by the students, during the months of their attendance upon medical lectures, is impracticable. The time for such study, eminently important as it undoubtedly is, must be before and after that period. Hence, the preparation of manuals for use in connection with lectures, and for subsequent review."

This seems to mean that the contents of this book represent the subject matter of lectures, and just that amount of knowledge the student need acquire to satisfy his examiners when he appears as a candidate for a degree or licence in medicine; and that he may postpone the study of complete works on the several branches of medicine to a more convenient season, which may be found after he is engaged in the duties and anxieties of practice, and when for his patients' sake he should be already in possession of the knowledge to be thereby acquired. As to a time of study of extended works being found at a period prior to a student's career at a medical school; such a supposition is extravagant. The deduction from the paragraph cited amounts then to this;—students need not trouble themselves with the perusal of special and extended works on medicine and collateral sciences, inasmuch as they have no time for so doing, but they should study this conspectus, and after they are admitted as qualified members of the profession proceed to educate themselves for practising it.

We fain would rather encounter Dr. Hartshorne as the author of an "extended text-book," or of some other more worthy subject for his industry and for his position as a professor in the University of Pennsylvania, than the preparation of such a superficial introduction to the various departments of medical science as is furnished by the treatise now noticed.

Glasgow Medical Journal.[1]

The appearance of this medical journal in a revised shape should have been notified to our readers long since, but various accidental circumstances have interfered to prevent our so doing; and we owe an apology to the editing committee, which has kindly forwarded the several numbers as published.

'The Glasgow Medical Journal' appeared for the first time in 1828, under the editorship of the late Dr. W. Mackenzie; but its somewhat languishing existence appears some two years ago to have struck the University professors and some leading physicians in Glasgow as not creditable to a city possessed of two universities, a large infirmary, and other medical institutions, nor to the host of medical graduates scattered over the world, enjoying, as private practitioners, and also as holders of hospital appointments, vast opportunities for observation and research. The upshot was the holding of a public meeting of the profession in Glasgow and its neighbourhood, and as private enterprise seemed to have failed in putting forth a medical journal sufficiently appreciated to be profitable, the formation of an association whose principal object should be "the publication of the 'Glasgow Medical Journal' in an improved form, and under the superintendence of an editorial committee of the association," was agreed upon. To give stability to the adventure, a class of guaranteeing members, responsible for a certain sum of money to meet possible contingencies, was likewise instituted.

Such is the history of this fresh issue of the Glasgow quarterly. A well written introductory address appeared in the first number, setting forth the necessity of co-operation on the part of the practitioners of Glasgow and the medical graduates everywhere, as well as of those specially engaged as teachers in the medical schools. It solicits contributions from the practitioners, pointing out the advan-

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[1] *The Glasgow Medical Journal.* Edited by a Committee of the Glasgow and West of Scotland Medical Association. Published Quarterly. Glasgow. Vol. I, and Nos. 1 and 2 of Vol. II.

tages accruing to themselves from noting and recording in such a medium as the journal the results of their daily experience. But while doing so it refers especially to the value of the journal to the medical school:—"It seems [it is written] self-evident that it is of importance to the medical school, as a proof of vitality and a measure of energy and talent, to have a medical periodical bearing its name. This periodical ought to be interesting to the numerous pupils of the school scattered over the globe," as representing the written opinions and teachings of its lecturers.

According to the regulations adopted, the journal is to consist of nine sheets, and to contain "original articles, a clinical record, reviews, the transactions of the Medico-Chirurgical Society, and, as regularly as may be, an article upon the more important medical topics of the period."

This programme has been faithfully followed, and the resuscitated journal has displayed much vigour and talent. The original articles have the precedence accorded them, and the value of the journal as a scientific periodical will be assessed by that to be attached to those articles. An analysis of those articles in these pages is, however, out of the question; nor will we attempt even to catalogue the whole of them, although we may commend all to the reader. Among those that will attract attention by reason of their authors, their subject, and the way in which they are handled, may be mentioned the articles on the limits of alcoholic stimulation in acute disease, by Dr. Gairdner; on the distribution of enteric fever in Glasgow, by Dr. J. B. Russell; on embolic and cardiac coagula, by Dr. R. Scott Orr: on the normal temperature and on the temperatures of children in phthisis, by Dr. Finlayson: on the cooling of dead bodies, as indicating the length of time elapsed since death, by Dr. Rainy; on the excretion of urea in typhus in relation to the temperature, by Drs. Russell and Coats; on the forces which carry on the circulation of the blood, by Dr. Buchanan; on epidemic scarlet fever, by Dr. Easton; and on hydrate of chloral as a hypnotic in typhus, by Dr. Russell.

As representative articles of the teaching and practice of the Glasgow medical school, we have also clinical reports of cases treated in the surgical wards of the infirmary, by Dr. G. H. B. Macleod. We might also cull from the clinical record notes of interesting cases sent by various contributors, but such a proceeding is not called for in a general notice of this sort.

In conclusion, we would commend the 'Glasgow Medical Journal' for its solid worth to professional men at large, and particularly to those among them who have been educated in Glasgow, and who, consequently, may be expected to feel a more lively and appreciative interest in the literary productions of their former teachers and fellow-students. A healthy rivalry with Edinburgh in maintaining a journal representing the teaching imparted and the work done in the Glasgow schools, and as reflected in the professional status and scientific acquirements of former pupils, must exercise a beneficial influence upon these Scottish universities and their students. Such rivalry must be felt, and deserves to be cherished; and considering the much larger population of Glasgow, the various manufactures carried on in it, and the important position of the city as a great commercial seaport, it must be admitted that this western capital of Scotland possesses advantages for the pursuit of medical inquiry superior to those to be found in Edinburgh, and from which her scientific practitioners might be expected to produce superior results.

Cornil and Ranvier's Pathological Histology.[1]

THESE two authors, who are well known in Paris by their researches in morbid anatomy, have undertaken to write an elementary work on the microscopical appearances of diseased structure. The first part, which is now before us, deals with general pathological anatomy, the second part will describe the diseases of the various tissues of the body, and a third one will treat of the diseases of each organ. It will be of great use, not merely to the student, but also to every practitioner, as supplying him with a clear and brief account of the very latest views of scientific authorities on these subjects. Its tone is studiously moderate throughout, as is fitting in a manual, and the judgments expressed on such questions as Cohnheim's theory of suppuration, and on Virchow's opinions on tumours and inflammation, are well grounded and clearly expressed. If the work sustains its present high character, it will prove one of the most valuable recent additions to medical literature.

Averbeck on Addison's Disease.[2]

THIS work appears to have been an inaugural dissertation founded on the observation of two cases related in full by the author, and expanded into a monograph by the addition of material gleaned from all the current sources of information. Some of our best treatises on medicine have originated from the author of an interesting case having sought through the literature of our profession for facts which could illustrate his theme. So in this work we find a good compilation of all the cases of Addison's disease gathered from every country of Europe accompanied by the opinions of writers on the nature of the disease. Upon a careful perusal of the treatise before us, we have not been able to discover any clue afforded as to the con-

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[1] *Manuel d' Histologie Pathologique.* Par V. CORNIL et L. RANVIER. 1re Partie. Anatomie Pathologique Générale. Paris, 1869.

[2] *Die Addisonsche Krankheit.* Eine Monographie von D. AVERBECK, Praktischend Arzte in Bremen. Erlangen, 1869. Pp. 114.

nection between the disease of the supra-renal organs and the remarkable train of symptoms which accompany it.

The author commences by describing very minutely and clearly the three conditions which constitute the affection known as Addison's disease, viz., the pigmentation of the skin, the asthenia, and the peculiar disease of the capsules. The latter he regards as uniform in character, and styles it a chronic inflammation of the organs. He explains how the capsule becomes enlarged by the exudation of an albumino-fibrinous deposit, and how this subsequently shrinks and hardens. He then describes the pigment changes in the skin and mucous membrane of the mouth, and subsequently the constitutional symptoms indicated by intense feebleness, often great disturbance of the stomach and bowels, so that in some of the more acute cases the symptoms were mistaken for those of a gastro-enteritis. In only four out of a large number of cases was it ascertained that the hair had grown darker. The author subsequently considers the question of its alliance with other recognised constitutional maladies by taking into consideration the frequency of its association with various diseases, and can discover a relation to tuberculosis only from the fact of a certain number of cases of Addison's disease being met with where so-called tubercle was found in the lungs and other organs. As regards the duration of the complaint, some instances of the disease had shown a duration of only a few months, whereas others had endured for four years or more. The average duration of twenty-seven cases was fifteen months. The termination of the disease is gradual, though sometimes it is by diarrhœa, convulsions, or coma. Addison's disease occurs mostly in young adults, and twice as often in males as females. The prognosis is necessarily unfavorable, and hope there is none, seeing that all evidence corroborates the opinion, that as soon as the disease is recognisable the capsules are irretrievably destroyed; the records of cases of cure, therefore, must have been fallacious. The only treatment which the author recommends is that which is best suited to support the enervated powers of the patient, and thus advises tonic medicines, with meat, milk, eggs, wine, tea and coffee.

As regards the nature of the disease, this was endeavoured in the first place to be elucidated by physiologists, who, after Addison's discovery, set to work with fresh vigour to make out the functions of the supra-renal bodies. Brown-Séquard believed he had tended to confirm Addison's views, when he stated that death speedily followed their extirpation in animals, but it was not long before these results were contradicted by other physiologists. Then followed those who experimented chemically on the composition of the organs, believing that they were intimately connected with the blood-making process; and various comparisons were made with hæmatine, as well as with the colouring matter of the bile and of the urine. These experiments threw no light upon the cause of pigmentation of the skin. Other physiologists, who carefully dissected the organs, were struck with their close approximation to those great centres of the sympathetic system, the semilunar ganglia; and thus it was surmised that, whilst the cortical portion of the organ might be associated with the blood-making process, the medullary might be intimately connected with the nervous system. Analyses of the blood were also made, but no striking results were arrived at; and as regards the amount of blood corpuscles, some observers declared that the amount was less and others greater in Addison's disease. The author then gives the following summary:—1. There is a certain combination of symptoms shown by skin-discoloration and anæmia (asthenia?), and with these is found a chronic inflammatory process of the supra-renal capsules. 2. The specific anæmia and diseased capsules are essential and primary. The discoloration, though intimately connected with the disease, is perhaps not essential. 3. The chronic inflammatory disease affects both capsules simultaneously. 4. Constant changes in the blood not yet discovered, but frequent disturbances of organs which accompany blood-changes. 5. Pigmentation of skin and mucous membrane of mouth, in the deep cell-layers of the rete-mucosum. 6. Causes unknown and the moment the disease begins; also whether the causes be within or without.

We may here mention that the "Nouveau Dictionnaire de Médecine et de Chirurgie," and also the "Dictionnaire Encyclopédique des Sciences Médicales," contain each an article on "Maladie Bronzée," by Jaccoud and Ball respectively, in which the authors have collected a large array of cases, but have not thrown any additional light on the malady, nor added anything which may not be found in English treatises.

Petersen—Contagiousness of Phthisis.[1]

Hr. Petersen commences his clearly written and well arranged essay, with an interesting history of the literature of the contagiousness of phthisis, which forms the subject of the first part of this work. From his researches he shows that the belief in the existence of this contagion is not derived exclusively, as many suppose, from a traditional popular superstition, but that it has in science also a more than ordinarily solid historical foundation. He adds that it is a striking result of his investigation that the inquiry has not in past centuries made any remarkable progress, and that little more is known in the present day than what Fracastorius ('De morbis contagiosis,' Geneva,

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[1] *Lungesvindsotens og Tuberculosens omtvistede Contagiösitet og Inoculabilitet.* Afhandling for Doctorgraden i Medicinen. Af Julius Petersen, cst. Distriktslæge paa Kjöbenhavns Nörrebro. Kjöbenhavn, 1869. 8vo, pp. 123.

The Disputed Contagiousness and Inoculability of Pulmonary Consumption and Tuberculosis. A Thesis for the Degree of Doctor in Medicine. By Julius Petersen, *ad interim* District Physician in Copenhagen.

1621), and Sylvius ('Op. Med.,' 1679), taught. But very little has been ascertained as to what anatomical and clinical peculiarities characterise the infecting or infected cases of phthisis, or how far the latter, on the whole, are distinguished from those occurring in another mode; only in Morton ('Phthisiologia,' Londini, 1689), do we find definite statements of a clinical nature.

The author further observes, that it would appear from his investigation that the discrepancy he has found to exist in the views of European physicians on the subject of contagiousness in phthisis, is connected with well marked climatic differences; in southern Europe proper the views in its favour decidedly preponderate; in France, opinions are more divided; in the extreme north an anticontagious theory prevails. He asks, may not climatic conditions have positive influence, so that in each case the contagion may be more active, more prominent in the warmer than in the colder countries? He justly adds that in the present state of science it is necessary to turn our thoughts towards parasitic organism, the existence of which would render climatic influences more explicable. We may observe that the discovery by Davaine, and the confirmation by Malmsten, Ekecrantz, Lambl, and others, of the existence of infusoria in the dejections of patients labouring under cholera and typhoid fever, as well as the recent researches of Professor Tyndall upon dust, add force to this remark.

Hr. Petersen next considers the question as it affects Denmark, and while he is unable to produce any absolute proof of the existence of contagion, he sums up the results of his inquiries and of his own observations in the following propositions:

"1. That a contagious origin of some cases of phthisis cannot on sufficient grounds be denied; 2, That phthisis caused by contagion is in general of a very dangerous and inflammatory character; 3, That it must justly be considered hazardous to lie in the non-disinfected bed of a phthisical patient, and to be habitually in too close contact with such a person; 4. That this danger in Denmark seems to be greatest in the warm period of the year."

In the second part of his work the author proceeds to consider whether we really ought, on the ground of experimental investigations, to take part with Villemin ('Études sur la tuberculose, Paris, 1868), in looking upon phthisis (tuberculosis) as a specific and virulent, that is to say, as an exclusively contagious disease. In dealing with the question prefixed to this section: "Is phthisis or tuberculosis inoculable?" the same plan is followed as in the first part, an historical sketch is first given, and to this an account of the author's own experiments is appended. The latter consisted of—1, subcutaneous injections of actual tuberculous and caseous substance in dogs, rabbits, and cows; 2, of feeding experiments on rabbits; 3, of the application of sputum in the respiratory organs by means of tracheotomy and injection in rabbits; 4, of the introduction of an indifferent foreign body into the peritoneal cavity. From his researches the author is led to prefer Sanderson's (Niemeyer's) theory to Waldenburg's, and with the former to infer that—

"Miliary tuberculosis is a disease of resorption and infection arising regularly from the action of peculiar caseously metamorphosed substances developed in the organisms concerned, which are at least most frequently results of an inflammatory condition. Purely contagious properties in the caseous substances have, on the contrary, not been demonstrated. The question of inoculability in the proper sense of the word must be answered in the negative."

In the third and last division of the work the author studies from a clinical and anatomical point of view, the genesis of tuberculosis in the human subject. Towards the close of the chapter he briefly states the conclusion at which he has arrived, viz. that—

"It cannot be premature to infer that miliary tubercles in man also are in general developed secondarily, as the result of a previous infecting influence in the individual himself, from retrogressive products of caseous appearance, which have undergone peculiar metamorphosis, the proper nature of which is unknown. A certain predisposition in the individual affected is necessary. A purely contagious origin of actual tuberculosis has not been proven."

Jaccoud's Internal Pathology.[1]

We have much pleasure in calling our readers' attention to the first instalment of a treatise on the practice of medicine, by Professor Jaccoud, whose 'Clinique Médicale' has been analysed in a recent number of this Review.

The part before us treats of the general morbid processes (as congestion, embolism, pyrexia, and the like), and of diseases of the brain and spinal cord. It is particularly valuable, as giving a bibliographical summary at the head of each subject, with full references to the periodicals in which particular subjects have been treated. Probably the best portions of the volume are the general rules for diagnosing the seat of intracranial disease, the description of spinal diseases, and the history of embolism, inflammation, and fever. The various modes in which the cerebral arteries may be blocked up are very clearly described, and there is a sufficient account of several matters which have been too much neglected in systematic treatises; for instance, the use of the thermometer in febrile diseases, sclerosis of the brain, and atrophy of the nerves. On the other hand, an English reader will observe that there is an undue

[1] *Traité de Pathologie Interne.* Par S. Jaccoud, Professeur agrégé à la Faculté de Médecine de Paris. Tome I, Part I. Paris, 1869.

preponderance of German quotations, while English medicine is imperfectly represented (we do not remember to have seen Reynold's 'System of Medicine' once referred to); and the articles on treatment are strikingly meagre and unsatisfactory compared with those on diagnosis.

With these exceptions, the book is one which we strongly recommend to all who desire to have a good idea of the present state of medical science in France and Germany.

Basham on Renal Diseases.[1]

The steady reaction in favour of the clinical study of disease, as opposed to the systematic teaching of former days, is one of the most hopeful signs for the future of medicine. Going back again with all our powers of observation sharpened, and aided by instruments and appliances which give a precision to our investigations formerly unhoped for, we are studying the phenomena of diseases as they actually are, and learning more in months than years of dogmatic teaching could ever tell us. That this wise retreat is really an advance the progress of medicine in recent years amply proves, and that it makes better practitioners none would deny. As a product of this reaction in favour of the clinical method we welcome the little book before us, which the author says has been prepared "with the view of promoting a practical and clinical knowledge" of renal diseases. Dr. Basham has divided his work into three parts, of which the first treats of inflammatory diseases of the kidney; the second of non-inflammatory diseases; while the third part is devoted to the urine and its clinical significance. The classification of renal diseases adopted in the first part of the book, and which the author tells us he has used for some years in his lectures on medicine, however admirably suited for a systematic course of lectures, is, we think, quite out of place in a clinical treatise. There is too much of the lecturer and too little of the clinical teacher in it. The modern method has conquered the older teaching but not destroyed it, and thus at the basis of Dr. Basham's clinical superstructure we find the stones on which his predecessor built. This is, we think, the single fault of the book, and throughout there are traces of the difficulty the author has felt in reconciling his pathological grouping of his forms of disease with their clinical study. Thus he is forced to include cancer under the head of inflammatory diseases, and to term it "cancerous nephritis." Then, again, we find scarlatinal albuminuria among inflammatory affections in Part I, and among the non-inflammatory diseases of Part II. If more than two groups had been made these defects might have been

[1] *Renal Diseases: a Clinical Guide to their Diagnosis and Treatment.* By W. R. Basham, M.D., Fellow of the Royal College of Physicans; Senior Physician to the Westminster Hospital; and Lecturer on Medicine, &c. &c.

avoided, and the classification improved. The grouping together of such conditions as cancer, tubercle, acute morbus Brightii, and endemic hæmaturia, is an imperfect method, and unsuited for clinical teaching. In a new edition, we hope Dr. Basham will substitute for his pathological classification one built up on the purely clinical plan. The chapters on diagnosis and treatment are very good, and the student and young practitioner will find them full of valuable practical hints. The third part, on the urine, is excellent, and we cordially recommend its perusal. The author has arranged his matter in a somewhat novel, and, we think, useful form. Here everything can be easily found, and, what is more important, easily read, for all the dry details of larger books here acquire a new interest from the author's arrangement. This part of the book is full of good work. Many a student referring to it in the hospital wards, many a busy practitioner glancing over it in the evening, will have reason to thank Dr. Basham for placing so much useful information before the profession in so small and pleasant a form.

Murray on Treatment of Cholera.[1]

The documentary evidence on which this Report is based, consists of replies from more than five hundred medical officers of the British and Indian medical services to a long series of queries about the treatment, prophylactic and curative, of the disease. There is, of course, great discrepancy of views on a variety of points in this mass of statements; and indeed it may be doubted whether any real good could be fairly anticipated from collecting such an accumulation of mere opinions, founded often on very insufficient observation, and generally somewhat loosely expressed, especially in regard of the second or therapeutic section of the inquiry. "In no disease," Dr. Murray remarks, "has there been such diversity of opinion as in cholera, and in none has such a multiplicity of opposite remedies been so strongly urged." All that can be said is that "the general impression is that much good may be done by limiting the ravages of the disease, both in extent and intensity, and many lives saved by recognising and treating it in its earlier stages, and much distress relieved even when life cannot be saved." This is unquestionably true, and is one of the very few general truths that may be considered an axiom. Dr. Murray's own views on the subject will be gathered from the following remarks:—"The indications of treatment in the earlier stages are to assist Nature by supporting the strength, counteracting the depressing action of the poison, promoting its elimination, and defending the parts through which it is discharged from the irritating effects of the poison, or vitiated con-

[1] *Report on the Treatment of Epidemic Cholera.* By John Murray, M.D., Inspector-General of Hospitals, Bengal Medical Department. By order of the Government of India. Folio, pp. 88. Calcutta, 1869.

tents of the bowels." He accepts, as if it was an established verity, that the cholera poison is contained not only in the alvine evacuations, but also in the secretions from the kidneys, skin, and lungs; and he is thus constrained to adopt, to a certain extent at least, the "elimination" doctrine in the treatment. Nevertheless, he admits that the use of purgatives is highly objectionable, and that "one of the first points of treatment is to soothe the irritation which causes the diarrhœa, and remove any extraneous matter (as undigested food) which ordinarily induces irritation in the bowels." It would have been better, it seems to us, if Dr. Murray had kept more closely to the strict results of repeated experience in dealing with the subject he had in hand, and not allowed himself to be biassed or in any way trammelled by speculations as to the theory of the diseased process in cholera.

In respect also of the preventive and prophylactic treatment, he has assumed as proved various propositions which are still *sub judice*, and which it is well known are anything but fully accepted by the profession in India. The following passages will indicate to what we allude:—

"The human body appears to be the chief medium of reproduction or multiplication and dissemination of the poison. This is fully proved by the history of the progress of the epidemic attacks in India, Europe, and America." . . . .

"There are numerous well-authenticated cases of the poison having been mixed with the water of wells and tanks, those using the water being attacked by the disease."

And in illustration of this position, reference is made to two instances, during the Hurdwar outbreak, of persons being attacked "on the second day after the poison had been communicated to the village tanks; in one instance from a pilgrim, suffering from cholera, having bathed in it; and in the other, after the clothes of a man who had died from cholera were washed in it." No detailed evidence for these vague statements is related.

"There is abundant concurrent evidence that contact with the evacuations from cholera patients, or with articles of clothing, &c., contaminated by them, and using public latrines, have been followed by attacks of the disease." . . . . "The (cholera) sick are a source of danger to those near them, and it is not prudent to collect them in ordinary hospital wards with other patients. Even special wards in ordinary hospitals would bring the disease in dangerous proximity to the other sick. Communication between the outside world and the inmates should be restricted to near relations. The attendants should limit their intercourse outside as much as possible. Perfect isolation is impossible, but that should indicate the course to be followed."

That Dr. Murray's views are not shared by other experienced men in India, we have only to refer to the notice of the report by Dr.

D. Smith, Sanitary Commissioner for Bengal, in the number of this journal for last October, pp. 428-9. With such marked discrepancies of opinion among even the chief men in India, it was certainly high time that a more searching and more rigorous method of investigating the phenomena and relations of cholera in its home should be instituted than has yet been undertaken; and we cannot but think that much good will result from the adoption throughout the military service of the code of instructions for conducting the necessary inquiries respecting cholera recently issued by the War Office, and of which an abstract was given in our last number.

Fenwick's Diagnosis.[1]

This is strictly, as it professes to be, an elementary treatise on medical diagnosis, yet the mature practitioner may study it with profit. It owes its origin to an attempt to supply the wants of his own students in pursuing the plan of clinical investigation carried on at the London hospital. It is illustrated by woodcuts, and has appended to it regional figures and a piece of carbon paper for transferring the figures to paper to be filled up with the delineation of the results of thoracic and abdominal exploration obtained by percussion, palpation, and auscultation.

The author introduces only so much pathology into his instructions for diagnosis as to afford an interpretation of the symptoms recorded of each special lesion of importance. Altogether this little book may be cordially recommended to all learners of their profession.

Ellis on Diseases of Children.[2]

This small treatise on the diseases of children has attached to it the title of a practical manual, and we may take it as addressed especially to the class of busy practitioners who would rank themselves under the beloved term of practical men, and plead want of time for the study of works more fully representing the pathology of the day. In short, Dr. Ellis's work is essentially representative of the ordinary routine practice, and sets forth, well enough for the unambitious pathologist who is satisfied with opinions that have become popularised in the profession, the principal features of children's diseases.

The book is a compilation; we do not observe that it contains any information of importance beyond what is contained in previously published and well circulated works on the subject. The description of diseases and of treatment is superficial; and, if enough for the learner who wishes to acquire a general knowledge, it would

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[1] *The Student's Guide to Medical Diagnosis.* By Samuel Fenwick, M.D., &c. London, 1869. Pp. 176.

[2] *A Practical Manual of the Diseases of Children, with a Formulary.* By Edward Ellis, M.D., &c. London, 1869. Pp. 279.

fail the painstaking practitioner who sought for help and enlightenment in dealing with difficult and anxious cases. We cannot, therefore, congratulate the author on having produced, in the words of his preface, "a handy book of reference upon a class of diseases, presenting peculiar difficulties, and yet of the deepest interest because so continually met with in practice;" inasmuch as reference to its pages would rarely, at least, furnish the practitioner with information over and above what every one may be supposed to possess, and such as is calculated to clear up the pathology and treatment of "a class of cases presenting peculiar difficulties."

A very lengthy formulary is appended, but we cannot recognise in it any peculiar value. A considerable part is nothing more than a catalogue of drugs, classified according to their accredited therapeutical action, and looking not much unlike a student's synopsis after the perusal of a work on materia medica, and very much out of place in a special treatise on the diseases of children. For example, the greater part of a page is occupied with the following catalogue of astringents:—

"Astringents. The following remedies may be grouped under this head: Aluminium; Plumbum; Quercus (chief external use, as injections and lotions); Galla; Krameria; Rosa; Tormentilla (seldom used); Granati Radicis cortex (chiefly as anthelmintic); Hæmatoxyllum (*sic*), (decoction, a good remedy, but stains linen); Kino (the compound powder contains opium one in twenty. Dose according to opium); Catechu ball (Extract. liquid. Balæ Fruct. Dose ♏v—ʒss in dysentery); Matico (has a tonic action on the urinary passages); Gummi Rubrum (a new remedy. Dose gr. i—vi); Syrupus Gummi rub. (dose, ♏xx, ʒi; much used in diarrhœa and dysentery)."

Then follow three formulæ: two of them for prescribing alum, and the third for giving acetate of lead; but neither of them having any special claim to notice; and afterwards a notice of "Acidi Gallici" (*sic*), a formula for giving tannic and nitric acid together; a notice of "glycerine of tannin," and the common-place remarks that "combinations of the infusions and tinctures of kino, catechu, and krameria, with the mineral acids, chalk, or opium, are the ordinary remedies for diarrhœa." Thereupon, by adding two formulæ, the one for an astringent tincture, the other for an electuary recommended by Trousseau, above another page of the treatise is made up.

Scope might readily be found for criticism in the above notice of astringents, but we limit ourselves to producing it as a sample of a well nigh valueless, or at the best of a misplaced addendum to the book, occupying fifty-seven pages, or a fifth part of the whole contents. We do not deny the fact that the majority of the formulæ are really useful preparations, yet most of them are, with more or fewer modifications, in constant use by medical men who have ingenuity enough to write their own prescriptions; and if some of them possess peculiar merits they might have taken their place in the text under the heading of the diseases for which they are presumed to be useful, just as indeed the author has done with many others. Our impression is, that the copying and learning of special formulæ, except in the case of some peculiar medicines, is out of date, at least among those who are well acquainted with the properties of the medicines they prescribe, and who aim not at firing off some special formula at a duly catalogued and scientifically named malady, but endeavour to treat each individual case of disease with reference to its peculiarities of diathesis, and its existing pathological conditions, and its physiological requirements.

Marcet on Diseases of the Larynx.[1]

THE title and the substance of this little treatise are in accord; a matter of merit in this age of book-making. Dr. Marcet has written from experience, and illustrates his teachings by experience. He has wisely not attempted an exhaustive description of laryngeal diseases, for systematic treatises on those diseases are sufficiently abundant, yet the practitioner may acquire from these clinical notes a sufficient insight into the nature, symptoms, and treatment of the lesions in question. From his position as physician to the Brompton Consumption Hospital, he has had a wide field of observation of the laryngeal lesions associated with tubercular phthisis, and that portion of his book relating to those lesions holds a prominent position in its pages, and will especially attract the reader's attention. The chapter on laryngeal phthisis is likewise illustrated by some excellently executed coloured figures, as well as by engravings. The essay is divided into three parts, the first devoted to an account of laryngoscopic instruments and their application, and to simple laryngitis; the second to hysterical and nervous aphonia, and the third to laryngeal phthisis. In his notes on chronic laryngitis, he asserts his conviction, that the complete disappearance of the true vocal cords must not be considered as an indication that the voice cannot be recovered. This proposition is supported by a case cited. Again, in the course of his observations on loss of voice, he recounts a very interesting case of aphasia in which the vocal cords could not be discovered by the aid of the laryngoscope, and in which considerable benefit resulted from galvanism.

In the chapter on laryngeal phthisis Dr. Marcet takes occasion to insist upon the value of the laryngoscope in the diagnosis of tubercular consumption, pointing out that the recognition of the well-

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[1] *Clinical Notes on the Diseases of the Larynx investigated and treated with the assistance of the Laryngoscope.* By WILLIAM MARCET, M.D., F.R.S., &c. London, 1869. Pp. 135.

ascertained signs of tubercular lesion of the larynx becomes confirmatory of what may be obscure and doubtful indications of the disease as presented within the chest. Indeed "the appearance of the larynx in many cases of phthisis, and long before any positive signs of the disease have set in, is peculiar." What this appearance is, and what are its concomitant symptoms are well described, and should be read in the author's own words.

Legg on Examination of Urine.[1]

THIS little pocket volume is precisely what its title imports, a handy guide for clinical clerks and students who want instruction in the modes of examining the urine for the purposes of recognising disease. It pretends to no originality of matter, but only to a convenient bringing together of the best information to be got concerning the subject. That it has succeeded in its purpose is sufficiently attested by the early appearance of a second edition, and although the medical practitioner may find the same instruction conveyed in most works on the urine and urinary diseases, yet he will often be thankful to have it in a small compass such as in this book, when he cannot conveniently refer to his larger treatises to refresh his memory.

Squarey on Administration of Anæsthetics.[2]

THIS small book furnishes in a convenient compass such lessons in giving the most used anæsthetics as cannot fail to be valued by those called upon to administer them; and such likewise as are not to be gained from ordinary medical works, in which, indeed, the mode of giving those agents, the dangers to be avoided, and the conditions to be looked for, are only incidentally referred to.

The rapidity with which the nitrous oxide is making its way into practice, particularly amongst the dentists, will secure wide attention to the section of the treatise devoted to the consideration of that agent.

To facilitate the understanding of the method of giving nitrous oxide, the author has introduced woodcuts illustrating the instruments employed. Later improvements have been suggested by the ingenious, but the notice of apparatus given is sufficient for the full comprehension of the instructions for administration accompanying it.

Turner's Manual of Diet.[3]

MR. TURNER'S first aspirations in writing this brochure were

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[1] *A Guide to the Examination of the Urine, intended chiefly for Clinical Clerks and Students.* By J. WICKHAM LEGG, M.D., &c. London, 1870. Pp. 89.

[2] *On the Administration of Chloroform and Nitrous Oxide.* By CHARLES SQUAREY, M.B., &c. London, 1869. Pp. 44.

[3] *A Manual of Diet for the Invalid and Dyspeptic, with a few Hints on Nursing.* By DUNCAN TURNER, L.R.C.P., &c. London, 1869.

confined to producing something useful to his own patients, but the benevolence of his heart soon urged him on to make his teachings useful also to the patients of others; for he writes with particular reference to non-professionals, although, as he remarks rightly enough, "the busy, medical practitioner, who so frequently finds his instructions either forgotten or misunderstood, may likewise find the book a help to him."

Chapter I, "On Digestion and Nutrition," is written for the public, and the subject-matter of the book, viz., diet for the sick, is contained in the second and third and fourth chapters. The fifth chapter has no true place in the book; it details the causes, symptoms, and the dietetic and medical treatment of dyspepsia, and puts forth some prescriptions written in English for the edification of dyspeptic patients. There are, indeed, sensible remarks in it on regimen, but the medical instructions concerning dyspepsia are not needed for the public, and are likely to prove, as the like have heretofore done, mischievous to patients by encouraging them to treat their own maladies.

The sixth chapter is occupied with notes on nursing, on the requisites of a nurse, on the casualties of the sick-room, and on some of the ordinary appliances used in cases of sickness. So far as it goes this chapter is well enough, and may be read by the public with advantage. An appendix of receipts for drinks and articles of food for the sick brings the work to a conclusion.

Had the author left out his article on the nature and treatment of dyspepsia, the book would have been more commendable, and he might have had more space to deal with the subject he undertook to teach. We have a notion that too much in the way of teaching the public the features and treatment of disease is undertaken by medical men, and with no real benefit to those to whom it is given.

Knapp on Intraocular Tumours.[1]

We consider Dr. Knapp's work to be one of great value, first, because it treats of a subject that has scarcely received any attention at the hands of ophthalmic surgeons, and secondly, because the conclusions at which he has arrived are capable of very wide application to the development of tumours in general. Considering the various names and descriptions that have been given of intraocular tumours, it is with an agreeable feeling of surprise we find Dr. Knapp stating that, so far as his own observation has extended, ocular tumours would hardly admit of more than two varieties being dis-

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[1] *A Treatise on Intra-ocular Tumours.* By H. Knapp, M.D., late Professor of Ophthalmology and Surgeon to the Ophthalmic Hospital in Heidelberg. Translated by S. Cole, M.D., of Chicago. With 1 chromolithograph and 15 lithographic plates. New York and London, 1869.

tinguished, viz. glioma, originating in the retina; and sarcoma proceeding from the choroid, and being in part unpigmented and in part melanotic. Dr. Knapp's work therefore is naturally divided, into two parts, in which these affections are severally considered.

The plan pursued in both portions is similar, consisting of a section devoted to the reports of cases, which is succeeded by a general description of the two affections. Our remarks will be limited to the latter, though we cannot pass by the former without observing that the history, symptoms, and examination of the tumours are most instructively and admirably detailed; the examination, both with the naked eye and under the microscope, in particular, constituting excellent models, and contrasting strongly with the imperfect descriptions too often met with in English works.

We may just add for the benefit of those who may have the opportunity of examining a few cases of this kind (and they are rare even in the largest clinique) the mode of procedure adopted by Dr. Knapp. Immediately after the removal of the globe, or within as short a time as possible, it should be carefully divided vertically from before backwards into lateral halves, or through the equator into an anterior and posterior half, and a good microscopic description written out. The fluids contained in the globe, together with portions of the several tissues and of the tumour, should be submitted to microscopical examination, and the eye should then be hardened for some days or weeks in Müller's fluid, which consists of a solution, in four ounces of distilled water, of about forty grains of bichromate of potash and fifteen grains of sulphate of soda, which constitutes an admirable preserving and hardening liquid. When thus prepared sections should be made with a sharp razor through all parts, and carefully recorded.

In regard now to the first of Dr. Knapp's two forms of tumour, "Glioma of the Retina," its appearance is that of a soft brain-like substance, which, according to its vascularity, is of a more or less red or yellow tint, and when fully developed presents a number of small cells, separated by a variable quantity of a finely granular or amorphous matrix. The young cells, when examined in the recent state, are about the size of lymph corpuscles, and have a large nucleus provided with one or several nucleoli, or are without any. They have no obvious cell wall, and become contracted when preserved in Müller's fluid. The whole mass is liable to fatty pigmentary, and to calcareous degeneration in the former case, as we have observed in several instances abscesses appearing to be present in it, though true fluid intercellular substance, however, as in pus, being very rarely present.

In regard to the origin of glioma, that it commences as a proliferation of the granules of the granular layers of the retina, the enlargement of these layers occurring at first at the expense of the intergranular layer, the other layers, including the columnar, being for some time preserved; gradually, however, the hyperplasy of the granules becomes so considerable that the various layers of the retina become destroyed, the limitans interna, and the radiating fibres which undergo hypertrophy, offering resistance the longest. The retinal vessels ultimately vanish, and the tumour now vegetates in the space between the choroid and the remains of the detached retina.

The choroid now becomes implicated, and, as Dr. Knapp's observations show, even before the glioma clusters actually come into contact with it. It suffers atrophy, the pigmentary epithelial layer offering the longest resistance. The iris and ciliary processes subsequently undergo the same change. The implication of these tissues clearly takes place in two different modes, both by immediate contact and by dissemination of germs.

The prolification of cells now continues till the globe of the eye is completely filled, and two ways are then open for the exit of the pseudoplasma, the optic nerve, or some part of the fibrous capsule of the eye. "The latter for a long time resists the progress of the growth, but is finally ruptured either through the cornea or the sclerotic. The glioma cells produce a parenchymatous inflammation, with distension and softening, then penetrates into its stroma, separating and absorbing the bundles of connective tissue. Arrived at the outer surface of the capsule of the eye, the growth vegetates, rapidly attacks the tissues round about, ulcerates, crowds the lids asunder and the globe forwards, at the same time penetrating into the depths of the orbit. The further prolification is then only limited by its decay, or by the death of the patient." We must refer our readers to the excellent section on the symptomatology, diagnosis and prognosis of this disease. In regard to its treatment Dr. Knapp thinks that enucleation is decidedly indicated where the disease is unilateral and recognised early, and that even in cases of bilateral glioma we should be fully justified in extirpating both eyes. We have ourselves recently had two cases with all the symptoms of retinal glioma in both eyes, but in both the parents were unable to make up their mind to what of course appeared to them the barbarous treatment of enucleating both eyes. One of these cases has since died, the other is wasting, though the eyes have not presented any marked alterations.

We pass to the consideration of choroidal sarcoma, as given by the author. This form of tumour is much more varied in appearance than retinal glioma, from which, in many instances, it is impossible to distinguish it without the aid of the microscope.

When carefully examined it is found to present cells of various forms, round, elongated, and stellate, imbedded in an intercellular substance, which is traversed by numerous blood vessels, and often infiltrated with abundant pigmentary deposit. The tumour may undergo fatty ossific or amyloid degeneration. The origin of the tumour appears to be either from migrating white corpuscles, or from multiplication of some of the choroidal cells. Its progress is frequently very slow. The treatment to be adopted is unquestionably that of enucleation.

We trust we have said enough to induce every ophthalmic surgeon to purchase Dr. Knapp's treatise, and we are sure that the study of its contents will materially facilitate the diagnosis of these diseases, though unhappily we possess no means of arresting their progress without the removal of the whole of the affected organ.

In conclusion, it only remains to say that the translation, though rather hard and crabbed at times, is very faithful, and sufficiently intelligible. The drawings, as may be surmised, from their having been made by Dr. Knapp himself, are spirited and instructive, and very materially assist in rendering the text perspicuous.

Tuson's Veterinary Pharmacopœia.[1]

MR. TUSON tells us he has written this treatise for veterinary practitioners; but as most medical men devoted to doctoring the human race are more or less "horsy men," and especially so in country districts, the book is likely to be useful also to them.

As the drugs described are those employed also for human beings, medical men will learn nothing from the materia medica portion of the work—from the notice of the composition, characters, tests, and general uses of the several medicinal agents, but they will obtain useful and necessary information relative to the doses to be given to horses and cattle, and also to various stable preparations and the mode of administering them, not found even in the bulky and comprehensive treatise of Pereira.

In the arrangement of his matter, and in the appendices he has introduced, Mr. Tuson appears to have had Mr. Squire's "Companion to the Pharmacopœia" before him, though he makes no reference to it among the works he has consulted.

Sewill on Teeth.[2]

THIS treatise consists of a series of papers that first appeared in the 'Lancet,' and in the 'Journal of Dental Science.' The paper on

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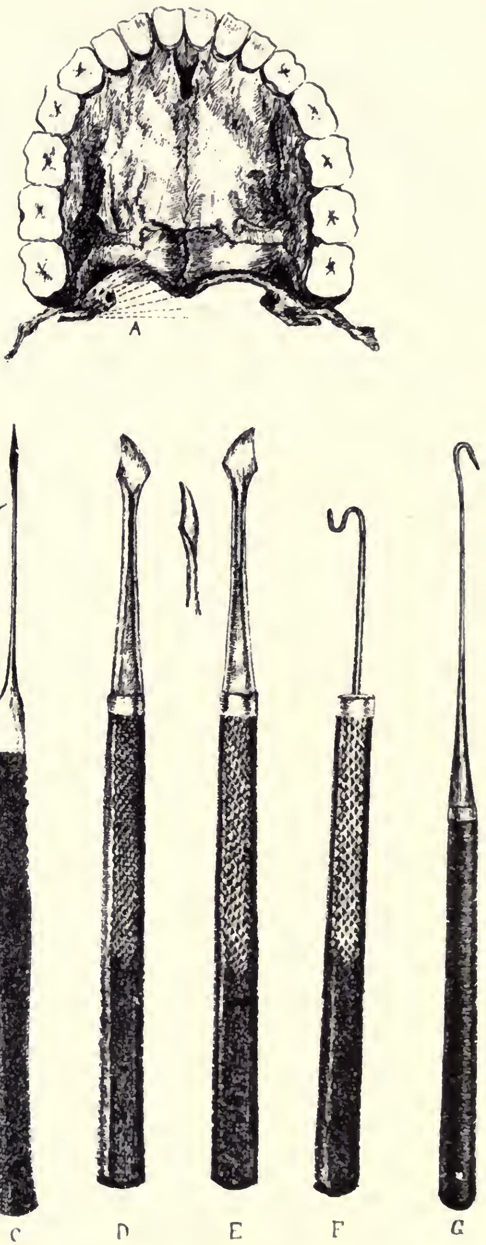
[1] *A Pharmacopœia: including the Outlines of Materia Medica and Therapeutics, for the Use of Practitioners and Students of Veterinary Medicine.* By RICHARD V. TUSON, F.C.S., Professor of Chemistry and Materia Medica at the Royal Veterinary College, &c. London, 1869. Pp. 311.

[2] *Irregularities and Diseases of the Teeth.* By HENRY SEWILL, M.R.C.S., &c. London, 1870. Pp. 66.

irregularities of the teeth is clearly written, and illustrated by woodcuts, and is calculated to be of material assistance to medical men who happen, in the absence of a dentist, to be called upon to deal with such abnormal conditions. Other papers are occupied with showing how much pain and misery in the form of neuralgia may result from bad teeth, and how much good manducatory organs favour digestion; and others, again, in illustrating the dependence at times of necrosis and abscess of maxillary bones, and of ulcers of the tongue upon neglected carious and otherwise damaged teeth. There is no novelty in such teaching, but the little book is well put together, and cannot fail to afford some instruction to its readers.



M^r LAWSON TAIT ON STAPHYLORAPHY.

Photo Lithographed by Whiteman & Bass, London

Original Communications.

I.—On the Treatment of Cleft Palate. By Lawson Tait, L.R.C.S. Ed., M.R.C.S. Eng.; Member of the Surgical Society of Ireland; Fellow of the Medical Society of London; Fellow of the Medico-Chirurgical Society of Edinburgh, &c.

It is not my intention here to speak of the history of the various operations for the varieties of this deformity, as these have been already sufficiently made known; nor do I wish to mention anything except my own experience of the treatment of the condition, with the single exception that I should like to draw attention to two circumstances in connection with its etiology. These are, first, that the deformity seems to be endemic in some localities, cases occurring with extraordinary frequency; while in others it is absolutely unknown.[1] This seems to be the case in different districts in Yorkshire. Secondly the deformity is, invariably in my experience, hereditary. I have never yet seen a patient with cleft palate in whose family, if the family history is known for any distance back, it has not occurred before. Very frequently in the same family of children two or three will have different varieties of cleft. The deformity is often atavic, and I have known it to miss as many as three generations. I mention these facts because I believe they are not sufficiently known, and are not mentioned by the authors I have read.

The most frequent variety of intermaxillary cleft is the simple unilateral harelip. The mesial cleft lip is so rare, that only two specimens are known to exist. The bilateral cleft is, as far as surgical proceeding is concerned, practically the same as the unilateral, save when the premaxillary bones project from the end of the vomer. In this case the operation must be performed at two sittings, the first consisting of the division of the bony attachment of the displaced bones, the paring of their edges and of the corresponding edges of the maxilla, and the insertion of the premaxillaries into the cleft. This has in my hands always been successful. For the after part of the operation, for the union of the lip, a variety of operative measures have been recommended, and most of them I have tried without having found that any of them are, in their results, in the least superior to the old-fashioned incision straight through the lip with a

[1] I understand that nearly all the lion cubs born in the Regent's Park Gardens have cleft palate, and that this is not known to occur in other collections.

slight deviation inwards at the bottom, to form a prolabium, with the removal of plenty of tissue. I have seen very disappointing results when a thin shred of tissue has been removed.

There is one very objectionable result of the operation for harelip which I have endeavoured to obviate, and I believe successfully. I mean the scars left by the needles when used transversely to close the wound. The only difficulty about the operation is with regard to the time during which needles so employed ought to be retained. If removed too soon, the wound is torn open; if removed after the second day, hideous and ineradicable scars are produced.

To avoid these scars I use ordinary sempstress's needles, strong and threaded with a few inches of silver wire double. I introduce each needle through the lip (in the plane of its surfaces), about half an inch from the prolabium, and bring the point out at the middle of the cut surface. I then introduce the needle at the corresponding point of the opposite cut surface, and again bring it out at the root of the ala of the nose. Thus, when both needles are *in sitû* they form a St. Andrew's cross, the point of intersection being in the centre of the wound. The needles are then pushed home up to their eyes, the wires twisted firmly together, the points cut off close to the skin, and the stumps retracted into it. Thus all possible scarring is avoided, save on the mucous surface of the lip, where it cannot be much noticed. I have a modification of this plan to secure a prominent prolabium, but I cannot by description render it very intelligible. The wires are made by it to pass through the prolabium instead of over it.

The next most common variety of intermaxillary cleft is that involving the soft palate, and from a third to two thirds of the hard palate, sometimes with, but more frequently without, harelip. Next in order of frequency is the complete cleft, and least frequent is the simple cleft of the soft palate, with which I have only twice met. It is, perhaps, not strictly correct to include all these varieties under the term *intermaxillary cleft;* but as this is the best name for the complete cleft, and as all the others are but modifications of it in their origins, and in the treatment I propose differ but very slightly, I find it the best term to use for all.

In detailing the method of treatment which I have pursued in cases of complete intermaxillary cleft I shall, with very few exceptions, give all the details necessary for clefts of more limited extent. I must first of all, however, take exception to a statement by my friend Mr. Thomas Smith, in his paper in the fifty-first volume of the 'Medico-Chirurgical Transactions,' to the effect that Sir William Fergusson has brought nearly to perfection the operation for cure of the soft palate by division of the muscles, and that almost as much has been done for the hard palate, so that there is "as little room as need of improvement in the *modus operandi* for the cure of these deformities."

I think it premature to say so much when we know very well that at least half of the children born with extensive clefts die within a few days after birth, from starvation; and when we know also that the improvement effected in speech by the majority of the successful operations is very trifling indeed, and that these successful operations are a small minority of those attempted.

Most unquestionably the greatest step in the development of the operation was the conception of Mason Warren of shifting the muco-periosteum for the purpose of forming a hard palate,[1] or at least an anterior palate. The next step in importance was the successful performance of the operation in infancy by Billroth; and after this the demonstration, I believe by Mr. Thomas Smith, that chloroform can and ought to be used.

The proposal of Sir William Fergusson to divide the muscles was brilliant and ingenious, and when performed in his way certainly effective enough in rendering the flaps passive; but the result has not, in my experience, been any improvement in the voice when the operation has been successful, nor has the operation always resulted in the closure of the cleft, either in my hands, or under the more skilful manipulation of one of Sir William's most able pupils, recently deceased; nor, I understand, does it always succeed, even in the hands of that most expert of British surgeons, the distinguished deviser of the process. After the division of the muscles and contiguous structures by the method alluded to, and also by that of Mr. Pollock, and when the operation has been successful, I have seen the flaps of the soft palate atrophy and almost disappear in less than six months. Fergusson's operation, moreover, is one which I think no one without Sir William's own consummate dexterity would venture to perform on an infant, and the same may be said of Pollock's; and unless we can close the cleft of the soft palate in infancy it will be of little use to attempt it. Finally, Mr. Annandale's cases and my own show that the operation may be performed with the most perfect success without the division of any muscle. I believe I have discovered the reason why a very great number of operations are unsuccessful, and that I have overcome the difficulties. To these I shall again allude.

The success Billroth had in a child aged twenty-eight weeks was due, I am convinced, to his performance of the operation at three sittings. In complete cleft I never do it in less than two, and I am quite certain that many operations are unsuccessful because too much is attempted at once. Experience has taught me that the cleft of the hard palate ought to be closed first, and then that of the lip and soft palate at another operation, not earlier than three weeks after the first.[2] The reasons for this order are that the anterior

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[1] 'New England Quarterly Journal of Medicine and Surgery,' April, 1843.

[2] My reason for delaying the operation on the lip is that if the lip should

palate is of the utmost importance in deglutition, and its formation is the easiest and most certain part of the process. Indeed, I have only once failed in getting it closed, and that was in a case where I did the whole cleft at one sitting, and the whole reopened. A child can feed very well with a cleft lip and a cleft in the posterior palate, so that if the hard palate is closed *immediately after birth*, as I propose in future to do, the operation may be completed at any time within the next six months. I perform this preliminary operation as follows:—With an ordinary scalpel I make an incision not more than three quarters of an inch long, as close as possible to the alveolar processes immediately behind the spot at which will appear (or have already appeared) the canine and premolar teeth; another along the edge of the cleft, nearly as far as the posterior internal angle of the horizontal plate of the palate bone. If one or both of the walls are vertical, I make the inner incision far enough up to allow sufficient breadth of flap to drop down, and in this case I make the incision extend rather further back than if the palate plates are horizontal. Then passing the raspatory proper to the side (D or E in plate)[1] through the incision near the teeth, I lift the mucoperiosteum carefully off the bone, coming close behind the incisor teeth (or where they will be). If the raspatory figured be used, and it be properly made, there is little fear of injuring the anterior palatine arteries. I have repeatedly performed the operation on the dead body, and have always found that with this instrument, even in the fœtus, the vessels are pulled slightly out of their canals, but never torn. Having lifted the flap at the anterior part, it must be cleared as far as the internal incision, care being taken not to go near the posterior palatine arteries. Generally the arch is high, and it will be found that flaps so formed will fall into their positions, but sometimes more traction will be required than would be safe to secure the closure of the aperture, and in such a case the external incision must be carried further back; it must be borne in mind that to do this safely the incision must be made actually *on the alveolar processes* external to the artery. I know an instance where an almost fatal hæmorrhage occurred from the division of a posterior palatine artery. Great care must be taken not to tear the internal edges of the flaps, and if they are not quite free the knife must be used.

I may here say all that is needful about the stitches. In the first place, it is more prudent to put too many in than too few; for if there be any point where the flaps do not come together, especially

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require to be lifted from the subjacent textures, as it frequently does, its removal will interfere with the nutrition of the flap of the anterior palate; and if the operation on the lip is not successful that on the palate is still less likely to be.

[1] The figures in the plate are reduced about one-third in size.

in the soft palate, the chances of the success of the operation are very much lessened, and it is no easy matter to put in an extra stitch after the others are *in sitû*. I use silver wire always, and introduce it directly with the greatest ease, by the use of the two needles figured (B and C in the plate), using them together. One is a fine tubular needle, carrying the wire, and generally held in the right hand. Both flaps are transfixed at the same moment for one stitch, thus securing that the points are exactly opposite. The wire projecting from the tubular needle is at once caught by the notch of the other, and brought out. The insertion of a stitch always, except in the uvula, takes me less time to do than to tell how it is done. I insert the stitches in order from before backwards, and close them in the same order, and am thus enabled to see exactly the effect of each step in the process, and to remedy any mistake, without having to undo work which is not faulty. There is also the advantage in this plan, in opposition to that recommended by Mr. Smith, that there is no risk of the posterior stitches being dragged out by the separation of the flaps during the operation, if they are supported by those already closed anteriorly. It is not long since I witnessed the operation done by a distinguished but not very self-possessed surgeon, who put in the stitch in the uvula—by far the most difficult part of the operation—twice, and twice it was torn out because it was unsupported, and the patient was not under chloroform. If the patient is in a conscious state the dragging is very great, and the absence of anæsthesia has contributed greatly to the want of success of the operations attempted.

I have figured (F) a little double hook which is very useful in arranging the wires, and in passing stitches where there is any peculiar difficulty; the other (G) is useful sometimes in putting on the stitch a point where a little further division by the knife is required.

I never remove the stitches until the fourteenth day.

Having succeeded in closing the anterior palate, it is advisable to wait some time, at least three weeks, to allow the new vascular connections to be well established; and in relation to the statements that bone is developed in the transposed flaps, I may say that I have now five cases where its presence can be determined.

At the second sitting the harelip and posterior cleft may be closed. What I have to say in reference to this second operation on the palate applies equally to all cases where the cleft is limited to the soft palate and posterior third of the hard. There are many cases, however, where the cleft extends far forward, which it will be much safer to treat as if they were complete clefts, and perform two separate operations on them.

Let me here say that while the patient is under chloroform I have found no difficulty in keeping the mouth open by an intelligent assistant using no more complicated apparatus than a strong silver spoon. I seldom have had more than two assistants; frequently, and in some of my most successful cases, I have had only one, as in the case where my friend Dr. Atkinson both administered the chloroform and kept the mouth open. I have long since discarded all gags, and I have an especial objection to such complicated and expensive, though extremely ingenious, mechanism as the gag devised by Mr. Thomas Smith. It is not long since it was broken during the operation by the patient of another surgeon in my presence.

It is confessedly in the treatment of the cleft of the posterior or soft palate that the great difficulties exist in the way of success, and the greatest of these difficulties is a tendency to separation of the flaps. To obviate this we have the various plans for dividing the muscles, and for lateral incisions to relieve tension. The earliest attempt of this kind is one, unfortunately not published, which I have found in the private note-book of my friend Mr. Nunneley, and of so early a date as October, 1841. The case was not, however, successful, apparently owing to the starvation of the patient. The anterior pillar was divided, and the proceeding generally was similar to that recommended in 1843 by Dr. Mason Warren, and recently revived by Mr. Francis Mason. One point noted in Mr. Nunneley's case I wish to draw special attention to, namely, that the only tension seemed to be at the upper suture, opposite the junction of the hard and soft palates. Nearly every author who has written on this subject has noticed this, and all allude to some of their cases having been quite successful, with the exception of a small aperture at this point. My late lamented friend Maurice Collis especially points out this; he says, "Where the hard and soft palates meet there is, as a rule, such deficiency of bone, from the angles being rounded off, as to make it almost impossible to complete the closing of the fissure when the operation is taken at two times. At least one may esteem himself fortunate if there is not a pin-hole or something larger at this spot, which will only close after repeated touches of caustic or tincture of cantharides."[1] In the unsuccessful cases which I have seen in the practice of others and in those which I have had myself, I have observed that, with only a single exception, the separation of the flaps has begun at this point; in fact they very seldom meet at this part, at least in my own cases the flaps did not meet until I had employed the means I shall shortly describe. This point of separation seemed to be the great cause of non-success; for often, after union was satisfactorily completed everywhere else, I have seen ulceration spread from this point slowly along the line of union until everything was undone.

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[1] 'Dublin Journal of Quarterly Science,' 1865. Warren and Pollock also refer to this fact.

I have never been fortunate enough to get a cleft palate to dissect; but in order to discover the cause of this separation I did what was next best—I made some intermaxillary clefts on the subject, performed the operation, and then dissected the specimens. I soon discovered that the cause of the want of union at the point spoken of was the peculiarity of insertion of tendon of the tensor palati, or what I propose to call the circumflex fascia (shown diagramatically at A). This peculiarity is well enough known to anatomists, but its practical importance in the operation of staphyloraphy has been quite overlooked, and what I believe to be an important physiological function belonging to it has been equally missed. After passing round the hamular process, the tendon of the tensor palati, in the normal palate, forms a tough, glistening expansion, which is inserted partly into the lunette of the posterior edge of the horizontal plate of the palate bone, partly into the posterior internal angle of that bone, and the remainder of the fibres are interwoven with those corresponding of the opposite side, and form a fascia of about three eighths of an inch wide in the mesial line. When both muscles are drawn tight this fascia may be felt under the mucous membrane, presenting a distinct edge, and I have little doubt that this fascia and its muscles exert an important influence in the modulations of the voice, acting by increasing the length of the hard palate.

The muscles which Sir William Fergusson divides are the *levator palati, palato-pharyngeus,* and occasionally the palato-glossus. It is with great hesitation that I venture to say that I think the division of any of these muscles unnecessary, with the exception, perhaps, of the last, which is occasionally necessarily divided along with the whole of the anterior pillar, to give more freedom to the flap. I think, however, that I may say this, because since the first case of the kind, operated on by Le Mounier and recorded by Velpeau, many others have been operated on successfully without the division of any muscles. My own cases also have satisfied me that the division of muscles is not only unnecessary, but actually injurious to the after results. Mr. Skey has written, "But while using the knife so extensively (as to divide these muscles), it must be borne in mind that every application of it cuts off a proportionate supply of blood, and thus interferes with the union of the flaps. This important fact has hitherto been overlooked. May not some of the untoward results be dependent on this cause?" I have already said that this proceeding has, in my experience, resulted in atrophy of the flaps after complete union.

My dissections lead me to believe that the *levator palati* has very little effect as a separating agent. Its action is almost directly backwards and upwards, and it may be that its division may rather tend to endanger the recently united flaps by preventing their elevation during the act of swallowing.

In Mr. Pollock's operation the tendon of the *tensor palati* is almost certain to be divided, but in that of Sir W. Fergusson's I think it cannot be. It serves, however, no good purpose to divide it, because the amount of action of the muscle is very limited. The real source of difficulty is the bony insertions of the circumflex fascia. When, in a case of cleft, the horizontal plates of the palate bones are far separated, and when in the operation for the cure of the cleft the stitches are, as they must be, passed through this fascia, no amount of traction will bring the parts of the flaps containing this unyielding structure together, and any division of the tendon between the muscle and the bony attachment of the tendon is not likely to be of much assistance. The only proper course, then, is to effect the dislodgment of the bony insertion; and, fortunately, this may be easily done.

The following operation, as the second stage of the treatment of a complete cleft, or for the treatment of a cleft involving only a small part of the hard palate, has been in my hands invariably successful. The flaps are to be pared from below upwards, especial care being taken that the fork of the cleft be cleanly pared. An incision is then to be made on each alveolar process corresponding to the position of the first and second molars, and the raspatory introduced as far as the fork. From this point the flap is to be raised along the edge of the cleft as far as the posterior internal angle of the horizontal plate of the palate bone. Some difficulty is always experienced in raising the periosteum from the palatal plate of the maxilla, but from the palate bone it is easily stripped; and I need scarcely say that it is of the utmost consequence to keep close to the bone, and not to *button-hole* the flaps—the raspatories, therefore, must not be sharp. Having reached the point indicated, the blunt convex edge of the raspatory is to be used to scrape the tendinous attachment of the muscle off from the semilune of the palate bone; it rises easily along with the periosteum, and in doing this the thin mucous membrane on the superior surface of the velum is to be torn through. This is to be continued until the apex of the hamular process is reached, and in this proceeding it will be found that the muco-periosteum round the posterior palatine vessels will be loosened and raised without injury to those important structures. The point of the raspatory must not be introduced into the canal containing these vessels. The stitches may then be introduced, and, if necessary, the external incision may be continued backwards to the tubercle of the maxilla, and the anterior column be snipped across. In this way the flaps may be made to meet accurately, and a successful result may almost certainly be obtained.

The following brief notes of all my operations may serve to illustrate the foregoing remarks:

A lad, aged 10, on whom I operated without chloroform, and divided the muscles completely after Fergusson's plan, was not benefited in the least. The cleft involved the hard palate to a very slight extent; the wound opened on the third day, beginning at the usual point, and the flaps subsequently atrophied completely.

A girl, aged 14, in whom the cleft involved about one third of the hard palate, was operated on under the influence of chloroform, the muscles divided after Fergusson's plan, and the muco-periosteum lifted to close the cleft in the hard palate. On the third day she ate a lot of half-ripe gooseberries, and succeeded in tearing open the posterior third of the wound. The rest united perfectly; but now, nearly three years after the operation, the soft palate has completely disappeared, and there is not the slightest improvement in the voice.

I operated on a lad, aged 21, with a complete cleft, and closed successfully, at the first sitting, the whole of the cleft in the hard palate, without using chloroform. A month afterwards I operated on the cleft of the soft palate, dividing the muscles of the right side by Sir W. Fergusson's plan, and those of the left by Mr. Pollock's. This operation was completely successful, save the usual pin-hole; but the soft palate has completely disappeared now, two years after the operation. There has not been the slightest improvement in his speech, but his comfort in eating is much increased.

I operated on a cleft of the soft palate in a young lady, dividing the muscles by Mr. Pollock's plan. The operation was successful, but the flaps had quite disappeared within six months after the operation, and the speech was as indistinct as ever.

In the case of a lad, aged 19, on whom I operated without the division of any muscles, and in whom the cleft involved nearly a half of the hard palate, the wound united all but an aperture large enough to admit a cherry-stone, at the usual spot. This hole shows no disposition to close; but now, nearly a year since the operation, there is no atrophy of the flaps, but the voice has not improved in the least.

In the case of a child, aged 10 weeks, with a complete cleft, a patient of Mr. Kemp, of Wakefield, I closed the entire cleft at one sitting, without the division of any muscles and without the use of chloroform. On the sixth day the wound was completely united; but on the tenth a speck of ulceration appeared at the junction of the hard and soft palates, and spread slowly along the line of union, completely backwards and forwards for a third of the hard palate. The failure in this case was due, I think, partly to the performance of the operation without chloroform, but principally to my having done too much at one operation. In a child, aged 13 weeks, with a complete cleft, I succeeded in the first operation in closing the cleft in the hard palate, without the use of chloroform. The second operation, on the soft palate, failed, because it was performed without chloroform.

In the remaining cases I have always kept the patient under chloroform, and have employed the measures described in the body of this note, and I have been invariably and completely successful. In a lad, aged 15, I closed a complete cleft by two operations at six weeks' interval. Five months after the operation the flaps had not atrophied in the least, but the voice had not improved in the slightest degree. He expressed himself very grateful for the increased comfort he had derived from the operation in eating and drinking. I closed a complete cleft in a patient of Mr. Greenwood's, of Ossett, a lad, aged 7, by two operations at a month's interval. The cleft was very wide, and the walls vertical; but now, eight months since the operation, very little trace can be found of the operation, and there is no atrophy of the flaps. The voice, however, has not improved in the least.

In the case of a female child, aged 9 months, in whom the cleft involved a third of the hard palate, I completed the operation at one sitting, with a success so perfect that now, six months after the operation, there is no trace of the operation discernible. In this case my friend Dr. Atkinson was my only assistant, and to his admirable skill in the difficult task of combining the administration of the anæsthetic with the management of the spoon the perfect success of the case is in no small measure due. There have been several cases of intermaxillary cleft in the family of which this child is a member, so that its relatives are perfectly familiar with the peculiarity of the voice in those in whom this deformity exists. They inform me that the child is now beginning to talk, and that no abnormality can be detected in its voice; that it is quite different from the others of their family similarly affected.

Several distinguished surgeons have seen the last two cases, and have been pleased to signify their satisfaction with the results.

Four months ago I successfully operated on a cleft very similar to the last, in a male child, 23 months old. The parents and child have disappeared from the neighbourhood, so that I cannot say what the result has been on the voice.

These are all the cases on which I have operated, and it may be said with some justice that I speak from limited experience; but the success which has attended my efforts in the last four cases has been so marked that I believe I am justified in recommending the process I have adopted. I am, moreover, quite convinced that, if the operation is to have any beneficial influence on the voice, it must be performed during infancy, either before or very soon after the child begins to speak. I had a case lately of an infant in whom I had intended closing the cleft of the anterior palate a few days after birth, but the little unfortunate died of starvation during my un-avoidable absence. I think that we might save many of these children if we could help them to suck by giving them a roof to their mouths.

II.—Othæmatoma, or the Asylum Ear.—By PATRICK NICOL, M.A., M.B., Assistant Medical Officer, Sussex County Asylum; late Clinical Clerk, West Riding Asylum, Wakefield.

OTHÆMATOMA, or sanguineous tumour of the ear of the insane, has at intervals, for a good many years past, attracted a certain amount of attention, both in this country and in Germany. A summary of the facts elicited and the theories built upon them up to a date a few years past is to be found in Fischer's essay, translated by Dr. Arlidge, in the 'Asylum Journal' for 1854, pp. 45—107; and additions are made to these in Dr. Stiff's paper in the 'British and Foreign Medico-Chirurgical Review' of January, 1858.

The account of the hæmatoma given in these papers is briefly this:—It is a tumour varying in size from that of half a bean to that of half a hen's egg, generally of a bluish-red colour, and tender to the touch, affecting commonly the antihelix with the concha and fossa triangularis, and the helix with its fossa. It may rise in from four to six hours, and subside in the course of ten days; but generally it is slower in its course, the walls, and at first especially the outside wall, getting more expanded and thinner for the first six or eight days or even three or four weeks, and then getting gradually thicker and becoming pasty to palpation as the tumour slowly disappears. The whole process, in these severer cases, may occupy four or six weeks, but often requires a much longer time from the slow progress of the absorptive stage. A greater or less degree of thickening and misshapement of the cartilage of the ear remains in almost every case. The contents of the tumour are at first fluid blood, afterwards bloody serum and coagulated blood; in the last stage they may consist of a viscid yellowish fluid or a tough fibrous mass, or the walls may be in apposition. These walls are made up, speaking in a general way, the outer of integument and perichondrium, the inner of integument, perichondrium, and cartilage; but a shreddy separation of the cartilage from the outer layer of perichondrium may have taken place. New cartilage, secreted by the perichondrium, is added to the walls during the last weeks of the complaint. Further, it is stated in the translation of Fischer's essay that the class of lunatics in which the state occurs is "those weak and anæmic, showing bluish patches where touched, having petechiæ, boils, sores, prostration, gangrenous wounds, and bleedings;" and that "the patients are either completely demented or becoming so." The same authority asserts it to be more frequent in men than in women. Two cases are given in the essay where the hæmatoma occurred in sane persons. As to the cause of the lesion, it is stated to be the generally received opinion that some dyscrasia, almost or quite peculiar to the insane, as predisposing, and some external irritation or violence as exciting cause, are the invariable antecedents of its occurrence. Fischer had observed small non-sanguineous tumours in the ear cartilages of cachectic patients, and he believes that such occur in the subjects of hæmatoma, and that effusion of blood into them is rendered very easy by the altered state of the latter in the insane. He thinks also that inflammation has a prominent part in the early stages of the disease. It may be added that the tumour is generally recognised to affect little or not at all the sense of hearing.

In the course of the observations, of which the table given beneath is an abstract, a few facts were brought out bearing on some of the above points.

In one case—of chronic mania, attended with phthisical symptoms—the origin of the tumour in the left ear of the patient was closely observed, the complaint having already advanced considerably in the right ear. Congestion and thickening about the left antihelix were noticed for some days before a distinct swelling could be said to be present; during the eighteen days of further observation an appreciable tumour was present and growing; it expanded most rapidly in the region of the antihelix, but was not clearly circumscribed, and till within about a fortnight of the conclusion of the observations was not evidently accompanied by the presence of fluid, but rather of spongy feel when handled. At the end of the above period distension so as to thin the walls appreciably had not taken place. This case, therefore, exhibited in the tumour of one ear unusual chronicity of the earliest stage—the stage antecedent to thinning of the walls. Meanwhile, the hæmatoma of the right ear continued to increase.

In a second case—of general paralysis with long-continued excitement—where both ears were affected, the tumour of the left ear continued distended, red, and tender throughout the two months during which the patient was under observation; it increased also in size. The nature of the contents possibly accounted for this long duration. The tumour of the right ear, which was smaller, remained stationary.

In each of these cases the contents of one tumour were examined, by puncturing the outer aspect with a grooved needle and collecting the fluid that escaped. In the first case, where the puncture was made eight days after the tumour was evident, blood only escaped; it appeared to be coagulable; under the microscope the corpuscles were seen to form *rouleaux*. On the day after the puncturing the patient burst the opposite ear, at that time much distended; from this a large quantity of coagulable blood escaped, and, as far as was seen, nothing else; the tumour was reduced to half its former size. On the day following it was about two thirds its former size. In the second case, the left ear—the one actively growing—was punctured; a fluid of a dirty cream colour escaped at first; this got, in the end, more bloody, the blood appearing to come from the margins of the wound; under the microscope pus-cells alone were presented in the first portions obtained, in the other specimens pus-cells and blood-corpuscles were seen; the latter did not seem to form *rouleaux* readily.

As to the nature of the patients in whom hæmatoma of the ear was found, it has to be remarked that the descriptions of the physical and mental state of such patients, as given by Dr. Fischer (and as quoted above) did not by any means apply in all their force. Moreover, cases where the patients were in good health at the time of the attack are recorded by Dr. Stiff; and cases of recovery from the mental disease are met with now and again, which would hardly be were the patients invariably "completely demented, or becoming so." There are other reasons for thinking that the mere want of the ordinary cerebro-mental processes—the characteristic feature of pure dementia—as distinguished from morbid changes which may result in it, is not likely to become a cause of such a lesion as hæmatoma, for it is matter of common observation that organic processes often thrive well rather than otherwise when dementia has fairly set in, and, moreover, the same want of cerebro-mental process is present in idiots, among whom hæmatoma is not recorded as in any way usual. Moreover, cases of past hæmatomas have occurred to the writer, where the patients, though decidedly insane, were decidedly not demented, and, if in some danger of becoming so finally, were not, long after the tumours had become absorbed, making any notable progress towards such a cessation of intellectual and emotional life.

If, then, neither prostrate bodily powers, nor the waning and extinction of the light of mind are affirmed by the later cases as invariable accompaniments of hæmatomatous tumours, are there other signs more generally connected with their occurrence? So far as bodily powers go, we must probably be content, while refusing to acknowledge that extensive weakening or vitiation of the vegetative processes is invariably present, to allow the existence of some special weakness such as is indicated by the term "blood dyscrasia." Hardly otherwise is it possible to account for the occurrence of so definite a lesion from external causes, which must be rather varied; besides that the restriction (almost total) of the complaint to one class of patients encourages the conclusion.

To answer the question as regards mental symptoms from Fischer's numerous cases is now impossible; but there is one circumstance, present, no doubt, in many of his cases, and possibly in most or all of them, which comes forward in the later cases with at least sufficient frequency to excite inquiry. This is the occurrence of general excitement. The two cases spoken of above were both instances of prolonged excitement, and the tables given below show how large a share this state had in the mental ailments of most of the subjects of hæmatoma there noted. Nor is this surprising. The state of general excitement is one which, from its steady course when present, its habits of recurrence, and its behaviour under drugs, seems to be connected intimately with a disturbance of the vegetative processes of the body. Not like monomania, where but a small part of the cerebral machinery is involved, and that only in respect of processes so removed from material observation that their manifestations would be designated by most, even in the present day, as purely mental, nor like melancholia, in which the primary lesion, however much its influence may permeate the whole conduct, seems to be of the same most airily material nature—unlike these, general excitement, whether occurring in the maniac, the paralytic, the epileptic, or the idiot, is a state which, once set on, seems, from its equal appearance in whatever act takes place, in obedience to whatever internal or external stimulus, from its equal persistence in all acts, and from the amount of vegetative disturbance generally observable even by ordinary means during its course, to owe its advent to some general and comparatively coarse alteration in the vital processes of the parts concerned, such as the "general diseases" of the physician exemplify for the body generally, and its progress to the steady course of a definite and extensive pathological change. To take advantage of the analogy just mentioned, general excitement seems to constitute the fever of the sensory and hemispherical ganglia.

Such being the case, we need not wonder if the presence of this state should influence in some degree the nutrition of the body generally, nor if its past occurrence should be stamped lastingly upon the finer activities of the tissues. Its sufficiency as a *vera causa* of nutritive dyscrasia seems, from the above considerations, tolerably well established. And further, it might be alleged that the superficial parts of the face and head are those which are likely, if we except the nervous tissue itself, to be in the most lively way affected, for both by vascular and by nervous distribution these parts are very closely connected with the intracranial ganglia.

The state of general excitement, therefore, might be accepted as a possible considerable element in the cerebro-mental factor, which goes with some influence of a mechanical sort to make up the joint result—the hæmatoma. The mechanical factor is not to be overlooked, for of late years, with the amelioration of the surroundings of the insane, the sanguineous tumour has got rather scarce than otherwise. The ear is a part which benefits in quite a peculiar, if a limited way by such amelioration. During the hours of sleep, or at least of recumbency, while the head is supported on one of its sides, as it generally is for considerable periods with most sleepers, and, moreover, frequently on one side more than another, the pinna of one ear, and more especially the antihelix, the part generally first affected, is pressed steadily between the mastoid part of the temporal bone and whatever happens to be beneath. A certain amount of derangement for the time of the processes of nutrition in the part must ensue, as well as reaction when the pressure is removed. With persons in ordinary health neither of these processes is so energetic as to cause any evident structural change in the part; but with the presence of some dyscrasia of the sort already discussed, and with pressure equal to a large part of the weight of the head reacting from a stiff pillow upon the delicate and peculiar tissues of the pinna, we can hardly wonder that, in a patient suffering from prolonged mental disorder, serious structural lesions should result in the part. No doubt other and very various injuries and irritations of the external ear assist in individual cases, but this fact of pressure is present in every case.

It seems, therefore, that the elements most probably constant among the antecedents of hæmatoma are a dyscrasia causally connected with general excitement, and some external irritation; and that the latter factor is sufficiently supplied in almost every case by the unfavorable position of the ear during the hours of recumbency.

It need hardly be pointed out that the great variability within its own range of the physical factor—varying, as it does, with the shape of the ear, of the skull, the mode of wearing the hair, the usual decubitus, the accommodation of the patient, and so on—accounts for the inconstancy of this tumour, even in cases of excitement. In respect of natural and artificial covering to the external ear, women have generally a considerable advantage over men, and this, taken along with the fact of the exemption of women in a great degree from one disease attended with frequent paroxysms of excitement, viz. general paralysis of the insane, may serve to explain the comparative infrequency of hæmatoma with the female sex.

Before concluding these remarks on the causes of hæmatoma in the insane, it is interesting to observe that the only two cases of this tumour occurring in sane persons, which the writer has seen mentioned along with a hint as to their mental state, are those given by Fischer, one that of a "very ignorant and *wild*" girl, the other that of a man "who had *delirium tremens*, and was, therefore, transferred to an asylum for treatment."

As for the immediate pathological antecedent of these tumours, according to Fischer's account of them, viz. the formation of cavities in the pinna—easily distinguished by palpation—into which the sanguineous effusion takes place, and thus forms a definite tumour, not a diffused infiltration, it may be remarked that careful and repeated palpation was exercised on the right ear of the first patient mentioned above, without the discovery of any such lesion, and that the course of the disease in that case rather indicated a diffused and indefinite primary seat than one definitely bounded, and, as it were, prepared for the further progress of the morbid process.

The tables given below have been drawn up for the purpose of showing roughly the frequency of the complaint in the present day. It will be seen that only nine patients out of about three hundred of the more seriously insane in the male wards of the Wakefield asylum, were found to have, or to have had hæmatoma, five coming under the former and four under the latter head. Of the five mentioned, only two had tumours in a thoroughly active state.

In examining for these tumours attention was drawn to other aural deformities, and all such were noted as showed fairly evident signs of not being congenital; but necessarily a certain amount of indefiniteness must attach to the results connected with these affections, since they have neither fixed names nor very definite marks. The method of examination employed was inspection, aided in all noteworthy cases by palpation.

Stage when observed.	Source of cases.	Nature of cases.	Bodily health at time of hæmatoma, or as far as known.
Tumour absorbed.	Cases in West Riding Asylum, Yorkshire.	1. Chronic mania; prolonged excitement. 2. Chronic mania.[1] 3. Idiocy, with frequent excitement. 4. Mania; two prolonged attacks.	All these patients rather weakly, some more so than others, but all able to get about.
Tumour present.		5. Chronic mania. 6. Epilepsy; excitement. 7. General paralysis; prolonged excitement. 8. Epilepsy; no history of excitement.[2]	
	Published cases of Dr. Stiff.	1. Dementia, acute, traumatic; "conduct generally tranquil." 2. Confirmed mania. 3. Mania. 4. Acute mania.	"Bodily health good." "Not remarkable." "Robust health." General health bad.
	Cases in St. Luke's (Dr. Arlidge in notes to Dr. Fischer's essay).	1. Mania, with restlessness. 2. Mania, with constant restlessness.	Great debility in both cases.
	Case communicated by Dr. Arlidge per letter.	Recurrent mania; recovery; "restless and noisy."	In good bodily health.
	Cases in Perth County Asylum, Murthly (from Mr. Cruikshank)	1. Chronic mania, with excitement. 2. Chronic mania, with excitement and incoherence	"Both in robust physical health."
	Maudsley's 'Mental Physiology and Pathology,' p. 398.	Acute melancholia; "much excited."	
Tumour incipient.	Cases in Hayward's Heath Asylum, Sussex.	1. Epilepsy; excitement. 2. Mania; arachnitis. 3. Prolonged acute mania.	Moderately good. Weak. Very weak; aortic disease present.

[1] The word 'mania' is understood to imply a certain amount of excitement.
[2] In one of the Wakefield cases the history is not at present accessible.

Number of ward.	Character of patients in ward.	Number examined.	Number abnormal.	Character of abnormality. Hæmatoma. Present.	Hæmatoma. Past.[1]	Fossæ more or less obliterated in one ear.[1]	Appearance of contraction throughout one pinna.	Congestion and slight thickening of one pinna.	Other deviations from the normal.
18	Weak, ailing, and demented patients	49	8	1	3	3	1		
2	"Suicidal" Ward	63	7	1	...	...	1	2	Outward deflexion of top of helix in one ear 2; Remains of erysipelas 1
4	Undefined	36	3	1	...	...	1	...	Both cartilages abnormally thick 1
1	"Refractory" Ward	36	3	1	...	1	1		
14	Undefined	57	2	...	1	...	1		
Totals in non-working wards[2] . .		241	23	4	4	4	5	2	4
35	......	46	1	1					
6	......	40							

NOTE.—Only two women, subjects of hæmatoma, could be found in the female wards, out of about seven hundred patients.

[1] 'Past hæmatoma' is distinguished by the great irregularities of the cartilage from mere 'obliteration of fossæ.'

[2] The examination of the working patients was accomplished only in wards 35 and 6; the other results are therefore summed up irrespectively of a fragmentary result which would complicate them.

III.—Surgical Dressings; remarks on some of the Methods of dressing Wounds which are in use at the present time. By Wm. Fairlie Clarke, F.R.C.S., Surgeon to the West London Hospital, &c.

The various methods of dressing wounds is a subject which must always have a great interest for the surgeon. It is a matter which meets him at every turn of his professional career; and the influence it has upon the welfare of his patients and the success of his operations is incalculable. And yet the most diverse opinions have been held, the most opposite practice has prevailed, upon such an elementary and such an important matter as this. The present generation of surgeons have seen the most violent changes. Many now living can remember when it was the custom to cover up wounds with two or three layers of different substances, so that a mass of materials was heaped upon the raw surface. This complicated system was followed by a reaction, and it was recommended to leave wounds entirely open and uncovered; and then the pendulum seemed to settle between the two extremes, and it became the custom to lay a few folds of lint dipped in water over the wound, enough to promote a little warmth and moisture without being uncleanly or burdensome to the patient. And this would seem to be a reasonable practice. It has its advantages, and accordingly it has obtained a wide-spread currency, and has held its ground for many years.

It is very instructive to read a paper which was written by Mr. Syme in the 'Edinburgh Medical and Surgical Journal' in the year 1825, in which that distinguished surgeon pointed out the inconvenience, and disadvantages of the old method—the method of mundifying, digesting, incarning and cicatrizing applications,—and recommends that the edges of incised wounds should be brought together with stitches, and that they should be very simply and lightly covered. The previous practice had been to bind the lips of the wound together with adhesive plaster, and then apply many and complicated dressings and bandages, and these were, as a rule, allowed to remain three or four days before they were changed. The result was that union by the first intention was rare, and the formation of abscesses was extremely common. It was mainly to the exertions of Mr. Syme, Mr. Liston, and others, zealously seconded as they were by succeeding surgeons, that we are indebted for the great improvements which have been made in the methods of dressing wounds. But, though they have been so much improved they are still far from being perfect. At least, the difference of opinion which prevails at this moment upon the subject seems to indicate that we are still unable to point to any one method which is so satisfactory that it must of necessity supersede all others.

It is interesting to observe that at the close of the paper to which we have referred, Mr. Syme expresses his conviction that if his advice were to be followed union by the first intention would become an ordinary occurrence, at any rate, in the case of simple incised wounds. Since that time Mr. Syme has had a large practice, and his opinions have always carried great weight not only with his many pupils but also with surgeons at large. But have his hopes been realised? Has union by the first intention become an ordinary occurrence? We fear not. Though our methods of dressing wounds are as simple, as cleanly, as little irksome to the patient as possible, yet it is seldom that we can reckon upon union by the first intention with any degree of certainty.

It may, therefore, not be a lost labour to consider how we stand in respect to this matter of dressing wounds, what methods are in vogue at the present time, and what advantages each seems to offer.

When the old fashion of plastering and bandaging up wounds was superseded, "water-dressing" took its place; that is to say, the wound was closed by means of sutures, and then a few folds of lint dipped in water were laid over it. These were then fixed by strips of plaster or lightly held by a roller. Such a dressing is very simple and cleanly, but it has this disadvantage, namely, that unless it is kept constantly wet it soon becomes dry and hard, and adheres to the edges of the wound, so that when it has to be removed it may cause the patient no small pain. If, however, a piece of thin gutta percha or oiled silk or calico is laid over the wet lint, it will retain its moisture for a long time, and this is the way in which "water-dressing" ought always to be used. In this form it makes an admirable application for a wound. It is cleanly, it is soothing, it promotes granulation, and it gives no pain in removal. It acts, in fact, something in the same way as a poultice, only its effect is milder. It keeps up a degree of warmth and moisture which is very agreeable to the patient's feelings, and which assists the natural processes of repair. "Water-dressing" is a method which is now in very general use, and it has such advantages that it will probably long continue the favorite plan with medical men; and, indeed, if it were always used, as it ought to be, with a covering of some impermeable material, it is probable that nothing simpler or handier could be employed for the generality of cases.

But the results which have been obtained under the use of water dressing do not satisfy some surgeons, and it has been asked whether other and better modes of dressing cannot be devised. The faults which may be found with it are these: that it does not seem to increase the chances of union by the first intention; that it rather provokes suppuration, and, as a consequence, it does not help us to get rid of erysipelas, pyæmia, and other hospital scourges.

Thus it has come to pass that, though water-dressing is at the present moment in very general use, other methods are also being tried; of these we shall only notice two, because they are the most notable, and because they are so diametrically opposed to each other that something may be learned from the contrast, and because they include between them the whole range of the subject. These two methods are the plan adopted by Professor Humphry of Cambridge, who leaves all wounds, as far as possible, entirely uncovered; and the plan recommended by Professor Lister, of Edinburgh, and which is known as the antiseptic method of dressing.

And, first, with regard to the plan adopted by Professor Humphry. It is a matter of common observation that no wounds heal so readily as those about the face. The incision which is made in the case of hare-lip, almost invariably unites by the first intention, and leaves as slight a scar as possible. The same may be said of other wounds about the face: for example, the incision made for removal of the upper jaw. Whence then does it arise that these wounds heal so favorably? Various reasons may be assigned for the fact. The parts are highly supplied with blood. The incision can be brought together almost throughout its entire depth, and there is a free escape for the discharge. And, be it observed, these wounds which do so well are left almost entirely without dressings. Sometimes collodion is painted over them, but as a general rule they are left altogether exposed. Their very situation makes it difficult to keep dressings upon them, so that they are of necessity open to the air and to the secretion of the mouth; and yet these are the wounds which heal more rapidly and more satisfactorily than any others that can be mentioned. It is not wonderful, then, that some surgeons should say let us leave all wounds uncovered and exposed to the air in a similar way; and this is the plan which has been carried out by Professor Humphry. I believe it is the practice of this distinguished surgeon to treat all suitable cases in this way until he sees some special reason for changing his method. This plan is as simple as possible, and it saves the patient from all the pain and discomfort which are caused by changing the dressings. What can be better than merely to lay a stump upon a pillow in a position which is agreeable to the feelings of the patient and to allow it to remain there untouched until the healing process is complete? What can be simpler? What less irksome to the sufferer? And, in many instances, I have no doubt this plan has been found to answer all the hopes and expectations of the surgeon, and that many wounds in different parts of the body have healed as kindly as the generality of wounds about the face do under similar treatment. But whatever advantages it may have it does not prevent suppuration. In a paper upon excision of the knee-joint which Professor Humphry has lately published in the 'Transactions of the Medical and Chirurgical

Society,' he speaks of the free and continued discharge which took place in several of his cases, and which told prejudicially against the patient. And not only is suppuration an evil in itself; not only does it weaken the patient and retard his recovery, but when there are many discharging wounds in a ward, we are more apt to see an outbreak of erysipelas or pyæmia than we are at other times. It is, therefore, very desirable to limit suppuration or, if possible, to prevent it altogether. Can this be done? And if so, by what means? This is the problem that Professor Lister has set himself to solve, and that with no small success. It is now several years since he first addressed himself to this inquiry, and, perhaps, some estimate of the value of his methods may now be formed.

We shall not attempt in a paper like this to give any detailed account of the theory upon which Professor Lister's practice is based. Suffice it to say that he believes that putrefactive suppuration is due to the contact between the organic germs which are floating in the atmosphere and open sores. If, therefore, we can filter out these germs and exclude them altogether from touching raw surfaces, we shall be able to prevent suppuration, or, at any rate, to reduce it within very narrow limits. But how is this to be done? Professor Lister replies—by means of carbolic acid. Carbolic acid, even in weak solutions, has the power of destroying the organic particles which abound in the air, so that, if we can keep the lips of the wound or its raw surfaces constantly covered by a solution of carbolic acid, we shall be able to exclude all organic germs, and thus prevent suppuration. The experiments of Pasteur and those which have been made by Professor Lister himself go to prove the truth of the germ theory; and some recent investigations of Professor Tyndal upon the nature of the solid particles in the atmosphere tend in the same direction. There is much to be said in favour of this theory: but whether it be true or false, the practice which has been founded upon it seems to me likely to mark an era in the progress of surgery.

There are two ways in which carbolic acid may be used, and in both I believe that it is a valuable addition to our resources. It may be employed as a disinfectant and depurative lotion, just as Condy's fluid or any other similar compound may be used. Or it may be employed with the professed object of excluding all organic germs which can possibly find their way to the wound, and with all those precautions which constitute the "antiseptic dressing."

When it is used merely as a disinfectant, we are not, of course, to expect the full effect which Professor Lister has claimed for it; and much prejudice, I am sure, has been done to his views by confounding cases in which the carbolic acid has been thus applied with those in which the antiseptic dressing, as he recommends it, has been fully and thoroughly carried out. But even as a simple lotion, it is wonderful how much it will do to limit suppuration; and this of itself is no slight matter, for it carries some important advantages in its train. If all the wounds in a hospital were to be dressed with a disinfectant lotion, as the rule of the house, I believe we should hear less than we now do of erysipelas, pyæmia, and gangrene. But the faith in carbolic acid must become more general than it is before we are likely to see any such rule applied upon a large scale.

The other way in which carbolic acid may be used is with all the precautions which constitute the "antiseptic dressing;" that is to say, with the intention of destroying any organic germs that may already have settled upon the broken surface, and of preventing the access of others. The way in which this should be done, must vary according to the nature and circumstances of each particular case. But I believe the surgeon who has at hand two or three solutions of carbolic acid of different strengths, and some of the plasters recommended by Mr. Lister, will find no great difficulty in adapting his means so as to carry out the system efficiently. The minute care and attention which the distinguished Edinburgh professor bestows upon his cases is beyond all praise, and something, no doubt, of his success must be attributed to this cause. Without care and attention it is impossible to carry out his principles, or to give his system a fair trial. But let no one hesitate to try it on this account. The methods of applying the antiseptic dressing have of late been much simplified and improved; and if for the first few days after the commencement of treatment the dressings have to be changed every day and with considerable precaution; yet the time soon arrives when the discharge is so trifling that they may be allowed to remain undisturbed for several days, and then the dressing becomes a much less troublesome thing than it is under the ordinary methods.

But what are the advantages which the antiseptic dressing offers us, and is it worth an effort to obtain them? The advantages which it offers we have already implied; it would reduce suppuration to a minimum, and it would probably rid us to a great extent of those diseases which are the pests of our hospitals and the bane of surgical practice. Moreover, if we felt sure that we had in our hands the means of controlling these evils, we should be able to do more by the knife than we now can, and operative surgery would take a stride in advance. There is, too, another advantage which has struck me, and that is the comparative absence of irritative fever in patients treated by this method. In several cases that I have seen, in which under ordinary dressings one would have expected some constitutional irritation, the total absence of it was very marked. Indeed, it would seem that the power which carbolic acid has of limiting cell growth and suppuration removes also the febrile symptoms which are apt to attend the rapid prolification of cells.

The local action being restrained, there is proportionably less constitutional disturbance. If on a more extended trial these advantages should be found to be real, antiseptic dressings will be a most important weapon in our hand. Every surgeon has seen sad cases in which patients appear to have *suppurated to death*, where nothing seemed to be so much wanted as the means of arresting the profuse discharge of pus. But if, under Mr. Lister's guidance, we can boldly open large abscesses, with the certainty that we can control or prevent the discharge of pus, we shall have made a most important advance in surgery. All wounds, or, at all events, all wounds suitable for antiseptic dressing, would thus be reduced to something like the position of subcutaneous wounds. The broad line of demarcation between simple and compound fractures long ago attested the important difference between injuries which are and injuries which are not open to the air. But it was not till the early part of this century that surgeons advanced a step farther and learnt that many operations may be performed subcutaneously, and that so long as the aperture made is small, and the amount of air admitted is trifling, no fear of suppuration need be entertained. And the advantages which antiseptic dressing appears to offer us carry the same benefits still farther, and seem in a manner to convert all wounds into subcutaneous ones. Whether it be by destroying organic germs, as Professor Lister holds, or by limiting cell-growth, or in some other way, it seems at present impossible to determine, but the fact remains that wounds treated by this method approximate, both in their phenomena and in their results, to those which are subcutaneous. And here I may say that it appears to me unwise to link the use of carbolic acid with the germ theory in such a way that they stand or fall together. The germ theory is *only* a theory, and its most strenuous advocates would probably admit that it is still far from being established. But carbolic acid has taken its place in the 'Pharmacopœia;' it is everywhere recognised as a remedial agent of high value, and it would not drop out of use even if the germ theory were proved to be fallacious. Seeing, then, that the germ theory is open to a question, while the value of carbolic acid is undoubted, would it not be better to keep the two things apart, and to examine the germ theory on its own merits, and at the same time to try the carbolised lotions, plasters, and ligatures, as fully and thoroughly as Mr. Lister recommends. In saying this, I believe I express the opinion of many besides myself. As far as I have been able to judge, there is a growing feeling among surgeons in favour of the use of carbolic acid, according to the plans and with the precautions laid down by the distinguished Edinburgh professor. But the theory upon which these plans is founded is quite another thing, and I am not at all sure that it is gaining ground among us. It seems, therefore, a great pity that a doubtful theory should be a drag upon the acceptance of an admirable mode of dressing wounds, for I am sure that those who have given the antiseptic method the fullest trial will be among the first to acknowledge its value. To this suggestion I apprehend Mr. Lister would reply that it is only because he believes in the germ theory that he takes so many minute precautions, and that if the theory were abandoned, the minute precautions would very soon be abandoned also. It may be so; but I cannot help thinking it would be better, notwithstanding, to separate the germ theory from the antiseptic dressing, and to advocate the thorough and complete use of carbolic acid for its own sake, in the same way that chloride-of-zinc lotions have been used elsewhere. Perhaps the time has hardly arrived yet for us to draw any statistical conclusions, but by and bye I hope it may be possible to compare on a large scale the results obtained by the use of carbolised dressings with other modes of treating wounds. One would like to know whether there is a marked decrease of pyæmia, erysipelas, and gangrene; whether the mortality after any selected class of cases, as, for example, amputations in the leg, is less than it is when ordinary dressings are employed; whether compound fractures leave the hospital earlier than under other modes of treatment, and so forth. I believe myself that, as I have said before, there would be found a notable decrease of hospital scourges if carbolic acid lotions were used as a general rule, and for this reason, if for no other: there would be a diminution of the death-rate after operations. But these are points upon which one would like to know the verdict given by collecting a large number of cases. Mr. Lister has lately furnished us with some information on this subject, and the tendency of it is most satisfactory. In some recent numbers of the 'Lancet' he has told us that during the three years in which he carried out the antiseptic treatment at Glasgow, previous to his removal to Edinburgh, the cases of erysipelas and pyæmia which occurred were extremely few and far between, and that although his wards were situated in a most unfavorable position in close proximity to an old graveyard. If we had more such testimony it would go far to establish the use of carbolic acid. If antiseptic dressing were to be tried on a large scale, and if it were found to yield results similar to those with which Mr. Lister has furnished us, it would be hard to gainsay its great practical value; and it is such statistics as these that we hope we may have at no very distant date, and by them the verdict must be pronounced upon the antiseptic method of dressing wounds.

But though the term "antiseptic dressing" has come to be used almost entirely with reference to Mr. Lister's method, he is not the only surgeon who has been endeavouring to arrest suppuration and to promote the safe and rapid closure of wounds. The minds of many other men in the profession have been turned in the same direction, and experiments of various kinds have been made. But nowhere has more attention been paid to this subject than at the Middlesex Hospital, and great credit is due to Mr. Moore, Mr. De Morgan, and other members of the surgical staff, for the additions they have made to our means of dealing with wounds. Mr. De Morgan, in particular, has made some admirable suggestions, and has carried them out with great skill and perseverance. In the year 1866 he wrote a paper in this Review, in which he recommended strong solutions of chloride of zinc as a dressing for wounds. The evidence which he adduces in its favour is very strong, and I believe that increased experience only serves to confirm the opinion of its value which he expressed four years ago. Of late, he has used solutions of sulphurous acid, and they promise to be a satisfactory application for wounds. In these trials of various solutions which he has made, I am not aware that he has proceeded upon any preconceived theory; and herein I cannot but think that he has acted wisely. If the germ theory be true, and if it be the access of organic germs which makes all the difference between open and closed wounds, between compound and simple fractures, then it is possible that solutions of chloride of zinc or of sulphurous acid may act beneficially by destroying these germs. But while the germ theory is still *sub judice,* while it is still uncertain what it is that constitutes the difference between wounds which are open to the air and those which are not, it seems to me wisest to try a variety of lotions and dressings empirically, using them for their own sake alone, to test the value they may have, and allowing them to stand or fall by their own merits. The chloride of zinc lotion seems to offer two chief advantages; it is highly caustic, so that, when it is used in malignant cases, it destroys any particles of cancerous matter which may remain in the wound after operation; and it forms a coagulum on the surface, which seals the open vessels, and appears to prevent the formation of pus and the absorption of putrid material. Mr. De Morgan's experience, as related in the paper to which we have already referred, is, that the caustic solution acts only upon a very thin layer of the raw surface, that it gives rise to little or no irritation, and that wounds which have been dressed with it heal rapidly and with hardly any suppuration; while, I believe, under its use erysipelas and pyæmia have become rare and infrequent at the Middlesex Hospital. Of the sulphurous acid lotion I can only say that it promises well, and that it deserves to be thoroughly tried; but I am not aware that any detailed account of the results which have been obtained with it has yet been made public.

In these days, when so much attention has been turned to the subject of putrefactive suppuration, it is not wonderful that unguents should have fallen somewhat into disfavour, for they are apt to become rancid, and thus give rise to very offensive discharges. Nevertheless, ointments have this great advantage, they do not dry and adhere to the edges of the wound. All the watery solutions are liable to this drawback. They must either be kept constantly moist or else they stick to the raw surface, and when they have to be removed they lacerate the tender granulations, and give the patients great pain. In some cases this may be avoided by mixing a small quantity of glycerine with the lotion; but nothing acts so efficiently as an ointment, nothing serves so well to soften the skin and to prevent the lint from adhering to the wound; and if it seems desirable to adopt this mode of dressing, there can be no objection, provided that it is frequently changed and the part bathed with warm water.

I cannot conclude this short paper upon some of the modes of surgical dressing most in vogue at the present day without saying that it appears to me that there is nothing we want so much as the ready and complete means of arresting hæmorrhage. If we could do this, if we could within a reasonable time after an operation, and before the patient was removed from the couch, thoroughly arrest the bleeding so as to produce clean and dry surfaces, I believe this would do more to promote immediate union than anything else. We should then get rid of the clots of blood which are so apt to accumulate in wounds, and which not only prevent their closure by acting like a foreign body and keeping the lips apart, but also break down and increase the amount of putrescent discharge. And we should have dry clean surfaces upon which lymph—the natural medium of repair—would be rapidly poured out, and which would put the wound in the best possible position for immediate union.

Much as one must approve of the various efforts which are being made to improve the character of dressings, I cannot but think that what we want still more are styptics, and that if we could find means of completely and rapidly arresting hæmorrhage, we should find also the best means of bringing about the safe and speedy closure of wounds.

IV.—An Inquiry into the Real Nature of Hysteria.

By D. DE BERDT HOVELL, F.R.C.S.E., &c.

(*Continued from* vol. xlv, *p.* 211.)

THE more this subject is investigated the further we are led away from the interpretation usually put upon it, and the more we are obliged to inquire into conditions and circumstances which appear at first sight to be foreign to it. Side by side with these we have to reconsider the erroneous views which have only too often represented as fictitious what has proved to be, in fact, a sad reality. We thus come gradually to see that much of the difficulty with which the subject is beset arises not so much from the intrinsic difficulty as from the wrong interpretation which misapprehension and misconception have placed upon it, and that the disease represents, by a wrong name, not so much an imaginary complaint on the part of the patient, as a phase of medical opinion which is unsound and untenable.

Some remarks on cases of purposeless or motiveless malingering appeared in a recent number of the 'British Medical Journal,' Jan. 1st, 1870, in which it was distinctly stated that they were "almost invariably of the class known as hysterical." In other words, that "*they were of the female sex, arrived at puberty and unmarried*," and the morbid conditions of the disease were attributed "to vanity, the love of attention, the desire to attract sympathy, and, perhaps," it was added, "the sheer pleasure of deception." These were followed the next week by "Remarks on Feigned or Hysterical Diseases of the Skin," in which it was said that "hysteria was essentially an imitative disease," and although the morbid condition of system from which it originated was "chiefly confined to the softer sex, *the result, probably, of reflex action connected with the organs of reproduction*, yet there were rare instances of similar *feignings* in the male, though these were for the most part examples of malingering."

The pathology and psychology of these remarks appear to be as loose and incorrect as the wording which expresses them. An imaginary reflex action—reflex may be presumed to mean indirect, the hypothesis of direct action being inadmissible—of the organs of reproduction in the female is seriously represented as a substantial explanation of similar feignings and purposeless malingerings in the male; or, in other words, the malingering of a private soldier has its origin in the reflex action of his organs of reproduction, because similar conditions in the female have, by a purely gratuitous assumption, been designated hysterical. Again, in the medical evidence given before the Court of Common Pleas, Dec. 5th and 7th, 1869, in the case of a railway accident, the absurdity of connecting the hypothesis of uterine irritation with physical shock is even still more plainly demonstrated. A physician—names are intentionally omitted because it is opinions, not individuals, that are sought to be contradicted—is reported to have said, "that the vomiting and *sexual symptoms* were also consistent with there having been a shock; and another physician, that there was no symptom present that could not be accounted for by hysteria."

The medical profession never appears in a more unfavorable light than in the contradictory and opposite views which it takes of the effects of railway accidents. Putting out of the question the possibility of medical opinion being suborned for purposes of interest, it is certainly remarkable that one man should stand up in the witness-box and ignore, deny, and refuse to recognise the validity of the very symptoms which other members of the same profession have come into the same court of justice to advocate and maintain. Such a state of things can only be compatible with imperfect knowledge and experience of the subject: but that not only fancifulness, purposeless malingering, &c., but *sexual* symptoms should be brought on by collision in a railway, results which George Stephenson most undoubtedly never dreamt of, requires some further proof than mere assertion. Through this tissue of random hypotheses and illogical deductions runs one reliable thread, which affords a clue to a fair inference. It is admitted that the shock of a railway accident produces the morbid state under consideration, and it cannot be denied that the same condition is the result of moral shock; we are thus brought to the conclusion that the results of physical and moral shock find the same mode of expression in the same physical effects upon the system; and that not only does moral shock affect the physical system, but that physical shock affects the moral condition. This pathological fact is the more remarkable inasmuch as Mr. Le Gros Clark considers the sympathetic system to be mainly affected by physical shock; and there are many reasons for concluding that moral shock finds its expression on the physical frame through the same portion of the nervous system. That moral shock should be followed by loss of moral power does not require any great stretch of the mental powers to admit, or that the power of the will should be infringed by loss of moral control; but we are not altogether prepared to find that these consequences appertain also to physical shock, although less frequently and in less degree, and it may be for the most part, in persons who are in some measure predisposed to it. Still the fact remains that so it is, and some physiological explanation is demanded if it be only an hypothetical one.

The integrity of the nerve centres and the right discharge of their functions depend upon a proper supply of healthy blood, and its normal circulation. If the circulation of the nerve centres be affected by any failure or disturbance of the vaso-moter nerves, upon which its regulation depends, it seems to follow as a necessary consequence that their nutrition and integrity of function will be impaired also.

If the faculty of volition be so much higher than excito-motory action and simple motion as the psychical condition of man places him above the mollusc and the brute, we must expect that it will partake of the higher qualities with which he is endowed, and that it is capable of improvement by cultivation and of deterioration by neglect. Dr. H. Maudsley remarks, "we have as physiologists to deal with volition as a function of the supreme centres following reflection, varying in quantity and quality as its cause varies; strengthened by education and exercise, enfeebled by disuse, decaying with decay of structure, and always needing for its outward expression the educated agency of the subordinate motor centres. We have to deal with Will, not as a simple undecomposable faculty, unaffected by bodily relations, but as a result of organic changes in the supreme centres, affected as seriously and as certainly by disorder of them as our motor faculties are by disorder of their centres." It is thus evident that not only the material quality of volition which directs our bodily functions, but the immaterial quality of Will which directs our bodily as well as mental and moral powers, is affected by our physical as well as by our moral condition.

In order to work out this subject we must once more depart from the beaten track of physiology into the higher region of metaphysical principles, and trace Will through the principle of Hope up to the still higher principle of Truth, Reason through the principle of Faith to the still higher principle of Justice, and human Sympathy through the principle of Charity to the still higher principle of Mercy; and having arrived at the three great principles which appertain to a state of being far beyond the capabilities of man's nature—for administration in perfect accordance with these high principles is a matter of such intense difficulty that it at once becomes the special attribute of Infinite Power—proceed to inquire how far departure from these principles in the concerns of life is attended with not only negative but positive evil, and proves to be instrumental in bringing about the morbid state under investigation.

The career of Cardinal Wolsey, which has been alluded to before, was not directed by the higher principles of right, but indicative of that inconsiderate and greedy selfishness, that utter disregard of the rights and feelings of others so long as the attainments of his own objects was advanced, which belong to a class of characters "whose heads are acute and active as their hearts are hard and callous; of a diseased intellectual activity with an almost perfect indifference to moral good and evil," of not only the love of power and position, but of what the Italians call "il affeto di signiori reggiare," the love of dominion over others, a lust for rule without consideration for the benefit of those who are sought to be ruled, save so far at least as it contributes to their own ends. It is true that the results of these wrong principles of action recoiled eventually on his own head, but a vast deal of mischief and unhappiness must have been created meanwhile. If Queen Katherine, one of those whose feelings and interests he disregarded so long as they were an obstacle or a footstool to his own ambition, had not possessed a high spirit and great determination in the first instance, as well as perfect resignation in the second, she might have been an example of the class of case under consideration. It is so usual to keep in view and regard the prominent object, and to lose sight of the subordinate effects, that we are apt not to think of the evil produced until it is plainly brought under notice. Thus many are apt to regard the character of Hamlet too much by itself without reference to attendant circumstances. The alteration in his dress and demeanour are looked upon simply as indications of madness, and essays have been written to prove that he was or was not mad, but we might say with Polonius, though it may be in another sense,

> "I am sorry that, with better heed and judgment,
> I had not quoted him."

He, too, in many respects, represents the state which is sought to be elucidated. Not only is his conduct eccentric, but he lacks decisive action; he does many things that are imprudent, yet "he is more sinned against than sinning." Consider how not only his feelings and affections, but how violently his whole moral nature and his principles have been shocked. He has the desire to do what is right, but he lacks the power to carry his designs into execution. The tyranny of power is too much for him, he cannot act straight, so he is constrained to pursue a tortuous policy and act indirectly; "there is a lion in his path," not the passive lion of indolence and sloth, but the raging lion of tyranny and power. He is not a motiveless and purposeless malingerer, and no sound reasoning can make him so; he is rather an example that "surely oppression maketh a wise man mad."

Malingering, in point of fact, indicates an unwillingness to submit to arbitrary power, which, however necessary it may be deemed, is not altogether just. It implies also a preference of certain disabilities in accordance with its principles of action, to the enjoyment of advantages with the infringement of those principles. It involves a certain dereliction from duty, but it is by no means indicative of vice. It is a very inconvenient form of insubordination. In all cases it implies the existence of something in the background which does not appear at first sight, of something which, possibly, is intentionally kept out of sight. It would be very unscientific to treat the effect without reference to the cause, the subject to the exclusion of the object.

So far we have discovered loss of physical and moral power, want of physical and moral control, alteration of conduct and general demeanour, imperfect volition and impaired will. We do not find that the intellectual powers are affected so much as that they are diverted from the even tenor of their way; we find that the conduct of the patient is not intentionally perverse, but perverted by coercion. We have seen how the conduct of eminent persons, rulers of the people, is obnoxious to these circumstances; we come now to consider that of ordinary mortals.

"But now, it seems, some unseen monster lays
His vast and filthy hands upon my will,
Wrenching it backward into his, and spoils
My bliss in being; and it was not great."

We will adopt the poetical so far as the condensation and terseness of expression represents the condition, but decline to accept the figurative interpretation. We recognise that the patient is, according to transatlantic phraseology, "in a difficulty," from which she cannot extricate herself. We must next inquire how far the representation of this condition is confirmed by the experience of practice.

Case I.—A gentleman about sixty years of age, of stout build, but active habits, "bearded like the pard," but of exceedingly sensitive temperament, who had been engaged for many years in a large manufacturing business, of gouty diathesis, suffered from dyspepsia attended with excessive flatulence, palpitation, and disturbed action of heart. His symptoms being in excess of the apparent physical cause, I inquired whether he had had any cause of anxiety, which question he evaded. On Monday, the 29th November, I received an urgent summons to see him, and found him walking about the room, muttering to himself, and apparently unconscious. I learned that the Wednesday before he had received a letter which greatly annoyed him, and he became much depressed in spirits. On the Saturday he suffered much from want of breath, and the next day from violent retching. On the Monday following, the day he sent for me, he was most anxious to answer the letter, but too unwell to finish it. He left the room, and his wife following him upstairs found him groping about, apparently not knowing what he was doing, and she was only just in time to save a sudden fall. He looked wildly about and talked incoherently. As there was no longer any doubt as to the cause or nature of the case, I requested one of the family to write to the relative with whom the difference lay, to ask him to call on me without delay. He very properly came immediately. I declined to enter into any particulars, but said, that not only the health but the life of the patient was at stake, and that if anything happened, the responsibility would lie with those who refused or neglected to settle the existing difference. This led to some temporary arrangement, and the patient was partially relieved, he became more composed, but was weak and depressed on the following days. On Thursday gout appeared, in consequence of which he refused to take his usual meals. On Friday he took two or three glasses of champagne with a friend, after which he appeared to be quite giddy and begged to be taken to bed. He became gradually worse on Saturday, and on Sunday began to be unconscious, his limbs became quite rigid, he was bedewed with profuse perspiration, and apparently in pain. He also suffered from distressing hiccough which lasted nearly seven days. He remained unconscious for about five days, during which time he lay like a log, with all the dead weight of an inanimate object; swallowing and the other functions of life were performed automatically. On the Thursday morning he suddenly exclaimed, "I have the cramp," and recovered consciousness. During this period the pupil of the eye was fixed, but neither dilated nor contracted; he frequently put his fingers to his lips, apparently to indicate that he wanted nourishment, but he said afterwards that he did so unconsciously and was not sensible of anything that was done for him, neither could he be roused to consciousness. I felt at times very doubtful whether he would not pass into the profound insensibility of apoplexy. In a woman this state would have been termed hysterical coma.

The matter in dispute in this case was not of great moment. His wife said that he would not concede the point in question, *because he considered it to be his right;* in other words, he would not sacrifice principle to expediency. Thus the abstract explanation of this case may be regarded as an infringement of first principles causing psychical depression, to which irritation was added in the form of continued opposition. One thing is certain, that after the removal of the irritating and depressing causes the patient recovered a better and stronger condition of health than he had enjoyed for some time.

CASE II.—A lady, twenty-six years of age, had considerable hæmorrhage eight days after her confinement, and she continued weak and out of health for several months; she attributed her ill health and want of tone to fright and terror on having been pursued by a rough fellow in the street seven or eight years before. The subsequent illness and death of her mother had been a cause of much anxiety and grief to her. She had consulted Scanzoni, whose first remark was, "This young lady has had some severe shock to her nervous system." She was a person of cultivated mind, sound sense, and excellent judgment, and herself frequently deprecated her nervous condition. On one occasion she said, "I am quite aware that it is a want of physical strength which prevents my having the perfect control over myself which I ought to have, and do possess at other times." On another occasion when I ventured to remonstrate with her because she fretted, she replied, "Do you not see that I only fret because I am not well; if you, as my doctor, will enable me to regain strength, I shall no longer fret." Her sense of helplessness and want of support were at times very great, and I told her that in order to get well it was necessary that she should cultivate the self-reliance which she had lost. She saw the truth of the remark and acted upon it; moreover, she afterwards thanked me for my advice. But from time to time she continued to ask for medical assistance to improve her physical power, as a necessary step toward the recovery of her moral tone. This case I regard as essentially psychical or emotional, the paresis of fright having impaired her nerve centres, and lowered both physical and moral tone as well as that of will and determination, and this in a person of naturally sound endowment.

Case III.—Mrs. ——, aged sixty, married, but without family. Has suffered from hysteria for forty-four years, during which time she has had much ill health. She says that she is often surprised at herself, because she feels at one time so different a creature from what she is at another. She does not wonder that others do not understand her, because she does not understand herself. Sometimes in the morning she is unable to walk, or bear the slightest sound or movement, and in the evening she feels comparatively well, the disability having passed away. On one occasion when undergoing a minor operation on the uterus, which was regarded as the fons et origo malorum, she began to speak French, which she often did when under the influence of hysteria. She could not control herself or prevent it, and what was remarkable she could not speak half so fluently in that language at another time. This case may be regarded as one in which the peculiar form of neurosis, commonly designated hysteria, but which I do not know how better to describe than as emotional neurosis or susceptibility, was indigenous. It first manifested itself at the age of sixteen, and recurred at frequent intervals up to that of sixty. It has been the bane of her existence. This case brings to an issue the question of the origin of this disease in uterine or ovarian irritation. The patient suffered from both of these in conjunction with hysteria, but were they cause and effect? They may have induced it, did they produce it? Is uterine irritation the cause, or is hysteria produced by various forms of irritation acting on a susceptible condition? We know that at certain ages there is an erethism of the nervous system, the precise molecular condition of which has not been ascertained. It has been said "mulier propter uterum," why not mulier propter hysteriam? If men become at times hysterical, is it not because they possess a susceptibility of nervous system which is allied to the feminine? Treating of this subject, the late Dr. Addision said, "Hysteria, is the result of uterine irritation, but I cannot explain the modus operandi." This implies weakness of argument. I think we have a right to demand from the advocates of this hypothesis as complete an anatomical explanation as that which Mr. Hilton has given of pain in the shoulder indicating hepatic disturbance, but I venture to think that this explanation will never be forthcoming, but "like the untimely fruit of a woman, it will not see the sun." In the meanwhile there is much collateral experience which tends to disprove it. I called the other day to see an old lady, eighty years of age, who was suffering from gout, and her servant told me that her mistress had been *quite low and hysterical.* It is rather out of date to look out for uterine irritation in a great-grandmother! A lady whose husband is abroad suffers from attacks of vomiting and the paroxysm commonly designated hysteria. "I know you will laugh at me," she said the other day, "the mail is in, and there is no letter for me." Another, expecting her son home from a long voyage, became agitated and hysterical at hearing of frequent shipwrecks, but the uterus was not at fault. In the case more particularly under consideration the attack came on whilst undergoing some operation on the uterus. How many different operations are performed upon both uterus and ovaries without the production of this bugbear? I am not aware that hysteria is an attribute of prostitutes, or of vicious persons generally, but there seems to be a rather prurient taste for a suppositious morbid anatomy of these cases with some persons. Again, by the report of this patient, the attack came on frequently in the morning and subsided in the course of the day. On investigation I was generally able to trace it to some excitement or over fatigue, which afforded a very reasonable explanation. But uterine irritation does not come on and go off like a thunderstorm. The treatment consisted, not in attention to the uterus, but in promoting rest, and the absence of exciting circumstances. Iron as a tonic was also of great service. I submit that these three cases were treated successfully upon sound principles, and were relieved not by accident, but by design, and these particulars are published with the consent of those patients.

Hysteria, then, is a pseudonym as regards the origin of the disease from the womb, and a false metonym so far as it assumes or pretends that uterine disease is the type of disorder from which it arises. It is a disordered condition of the nervous system which has two essential features, 1, a condition of depression, and 2, emotional susceptibility. This may be produced in the first instance by shock or emotion; it may be induced by depressing physical influence, as fatigue; or it may be caused by the joint effect of both. But depressed nerve power and emotional susceptibility do not suffice to produce all the symptoms which are included in the state. In addition to these there is generally some source of irritation. Irritation alone, or depression alone, does not suffice to produce the state, but a combination of the two is necessary. One reason why treatment has so often proved ineffectual, is that both these causes must be removed in due order to make it effectual, and the distinction between the two must be carefully made out before this can be done properly. Another bar to success has been that not only has the condition not been rightly understood, but it has been altogether misinterpreted. A third reason is, that the actual condition as the effect of the causes which really produce it has hitherto been very imperfectly investigated. It is necessary therefore to consider not only the condition of paresis, which is an essential element, but also the great susceptibility of that state to irritation, from the various causes which act upon it; whence the protean character of hysteria.

The general effects of shock upon the system have at all times been evident, the precise results are even now far from palpable. The state of collapse clearly indicates the suspension of some faculties and functions to a greater or less extent, the interruption and cessation of the power which appertains to them and regulates them. Some molecular change disturbing integrity of structure is, no doubt, the direct effect of moral as well as physical shock. It is not too much to say that impaired power has not always been sufficiently recognised as the result of an accident, and this remark applies with even more force to moral shock. In both instances, there is loss of power, and, what is very important to bear in mind, the recovery of power thus lost is not *immediate* but *deferred* for a shorter or longer period of time.

The contingent effect of moral shock is to cause more or less physical disability; on the other hand, a low condition of physical power tends also to produce susceptibility to emotional influences. Patients so affected are no longer upheld by strength, energy, and that vigour of will which is closely associated with a sound state of health, but kept prostrate by listlessness, irresolution, and indecision, subject to all the evils attendant on sluggish action and atonic function. Exercise brings fatigue instead of strength, and slight exertion is followed by exhaustion, because the stock of nerve power is low and imperfectly renewed, consequently there is no reaction.

1. This condition of exhaustion, though in itself *indolent, i.e.*, not suffering, is highly susceptible of irritation, and must be carefully distinguished from,

2, The causes of irritation which act upon it.

Patients so affected are not always in pain, but pain is readily produced; the sense and appreciation of pain are also heightened. We have no difficulty in recognising on the part of the sensor nerves, a general condition of hyperæsthesia, cutaneous or other, as consequent on this state of things, whether it assume the form of pseudo-peritonitis, inguino-cutaneous or submammary pain, head-ache, or chest-ache; *without the necessity of imputing a desire to exaggerate painful sensations.* Neither is it fictitious because the sense of pain is superseded by diverting the attention, or by instituting some decisive action or other. Or, on the other hand, that the motor nerves should be correspondingly affected, that dragging of the limbs, akinesia from paresis, should be associated with various forms of disordered motility, spasm, palpitation, &c., *without any occasion to impute a morbid craving for sympathy.* We need not be surprised if inattention to the wants of the bladder, coupled with the copious secretion of urine which attends emotions affecting the vaso-motor nerves of the kidney, should cause over-distension of the bladder and inability to evacuate its contents, *but we need not presuppose prurient motives.* In short, that various forms of irritation should be set up by various causes, mental and moral, as well as physical. Reflex paralysis, or rather paresis, is a depressed condition, a low state or ebb of nerve power acted on by some source of irritation; it may be produced by physical or moral causes; and the source of irritation may be physical, scybala for instance; or moral, in the case of worry. The right indications for treatment are to remove the cause of irritation and raise power above the condition of susceptibility to irritation; in the moral case, this is most effectually done by improving volition and restoring the power of the will. The causes producing this state are not single, be it observed, but compound; depression alone, or irritation alone, will not suffice to produce it, but the joint effect of these two is necessary; and for the cause of irritation to be able to act, there must be the susceptible condition on which it can act. Not only have these symptoms not been sufficiently investigated from this point of view, but there is also a difficulty on the part of those who are in rude health rightly to estimate the infirmities of the weak. "I am afraid," said a lady to me the other day, "that being in strong health myself, I cannot properly estimate my sister's condition of weakness and ill health." And there was much truth in the remark. Neither could she understand the length of time that some of these cases take to recover. "I am quite sure," she said on another occasion, "that my sister has had time enough *now*, and that she could do more if she liked;" and so she went on teazing the invalid and urging her to do more than she had the power to accomplish, and thus, with the best intention in the world, perpetually marring and deferring her amendment. There is a marked distinction to be drawn between the mere state of inertia which requires to be roused to action, and the condition of ill health and low power which is incapable of much exertion. It also becomes necessary to consider the indolent state with reference to the susceptible condition which appertains to it and is contingent upon it. There is the greatest possible difference between the dormant and quiescent stage of neurosis, with its liability to provocation from irritating causes, and the state of excitement which ensues when that liability has been called into play. There is no more apparent relation between the two than between the mere physical appearance of gun-cotton and its explosive properties. In the case of epilepsy this liability has been termed, by Dr. Russell Reynolds, "a readiness to take on perverted action." The perverted action of epilepsy, the hysterical paroxysm, the "mad spinal cord" of Mr. Paget, are all marked instances of the excited or active stage of neurosis. So in the case of pain, the indolent stage is not one of actual pain, but simply of liability and susceptibility of pain, and the amount of pain is not to be estimated by the trifling causes which provoke it, but rather by the exalted sensibility of the patient; the degree of pain by the appreciation of it; and it must not be forgotten that the very circumstances which create this susceptibility to pain, tend to lessen rather than increase the power to bear it.

The susceptibility to pain induced by emotional neurosis is a fact of great importance, which is not only not generally recognised but frequently ignored. Recently I attended a lady in her confinement whose pain was literally anguish. She said that for the last two months of her pregnancy she had not a single night's rest, in consequence of pain which compelled her to get up and walk about the room. She attributed the pain, which did not recur after delivery, and which she had not suffered on former occasions, to having been "upset" by the discovery of the pregnancy of a confidential servant. She was a cheerful, good-tempered person, wholly free from any tendency to make frequent or exaggerated complaints. But the evil did not end here—her baby, otherwise healthy, was exceedingly irritable and restless, at night especially; this condition was relieved by small doses of bromide of potassium.

"We know not how the bones do grow in the womb of her that is with child." But I regard the above as a very remarkable pathological fact, especially in connection with the effects of emotions in the vaso-motory system. This is not the only instance I have met with. Similar irritability even to a greater extent, and continuing to the seventh year, was remarkable in a boy otherwise healthy, whose mother had great anxiety during her pregnacy with him in consequence of the illness and death of one of her children; more than this, the mother suppressed her feelings and did not give them expression, and the boy in question has a difficulty in articulation, and speaks to this day a lingo of his own. His elder brothers and sisters talk quite plain; those younger than himself have followed his peculiarity of both speech and dialect.

The hypochondriac may be regarded as the "mauvais sujet" of medical practice, but his confessions, quoted above from a reliable source, give a picture of acute suffering which few would be so bold or so unfeeling as to call in question. He is not altogether a grumbler or a complainer, he regards his illness in the light of an affliction. Moreover he fully and highly appreciates the ministrations of a kind and skilful physician. But he denounces in language of no mean severity the conduct of others. We have all heard of the patience of Job, and he points out a similarity between the intensity of his own sufferings and the aggravation of those of his great prototype, in that both were caused by the misconduct of those who came to see him under the plea of affording relief, and took advantage of the opportunity to aggravate his complaint. It should not be forgotten that the friends of Job, his comforters as they are ironically and proverbially styled, did not scruple persistently to point out that his sufferings were the result of his own faults and wickedness, and we all remember that in the end Job was justified, and his officious monitors reproved.

But the "opprobium" of medicine is that "mauvaise sujette" the hysterical patient, for her sufferings there is no consideration, or toleration, they are hardly even listened to. She may crave for sympathy, but she does not get it.

But we must not altogether forget the young lady who, having lain eight years in bed, became quite well in three months. Assuming, for the sake of argument, that her case was genuine, but not rightly apprehended by her medical attendants, it will not be amiss to consider not only the disappointment but the irritation caused by unsuccessful treatment. Petty worry, long continued, is capable of becoming almost malignant in its effects, and the sense of helplessness which she was very liable to feel, which has been so wrongly interpreted into *a desire to attract sympathy*, must have been frequently sadly increased by the absence of sound opinion. I cannot imagine anything much more aggravating than for a patient who is simply conscious of illness and pain to be visited by a succession of medical men who ignore her complaint. Their manner must show to some extent the tenor of their opinion. A lady, about forty-eight years of age, the mother of several children, is at the present time under my care. She has been the round of the metropolitan physicians, and her report is "that none of them said much to her, no one did her any good, and that after a time they all *appeared as if they wanted to get rid of her.*" It would be difficult to invent a greater satire on the medical treatment of a complaining and suffering patient.

It has been asserted erroneously that I advocate the treatment of these cases with pity; it is rather with the "little help that is worth a deal of pity." Enough has been said to show how much depends upon the point of view from which a case is regarded and the construction and interpretation that is put upon it. Although the explanation here offered is extremely simple, yet it was by no means easy to arrive at it, but the difficulty lay, not so much in the intrinsic difficulty of the subject as in the extrinsic difficulty arising from the mass of, shall I say, prejudice and error with which it was enveloped. The main difference in the plan pursued from that of ordinary practice consists in this, that I regard the complaints of the patient is valid, and not as the idle tale which it is usually considered to be, and so looked upon as unworthy of belief. Thus, instead of fancifulness, wilfulness, and obstinacy, I have come to the conclusion that *patient endurance* is a frequent characteristic of this class of case. I have not found these objectionable qualities where they were reputed to be in the condition of the patient, I have found them in the persistent refusal to take a rational view which is so characteristic of the prejudiced mind. In other words, not in the hysterical (?) patient, but in the hysterical hypothesis.

Women are not only mortal, but have many mortal imperfections; they are naturally subordinate to man, and have less independent self-reliance. When not supported by high principles they more easily become desperate. The condition of irritated neurosis may be developed in them to any extent. The more reason why they should be treated justly and reasonably, which they frequently are not. It must not be inferred that I seek to justify or excuse the alterations of conduct and deviations from moral rectitude which ensue under such circumstances because I endeavour to explain them. The main point of which I desire to secure recognition is, that the imperfections of character and conduct which, it cannot be denied, form one phase of this morbid condition, are not intentional or primary so much as induced by the operation of adverse circumstances, and as it were secondary. To speak physiologically, they are not the product of volition, but of excito-motory action. But whether the result of deliberate intention, or provocative irritation, they alike require treatment; medical relief, not judicial condemnation.

Least of all should it be an attribute of the medical profession that it not only neglects to relieve but casts unjust imputations on those who seek its aid. I seek not only to remove unworthy and unjust reproach from the softer sex, but also from the harder materials of my own. Enough has been said to show that this class of case requires a clearer perception, more sound reason and judgment, and better feeling, but also the aid of the higher immaterial principles which so greatly influence our material or physical condition. I desire simply to see the physician approach his patient not to aggravate her distress, but to bring relief; not to reproach, but to give confidence; and, remembering that speech, especially as regards words of comfort, is silvern, and that silence, especially that which covers a reproach, is golden; to hear him utter, not the ambiguous phrases of uncertain hypothesis, but the clear opinion of knowledge and experience; not in a "spirit of fear, but of power and a strong mind," like the chief

"Whose voice was heard around
Loud as a trumpet, with a silver sound."

V.—Laws affecting the Public Health in England. By H. W. Rumsey, M.D., Member of the General Medical Council, &c.

A sketch, if it be only a *catalogue raisonné*, of the statutes which apply more or less directly to the health, either of the whole population, or of large classes and communities, may be of some use at a time when many, in and out of the medical profession, are expecting the final Report of the Royal Sanitary Commission, and may be reminded, not unprofitably, of the great extent of the field over which the commission has to travel, even under the single head of law. It is not my object to touch, except incidentally, upon other details of this vast inquiry, such as organization, administration, results, and indications for future legislation. Nor does the summary to which I limit myself in this paper pretend to be complete.

I have divided the laws herein described into three great classes. (1) *Special and Municipal*, having for their direct object the improvement of the public health in towns and districts. (2) *Social and Industrial*, having other primary objects, yet determining in many respects the methods of sanitary administration. And (3) *Medical and Dietetic*, applying to the profession itself, to particular diseases, and to other matters of state medicine.

I. (1) *Public Health Acts.*—Almost all details of local government bear (and some of them very closely) on the sanitation of the people; but special health-legislation for towns may be said to date from what was called by an euphemism, 'The Public Health Act of 1848,' constituting a General Board of Health, at the head of which was appointed in 1854 a President, with a seat in Parliament. This first step in direct sanitary legislation at all events recognized a great principle, although the titles 'Public Health Act' and 'General Board of Health' were misnomers. It is needless to recur to the controversies which led in ten years to the abolition of the Board of Health, and of almost all central control over local action. The Local Government Act, and the Public Health Act of 1858, established new dynasties; the function of medical inquiry and advice being transferred to the Privy Council, with Mr. Simon as its medical officer, while the little which remained of the function of control in matters of town-improvement was administered by the legal and engineering authorities of the Local Government Act Office, under the Home Secretary; the real responsibility being thrown upon popularly elected bodies in any towns or detached districts which might choose to adopt the Act.

The appointment of a Medical Officer of Health is optional with local authorities under this Act, and indeed unconditional, save that he is to be 'a legally qualified medical practitioner.' It is fortunate that in the present unsettled *status* of this office, the appointment is merely permissive.

(2) *Local Acts.*—The Statute Book is laden with a multitude of laws, each applying only to a particular city or town, and some towns delighting in a series of such enactments. Manchester has twenty-two of these Acts. The earlier Local Acts were obtained by boroughs under the *Municipal Corporations Act*. Some of these laws appear to have been employed in the construction of the Public Health Act. Local Acts of later date have either amended, or supplemented, or abated provisions in the general Acts, according to the circumstances, habits, and feelings of the popular leaders of the place. It has not been denied, even by their warmest advocates, that, with all their presumable advantages, they have complicated, and needlessly diversified, sanitary law and administration throughout the provinces.

The Metropolis Management Act of 1855 is altogether exceptional in its character and general outlines. That vast area, containing one-seventh of the population of England, is divided into districts, each governed by a vestry or elected Board, and all, except the old city, represented in a superior Metropolitan Board. This peculiar constitution may soon perhaps be superseded by an improved municipal organization for the whole of London.

The Local Acts of Liverpool, Manchester, Birmingham, Newcastle, Oldham, and other large towns, are notable examples of a method of legislation which I believe to be peculiar to this kingdom. It is curious to observe how almost every learned town clerk, examined by the commission, describes the Act of his own place, not only as specially adapted to its particular conditions, but as a model of general imitation. The Metropolitan and the Liverpool Acts, and perhaps others, define briefly the duties of the medical Officer of Health, and compel his appointment, though without any conditions for his independence.

(3) *Water Supply Acts.*—The law of public water-supply is contained principally in the general Acts already noticed, but it is modified by various local Acts, and seriously so by the private Acts of self-constituted water monopolies. These companies, it may be admitted, have often been the means of providing the inhabitants of towns with a prime necessary of life, but they have, nevertheless, as often stood in the way of comprehensive measures of local improvement, and have sometimes inflicted serious damage on the public health.

(4) *Nuisance Removal Acts.*—A series of these, commencing as temporary measures in 1845, developing into a permanent law in 1855, and amended by subsequent Acts, are now in force through-

out the kingdom; so, however, as not to affect the provisions of local Acts, or to interfere with jurisdictions conferred by the general Acts. These enactments, and notably the *Diseases Prevention Act* of 1855, are public health laws in reality. The latter, for the prevention of epidemic, endemic, and contagious diseases, is a purely medico-sanitary enactment, administered by authority of Privy Council. It may be put in force everywhere, but only by 'Orders in Council,' the local authorities being Boards of Guardians. Neither this, nor the Nuisance Acts, can be properly executed except under medical direction.

The legal meaning of the word 'Nuisance' was for some time very unreasonably limited, in striking contrast with the view taken by Mr. Simon, who has already been the means of extending the legal application of the term from mere annoyances to most sources of real harm. He would include under the designation every *nocumentum*, every removable or avoidable cause of injury to public health.

(5) *The Smoke Nuisance Abatement Act*, 1853, and the *Alkali Works Act*, 1863, are specimens of 'piecemeal legislation' against noxious trades. They may however be treated by courtesy as special applications of the principle of nuisance-prohibition to particular cases. Although they are now administered on different principles, there is no good reason why they might not form parts of a uniform law, by which special regulations might be applied to insalubrious occupations in populous districts, under various conditions, as in the *Code Napoléon*.

(6) *Sewage Utilization and Sanitary Acts.*—In 1865 commenced a new series of enactments. The object seemed to be, at first, to check that enormous pollution of rivers and streams which followed upon the earlier stages of the sanitary movement, and before the dangers to health and life of fouled water-courses were generally known or properly appreciated by sanitary authorities. From the evidence under consideration, this frightful abuse appears to be the common practice in the great majority of English towns, districts, and villages, and to be steadily advancing with the growth of manufactures and the extension of the water-closet system.

The *Sewage Utilization Act* of 1865 was followed by the *Sanitary Act* of 1866, and their amending Acts of 1867 and 1868. These laws have established some important principles, which had been but imperfectly enunciated in former enactments; but they have immensely complicated local administration by creating a new class of rival sanitary authorities in the country, and by promoting the formation, on arbitrary grounds, of small 'Special Drainage Districts,' under independent management.

(7) The *Dwellings* of the people are controlled, though very partially and imperfectly, by another group of laws. The regulation

of new habitations in great towns, and the prohibition of certain cellar dwellings, are provided for, partly under general or local acts, and partly by bye-laws; but there is no general *Buildings Act* for the whole country. The Public Health Act led to the enactment in 1851 of a most beneficial law for the inspection and well ordering of *Common Lodging Houses.* This, with a contemporaneous permissive Act for the establishment of *Lodging Houses for the Labouring Classes* in populous districts, and their amending Acts, may be taken as the second stage to house-legislation. The third —soon we hope to be advanced on sound principles—began with the *Artizans' and Labourers' Dwellings Act* of 1868, a well intended, but hitherto almost inoperative, method of substituting healthy homes for the fever-nests of closely packed towns. An officer of health must be appointed by this Act in all towns having 10,000 inhabitants. But he need not be a medical man, and this absurd provision would not therefore interfere with a future organization of superior medical officers of health.

Intimately connected with the habitation-laws are the *Public Baths and Washhouses Acts,* still permissive.

(8) To complete this division of sanitary legislation, several *Consolidation Acts* must be noticed. These preceded the Public Health Act, and are of technical utility for incorporating general provisions into special or local enactments. Such are the *Lands' Clauses, Markets' and Fairs' Clauses, Towns' Improvement Clauses,* and *Towns' Police Clauses Acts.*

To this category might be added various laws relating to rating, assessment, audit of accounts, and especially those concerning the advance of public money to local authorities by the Loan Commissioners for the execution of public works.

II. I come now to groups of laws which for the most part apply to the general frame-work of society, and are not primarily or exclusively directed to the improvement of the public health.

But in their working and results, and in relation to 'areas proper to be controlled by local authorities,' they are becoming more and more important to sanitary regulation.

(1.) The 'New Poor Law'—as it was called in 1834, and its long succession of amendment acts, may well head this class, for, under its first administration, it became the parent of direct sanitary legislation. By these laws, a vast system of *medical relief for the poor* was brought under uniform regulations. The whole of England and Wales was divided, in the course of a few years, into parochial unions, which in 1860 were employed as districts for the *removal of nuisances.*

(2.) The Act for the *Registration of Births, Deaths, and Marriages,* 1837, owes to the Poor Law Amendment Act a system of territorial divisions, now used not merely for the control and relief of destitution and for the care of the sick poor, but also for recording the facts of mortality and reproduction, and the reported causes of deaths. Those acts have thus supplied a machinery for the collection of vital statistics, which may be utilized to a very great extent for scientific inquiry and agency in legal and preventive medicine. The scientific value of the statistical record is due to the long and able superintendence of Dr. Farr.

(3.) The *Census Acts*, of which one has to be passed afresh for every single enumeration of the people, recognize the poor-law and registration divisions of the country, and furnish, at least once in ten years, all the information which is needed to determine the numbers of both sexes at all ages, the occupations and civil condition of the people, in every town and legally-recognized group of population. Accurate information on these points is universally allowed to be essential to sanitary science and administration.

(4.) The *Highway Acts.* A long series of statutes founded on the common-law liability of each parish to repair all highways within its boundary, were consolidated in the Act of 1835, which forms the basis of the present law of roads. As to turnpike roads, the parish or district is relieved of its main liability; and every road is not a 'highway.' In municipalities and places under the Local Government Act, the control of all roads is vested in the local authorities.

Facilities of internal communication are essential to a good sanitary police. The maintenance of old roads in safe and proper condition, the formation of new roads, and the proper drainage of all, are matters intimately connected with regulations and bye-laws relating to buildings, especially dwellings, in both towns and rural districts. The *Highway Act* has created districts, in the English manner, by authorizing parishes to group themselves for this purpose, at will or fancy. A principle, laid down in an admirable measure proposed by the late Sir James Graham, was unfortunately rejected by Parliament.

Highway districts are not necessarily coterminous with the poor-law unions. The result is, that their respective areas of management rarely coincide; while the grouping of parishes being only permissive, a large portion of the country still remains subject to the old inefficient treatment by parish road-surveyors. Travellers are still mulcted by toll-bar keepers. Petty local boards are still permitted to escape being included in a general system. Many persons of experience in local administration have recommended that the management of roads should be everywhere combined with existing machinery for the relief of destitution and the removal of nuisances.

(5.) *Rivers.*—If the rights of riparian proprietors and the claims of river navigators have been settled by numerous legal decisions and private acts, the interests of the public in the natural use of river water and in the maintenance of a pure supply have been neglected. This department of administration is in a very crude and rudimentary state. Earnest attention has, however, been directed of late to the conservancy of rivers and their tributary streams, the 'upper storage' of water, the improvement and repair of natural water-courses, and their protection from defilement by the refuse of towns, mills, mines, and manufactories. Stringent legal provisions against river pollution are found to be inseparable from the sanitary regulations and the water-supply of towns and villages. The management of rivers is also connected with what is now called 'arterial drainage' of land, as well as with works which, in the middle ages, were controlled by the old Crown Commissioners of Sewers. I may here repeat what has been clearly shown elsewhere, that sewers or sea-weirs meant, at that time, channels or embankments to protect cultivated land from incursions by the sea and from floods in tidal rivers. They had nothing whatever to do with the modern objects and practice of sewerage, against which they were sedulously guarded by penal enactments.

The *Thames* and the *Lea Conservancy Acts*, as well as important measures for other river basins, recommended by the two River-Pollution Commissions, under Mr. Rawlinson, and Sir W. Denison, directly bear on the question of sanitary organization.

Again, navigable rivers are, in a legal sense, highways; and the construction and repair of bridges, as belonging to the 'king's highway,' was always a matter of county jurisdiction, not a liability of the parish, but a part of the ancient '*trinoda necessitas*' of the shire. Thus the proposal for a reformed system of county government, as a step towards the constitution of authorities over wide natural areas within defined watershed boundaries, seems to be justified by precedent, as it is also an obvious means of settling many difficulties of sanitary control.

(6.) *Labour.* From the year 1802 until the present time Parliament has been engaged—at first very reluctantly and inefficiently and at very long intervals—in legislating for the safety and health of operatives, especially women and children, occupied in manufactures of various kinds, and in mines and collieries.

But the *Factory Acts* and acts of the same kind applying to other branches of industry, worked by a state machinery of inspection, date from 1833. This series of enactments displays remarkably the growing ascendancy of humanity, reason, and justice, over a false and narrow political economy. By prohibiting the employment of children and young persons, under specified ages, or otherwise unfit for work; by limiting the hours of labour; by requiring that machinery shall be fenced; by the half-time system of education; and by the sanitation of work-places; their maintenance of health, and their arrest of physical and moral degeneracy—under a system of medical certification and skilled supervision—entitle these acts to prominence in any sanitary code.

Legal protection having long been granted to workers in various kinds of factories, etc., to chimney-sweeping boys, to dress-making girls, and to bakers (under the *Bakehouses Regulation Acts*)—the principle of legislative interference between labour and capital has recently been extended to almost every description of handicraft, by the *Workshops Regulation Act.*

(7.) *Burial of the Dead.* Under my second class may rank, lastly, the *Burial Acts.* These regulate a distinct branch of administration, under the Home Office and Burial Boards, the districts of which, if not local boards, are not generally coterminous with districts for other purposes of local management.

The Burial Acts—with certain provisions in the Local Government and Sanitary Acts—control, for the public safety, interments in towns, in extra-mural cemeteries, and in parochial grave-yards. Burials may be prohibited in any place which, on the report of a government inspector, may be certified to be in a state dangerous to the public health.

Mortuary houses may be provided by local boards and by nuisance authorities, for the reception of corpses dead of infectious disease or otherwise dangerous to the health of the living; as also for the performance of medico-legal examinations, ordered by the coroner or other competent authority. But local administrative bodies are not compelled to establish this most necessary accommodation in every district.

III. (1) If the laws regulating the medical profession are not sanitary as well as sanative, in their design, the public might reasonably demand that they shall be made so. It is not enough to enable persons to distinguish the qualified *curative* practitioner from the unqualified, by prohibiting fradulent assumption of medical titles. It is not enough to endeavour to secure the possession of adequate knowledge and skill for attendance on the sick and hurt, by registered practitioners of various grades and qualifications granted by the medical institutions which these Acts support and combine and in some sort control. For, if the coming amendment Act should do nothing more; if, at the furthest, it should merely establish a threefold concentration of medical corporations for granting the primary license; if it should provide no means of ascertaining special fitness for the public exercise of *legal* and *preventive* medicine; other measures may soon have to be adopted.

Before the passing of the *Medical Act* of 1858, legal qualification for holding certain appointments, or for performing certain public duties, consisted in the possession of a diploma or licence to practice, from one or more of the professional corporations, or from some university.

Less than forty years ago,—that is, before the Poor Law Commissioners required that every medical officer of a workhouse or district should possess both a medical and a surgical license,—no one could obtain the diploma of the College of Surgeons unless he were twenty-two years of age. The minimum age for that qualification was afterwards lowered to twenty-one, on the plea of the public necessities.

To be legally qualified for any public office, it is now only necessary that the candidate should be on the medical register, having attained his legal majority. But thoughtful men, in and out of the profession, are prone to inquire whether, *at that age,* students in general, though they be licensed and registered, can be properly prepared to undertake the sole charge of districts or public establishments, or to fulfil the more important duties of a medical officer of health—to say nothing about still higher qualifications for a medico-legal expert or a superintending inspector.

Now, in Ireland, the Poor Law Commissioners allow no one to be appointed to a dispensary district until he is twenty-three years of age. The attainment of at least that age, and the passing an examination in some subjects of state medicine, or giving to a scientific authority satisfactory proof of experience and practical efficiency in the duties of health officer, would be not unreasonably strict requirements of future candidates for that appointment.

(2) The *Medical Witnesses Act* regulates and remunerates the services of 'legally qualified' medical practitioners in coroners' inquests, the real value of which depends almost wholly on the manner in which medical science is brought to bear upon the inquiry into the causes of sudden or suspicious or apparently preventable deaths. No one can truly say that sufficient legal security is taken in this country for the effective application of science to forensic investigations.

(3) It is not surprising that in the native country of Jenner, the laws relating to *Public Vaccination* should be very full and precise. But it is also characteristic of England that these acts establish, for the prevention of a single disease, smallpox, a special organization, which is almost useless for other purposes of preventive medicine, and in some respects irreconcilable with general arrangements for medico-sanitary service. These acts extend from 1840 to 1867, and are administered under the Poor Law Board and the Privy Council. I cannot pass on without acknowledging the improvements which have been effected under Mr. Simon's direction in the arrangements for, and performance of, public vaccination,—arrangements already marked by successful results.

(4) Of late there has been special legislation against another

contagious disease, *Syphilis,*—no less formidable than smallpox, and as regards its constitutional and inherited effects—to say nothing of its moral and social bearings—far more dangerous. The *Contagious Diseases Acts* apply only to certain areas surrounding fifteen garrison towns and naval depôts in England, and three in Ireland. These very necessary and beneficial enactments also create a separate machinery in isolated districts.

(5) The *Quarantine Act,* 1825, in connexion with certain provisions of the Sanitary Act, 1866,—several laws relating to emigration and the mercantile marine,—and a few local harbour acts, make up another group of laws,—having the double object of preserving the health of merchant-seamen and ship-passengers, and of preventing the importation and spread of foreign pestilence. The execution of quarantine regulations is committed to the Privy Council, and, under that, to various local authorities on the coast and in tidal rivers. But this department of sanitary law and administration is in a most anomalous and unsatisfactory condition.

(6) Laws for the protection and remedial treatment of the *Insane,* —still called by us, in pagan phrase, lunatics,—form another group, the *Lunacy Acts.*

These relate to 'disorders of the intellect'—so described in the new nomenclature—and constitute, under the Lord Chancellor, the lunacy commissioners and justices of counties, a distinct medico-legal organization of inspectors, who visit asylums and private houses for the reception and care of those whose unsoundness of mind has been certified by any 'legally qualified' medical practitioners, not personally interested in such establishments.

To these inspectors and visitors are committed a variety of 'preventive' functions; but they are wholly unconnected with every other machinery for medical or sanitary supervision.

(7) *Acts relating to Pharmacy and the Sale of Poisons.*—Very obvious in theory is the connection between laws controlling the public supply of drugs and poisons, and those affecting the exercise of medical or sanitary functions. Practically, however, the two departments are so sharply separated in this country, that the late attempt to establish some normal relation between them, in framing the *Sale of Poisons Acts* of 1868, was defeated by the antagonism of their leaders. Yet in Great Britain alone of all European nations, is the practice of pharmacy legally undertaken by physicians and surgeons acting as apothecaries. The regulation of pharmacy, so far as concerns the examination and registration of pharmacists and dispensers of poisons is now committed by law to the Pharmaceutical Society, but apothecaries and veterinary surgeons are exempted from the operation of the Act, so that there is no law applying indifferently to all pharmacopolists. The Act of 1868 confers a few very limited powers of control on the Privy Council, the consent of which is

necessary (1) to any regulations made by the Pharmaceutical Society 'as to the keeping, dispensing, and selling of poisons,' and (2) to a modified examination of 'chemists and druggists' not being members of that society. In order to judge of the sufficiency of these examinations, the Privy Council has appointed Dr. Headlam Greenhow as assessor.

Another singular anomaly in the law of poisons is, that while the medical council is by law the sole authority in the preparation of the national pharmacopœia, it has nevertheless no control whatever over the selection of articles from that pharmacopœia to form the authorised *schedule of poisons,* which selection is left by the Pharmacy Act wholly to the Pharmaceutical Society.

There is all the rigidity of caste in the legal separation between the two registers. The list of pharmacists and that of medical practitioners may not appear in the same publication. No one can be entered on both registers. Yet a large proportion of the men on either register invade at pleasure the occupation of those on the other. Pharmacists have become, in fact, 'a new race of unqualified practitioners.'

There is no independent supervision of the practice of pharmacy and sale of poisons, in the public interest,—no inspection of druggists' shops and stores, as there is of slaughter-houses and markets. The old powers of the London College of Physicians to visit apothecaries' shops are rightly abolished, although they recognized a social necessity for which there is now no provision.

(8) *Acts relating to Food.* These include (1) the *Adulteration of Food Act,* intended to secure the purity and wholesomeness of articles sold for human food and beverage, the provisions of which Act are now extended to drugs; (2) regulations concerning slaughter-houses, markets, and cowhouses, under the Public Health Act, local acts, and bye-laws; and (3) the analytical action of the Inland Revenue department. All these matters have relations which require to be adjusted with other measures of public hygiene. The appointment of analysts and inspectors, under most of these acts is left to bodies which evince no proper interest in the subject, and which represent the sellers rather than the consumers of food. Hence the failure of the Adulteration of Food Act. No security has been taken for the proper independence and qualification of the analyst. To make the Act compulsory in its present form would be an error.

Important regulations affecting the care, the slaughter, and the sale of animals destined for food are merely permissive, and most imperfectly carried into effect. Many in force on the Continent are wholly wanting in England. The subject requires more stringent and comprehensive legislation, universally applicable, and connected with other branches of sanitary administration.

In all departments of public health, but in none more than this, is a public prosecutor required, in every county of England.

(9) *Cattle-diseases Acts.*—A number of statutes relate to the prevention and arrest of contagious diseases of cattle. By these acts the Privy Council and the magistracy are authorised to regulate by means of local inspection and orders in council, the importation, transit, removal, and sale of animals, when epizootic diseases of various kinds prevail or are threatened. The control of this department of food legislation, like that of pharmacy, has been abruptly severed from the administration of sanitary and medical laws. Yet it is generally acknowledged, as it has been forcibly shown by Mr. Simon and Dr. Budd to the Commission, that a scientific study of comparative pathology is inseparable from medicine in its wider and truer sense,—that the principal discoveries in animal disease are due to medical philosophers,—and that in practice the subject is essential to a proper organization of state medicine. Indeed, it is known to form an integral part of every other medico-sanitary system in Europe.

Chronicle of Medical Science.

CHRONICLE OF PHYSIOLOGY.

By Henry Power, F.R.C.S., M.B. Lond.,
Ophthalmic Surgeon to St. George's Hospital.

Blood.

1. Dr. E. Klein: *On the Fission of White Corpuscles.* (In the Report of the Institute for Experimental Pathology in Berlin for 1869.)
2. G. Saviotti: *Observations on the Entrance of Pigment Corpuscles into Blood-vessels.* (Centralblatt.)

1. Dr. Klein states that the segmentation of the white corpuscles can be easily observed if a drop of the blood of a triton be placed in a moist cell on Stricker's heatable stage, at a temperature of about 86° F. The white corpuscles present three not very well defined forms, viz. 1, granule cells; 2, large finely granular corpuscles; and, 3, small nuclear-like bodies. The mode of division is not always the same; sometimes the fission occurs in the ordinary mode, by the formation of a sulcus, which, deepening gradually, causes the separation of the two halves: this mode commonly occurs in the coarsely granular cells. A second mode is that the cell, usually one of the finely granular ones, flattens into a disk, from one part of which a protrusion containing a nucleus forms; this gradually enlarges, and ultimately, by what resembles a process of gemmation, becomes detached. He thinks a single cell may divide more than once. He has observed similar phenomena on a heated stage in the white corpuscle of man.

2. In the course of some observations on inflammation, Dr. G. Saviotti applied a drop of collodion to the membrane of a frog's foot, which occasioned the inflammation of a circumscribed spot. After a few days had elapsed, the pigment cells were observed to be contracted, and grouped by the sides of the capillaries, whilst those in the neighbouring parts presented their ordinary characters. A few days later they had disappeared altogether, leaving the inflamed spot of a whitish tint as compared with the rest of the web. He immediately set himself to determine in what mode their disappearance had been occasioned, and three alternatives presented themselves: they might have moved to the surface and been cast off; they might have been absorbed into the lymphatic or blood-vascular system; or, lastly, they might have become colourless.

In pursuing his experiments he induced inflammation in the frog's web by the application of a two per cent. solution of sulphuric acid;

and again he found that, after the lapse of a few days, the pigment cells, leaving their natural position, became massed around the blood-vessels, and although still giving evidence of contractility, their processes were much less branched than usual. He then remarked that some of these processes appeared to penetrate the vascular wall. If the process remained filiform, its position was difficult to ascertain; but there could be no doubt when it assumed a globular form, for then it to a certain extent arrested the passage of the blood corpuscles, whilst, on the distal side, a small clear space existed, filled only with blood plasma. He now proceeded to examine whether the introduced process was carried away by the blood current, or whether the whole cell followed the process, and found that both phenomena occur. A process may remain for hours in the lumen of the vessel without undergoing any remarkable change, and it may even be withdrawn, but generally the intra-vascular part gradually increases, and ultimately draws the rest of the cell into the vessel after it. The duration of the whole process is twelve hours or more. M. Saviotti has satisfied himself that the white corpuscles can migrate into the vessels from without in a similar manner.

Digestion.—Absorption.

1. Dr. Alexis Dobroslawin: *On the Action of the Intestinal Juice in Digestion.* (In the Untersuchungen aus dem Institut für Physiologie in Graz., 1870.)
2. Th. Eimer: *On the Path traversed by Oily Material in Intestinal Absorption.* (Virchow's Archiv., Band xlviii, p. 119, and Centralblatt, 1870, p. 21.)
3. Dr. Victor Subbotin: *Essays on the Physiology of Adipose Tissue.* (In Zeitschrift für Biologie, Band vi, Heft 1, p. 73).
4. C. Voit: *On Luxus Consumption* (Zeitschrift für Biologie, iv, p. 517); *On the Formation of Fat* (idem, v, p. 79); *On the Metamorphosis of Albumen with Albuminous and Fatty Food, and on the Significance of Fat in the Body* (idem, v, p. 329); and in conjunction with Pettenkofer's *Researches on the Respiration of the Dog whilst Fasting, and with exclusively Oleaginous Food* (in idem, v, p. 369).

1. In Dr. Dobroslawin's investigations the intestinal juice was obtained in as pure and natural a state as possible, by isolating a loop of intestine, the application of two ligatures, and making a fistulous orifice into it. A canula was properly secured in the wound, its thick extremity entering the intestine. The dog was fed with a pound of horseflesh and a single supply of water daily. Attempts were, in the first instance, made to ascertain the absolute quantity of intestinal juice secreted. The fluid obtained was partly a thin liquid, and partly consisted of mucus, the relative proportions varying considerably. In one instance, when the length of the loop of intestine was 13 centimetres, 34 grains of fluid were obtained per hour; in another case, the length of loop 17 centimetres, and the quantity of fluid 28 grains per hour. The results of electrical excitation by means of induced currents were very similar to those pre-

viously obtained by Thiry, and showed a considerable increase in the amount of the secretion during the passage of the current; and further researches showed that the secretion thus obtained did not differ materially from that produced in the healthy animal by the application of electricity to the freshly exposed intestinal tract. The most interesting part of his researches bears upon the action of the intestinal juice on starch, albumen, and fat respectively. In regard to the former, he was able to convince himself that the intestinal juice possesses a distinct power of converting starch into sugar, and this in whatever state the juice might be, whether clear or troubled, or filtered, or mingled with flocculent masses of mucus. The time required was, in all instances, nearly the same, about two hours; in one instance evidence of the presence of sugar was, however, obtained in a quarter of an hour. In regard to albumen, his experiments were made with portions of raw fibrin. These were kept at a temperature of about 100° Fahr., in contact with some of the recently obtained intestinal juice, and it was found that a solvent action did occur, but very tardily, from 20 to 40, or even 48 hours being required. The dissolved fibrin underwent conversion without the development of any putrefactive odour, into peptorus. The researches made to determine the action of the intestinal juice on fats, as olive oil and butter, had a negative result; he was never able to discover any of the fatty acids.

2. Dr. Eimer, from renewed observations made with immersion lenses, still maintains the views he had previously expressed, to the effect that the fats enter in the form of fine molecules into the epithelial cells, and are conducted by their inner prolonged extremities to a vascular plexus lying beneath the basement membrane, and formed of connective-tissue corpuscles, from whence these molecules pass into the central lacteal. He describes the basal hem or border of the epithelial cells as traversed not only by vertical lines or porous channels, but by extremely fine lines at right angles to them.

3. Dr. Subbotin has undertaken, first, the inquiry respecting the occurrence or non-occurrence of a direct passage of fat in the animal organism from the intestinal canal into the adipose tissue; secondly, whether the fats develop within the cellular elements of the adipose tissue, and if so, whether they take origin from the albuminates, from the hydro-carbonaceous compounds, or from both of these together; thirdly, whether, as Kühne supposes, for fat to be produced, either glycerine or the fatty acids must be absorbed from the intestine, the complementary compound required to make the perfect fat being formed in the body at the expense of albumen or albuminous compounds, the fat cells being the principal agents in effecting the union. The conclusions at which he arrives are, first, that a direct passage of fat into the adipose tissue does *not occur*, for if a dog be fed with spermaceti it is impossible to discover that fat in the adipose tissue; and secondly, he finds that the fat cells *do* form fat, since if fat-free meat and palm oil be administered to a dog its adipose tissue contains stearine, which must have been generated in the body, and in all probability from the albuminous compounds. So also when no

olein was supplied it was still found in its normal proportion in the adipose tissue, and he thinks it highly improbable that it would exist in its normal proportion, unless formed by the elements (cells) of the adipose tissue. The fats of the internal organs contain more of the less easily fusible fats, stearine and palmitin; the fats of the surface, on the other hand, contain more olein, the disintegration or oxidation of the albuminous compounds being more perfectly accomplished in this deeper, and therefore warmer, situation. He thinks there is no evidence to show that fat is formed from the carbo-hydrates; these aid in storing up fat in the bodies of animals, only by being more readily oxidized than the fat formed by the cells of the adipose tissue, from the albuminous compounds. He does not, consequently, agree with Kühne in his interpretation of the origin of fat.

4. So good a résumé of the long and important papers of M. Voity are given by M. Hermann in the 'Centralblatt,' No. 55, 1869, that instead of attempting to present our readers with our own extracts, we shall, in preference, give a translation of M. Hermann's paper. The first of the above essays contains an historical summary of the doctrine of the so-called "luxus consumption," *i. e.* of the formation of urea when an excess of albumen is consumed, without such albumen having at any time become a constituent of the body, the combustion or oxygenation of the albumen, according to those who entertain this view, taking place in the blood. Voit considers that this question is altogether settled, because it is now established that the whole of the urea excreted is dependent on the supply of albumen in the food, and that the amount of albumen required to preserve the body at a uniform weight must be so much the greater, the richer it has become in albuminous compounds; all excess of albumen consumed leads to an increased metamorphosis of the albumen already present in the body, with the final establishment of a new condition of equipoise, in which the body contains a larger amount of albumen.

The second work contains a very complete historical account of the doctrines that have been held in regard to the formation of fat in the animal body, of which the following is an abstract:—

In opposition to Beccaria (1742), and to Prout and Dumas (1841), who held that animal fat simply proceeded from the fat consumed as food, Leibig, in 1842, first suggested the origin of fat from the hydro-carbonaceous compounds, in favour of which a variety of arguments drawn from chemistry and vegetable physiology were adduced, and especially the fact ascertained by Gundlach and Huber, that bees fed on pure sugar produced wax. Numerous cases were also recorded by himself, Playfair, Dumas, Milne Edwards, Persoz, Thomson, and Lawes and Gilbert, in which animals carefully fed consumed less fat than was excreted by them in their milk and fæces, though some, as Boussingault and Payen, denied this. The chemical grounds of the theory were undoubtedly weak, for in the frequently cited saccharine fermentation, only fatty acids, poor in carbon, with traces of glycerine and glycerides occur, whilst no certain conclusions could be drawn respecting animal processes, from a consideration of those taking place in vegetables.

The possibility of the formation of fat from albumen was alluded to, but not admitted by Liebig. The arguments adduced in favour of this were the occurrence of fats and fatty acids in the putrefaction of albuminous compounds, and in the ripening of cheese (Fourcroy, Berzelius, Liebig, Iljenko, Balard and Laskowsky, Wurtz), the formation of adipocere, first observed in 1786 (Fourcroy, Gibbes, Quain, Gregory, G. Liebig, Virchow, and Wetherill), though according to Voit, it has not in this instance been established whether a true fat or a fatty acid is produced; the conversion into fat of fragments of albumen introduced into the abdominal cavity of animals (Hunter, Berthold, R. Wagner, Husson, Middeldorff, Donders, Michaelis), against which it was objected by Burdach and others, that the fat was derived from neighbouring parts, and only infiltrated the albumen; the formation of fat during maturation of Roquefort cheese (Blondeau, Kemmerich), which was denied by Brassier; the formation of fat in standing milk (Hoppe, Kemmerich); fatty degeneration of tissue (numerous authors), which some observers explained without admitting a new formation of fat (H. Weber), and lastly, the development of fat in the development of the egg of the Limnæus stagnalis.

Hoppe, first in 1856, endeavoured to prove that the animal body was capable of forming fat from the albuminous compounds, though various objections were raised to the view he proposed, Pettenkofer and Voit positively demonstrated the fact in 1862, when flesh food being given to an animal, they obtained all the N, but only a portion of the C in the excreta, showing that a breaking up of the albumen had occurred, with retention of a portion rich in carbon, and analogous, therefore, to fat in composition in the economy.

Ssubotin also found that the amount of fat in milk increased in proportion to the amount of albumen in the food, and Kemmerich showed that the milk contains far more fat than is contained in the food, and that this could not well be derived from the body, since the animal increased in weight. From all this it was concluded that the albuminous compounds in the body are constantly disintegrating, with coincident formation of fat, that the body fat, exclusive of what is thrown out by the excreta is constantly burnt off, unless large supplies of easily oxidisable carbo-hydrates are consumed, which combine with the oxygen, and so preserve the tissue fat, producing fattening.

For the vegetable feeders, especially for the milk-yielders, special investigations were needed to ascertain whether the large quantities of fat in the milk could be derived from the albumen of the food. Voit undertook a preliminary inquiry, in which a milch cow was watched for six days and nights, the excreta being caught in clean vessels by scholars aud pupils. The numbers given need not here be introduced, but the conclusion at which Voit arrived from their inspection was, not only that the fat and albumen ingested were sufficient to explain the amount of fat contained in the milk, but also that the fat of the milk does *not* proceed from the carbo-hydrates of the food.

Voit then proceeds to show that the earlier researches of Boussingault, Playfair, Thomson, Arendt, Bähr, Knop, Ritthausen, Wolff, and Stohmann, also point to the origin of the milk fats in milch cows from the albumen and fat of the food, though the experiments of Kühne (1868), in which food containing very little fat and albumen were given, certainly cast some doubt upon the subject. He comments on the known independency of the composition of the milk on the kind of food supplied, though by affording pabulum to the gland structure albuminous compounds are much more influential in the production of milk than fats or the carbohydrates. Whilst thus, even with highly oleaginous food, mammals do not avail themselves to any great extent of the hydro-carbonaceous constituents of their food in the production of the fats of their milk, bees appear to form wax from sugar. But here also Voit endeavours to show that the wax is really derived from the stores of albumen they possess in their bodies, which, indeed, is so considerable that a queen bee, fed on sugar water, will lay eggs.

The later experiments of Voit are connected with the effects of the addition of fat to flesh diet on the metamorphosis of tissue, and he has obtained the following results:—Even with large quantities of fat in the food the metamorphosis of fat, indicated by the excretion of nitrogen, does not cease. Further, the increased metamorphosis of albumen, when an increased quantity of meat is consumed, remains constant if equal proportions of fat be supplied. Nevertheless the ingestion of fat diminishes the absolute metamorphosis of albumen, as is most clearly shown when fat is given whilst the animal is in a state of equipoise on a fine flesh diet. This may be due either to a diminished absorption of oxygen under the influence of the fat, or to the fat withdrawing the oxygen from the albumen, a point which must be determined by experiments on the respiration. The diminution of the metamorphosis of fat produced by the addition of fat is not great, amounting only to about 7 per cent.

The flesh, spared by the presence of the fat, for the most part enters into the formation of the tissues, but in some cases where much meat and little albumen is given as food it remains in the blood as "stored up," or "circulating" albumen. In the latter case the nitrogenous equipoise of the system is much sooner attained than in the latter. The fat present in the body has a similar influence. In a fat body an increase of flesh food causes increased tissue formation: hence to increase the albuminous tissues, *i.e.*, to make the animal more muscular, flesh food must not be given in excess, but combined with a considerable proportion of fat; in which case, however, a deposition of fat always also occurs. The influence of the condition of the body is such that for each state a certain proportion of flesh and fat must be contained in the food to produce the greatest effect. To maintain the body in its normal condition the addition of fat to the food renders less albumen requisite, but to enable it to perform the greatest amount of work it is preferable to keep the animal at its standard by larger supplies of albumen and less fat, since then the oxidation processes are exalted. Voit explains the leanness of a dog with biliary fistula on the ground of the imperfect absorption of fat that occurs in the intestine, owing to the want of bile.

We regret we have no space to give the researches of Pettenkofer and Voit on the exchange of gases in the fasting animal.

Nerve.

1. M. Grandry: *De la Structure interne du Cylindre de l'Axe et des Cellules Nerveuses.* (Journal de l'Anatomie, T. vi, p. 289. 1869.)
2. M. Ciaccio: *On the Pacinian Corpuscles.* (In Moleschott's Untersuchungen zur Naturlehre. Giessin, 1870.)
3. D'Albert Hénocque: *On the Mode of Distribution and Termination of the Nerves in Organic Muscle.* (In Brown-Séquard's Archives de Physiologie, No. 3, Mai—Juin, 1870.)
4. C. J. Eberth: *On the Mode of Termination of the Cutaneous Nerves.* (In Max Schultze's Archiv. f. Mikroskop. Anatomie, 1870, Heft i.)
5. K. Trütschel: *On the Mode of Termination of the Nerves in the Stomach.* (Provisional communication in the Centralblatt für die Medicin. Wissens, Feb. 19, 1870.)
6. Dr. W. Krause: *On the Termination of the Nerves in Glands.* (In the Archiv für Anatomie, Physiologie, etc., Reichert und Dubois Reymond, p. ix, April, 1870.)
7. C. Eckhard: *On the Innervation of the Parotid in the Sheep.* (In the Zeitschrift für rationelle Medicin, Band xxxvi, Heft 2, 1869.)
8. M. Schiff: *Recherches sur l'Echauffement des Nerfs.* (In Brown-Séquard's Archives de Physiologie, 1870, 2me et 3me parties, 1870.)

1. M. Grandry has made a series of experiments upon the effects produced on the axis cylinder and ganglion cells by the action of nitrate of silver; the perfectly fresh nerves, consisting of portions from the nervous centres, were macerated for five days in the dark, in a one-fourth per cent. solution of the nitrate, and were then exposed for two or three days to the full light. Under these circumstances the superficially lying axis cylinders appeared very sharply and regularly transversely striated, with alternate uncoloured, and deeply stained, transverse striæ. The breadth of the dark striæ varied from 1-25000th to 5000th of an inch, and they were separated by intervals, varying from 1-25000 to 1-8000th of an inch. There was no relation between the thickness of the fibre and the fineness of the striation. The longitudinal striation of the axis cylinder came prominently into view, so as to produce a still greater similarity to muscular fibre. Double refraction was not observed to be present. The bodies and processes of the ganglion cells presented a precisely similar striation. M. Grandry does not attempt to explain the cause of these remarkable appearances.

2. M. Ciaccio gives a very full and detailed description of the Pacinian corpuscles, which, however, does not appear to contain much

that is new either in regard to their structure or physiology. Those of birds are probably of simpler structure than those of mammals, and the terminal nerve-fibre less frequently undergoes division. The fibre always ends in a cell or in cells equal in number to its divisions. In man the Pacinian bodies of the hand and foot are abundantly supplied with blood-vessels, which enter not only at the two poles, but at any point of the circumference. Those of the mesentery of the cat, and of the foot of the horse and ox, receive but few vessels, and these usually enter at the pole by which they are attached. In the case of the Pacinian bodies of birds the blood-vessels do not penetrate, but only wind around the outside.

3. M. Hénocque has pursued his investigations on a variety of animals, and in some instances also on man, though this was practicable only in very cold weather. On following the nerves to a viscus containing many smooth muscular fasciculi, as the bladder, they are found to form a plexus of origin in the connective tissue, which invests and surrounds the muscular fasciculi. Amongst the fibres of this plexus numerous ganglia are distributed. From this plexus branches are given off, which, as they ramify and intercommunicate, form an intermediate plexus, and from this again are given off an intramuscular plexus, situated in the interior of the fasciculi of smooth muscle. The terminal fibrils are everywhere identical; they divide dichotomously, or anastomose and end in a slight swelling or button, or in a point. These buttons are found on various parts of the smooth muscular fibre, most frequently around the nucleus, or at the surface of the muscular fibres, or lastly, between them.

4. M. Eberth's observations have been conducted on man, rabbits, guinea-pigs, cats, and dogs, but chiefly on the two first-mentioned animals, because the presence of pigment-cells in the skin of the others interfered with the success of the preparation. For the same reason the skin of albino rabbits was preferred. Small portions were excised, and whilst perfectly fresh were placed, for a period of time varying from a quarter of an hour to four hours, in a solution of chloride of gold, containing from one-fourth to one part per cent. The nerves were found to be coloured of a deep black colour. He agrees generally with the observations made by Langerhans to the effect that the skin is supplied by a rich web of nervous tissue, which forms a delicate but close plexus of medullated fibres in the corium, and from this delicate axis cylinders are given off, which run towards the surface, and end in *free* extremities. In the deep layer of the epidermis peculiar cells are met with, which usually present a stellate form, sending off five to eight processes towards the surface, and one or two towards the corium; the connection of these with the nerves has not been clearly ascertained, though they become blackened with solution of gold.

5. From investigations undertaken in the laboratory of Chrzonszczewsky, at Kiew, M. Trütschel has arrived at the following conclusions: 1, a close-meshed plexus of fine fibrils, with scattered cells, lies beneath the muscularis mucosæ, the fibres of which are connected with nerves, and which Trütschel holds to be nerves because they

become coloured with chloride of gold and perosmic acid. This layer is in connection with the nervous layer of Meissner, which is strongly developed in frogs. Fine offsets from it penetrate the mucous membrane, and reach the epithelial layer. Superjacent to Brücke's muscle, or the muscularis mucosæ, is a layer of large multipolar cells, connected with each other by processes. Other cells, presenting the microchemical reactions of nerve-cells, are found beneath the epithelium, into which they send processes, whilst others of their processes anastomose with each other. Between the epithelial cells are terminal bulbs, from some of which at least extremely fine processes are given off.

6. Krause states that in 1863-64 he first, in opposition to the prevailing views, published observations showing that, 1, nerve-fibres were very numerous in the finest lobules of the salivary and lachrymal glands, the trunks presenting ganglia in both of these glands, and in the pancreas in all mammals; 2, that in the parotid, flattened multipolar cells are present, which might be regarded as nerve-cells; 3, that the doubly-contoured nerve-fibres terminate in the salivary glands of the hedgehog in small terminal bodies, the terminal capsules; 4. that the pale nerve-fibres ultimately apply themselves to the acini of the glands, and perhaps end in secretory terminal plates. These results led to further research. The presence of the ganglion cells has been established and corroborated by Reich, Schlüter, Pflüger, Bidder, Kölliker, and others: most are bipolar, a few have three poles, but they do not anastomose much, and his present investigations show that these do not belong to the nervous system nor to the connective tissue, but are in all probability connected with the development of the gland. In the salivary gland of the hedge-hog and in the pancreas of the cat, the dark-edged nerve-fibrils terminate in small terminal capsules, like the corpuscles of Vater. Pale nerve-fibres are always present in the salivary glands, and terminate between the acini of the gland.

7. M. Eckhard states that, in several experiments in the sheep, he had so completely isolated the parotid, that it only remained attached by its vessels, and yet still found that the secretion continued nearly unaltered; and hence concluded that no cerebral nerves governing the secretion penetrated this gland. Ludwig, it appears, had also noticed the same phenomenon in experiments on the kidney. Recently, however, Herr Loeb having made the observation in Eckhard's own laboratory, that in the *dog* the secretion of the parotid was under the control of the tympanic branch of the glosso-pharyngeal nerve, Eckhard was induced to re-examine the subject in reference to the sheep, especially with the object of ascertaining whether some nerve-fibrils might not have escaped his observation. In the first research he placed a canula in Steno's duct, and determined the amount of secretion discharged every five minutes: it amounted to 3·5, 3·9, and 4 grammes. The tympanic nerve was now divided, and the quantity fell at once to 1·0, 1·2, 1·2. Subsequent section of the sympathetic produced no alteration in these numbers. A second control experiment was performed, in which the tympanum

was merely opened, but the ramus tympanicus was not divided; a slight but not very marked effect was produced. The ramus tympanicus was now divided, and the effect immediately became strongly marked; the numbers ran thus: from the duct in the first instance 5·0; after exposure of tympanum, 3·9, 4·0, 3·5, 4·0; after division of tympanic nerve, 1·8, 1·9, 1·8, 1·9. Professor Eckhard has not been able to ascertain the cause which leads to this depression of the secretory process after section of the tympanic of the glosso-pharyngeal.

M. Schiff has performed a series of experiments, with a view of ascertaining whether any and how much increase of temperature occurred in the nerves and nervous centres on the reception of sensorial and sensitive impressions. The means employed consisted of delicately constructed thermo-electric needles, which were made to traverse various parts of the nervous system. Then, when the animals were at rest, the several senses, as of sight, hearing, or even of smell, were suddenly aroused; and the effect on the needle of a galvanometer, the movements of which were magnified by having a mirror attached to it, were carefully noted. In the first series of experiments he ascertained that excitation of one of the higher senses produces an elevation of temperature in the cerebrum, though it remained doubtful whether the disengagement of heat was the expression of the molecular movements accompanying the conduction of the excitation towards the centre itself, or of the changes produced by this excitation after its arrival at the central point. In the second series of experiments fowls were chiefly used, and minute batteries of copper and bismuth were introduced into the brain, and allowed to heat, two wires issuing from either side of the creature's head. By mechanical means absolute quiescence of the body of the animal was secured, and the interfering effects of any slight movement guarded against by the attachment of springs. Under these circumstances the exposed surfaces of the animal were slightly irritated, so as to excite the sense of touch; the auditory centres were aroused by sudden noises; the visual perceptions were excited by moving a coloured object in front of the eyes, and it was then found, in all instances, psychical activity was accompanied by an exaltation of temperature. The last-mentioned experiment was peculiarly valuable, since the psychical impression was twofold, and one of these could be eliminated; for, on waving the coloured object, the augmentation of temperature indicated was partly attibutable to fear and partly to the psychical operation involved in the perception of the visual impression; but after frequent repetition of the experiment the animal became accustomed to the movement, and no longer afraid of it; and the factor of fear being thus eliminated, it was found that the movement of the indicator of the galvanometer fell from twelve to a constant of eight.

Spinal Cord.

MM. Masius and Van Lair: *Experimental Researches on the Anatomical and Functional Regeneration of the Spinal Cord.* (In the Monthly Microscopical Journal, May, 1870.)

MM. Brown-Sequard and Lombard: *Experiments on the Influence of Irritation of the Nerves of the Skin on the Temperature of the Limbs.* (In Gazette Medicale de Paris, 1870, p. 142.)

F. Arlt: *On the time occupied in the Movements of the Iris.* (In Archiv. f. Ophth., Band xv, p. 294.)

M. Lombard: *Experimental Researches on some hitherto neglected Effects of Respiration on the Temperature of the Body.* (In Gazette Medicale de Paris, 1870, p. 142.)

MM. Masius and Van Lair observe that Voit has recently proved the reproduction of the cerebral tissue in the pigeon, and the coincidence of this reproduction with almost complete renewal of the cephalic functions, and add that the facts they have collected tend to show that, in the frog, this regeneration of the spinal cord also takes place. Various experimenters have demonstrated the cicatrization of the spinal cord when simply divided, but no one has as yet observed the reproduction of a segment of excised cord. They thus formulate the principal deductions that have resulted from their experiments: "The spinal cord in the frog can recover a loss of substance, which has taken place in its own tissues, and recover its primitive anatomical and physiological properties-" The anatomical reparation is effected by means of a yellowish translucent substance, containing delicate cellules, corpuscles, and fibres. The physiological can be demonstrated to have occurred in the course of a month by a return of the functions of the parts below the point of excision, and to completely restored in six months. Frogs operated on midwinter alone succeeded.

2. It is well known that MM. Brown Sequard and Tholozan demonstrated that a fall of temperature occurs in one hand when the other is dipped in cold water. They now show, with M. Lombard's delicate thermo-electric apparatus, the following facts:—1. That irritation of the skin of a limb by pinching is soon followed by an elevation of temperature; 2. This irritation produces a depression of temperature of the opposite limb; 3. Pinching the skin of one of the lower limbs often produces a change of temperature in the two thoracic limbs, to wit, depression in the arm of the opposite, elevation in that of the same, side; 4. All these effects, they consider, are effects of vascular contraction or dilatation, and are of a reflex nature.

3. M. Arlt, in accordance with Donders, finds that there is no difference in the time at which both irides begin to contract when light falls on one only. The commencement of the contraction occurs

half a second (0.49 sec.) after the light has entered the eye, and the maximum of contraction occurs about an eighth of a second later (0·58 sec. after the impression). Listing had already given for them two periods respectively 0·4 sec. and 0·6 sec., which shows a very close agreement in results. Donders found that the commencement of that contraction of the iris which occurs in accommodation for near objects, took place after the lapse of 0·41 sec., and reached its maximum in 1·13 sec., consequently at a much later period than when the movement was reflex on the admission of light.

4. By means of an extremely sensitive thermo-electric apparatus (indicating a variation of temperature amounting only to 1-2000th of a deg. cent. M. Lombard has ascertained that the temperature of the skin over the radial at the wrist falls a few seconds after the respiration has been suspended, and that the depression is proportional to the time during which it is suspended. This he shows is not due to a diminution in the activity of the molecular changes in the member, nor is it due to the temperature of the arterial blood being lowered, but it is due to the fact that the quantity of blood which circulates in the artery is diminished.

S. L. Schenk: *On the Rotation of the Embryoes of the Frog within the Egg.* (In Pflüger's Archiv, 1870, Heft ii and iii.)

Dr. Schenk observes that the embryo of the frog exhibits constant movements of rotation, the direction being the reverse of that of the hands of a watch, supposing the head of the animal to be directed away from him. These movements continue without interruption, and may be watched for hours together. They vary considerably in rapidity, but in a series of observations each rotation was found to be effected in from five minutes and thirteen seconds to twelve minutes and two seconds. It has not been accurately ascertained when these movements commence, since in the earliest stages of development the surface of the egg is in close contact with the capsule, and it is only after a little water has been imbibed that the two are separated; but Dr. S. shows that they result from the pressure of ciliated cells on the surface, first, because the presence of these can be demonstrated with the microscope, secondly, because they can be accelerated by the application of moderate heat, which is well known to render the movements of cilia more rapid, and thirdly, because they can be arrested almost instantaneously by the action of weak acids, which are known to operate in the same way on ciliary movements.

REPORT ON OPHTHALMOLOGY.

By J. F. Streatfeild, F.R.C.S.

Plastic Restoration of the Lower Lid.—M. Denonvilliers, at the Imperial Academy of Medicine, showed a patient in whose case, a year previously, he had removed a cancroid growth which had involved all of the right lower lid, excepting the mucous membrane, which was alone unaffected. This peculiarity rendered the operation very difficult. All the diseased parts were, in the first place, removed. Then it was necessary to make up for the loss of substance. To do this the surgeon cut a triangular flap, borrowed from the skin of the cheek, a flap which he made to turn by its pedicle, and which he inserted at the greater canthus. The palpebral mucous membrane was then to the same extent dissected, and its raw surface then combined with the raw surface of the flap to compose the lower lid.

The retractation undergone by the flap in its length was such that, inserted in the greater canthus, it seemed yet as if it had been towards the middle of the lower lid. M. Denonvilliers has never seen a case wanting this retractation of length of the autoplastic flaps, against which the surgeon must always take precautions.

The result is very fine, inasmuch as the flap is flat, and does not show those disagreeable lumps, hitherto observable after autoplastic operations of the face. One must look closely to see the scar of the flap.—*L'Union Médicale*, 25th Nov., 1869, p. 766.

Granular Lids.—Mr. Lawson Tait recommends syrup as a collyrium to be dropped into the eye (p. r. n.) of three drachms, or half an ounce of the best sugar to an ounce of water, and filtered quite clear. In the case of a girl whose cornea had been cloudy, ulcerated, and vascular for two years previously, by this treatment, they became perfectly clear in about nine weeks. The author says that by *any* other treatment he has *always* got entropion as a consequence.—*Lancet*, 12th Feb., 1870, p. 228.

Trachomatous Entropion, and Trichiasis.—The surgical treatment introduced by Dr. B. A. Pope, of New Orleans, as an addition to an operation in these cases proposed by Jäsche, modified by Arlt, remodified by Von Graefe, is briefly described in the title given to his communication on "the extirpation of the fibro-cartilage of the upper eyelids for the cure," &c., &c. He recommends it "where the so-called granulations have involved the cartilages profoundly" in the neglected insidious cases often met with. For six or seven years the author, having removed the piece of skin, has removed some muscle, "and then shaved off the convex and thickened portion of the cartilage down to its free margin." Now (three times) he has removed, piecemeal, the cartilage, all but its upper rim, to which the levator is attached. "The thickening of the conjunctva renders the removal of the cartilage much easier." Dr. Pope—when the cartilage is less atrophied than in the cases given, but much thickened and inverted—would advocate their *partial* extirpation at the (lower) part of them affected.—*Knapp and Moos, Archives of Ophthalmology,* &c., Vol. I, No. 1, pp. 10—16.

Lacrymal Excretory Apparatus.—Dr. P. Lesshaft, of Petersburg, has published a treatise on the orbicularis muscle, and its influence on the mechanism of secretion of tears. He describes the muscle very minutely in all its relations, and the (1) Depressor, and (2) Corrugator supercilii. He continues of the (3) Lacrymal muscle, which is wrongly called Horner's muscle, because it had been long before (1749 and 1761) described by H. von Duverney; it was also (1816) called by Rosenmüller the muscle of the lacrymal sac, whereas Horner first mentioned it in the year 1823. It has been discovered altogether already six times; the most incorrectly is its behaviour depicted by A. Weber. It arises from the middle of the orbital surface of the lacrymal bone, in a line, bow-shaped, 5—7 mm. broad; forwards the origin reaches to the posterior lacrymal crest. Its bundles form a quadrate body, which, 6—9 mm. from their origin divide into an upper and lower section, which correspond to the two lacrymal canaliculi. The length of the whole muscle is 12—15 mm., in infants 10—11 mm., the thickness amounts to 1—1½ m. From the division little fibres approach the lacrymal sac and the hinder wall of the common portion of the canaliculus. After the division every muscular offshoot is spread out over the hinder wall and the convex side of the corresponding canaliculus, its fibres becoming always thinner in proportion as it nears the puncta. At the convex side of the canal there appears between the bundles a small streak of connective or fatty tissue deposited.

With regard to the lacrymal apparatus, we would draw attention to one only peculiarity found by the author in his numerous preparations. In 112 eyes, only three times did he find a separate opening of the 2 canaliculi into the sac. The usual single opening is 2—3 mm. long, 1½—2 mm. thick. The aperture into the sac usually is found nearer its posterior than its anterior wall, 1½—3½ mm. from the upper, blind, end of the sac. The little fold on the posterior wall of the common orifice is formed by the prolongation of the inner margins of both canaliculi; only in four cases were they continuous into the lacrymal sac itself, as far as the Plica lacrymalis superior, and at the place of junction of the two was found a little nodule.

The author found the sac itself 11—14 mm. in length, the broadest part in the middle measured 4½—5, at most 6 mm., the passage into the nasal duct 2½—3½ mm. The nasal duct was found to be 16—28 mm. in length along the lateral wall, 11—24 mm. along the medial wall, and the width amounted to, above, 2—3½, below, 3—5 mm. The form of the nasal opening very rarely varied: it presented an oblique or transverse slit of a round, oval, triangular appearance; it was also in two openings divided by a bridge. From the lower wall of the nasal cavity the opening was only 2—4, but also as much as 11—13 mm. distant. Frequently it was even only by a bristle permeable. From above here it was bounded at the medial wall by a fold of mucous membrane, which very wrongly bears Hasner's name, for indeed Morgagni had portrayed it; Rosenmüller, E. H. Weber, Osborne (1835) have accurately described it.

Under the *valves* in the lacrymal sac the author could only discover an upper and lower *fold.* The upper fold commenced at the place of opening of the canaliculi, and extended towards behind and below the medial wall; their length amounted to $5\frac{1}{2}$—$7\frac{1}{2}$ mm. Only three times was this altogether wanting, twice it was ring-shaped; spiral the author never saw it; ten times there were found at the place of discharge little polypous (pathological?) nodules. The lower fold was often absent; it was found at the point of transition from the sac to the canal, only once in the canal itself. It was generally lying across at the lateral wall, but also at the medial, and had a length of 4—6 and a breadth of 1 mm.

In fine the author delivers himself as to the theory of dilatation, and reflects on the proceeding in the following manner. When the fibres of the circular muscle at the outer angle of the eye and those of the lacrymal muscle at the lacrymal bone are fixed, then, by closing the lids, must the innermost fibres of these muscles firstly become contracted. By this the lumen of the canaliculi becomes expanded in the direction from the puncta lacrymalia towards the sac, but finally the front wall of the sac itself becomes raised. In these expanded holes the tears now become absorbed. With the opening of the lids consists the contraction of these muscles, the natural elasticity closes the canaliculi and the sac, and the fluid becomes pressed downwards through the nasal duct.—*Schmidt's Jahrbücher*, Vol. 144, pp. 143-5.

Periodical Epiphora, due to Paresis of the Fibres of the Orbicularis which cover the Lower Tarsus.—Dr. E. Williams, of Cincinnati, records this case:—The weeping came on once in three or four days, and lasted only a few hours. The lower lid of the eye affected, held down for a few seconds, did not so quickly adapt itself to the eye again as on the other side. The weeping, in six weeks, entirely ceased after employing the galvanic current at intervals.—*Knapp and Moos, Archives of Ophthalmology*, Vol. I, No. 1, p. 55.

Stricture of the Nasal Duct.—Dr. E. Williams, of Cincinnati, abandons Bowman's and Weber's probes in favour of his own system of wearing a style constantly during the whole course of treatment, instead of only occasionally introducing the instrument. "A smooth silver style, even of large size, in a natural opening, *lined by mucous membrane,* is tolerated with impunity." His styles are of virgin silver: the smallest corresponding to Bowman's largest. The upper end, which is flattened, is bent and hooked over the lower lid. The sac is washed out every day, or every two or three days. As an astringent injection he uses sulphate of copper 2—20 grains to the ounce of water. To wear a style thus is *far less painful to the patient than its occasional introduction.*—*Ibid.*, Vol. I, No. 1, pp. 40—54.

Dr. John Green, of St. Louis, Missouri, would substitute "the softest and most flexible lead wire for the rigid silver styles employed by Dr. Williams," *vide supra.* His leaden probes are made tubular in order, for the smaller sizes, to carry a stylet of tempered steel wire, whilst they are being introduced, giving them thus sufficient rigidity, and for the larger sizes, to lessen their weight. They exhibit, after they are withdrawn, a very considerable curvature, varying in different cases, but uniform in the successive stages of the treatment of the same case. They not only adapt themselves to the sinuosities, but give little pain. They are to be worn a week or two.—*Transactions of the American Ophthalmological Society*, 1869, p. 15.

Why Obliteration of the Lacrymal Sac is not followed by Epiphora.—This, in a paper read at the Académie des Sciences, is explained by M. Bergeon as a loss of the stimulus to the gland, caused by the constant aspiration of the tears as they reach the nares, of which they are the chief source of their moisture, as well as, in some degree, of that of the lung itself. The author says, "By reason of the formation of the lower orifice of the nasal duct—always narrow, and usually capillary—the tears become 'stored up' in the duct, the lacrymal sac, the lacrymal canals, and the oculo-palpebral space, which is closed in by the unctuous edge of the eyelids. The tears in this way represent a small liquid column, certainly a very thin one, but still continuous, and extending from the secretory canals of the gland to the lower orifice of the nasal duct."—*Medical Times and Gazette,* February 5th, 1870, p. 154; also *Archives Générales de Médicine*, March, 1870, p. 372.

Dacryolith in one of the Excretory Ducts of the Lacrymal Gland.—The same author records also this case. The tear-deposited concretion, 5 mm. in length, was easily removed.—*Ibid.*, p. 56.

Dilatation of the Lacrymal Nasal Duct in Pulmonary Diseases.—Dr. Starcke, of Jena, refers to a case, in the 'Archiv. für Ophthalmologie,' I, 2, reported by W. Rau, and another similar, but more pronounced in his own recent experience. A middle-aged handicraftsman suffering, after a chronic pneumonia, from severe catarrh and dyspnœa, had, at the left inner canthus, near the plica semilunaris, and over the upper orifice of the tear canals, an almost hemispherical tumour, which showed a most remarkable, irregular, periodical diminution and enlargement. By each exspiration it became more convex, and *vice versâ,* and the skin tense or wrinkled. It became even more distended when the patient coughed. This increase of size outlasted somewhat the cessation of its cause. It felt to the touch somewhat like stiff paper. On the right side a similar but smaller tumour appeared only when an exspiration was forced, the mouth and nose at the same time being closed. At the lacrymal puncta no air or fluid ever escaped, and their absorption of the tears was uninterrupted. The catarrh and dyspnœa were cured, and with them the abnormal swelling. Other cases are given—associated with lung affections.

This remarkable circumstance, which also concerns about half of those suffering from difficulty of breathing (only a very much less per centage of the sound, only three in ninety-two showed any abnormality), seemed to me, therefore, to allow of the admission of a very close connexion between lung affections and insufficient closing of the tear passages, and of course in such diseases the conditions are favorable for their establishment in the greatest degree. The cough so often repeated (sometimes with the mouth shut), the preceding inspiration, with aid of all the respiratory muscles, must naturally enhance repeatedly very essentially the compass of the pressure of air in the nasal cavities, and so much facilitate the entrance of the air into all the spaces in connexion with it, in the way described above.

In however high a degree then, the resulting insufficiency becomes, whether it be complete or incomplete, this in part depends on the amount of the *vis à tergo*, in part, likewise, on the more or less favorable position of the opening of the nasal canal, and in part on the greater or less tension of the *periorbita*, covering the anterior wall of the fundus of the lacrymal sac. — *Deutsches Archiv für Klinische Medicin*, 1st April, 1870, p. 212.

Origin of the Bipolar Arrangement of the Lens-Fibres.—R. Woinow continues the history of the development of the lens constructed according to the bipolar type, which, as is known, shows no lens-cross, but at their front pole a single horizontal, at the hinder a vertical line, and finds in the lens of the rabbit and squirrel that in the place in which the elements of the inner periphery of the lens envelope have just altered to the lens-fibres, the hinder wall of the lens shows no single tortuous surface, but that out of the midst of it a hillock projects, which is surrounded by a deeper ring, and without this again, by a prominent swelling. How from this disposition the bipolar arrangement of the fibres proceeds, as the horizontal and vertical lines in course of development, is not well rendered in abstract, and reference to the original illustrations should be made. *Wiener Acad. Sitzungsber, Math.-Naturw. Cl.*, Part 2, 1869, lx, 151—5.

Luxation of the Lens—Mechanism of Accommodation.—M. Dufour reports three cases, of which the two first particularly allowed of conclusions being drawn as to the state of the accommodation. When, according to the theory of Helmholtz, the lens in the state of rest of the accommodation is kept by the zonula stretched out and flattened, then it may be expected that the (luxated) lens freed from the zonula would be the more convex; it must, therefore, by lens-luxation appear short-sightedness, which is yet more increased when the lens falls into the anterior chamber, so that the space between it and the cornea is abolished. The investigation of the cases (three brothers and sisters, all of them suffering from spontaneous luxation of the lens of both eyes) corroborated the above opinion completely.

In the first case the lens in the left lay chiefly, with exception of its upper border, in the anterior chamber, therewith myopia, about $\frac{1}{3}$—$\frac{1}{2}$ (with much amblyopia) subsisted; in the right, by simple dislocation of the lens inwards, the myopia reached only $\frac{1}{7}$; accommodation seemed not to be present. The myopia cannot be due to sclerectasia posterior, the ophthalmoscopic appearances of it were altogether wanting, and the eye was not of a myopic shape. Moreover, by extraction of the lens the left eye was very hypermetropic ($\frac{1}{3}$), more so than it ought to be by previous emmetropia. After a longer time the lens in the right eye fell into the anterior chamber, by which increase of the myopia to about $\frac{1}{5}$; after extraction, hypermetropia $\frac{1}{3}$—$\frac{1}{2}$, with some irregular astigmatism.

In the second case, in the left eye the edge of the lens extended across the pupillary region; the oblique position of the lens induced monocular diplopia, whereby the one image with half the lens, the other without it, was projected. The refraction corresponding to the part of the pupil independent of the lens was hyp. $\frac{1}{18}$, whereas the part corresponding to that yet furnished with lens, myop. $\frac{1}{2}$. The difference in the refrangibility of the two parts of the same eye, $\frac{1}{1\frac{4}{5}}$, is in this much greater, than the effect of the lens in a state of rest of the accommodation corresponds to, it betokens much more an accommodation of 4—5 inch distance. (In the right eye of this person the lens had fallen down altogether into the vitreous; it suffered from very considerable amblyopia.)

The action of the mere advance of the lens by evacuation of the aqueous humour Dufour found by repeated experimentation to be equal to an increase of refrangibility of about $\frac{1}{12}$—$\frac{1}{14}$, in accordance with previous definitions of Von Graefe's.—*Centralblatt für die Medicinischen Wissenschaften*, 12th March, 1870, pp. 169-70.

Depression of Cataract of an Ass.—The operation—performed by M. Hervez de Chégoin—succeeded perfectly. The patient, twenty-one years old, shed abundant tears during the operation. The animal—without need of any glasses—now sees distinctly.—*L'Union Médicale*, 6th November, 1869, p. 667.

Cataract Extraction-operation.—Dr. Taylor, of Nottingham, describes the operation he has devised, and which his experience since 1865, when he first announced it, only confirms. He has two knives, a line in width, and bent on the flat at an angle, one sharp, the other blunt-pointed. With the former he makes an incision, 6—7 lines, in the sclerotico-corneal junction at the upper part; the latter is used to extend the limit of this incision, in some cases he considers favorable, in which he would now make the usual iridectomy. Over-mature or tough-capsuled cataracts he would extract without opening the capsule. He operates on both eyes at once in many cases.—*Lancet*, 23rd and 30th April, 1870, pp. 581 and 614.

Cataract in both Eyes—which has precedence?—Mr. Haynes Walton says didactically, "In an adult the right eye is almost always the first to be involved." This is rather contrary to deductions made from the statistics of the Moorfields Hospital in the 'Ophthalmic Hospital Reports,' October, 1857, p. 4.—*Medical Times and Gazette*, February 5, 1870, p. 143.

He limits this assertion to cases of "hard cataract."—*Ibid.*, February 26, 1870, p. 224.

Von Graefe's Extraction-Operation.—Mr. Wilson, of Dublin, after reviewing the history of cataract operations generally and of extraction in particular, from the earliest times, speaks most highly of this the latest method. Escape of vitreous has been, in his experience, the most frequent—twenty-five per cent.—misfortune. The operator and his assistant require less previous education than for other methods in order to succeed in extraction. He gives no anæsthetic. He remarks on the Celtic peculiarities of his patients—small deep-set eyes, with prominent over-hanging eyebrows; therefore he would use a Graefe's knife, but "curved or angular," the handle next the blade tapered. He uses a right- and left-hand speculum, desiring always to have the nut by which it is kept open downwards. He points out the desirability of a distinguishing mark, conspicuously to indicate, on the handle of Graefe's knife, the side of the cutting edge from that of the back of it, to avoid any error in its introduction.—*Dublin Quarterly Journal*, May, 1870.

Non-mercurial Treatment of Syphilitic Iritis.—Mr. Gascoyen, encouraged by Dr. Boeck, who, at the Lock Hospital, treated successfully two patients undergoing syphilization, and who had severe iritis, with only atropine drops and opium, has been trying the simple method of treatment, even for syphilitic iritis. He tabulates 18 such cases; those of them which had been taking mercury for other syphilitic disorders, had it discontinued at the outset of the specific eye-inflammation; only 7 had had no mercury, and took none. Atropine and belladonna to the eye; opium, if indicated, externally and internally; perhaps iodide of potassium; tonics and salines; leeches and blisters, and other ordinary precautions and palliatives, were employed. Of the 7 cases above, only one had finally any synechiæ. Mr. Gascoyen distinguishes two forms of iritis, a truly syphilitic (parenchymatous) tertiary, and a false (common) form (serous) secondary manifestation of the disease. The conjunctiva is *in the first place* inflamed, in the latter cases, and *vice versâ.* "Irritation of the conjunctiva, from whatever cause, always occasions an immediate contraction of the pupil; and the narrowing of this aperture, which is so constant that it has been regarded as diagnostic of commencing iritic inflammation, may be thus explained, even though the iris is not yet involved." In tertiary syphilis (iritis) mercury is, by general consent, not to be given; the other cases are not syphilitic and so mercury is superfluous.—*Medico-Chirurgical Transactions,* vol. lii, pp. 265-76.

Division of Ciliary Muscle.—Mr. T. S. Walker, of Liverpool, says it "is useful in cases where there is a considerable amount of inflammatory action going on in the iris, sclera, or cornea, of a subacute character, and attended with moderately increased tension of the globe, and general vascularity . . . sometimes . . . in the slighter cases of increased tension, its adoption has rendered the performance of iridectomy unnecessary."—*Liverpool Medical and Surgical Reports,* vol iii, p. 74.

On the Defects of Shape and Disturbances of Accommodation of the Human Eye.—Dr. Albert Schumann, in this work of his, seeks to prove that the diagnosis of an error of refraction by means of the ophthalmoscope is not to be depended on. Especially is it false, from the circumstance that in the upright image the fundus only becomes plainly visible by greatest proximity to the observer, to argue upon emmetropic conformation. A truly emmetropic eye shows the background, at the least 6—8″ in the upright image; those eyes, which require a yet greater approximation being slightly myopic. Emmetropia is altogether much rarer than is generally thought. (That weak myopia, *e.g.*—$\frac{1}{120}$, probably more often appears as a completely normal state of refraction, Donders expressly mentions indeed). When the fundus oculi indeed is visible from afar, one is not yet permitted to infer hypermetropia, and to trouble the patient with convex glasses, but it may be also astigmatism with or without spasm of accommodation present. Further, the author yet makes a point, that the apparent size of the papilla is of importance in the ophthalmoscopic investigation. It always maintains an inverse proportion to the size of the image of the flame visible in the fundus of the eye. In all probability there should, with strikingly large papilla, be assumed spasm of accommodation, with one particularly small, on the other hand, a flattened lens. The author has, moreover, constructed a very simple model in which, by throwing forward or backward the focus, and by placing different glasses, the accuracy of his conclusions may be easily authenticated.—*Schmidt's Jahrbücher*, 1869, No. 3, p. 349.

Influence of the Choroid in Vision.—An inaugural thesis of Dr. Victor Bravais, presented to the Paris Academy of Medicine, is to be thus briefly understood:—In view of the well-known fact that most animals have a very reflecting choroid, and also that, with the ophthalmoscope, we every day see even in man, whose choroid is black, that light at the bottom of the eye is never altogether absorbed—it is difficult to admit that this absorption should be indispensable to good visual perception. On the contrary, it is more reasonable to ask if this returning light, which traverses the retina from behind forwards, is not rather useful than harmful to vision.

The author first calls to mind that good vision can only be got with distinct images on the retina, but he points out that these images, formed by the direct rays, may preserve their distinctness in spite of reflected light. As for that, it is only necessary that each returning ray again passes through the same point of the retina, and this condition is complete whenever the plane which reflects or diffuses the light is in contact with the sensitive screen. Each luminous ray which has traversed a cone or a rod, being arrested and reflected exactly at its extremity, can only return by the same sensitive element, thus re-enforcing the impression without at all subtracting from the distinctness of the image and its perception. Such is the case in the eye having a tapetum and in the normal eye, which arrests and reflects the light by its epithelial pigment layer backed by the rods; in one as in the other, the reflexion is so made as to serve for perception of light. It is quite otherwise if, instead of being made on a plane touching the retina, the reflection is further off, as, for instance, on the sclerotic, when the choroid is devoid of pigment (albinos, old people). The resulting disturbance is not to be attributed to the quantity of the reflected rays but to the defective manner of their reflection. The light which has traversed a cone not meeting the pigment layer, which should return it by the same cone, passes on as far as the sclerotic; in returning whence to light up diffusedly the retina, it is that it weakens, by the concussion of many cones, the impression of which should have been limited to one only. The image thus is cast on an illuminated fundus, and its perception is the less distinct.—*Gazette Hebdomadaire*, 7th Jan., 1870, p. 10.

Foreign Bodies in the Vitreous.—Dr. R. Berlin, of Stuttgart, has recently extracted two, of large size, with probe and forceps. Neither eye preserved any vision, but they became uninflamed, shrank, and were not extirpated. In the first case, before probing the wound, the author convinced himself that the foreign body (a small leaden bullet of an air-gun) would be found in the vitreous space by shooting—with the same gun, &c.—a deal door at a distance of four paces; only a mark was left on the door, so he rightly concluded "that the ball had not force enough to pass twice through the membranes of the eyeball," &c. In the second case the author diagnosed an extensive hæmorrhage in the lower parts of the vitreous space—(the position of a lighted candle was only noticed in the lower half of the field of vision)—caused, the author says, "not by concussion of the eyeball, but *by the direct blow from the foreign body.*" By gravity the foreign bodies sink; the greatest quantity of blood is found about the foreign body. He continues—"In the cases known to me, examined immediately after the accident, in which the body was found fixed in the posterior wall of the eye, an extensive hemorrhage was never observed, and I believe that in such cases there will be little or no limitation of the field of vision."—*Knapp and Moos, Archives of Ophthalmology*, Vol. I, No. 1, pp. 30—6.

Retinal Aneurysms.—M. Henry Lionville, in a note to the Académie des Sciences, records a new case in which his observations, made since 1868, are corroborated by M. Charcot, on the co-existence of aneurysmal changes in the retina with aneurysms of the minor arteries of the brain. The patient, an old woman of 72 years, had innumerable miliary aneurysms of the encephalon generally; so also in both retinæ. These latter corresponded to little hemorrhages infiltrated in the proper coats of the retinal layer. The microscope showed also the aneurysmal structure of these changes of the vessels in the retina as in the brain.—*Archives Générales de Médecine*, April, 1870, p. 503.

Embolism of a branch of the Retinal Artery with Hemorrhagic Infarctus in the Retina.—Dr. Knapp, of New York, relates a case, and gives a coloured illustration of the ophthalmoscopic appearances, and makes some valuable observations on the mode of their causation. A woman of 37 had peri- and endo-carditis, recurrent, and rather suddenly lost vision in the inner, lower quadrant of one retina. One branch of the retinal artery (the lower) was abruptly hidden between the centre and margin of the optic papilla by a reddish gray opacity, beyond this it appeared extremely thin, and further on somewhat enlarged, and pursued its regular course in subdividing to the periphery. The pressure of a finger would produce pulsation in the upper branch of the retinal artery only. The veins of the part of the retina affected were enlarged and tortuous; some could not be traced to the optic disk. There were numerous apoplectic spots here, chiefly around small venous twigs, on both sides of the larger branches. The retina had lost its transparency in the whole of this triangular space, showing a reddish yellow opacity. The defect in the visual field was never recovered from, but the morbid alterations of structure disappeared. The author proceeds to combat the statement that in twenty-four hours at most the central retinal artery obstruction is obviated by the collateral ciliary arteries, by analogy, &c. He has demonstrated to his ophthalmoscopic students the effects produced on the retinal circulation by pressure on an observed eye. After embolism or injury of the retinal artery, "as a rule, it was not before the end of the second week that pulsation could again be seen by applying pressure to the globe; and at this time, too, the calibre of the retinal vessels had regained half or two-thirds of its normal size," "a most valuable means of diagnosis, indicating not only the absence or presence of circulation in the retinal vessels, but enabling us to judge, to a certain degree, even on the strength of the current," &c.

That the retinal hemorrhages were not primary, so leading on to the blindness, the author says, "Even in cases of more extensive apoplexy I always found a fraction of visual power remaining, and though dark spots, to a certain extent, obscured the visual field, yet there was never a whole quadrant entirely missing, as in this case." The sudden increase in calibre of the retinal artery may fairly be accounted for as the place of junction of a ciliary and retinal artery. "The blood seems to have been driven out of the arteries by the contractility of their walls."

That the peripheral retinal veins are fullest, he explains by saying that their contractility is less than that of the larger veins, and the column of blood to be moved is longer and therefore heavier. Of the tendency to varicosity of the retinal veins, he says, it was a cadaveric change, due to the stagnation, and their partial restoration of the circulation collaterally through the weakened and inefficient vessels. That the short portion of the artery between the plug and the entrance of the communicating ciliary branch remained so narrow, is, by analogy, explained, that no outlet, by reversing to that extent the course of the circulation, was found.—*Knapp and Moos, Archives of Ophthalmology*, &c., Vol. 1, No. 1, pp. 64—84.

Embolism of the Retinal Central Artery.—Dr. T. C. Allbutt says in this one instance we can *see* it, and changes in the encephalon (he is writing of embolism of the sylvian artery) are similar. The loss of function is instantaneous. The œdema creeps on somewhat slowly, not as a "serous apoplexy," but as a gradual infiltration in some way consequent upon the emptiness of their vessels,—a *compensatory* process subsequent to the plugging—not a cause of pressure upon surrounding parts, but a support to parts which are in danger of collapse. Hæmorrhage, again, is a late process, and never simultaneous with the embolism. In the eyes, *where collateral vessels are few and small*, hemorrhages are few and small.—*Medical Times and Gazette*, 30th April, 1870, p. 464.

Analogy of Light and Sound.—Mr. W. F. Barrett says, "the English 'loud,' the French 'criard,' the German 'schreiend,' are identical expressions relating to sound, also applied to glaring colours. Faintness of vision and feebleness of voice were spoken of as one. Our own words, *dim* and *dumb* were probably cognate terms in Anglo-Saxon."

"Like the limits of hearing, so these limits of vision vary slightly with different individuals; some people are capable of seeing farther beyond the red, and not so far into the violet, whilst the converse is true with others. Hence, beyond the shadow of a doubt, certain sounds and certain lights perceived by some persons are totally unperceived by others. And when we pass from human beings to the larger animals on the one hand, and to insects on the other, we doubtless have the range both of hearing and of vision considerably extended. We are not aware that the limits of vision in animals have ever been studied; but analogy and experience lead us to suppose that it differs from our own in many cases. Assuming that the perception of light is due to a sympathetic vibration of the filaments of the retina, it merely needs that these filaments should be capable of vibrating only the one hundred millionth of a million per second slower, and what we call black heat would be perceived as light; and if these filaments could vibrate the same amount faster, what we call the actinic, or invisible chemical rays beyond the violet, would become directly visible. Now it is highly improbable that the retina of every animal in creation should be, as it were, tuned to the same pitch as ours; and if this be so, then forces unrecognised by our senses are perceptible elsewhere."—*Quarterly Journal of Science*, January, 1870, pp. 1—16.

Manifestations of Cerebral Compression in the Eye.—Prof. W. Manz says, "I have in the past year made experimental researches into the final influence of an increased intracranial pressure on the eye, especially on the retinal circulation, and have reported the earliest results thus obtained to the session of the Freiburg Natural History Society of the 30th April. The experiments were mostly made on rabbits, sometimes on dogs. The increase of intracranial pressure was effected by injection of different fluids in the aperture trepanned in the upper part of the skull of the living animal. The following were the chief results of the experiments:—

"1. The simple opening of the skull, at least to the small extent that was necessary for the introduction of the canula I have employed; neither in the eye, nor anywhere else in the body, produces any disturbance, at least any such is inconstant, and their appearance, as *e.g.* of convulsions, changes of the breathing and frequency of the pulse, were probably more to be attributed to the fortuitous collateral circumstances of the operation.

"2. The injection of any fluid into the cranial aperture produces generally as a first symptom a greater filling of the retinal veins, whilst the arteries behaving very differently, appear now thinner, now thicker, or even retain their prior fulness. The above appearances are always seen simultaneously, and almost equally in both eyes.

"3. The venous stasis begins in the central part of the papilla (rabbit), spreads thence on the two wing-like appendices, and by greater pressure betokens itself not only as an increase of the diameter of the vessels, but also in their winding course, which appear as movements in them.

"4. With remission of the injection these windings cease immediately, the greater fullness of the veins somewhat later, but in quite a short time, likewise ceases; it generally subsists longer in proportion as the whole experiment has lasted longer.

"5. If the injection is continued long enough, or so quickly brought about, the papilla becomes pale, the vessels bloodless—an appearance which always betokens the death of the animal. The emptying of the vessels appears first in the arteries, then in the veins, and in both proceeds in a centrifugal direction.

"6. Somewhat later than the repletion of the vessels there constantly, in the rabbit, appears a contraction of the pupil, even when this had previously been strongly atropinized; besides, generally it was accompanied by an upward direction, in some cases rolling of the eyeball; these movements ensued rather slowly.

"7. The anatomical results are various, according to the quantity and quality of the fluid injected, and according to the time which from the latter operation to the death of the animal has expired. As particularly noteworthy it should be in the mean while pointed out that in most of the experiments, even in a short time, the injection was found to have filled the subvaginal space (Schwalbe) of both optic nerves, but then also, when the animal, as soon as the first symptoms of stasis in the papilla had been remarked, was killed by the letting of blood, the fluid injected had not then as yet left the arachnoid space, correspondingly had not as yet penetrated the optic nerve sheath."—*Centralblatt für die Medicinischen Wissenschaften,* 19th February, 1870, pp. 113—14.

REPORT ON MIDWIFERY.

By Robert Barnes, M.D. Lond., F.R.C.P.,
Obstetric Physician to St. Thomas's Hospital.

1.—The Non-Pregnant State.

1. *On Imperforate Hymen.* By Dr. G. Conradi.
2. *On the Treatment of Dysmenorrhœa and Sterility by Bilateral Division of the Cervix Uteri.*
3. *A case of Absence of the Vagina.* By Dr. Pallen.
4. *The Pathology and Treatment of Dysmenorrhœa Membranacea.* By Dr. Mandl.
5. *On the Relations between Morbid Conditionc and Processes in the Sexual Organs of Women to Mental Disturbances.*
6. *Chorea complicating Pregnancy.* By Dr. Hall Davis.
7. *On Retro-uterine Hæmatocele.* By Dr. Rockwitz.
8. *Case of Death from a Strangulated Ovarian Tumour.* By Dr. Lawson Tait.
9. *Lardaceous Disease of the Kidney consequent on Abscess of the Ovary.* By Dr. Dickinson.
10. *Diseased Uterus of a Woman who died of Tubercular Peritonitis.* By Dr. Willoughby.
11. *A large Fibrous Polypus growing from the Lower Wall of the Urethra.* By Dr. Honing.
12. *Removal of an imbedded Uterine Fibroid.* By Dr. Whiteford.
13. *On the Tolerance of the Female Genital Canal against Traumatic and Septic Influences.* By Dr. Freund.
14. *Contribution to the Knowledge of Osteomalacia.* By Moers and Muck.
15. *Complete Inversion of the Uterus in a Multipara, caused by a fibrous growth in the Fundus.* By Professor E. Martin.
16. *The Histories of four cases of Chronic Inversion of the Uterus.* By Dr. T. G. Thomas.
17. *Thoughts on Chronic Inversion of the Uterus.* By Henry Miller, M.D.

1. Dr. G. Conradi contributes a memoir on *imperforate hymen.* A girl, æt. 13, had scarlatina; at 14 she complained of pains in the lower part of the back; at 16, the pains having increased in severity, she rather suddenly experienced a sensation as if something burst from the interior towards the genital organs. This feeling increased, and with it a frequent desire and difficulty to pass urine. The vagina was found occluded by a bluish membrane; fluctuation was felt in it, and also by rectum. The membrane was punctured, and thirty-two ounces of fluid escaped. The opening in the hymen was maintained by a tent. Menstruation was established.—*Med. Times and Gaz.*, 1869.

2. Dr. Gustav Braum communicates a report upon the *treatment*

of dysmenorrhœa and sterility by bilateral division of the cervix uteri. To divide the cervical portion he uses Küchenmeister's scissors. He then cuts the os internum uteri and the neighbouring portion by a blunt-ended lancet-shaped knife. On the third day he passes the sound. He keeps the patient in bed for seven or eight days, and during the succeeding menstrual period. Out of sixty-seven cases, the result was favorable in fifty-three, in eleven unknown, in four interrupted by subsequent affections of the abdomen. In eleven cases pregnancy followed. In no case did the operation cause any considerable disturbance.—*Wien. Med. Wochenschr.*, 1869.

3. Dr. Pallen relates a case of *absence of the vagina,* in which three operations ended in the establishment of menstruation. A girl, æt. 20, had since 14 experienced menstrual molimina without any show of menses. Her health failed. On examination it was at first thought there was retention of menstrual fluid from imperforate hymen. But it was found that there was no vagina, a small undeveloped uterus, and no accumulation. An artificial vagina was made by incision and laceration by the fingers, and maintained by wearing a glass plug and Barnes' dilators. A periodical flow of blood was established, and good health was regained. The case, Dr. Pallen says, is unique, the recorded cases of absence of vagina differing in some respects.—*St. Louis Med. and Surg. Journ.*, 1870.

4. Dr. Mandl discusses the pathology and treatment of *dysmenorrhœa membranacea.* He details a case, and submits the following conclusions:—It is a disease *sui generis;* its pathognomonic feature is the expulsion of a membrane resembling decidua, within and not later than forty-eight hours after the advent of menstruation; occasionally membranes are expelled during extra-menstrual intervals; they are formed in consequence of a chronic inflammation; it leads, after long duration, to retro- and ante-versions, congestions, and lastly, to marked disturbance of the general health; sterility is observed in all cases; the ætiology is quite unknown; chloride of potassium, applied immediately to the uterine mucous membrane, seems to be palliative.—*Wien. Med. Presse*, 1869.

5. *On the Relations between Morbid Conditions and Processes in the Sexual Organs of Women to Mental Disturbances* is the title of an elaborate memoir by Dr. C. E. Louis Mayer, presented to the Berlin Obstetrical Society. It is too long for analysis in this place. It is barely possible to indicate the questions discussed. These are—the influence of sexual excitations on the mind in childhood; of menstruation, of the climacterium upon the mind; the relations of individual diseases and processes to psychoses, such as inflammations, faults of development, displacements; hysteria; somnambulism; the different forms of mental derangement in their relation to diseased conditions and antecedents in the sexual organs; hysterical insanity; erotic character of psychoses associated with sexual disorders; states of psychical depression—hypochondria, melancholia; states of psychical excitation—exaltation, mania, dementia. He has analysed 145 cases of mental disorder associated with morbid states of the

sexual organs, excluding labour and puerpery. The memoir is enriched by numerous clinical illustrations, and especially deserves to be read by those physicians, special alienists, and others, who regard nervous symptoms in females as universally dependent upon primary or essential disorder of the nervous system, ignoring the intimate relations of the nervous system to the generative system, and who, neglecting this study, fail to appreciate the frequency with which the nervous system is attacked through the generative system. —*Verhandl. der Gesellsch. f. Geburtsk.*, 1869.

6. Dr. J. Hall Davis describes a case of chorea complicating pregnancy. A lady, æt. 20, in first pregnancy, three months gone, had been attacked by chorea three months previously. Hysterical; smooth systolic murmurs over heart's base. She took bromide of potassium, then bromide of ammonium, and recovered.—*Clinical Trans.*, 1869.

7. Dr. Rockwitz relates an interesting case of retro-uterine hæmatocele, and discusses the theory of Virchow and Schröder, which affirms that the source of the blood is the inflamed peritoneal surface. The case presents the following features:—The usual signs of shock and pain in abdomen set in during profuse menstruation, on the 3rd August. On the 9th September the tumour reached above the navel; the rectum was so compressed that defecation was greatly obstructed. Incision was made in the most projecting part of the vagina. A considerable quantity of blood, not mixed with coagula or pus, flowed. On the 13th the discharge became thin and fetid, and was mixed with firm coagula and pus; some hectic. The vagina was washed out with warm water; the tumour was sensibly diminished in size. She quite recovered. The case is a good example of the value of opening the blood-tumour.

Virchow states that the blood usually comes entirely, or in part, from the new-formed vessels on the peritonitic layers of the abdominal excavation. Schröder goes further, and says a tumour caused by a collection of blood, which can be felt in the vagina, can only arise when a cavity is pre-formed for it, that is, when Douglas's sac is first closed above by a partial adhesive peritonitis. He contends, like Virchow, that inflammation is the first event, and the hæmatocele the second; but he also says the blood-tumour is encapsuled, because the space was already converted into a bag by the adhesions.

Rockwitz seems to favour this hypothesis. On the other hand, Dr Ferber, in a note in the same journal, contends that the idea of a pre-formed cavity is a pure hypothesis; and that the blood-collection often takes place very gradually, *stillatim*, the peritonitis being secondary, and designed to isolate the blood-tumour. (The general history of cases certainly supports the latter view.—R. B.)—*Mon. f. Geb.*, December, 1869.

8. Mr. Lawson Tait relates a case in which death ensued from strangulation of an ovarian tumour. The patient, æt. 48, suffered from strangulated femoral hernia. This was relieved by operation, but tympanitis and vomiting continued. The temperature remained at

101°, and death took place four days after the operation. *Autopsy.*—A small ovarian tumour, consisting of two equal-sized cysts, one of which was totally gangrenous, the other partially so, was found in the right ilium. There was scarcely any peritonitis, a fact which had been diagnosed by the temperature. The pedicle of the tumour was like an umbilical cord; it had been twisted by four and a half revolutions of the tumour. (Mr. Tait thinks the case unprecedented; but similar cases are related by Rokitansky, and two have been described by the reporter.—R. B.)

9. Dr. Dickinson relates the following case of disease of kidney associated with ovarian abscess.

A young woman who died in St. George's Hospital had an abscess in the neighbourhood of the uterus shortly after labour; profuse discharge of pus took place by vagina; the legs became œdematous and dropsy extended; hectic set in; the lungs became affected; frequent vomiting; urine lithatic and highly albuminous. She died at end of seven months. The right lung was hepatized, recent vegetations upon the mitral valve, kidneys enlarged, Malpighian bodies enlarged and giving with iodine "amyloid" reaction. One ovary excavated by several abscesses.—*Pathological Trans.*, 1869.

10. Dr. Willoughby relates a case of diseased uterus of tubercular character.

A woman, æt. 35, mother of several children, had pulmonary tubercle, and died of tubercular pleurisy and peritonitis. She had not menstruated for years.

The pelvic peritoneum was beset with cheesy masses, one of which, the size of a walnut, was beneath the peritoneum; this mass had produced *rectangular anteflexion* of the uterine cavity. The Fallopian tubes were immensely distended with the same cheesy-looking substance, and curiously convoluted; the fimbriated extremities were entirely obliterated by coalescence with the ovaries. These organs were as large as walnuts, filled with the same cheesy material, and one contained an effusion or hæmatocele. No tubercular deposit was apparent in the lining membrane of the uterine cavity.—*Pathological Transactions*, 1869.

11. Dr. Höning relates a case of *a large fibrous polypus growing from the lower wall of the urethra.* A woman, æt. 41, suffered from dysuria and bowel obstruction. A tumour the size of the fist projected from the genitals; it sprang from the left side of the urethra; the meatus was an opening an inch long. A still larger tumour was contained in the vagina. Both, being turned out of the vagina, were removed by scissors. Actual cautery was applied to stop profuse bleeding. The mass was a soft fibroid, weighing three pounds. The patient recovered.—*Berlin. Klin. Wochensch.*, 1869.

12. Dr. Whiteford relates a case of *removal of an imbedded uterine fibroid.* The patient was nearly exhausted by hæmorrhage. Dr. Whiteford dilated the cervix by incisions and tents, then made an incision into the tumour, which projected from the anterior wall of the

uterus. After some days, when the tumour had been a little drawn down, it was seized by vulsella and dragged out, this operation lasting about two hours. The patient made a good recovery.—*Ed. Med. Journ.*, Feb., 1870.

13. Under the title "Tolerance of the female genital canal against traumatic and septic influences," Dr. Freund relates a marvellous case in which the fœtal head was retained for ten years in utero. A woman was delivered by extraction by the feet; the trunk was torn from the head, which remained in the uterus. It could not be extracted. It was, therefore, left to nature. Four years later a fragment of bone came from the vagina. After ten years examination was made, when a bone was felt by the sound to be in the cavity of the uterus. It was not removed. The author concludes that we should not be too meddlesome in removing the fœtal head! (The case is a curious instance of imperfect knowledge of operative midwifery. The removal of a head left behind in utero is not a difficult matter. It is simply necessary to perforate and crush down by the cephalotribe, or to extract by the craniotomy forceps. It is an affair of ten or fifteen minutes.—R. B.)—*Deutsch. Klinik*, 1869.

14. Drs. Moers and Muck relate three cases of osteomalacia. One case perfectly recovered. The urine showed abundance of lactic acid. She took phosphate of lime, iron, and cod-liver oil. The two other cases ended fatally. The last was remarkable for the fact that, together with the osteomalacic disease of the bones of the trunk, the long bones, especially the femur and humerus, became greatly atrophied.—*Deutsch. Arch. f. Klin. Med.*, 5 Bd.

15. Prof. E. Martin relates a case of *complete inversion of the uterus in a multipara, caused by a fibrous growth in the fundus.* A woman, æt. 46, was seized with profuse uterine hæmorrhage, which often returned. Two years later a tumour was observed protruding through the vulva and causing retention of urine. The tumour was so little sensitive that the patient cut off a piece with scissors. It was as large as a fist. It was found united by a pedicle to the fundus of the inverted uterus, no trace of os uteri being left. The tumour was cut off by an écraseur. Attempt to reduce the inverted uterus was postponed. A few days afterwards it was found that spontaneous reinversion had taken place. The patient recovered.—*Monatssch. f. Geburtsk.*, Dec., 1869.

16. Dr. T. G. Thomas gives the *histories of four cases of chronic inversion of the uterus,* and describes a bold course of treatment. He quotes first a case by Dr. Emmet. The uterus had been inverted about six months; there was great anæmia and hectic. Under ether attempt at forcible taxis, by pressure on the fundus, was made without much effect. Then Dr. Emmet allowed the fundus to drop into the palm of his hand, and passing the thumb and fingers around the mass, as high as possible within the cervix, he continued to enlarge the space between the neck and inverted body by rapidly expanding the fingers as much as possible, at the same time making steady up-

ward pressure, with a view of returning first the portion last inverted. This manœuvre, aided by endeavouring to roll out the inverted portion by the other hand on the abdomen, was successful in effecting considerable dilatation of the cervix and partial return of the uterus. After three hours and fifty minutes of this manipulation, in which Drs. Emmet, Sabine, Elliott, and Thomas relieved each other, the uterus was completely returned. She recovered and became pregnant.

CASE 2.—Inversion of four years' standing. After two unsuccessful attempts at forcible taxis, a Barnes' dilator was placed in the vagina for some hours. This was found to have supported the uterus well. Attempts were then renewed, trying Dr. Noeggerath's plan of pushing in one cornu; considerable hæmorrhage compelled to desist. Next day efforts on the plan described in the preceding case were sustained during nearly three hours, when the uterus was reduced. The woman recovered. During this proceeding Dr. Thomas held a plug of boxwood, with a handle a foot long, forcibly in the cervical ring through the abdominal walls. He believed this mode of exciting counterpressure is more effectual than by the fingers in supporting the uterus and in dilating the cervix.

CASE 3.—Inversion of ten months. Dr. Thomas began by putting the patient on the free use of belladonna, directed a stream of water against the inverted uterus three times a day, and placed suppositories of belladonna in the rectum. Under ether he grasped the tumour so that the fingers surrounded the pedicle; he then pushed the mass steadily upwards against the abdominal wall, where it met the counterpressure of his left hand. In exactly ten minutes the entire cervix yielded, and the body went up; then seizing the body with the thumb on one horn, and the index finger pressing the other, the horn pressed by the thumb became indented, soon the other horn followed, and in just twenty-five minutes the operation was completed.

CASE 4.—Inversion of twenty-one months. Several attempts at reduction by forcible taxis and by wearing a vaginal air-pressary had failed. Dr. Thomas resorted to the belladonna and douche, as in Case 3, for a week; but an hour's persistence in the methods narrated, aided by the abdominal plug, resulted in only partially expanding the cervix. A caoutchouc bag was then placed in the vagina, and attempts renewed next day, with no better success. Next, the uterus was drawn down, and an incision was made in the tissue of the neck towards the subjacent peritoneum. A free jet of blood followed; many attempts were made to tie the vessel, but failed; the bleeding was at last stopped by stitching the lips of the wound together. A week later the following remarkable proceeding was carried out. The uterus was lifted up so that the operator could feel the cervical ring against the abdominal wall. He then cut down in the median line, as for an exploratory incision in ovariotomy; then he inserted his finger into the uterine sac, and found there was no adhesion. He then inserted a short dilator, made on the principle of a glove-stretcher, into the cervix and expanded the blades. "The dilatation was easy and rapid," but the contraction returned as soon as the dilator was withdrawn. The uterus was drawn down and one horn pushed in, then the other, and the organ was reduced. The vessel which had bled so profusely a week before burst out again. This was, however, stopped. A finger passed through between the uterus and bladder. The abdominal wound was closed by silver sutures; the vaginal rent was not interfered with. The patient quite recovered. (The proceeding seems a desperate one, but it is better than amputation. It might, however, possibly have been avoided by more systematic application of elastic pressure.—R. B.)

17. Dr. Miller's paper is mainly critical. It is a plea for amputation. He discusses the value of Dr. Thomas's operation, as compared with amputation; he contends that the loss of the uterus is not so much to be considered, for the ovaries being preserved, the woman retains her sexual characters. He institutes a statistical comparison between the results of amputation and of manipulative measures, and appears to argue that manipulation is necessarily so severe as to show a mortality scarcely inferior to amputation. (He does not notice the reporter's case, in which reinversion was successfully effected after incising the cervix. The subject of this case is now far advanced in pregnancy, a sexual result which cannot be expected after amputation.—R. B.)—*Richmond and Louisville Med. Journ.*, April, 1870.

II.—The Pregnant State.

1. *The Duration of Gestation.* By Dr. Ahlfeld.
2. *Three cases of Retroversion of the Gravid Uterus.* By Prof. Halbertsma.
3. *Retroversion of the Gravid Uterus in advanced Pregnancy successfully Reduced.* By Dr. Riedl.
4. *Acute Leucocythemia in connection with Pregnancy.* By Dr. R. Paterson.

1. Dr. Ahlfeld investigates with great care the problem of the *duration of gestation.* Taking 219 cases observed by himself, by Hecker, and by Veit, he finds that conception took place on an average 9·72 days after the *first day* of menstruation, and in 161 cases on an average 5·28 days from the *last day* of menstruation; but it most frequently took place within three days. Faye arrived at a similar result.

As to the question whether the virginal os uteri is more easily disposed to conception than the gaping os of women who have borne children, he finds that, comparing 130 pluriparæ with 75 primiparæ, the same average of about ten days after the first day of menstruation was observed.

Taking 425 women, whose children seemed mature, the average duration of gestation was 269·91 days, reckoning from day of conception. Hecker's tables give an average of 273·52 days. The range was from 231 days to 329, so that there is manifestly a fault in determining the day of conception.

Ahlfeld gives a table of thirty cases, including six from Faye, of presumed single or well-defined coitus. Gestation varied from 233 days to one case of 313 days. Both these extremes are taken from Faye. The greater number ranged within 270 and 275 days. The average of all was 269·17 days, which corresponds closely with the period obtained by other modes of observation. (It is to be remarked that the weight of the child in Faye's minimum case was 3000 grammes, and in the maximum case of 313 days it was only 2530. Since 3000 grammes is below the average weight of a mature child, it seems only reasonable to infer that conception took place considerably within 313 days. With this exception no other case out of the thirty exceeded 287 days, and of the remaining twenty-eight all were below 282.—R. B.)

Ahlfeld then refers to the law enounced by Cedershjöld that labour takes place at the tenth menstrual epoch due, so that we should multiply the individual interval between two periods by 10. In many women this interval is not 28 days, but 27½, 28½, 29, 30. Hence a duration of 275, 285, and so forth, is explained. By most authors, says Ahlfeld, the duration is placed too high; 280, even 275 days, is too high. To estimate the expectancy of labour Naegele added seven days to the first day of the last menstrual appearance, and then reckoned three calendar months back. Thus he took as the date of conception the second day after the cessation of menstruation, with an average duration of menstruation of five days. Thus he arrived at an average of 273 days, which is very close to the reality. Ahlfeld's own plan is to take the tenth day from the beginning, the fifth from the end, of menstruation. There is a possible error in both ways of fixing the date of conception, and to illustrate this point he gives a table of 261 cases, calculated according to both, and showing the actual day of labour.

As to the sensation of the movements of the child, he shows that in 43 cases, in which the day of its occurrence was noted, it ranged from 108 to 134 days, the average being 132·77 days.

The duration of labour in primiparæ was, on an average, 20 hours 48 minutes, and in pluriparæ 13 hours 42 minutes.—*Mon. f. Geb.*, 1869.

2. Professor Halbertsma, of Utrecht, relates *three cases of retroversion of the gravid uterus.* The first is unusually interesting. A woman, æt. 24, was seized with retention of urine and fæces, and died unconscious, with stertorous breathing. *Section.*—The bladder was greatly distended; the rectum was stretched open by a tumour in which small parts of a child were felt. On opening the abdomen the distended bladder was cut into and emptied. Behind it was seen the retroverted and retroflected uterus, which could very easily be restored from the pelvic cavity. When the hand was brought behind the fundus uteri, and this was lifted up a little, the uterus sprang like a feather into its place. There was nothing else abnormal. The mucous membrane of the bladder was swollen and softened; the muscular fibres formed rounded and projecting bundles. In the second case, a multipara, the usual symptoms led to discovery of retroversion of the uterus—fifth month. Attempt to reduce the uterus produced so much pain that it was given up. The catheter

was left in the bladder, and next day the uterus had replaced itself. She was delivered at term of a live child. Case 3. Pluripara.—The history nearly resembles that of Case 2. Spontaneous reposition took place.—*Mon. f. Geburtsk.*, Dec., 1869.

3. Dr. Riedl relates a case described as *retroversion of the uterus in advanced pregnancy successfully reduced.* A woman had retroversion of the uterus with the usual symptoms in the fourth month of her first pregnancy; it was reduced by pressure from the rectum, and the pregnancy went on to a favorable termination. In her next pregnancy the same symptoms of distress became developed, when, according to her calculation, she was eight months gone. After emptying the bladder of a large collection of alkaline urine, and the bowels from hardened fæces, reduction of the uterus was effected by pressure through the rectum by means of a speculum-plug padded with linen. She was delivered next day. The child weighed only 4 lbs., and was very small. [The history shows that retroversion was, at least the second time, an original condition; although the child was small, and therefore probably the pregnancy was less advanced than was supposed, the uterus could scarcely have been wholly locked in the pelvis; the case was no doubt one of *partial* retroversion, the greater portion of the uterus being developed in the abdominal cavity.—R. B.]—*Ibid.*

4. Dr. Robert Paterson relates some interesting cases of *Acute Leucocythemia in connexion with Pregnancy.*

Referring to the researches of Vidal and others, which tend to show that the disease is essentially chronic, and only recognisable by the occurrence of splenic enlargement, he shows that it may be recognised before the splenic enlargement is detected or exists by the microscope. The splenic and glandular enlargements in pregnancy indicate an advanced stage rapidly tending to death.

Case 1.—Æt. 20, primipara. In early months she had severe sickness, which disappeared, leaving her in good health. One month before confinement she had a sallow look, hollow eyes, pulse 90, slightly feverish at night; urine giving no trace of bile. Labour, natural at term; child healthy. The uterus contracted well, but very troublesome hæmorrhage followed the expulsion of the placenta. About the sixth day a marked change for the worse took place; pulse, 120; skin, hot, with increased tawniness; decided enlargement of liver and spleen; slight increase in size in glands of neck. The blood containea an unusual number of leucocytes or white cells. A fatal issue was foreseen, and came in twenty-four hours. Within this time the glands of the neck enlarged rapidly, with increased difficulty in swallowing; any attempt to swallow in the horizontal posture produced such feelings of suffocation that she jumped upright gasping for breath. Death on the evening of the 11th day. Autopsy refused.

Case 2, also primipara. Seen in consequence of profuse post-partum hæmorrhage. History during pregnancy similar to Case 1; she became sallow towards end. Considerable enlargement of spleen and liver;

slight enlargement of lymphatic thyroid and other glands; pulse, 120; blood full of white cells. The gland swelling increased, and the patient was cut off by asphyxia fourteen days after labour.

Dr. Paterson has detected the disease in other cases during pregnancy. Vidal says it begins in four cases out of ten during pregnancy. Paterson gives two cases which ended successfully. Great pains were taken to counteract the hæmorrhage, which is so imminent in these cases. He makes the incidental observation that, although the mother's blood in all three cases was charged with leucocytes, yet the children suffered no way, and were born robust and healthy.—*Edinb. Med. Journ.*, June, 1870.

III.—Labour.

1. *On the Muscular Forces employed in Parturition.* By Rev. Dr. Haughton.
2. *The Condition of the Cervix Uteri during Labour.* By Dr. Breisky.
3. *Relaxation of the Pelvic Symphyses during Pregnancy and Parturition.* By Dr. Snelling.
4. *A Case of Rupture of the Uterus.* By H. P. Stearns.
5. *On the Production of Face-Presentation.* By Dr. M. Duncan.
6. *Pilo-cystic Tumours complicating Labour; Extirpation of Tumour by Abdominal Section.* By R. W. Gibbes.
7. *Extirpation of the Puerperal Uterus by Abdominal Section.* By Dr. G. H. Bixby.
8. *The Application of Electricity to Labour.* By Dr. Saint-Germain.
9. *On Using Short Forceps with the Patient in the Supine and Lateral Positions.* By Dr. A. Inglis.
10. *A Case of Ovarian Tumour Complicating Pregnancy.* By Dr. G. Kidd.
11. *A Case of Dilatation of an Osteomalacic Pelvis during Labour.* By Dr. Olshausen.
12. *Turning in Disproportion after Failure of Forceps.* By Dr. B. Hicks.
13. *Cæsarian Section performed Four Times on the same Person.* By Dr. Oettler.
14. *Two Cases in which Cæsarian Section was performed on Dead Women, the children being saved.* By Dr. Pingler.
15. *A Case of Cæsarian Section Post-mortem.* By Dr. Beckmann.
16. *On the Value of Artificial Premature Labour.* By Dr. Spiegelberg.

1. The Rev. Samuel Haughton, M.D., has contributed a very valuable memoir on the muscular forces employed in parturition to the Dublin Obstetrical Society. He says that in the first stage of labour, the involuntary muscles of the uterus contract upon the fluid contents of the organ, and possess sufficient force to dilate the mouth of the womb, and generally to rupture the membranes. In the second stage of labour reflex action calls in the voluntary abdominal

muscles, which aid powerfully the uterine muscles in expelling the fœtus.

Studying, first, the force of the uterine muscle, he finds the mean weight of this muscle, derived from Heschl, Montgomery, and Levret, to be 1·56 lbs.; and the mean thickness of the muscular wall to be 0·1519 inch.; and the tensile strain of uterine wall per inch to be 15·577 lbs.; and from these data he calculates that the maximum hydrostatical pressure produced by uterine contraction is 3·4 lbs. on the square inch. Then citing the experiments of Dr. Duncan on the pressure necessary to rupture the membranes, who found the greatest pressure was 3·1 lbs., and the least 0·26 lbs., giving a mean of 1·2 lbs., and combining the experimental result with his calculation he concludes that the uterine muscles are capable of rupturing the membranes in every case, and possess, in general, nearly the requisite power.

The extreme force of uterine contraction producing a pressure of 3·4 lbs. per square inch, acting upon a circle 4½ in. in diameter, which is assumed as the average area of the pelvic canal, is equivalent to a pressure of 54·1 lbs. Joulin's experiments give 110·23 lbs. as the maximum force, uterine, and abdominal muscular combined, required to expel the fœtus. Duncan considers 80 lbs. as the maximum. This would represent 5·03 lbs. to the square inch, which is greater than the uterine muscles unaided are capable of producing.

The author then discusses the force brought in by the abdominal muscles, which are four in number—the rectus abdominis, obliquus externus, obliquus internus, and transversalis. He found by experiment upon three young men, multiplying the curvatures into the tension of the abdominal muscles at the navel, that the result was an expulsion force of 32·926 lbs. to the square inch available to assist the uterus in completing the second stage of labour. Adding the combined forces we get—

Voluntary muscles	=	54·10 lbs.
Involuntary muscles . . .	=	523·65 lbs.
Total		577·75 lbs.

"Thus we see that on an emergency somewhat more than a quarter of a ton pressure can be brought to bear upon a refractory child that refuses to come into the world in the usual manner."

As a consequence Dr. Haughton insists that chloroform, used beyond the step of inducing drunkenness and indifference to pain, is positively injurious by cutting off the action of the voluntary muscles, without which the uterus is inefficient.—*Dublin Quart. Journ. of Med. Sc.*, May, 1870.

[Interesting as these investigations and calculations are, there are elements in the problem that appear to have attracted insufficient attention. First, experiments performed on dead tissues can scarcely be accepted as evidence of the properties of living tissues. Living tissues can probably resist a greater pressure. Secondly, the estimate of the expulsive power of the abdominal muscles, drawn from observations made upon men, cannot safely be applied to women at the time of labour. The curvature in the two cases is widely different; the tension and the pressure must also be different. Moreover the diaphragm, which undoubtedly is a power in labour, seems overlooked. Thirdly, the *reflex* muscular force employed in labour is too much confounded with voluntary power. Chloroform does not, unless carried to an extreme degree, annul reflex muscular power; and in the minor degrees of anæsthesia, found efficient to blunt the sense of pain in labour, even voluntary power is not altogether subdued. —R. B.]

2. Dr. Breisky describes the *condition of the cervix uteri during labour*. The peculiar condition is that after recent labour the whole cervix hangs down like a limp curtain around the os uteri internum. The appearances develope themselves gradually, first softening, second elongation, third paralysis. The softening does not extend beyond the sphincter of the os internum. The elongation takes place not only from distension of the membranes and head, but also independently, as in placenta prævia and cross presentations. The paralysis is developed with the preceding conditions as a progressive diminution of the contractile property of the cervix during labour. These three states are not explained by bruising and consecutive œdema of the cervical tissue, for they may be observed in abortion at the third month. Their relation to uterine contraction is unmistakeable; they are recognised in uterine action from polypus.—*Monatssch. f. Geburtsk.*, 1869.

3. Dr. Snelling gives an historical *résumé* of what has been said about relaxation of the pelvic joints during pregnancy and labour. He also investigates the anatomical structure of these joints. Dr. Fordyce Barker and Dr. I. E. Taylor, discussing Dr. Snelling's paper, added original cases and observations.—*Amer. Journ. of Obstetrics*, Feb., 1870.

4. Mr. Stearns relates a case of rupture of the uterus in labour at term, when the usual symptoms were postponed until twenty minutes after the accident. It was her fifth labour. Three conditions were present. The child was very large, weighing thirteen and a half pounds; it had been dead apparently some days; and the uterine wall in the seat of rupture, which was in the anterior wall, was extremely thin.—*Journ. of Gynæcolog. Soc. of Boston*, Nov., 1869.

5. Dr. Duncan, citing the evidence put forward by Hecker in favour of the opinion that face-presentations are the result of the dolicocephalous formation of the fœtal head, discusses the general question as to the causes of face-presentations. Hecker shows that a great majority of children born with the face presenting, have the dolicocephalous form; the height of the skull is small, it is prominent posteriorly, and has narrow but slightly bulging parietal bones. These conditions he affirms to be the original and permanent states of the heads of children born in this manner, and not the result of labour.

In ordinary labour, Dr. Duncan observes, with the vertex presenting, transformation into a face case is prevented by the greater length of the anterior cranial lever-arm, which maintains flexion. Not doubting the influence of the dolicocephalous form, he finds in the frequent lateral obliquity of the uterus, the most probable cause. This obliquity imparts a curvature to the genital canal at the pelvic brim, where the face transformation begins. If the uterus is deflected to the right, the occiput being turned to the left foramen ovale, will lie in the concavity of the uterine curve. If the head meets with much resistance the occiput will tend to advance first from the comparative shortness of the posterior cranial lever, and from its being nearer the line of the propelling force, which must incline towards the left in consequence of the deflection of the uterus. The vertex presentation will be maintained.

But nearly once in three and a half times the occiput is turned to the right, that is the convexity of the canal. The propelling power will then tend to make the forehead descend.

Dr. Duncan incidently calls attention to the far greater frequency of face-presentations in Germany as compared with this country, and refers to the dorsal and lateral decubitus in labour as possibly influencing this result. [With regard to Hecker's suggestion that face-presentation is due to the dolicocephalous form of the fœtal head, we cannot avoid remarking that the existence of this form previous to labour is not proved. The measurements and drawings of heads taken by the reporter afford strong evidence that the dolicocephalous form is the result of labour.—R. B.]—*Edinb. Med. Journ.*, 1870.

6. Dr. Gibbes relates a remarkable case of a pilo-cystic tumour complicating pregnancy. A pluripara was taken in labour at term, and was delivered by forceps. Some days afterwards a tumour the size of a turkey egg, firm, tender, movable, was discovered to the right of the uterus. About two months later it had increased to the size of the largest shaddock, and there was great suffering. Gastrotomy was determined upon. The tumour was fluctuating; a trocar plunged in let off a pint of pus. The attachment was to the left broad ligament; it was secured by ligature, and the tumour, with three inches of the Fallopian tube, were removed. The tumour contained thick, curdy pus, and a mass of fine black hair. She recovered, and afterwards menstruated by the stump of the pedicle. —*Amer. Journ. of Med. Sc.*, Oct., 1869.

7. Dr. Bixby relates a very remarkable case in which Dr. H. R. Storer extirpated the puerperal uterus complicated with a tumour by abdominal section. The tumour contracted the pelvis to such an extent as to leave only 1½ in. of space for passage of finger. It was decided to be impossible to deliver *per vias naturales*. It was determined to make a small exploratory abdominal section so as to be able to tap the tumour should it turn out to be ovarian. It was found however to be a fibro-cystic tumour of the left and lower anterior wall of the uterus, with an outgrowth nearly the size

of the fœtal head, originally pediculated, but now firmly adherent low down to the walls of the uterus. The uterus itself was much retroflexed. The tumour was cut into and found to be undergoing degeneration; profuse hæmorrhage set in. A child weighing 8 lbs. was removed, it and placenta being also decomposing. The hæmorrhage continued, and the uterus had no power of contracting. There appeared no resource but to remove the whole mass as far as possible. A large trocar was passed through the upper segment of the cervix uteri, and a metallic cord passed through its canula, and the whole was firmly tied in two parts. The mass was removed by écraseur, the stump seared with hot iron, and clamped. A portion of the outgrowth which adhered to the pelvic wall was necessarily left behind. The patient died on the fourth day.—*Journ. of Gynæcological Soc. of Boston*, Oct., 1869.

8. Dr. Saint-Germain has subjected to fresh experiments the use of electricity in labour. He never found uterine contractions excited when they had not previously set in spontaneously. When labour had begun, the conductors applied to the sides of the abdomen always induced acceleration of the pains. Dilatation of the neck was always rapidly produced. In every case the expulsion of the placenta immediately followed that of the child. In conclusion, he says, without sharing the enthusiasm of Barnes as to the application of electricity, he thinks it ought to be fully investigated. [The reporter begs leave to add that his enthusiasm, if he ever had any, was only temporary. He fully proved, in 1854, the power of galvanism to accelerate labour, as well as to provoke labour. Whilst still thinking that galvanism may occasionally be of service, he finds that the indications it meets aro much better accomplished by other means.—R. B.]—*L'Union Med.*, Nov., 1869.

9. Dr. Inglis contends for the superior advantage of applying the short forceps with the patient in the supine position. He places her close to, and nearly parallel with, the side of the bed. He says, as objections to the lateral position, that there is great difficulty in keeping the patient's breech *over* the edge of the matrass; that it puts the perinæum more on the stretch; and that it is necessary to force back the perinæum to introduce the blades. [There are real advantages in the supine position, but the objections urged against the lateral position are void of force if a proper double-curved forceps is used. It is not at all necessary to drag the patient to the edge of the bed.—R. B.]—*Dublin Med. Press*, 1870.

10. Dr. G. Kidd relates an interesting case of ovarian tumour complicating pregnancy. The tumour occupied the brim of the pelvis at the commencement of labour, the membranes ruptured, and the tumour became tightly pressed into the pelvis; it was solid. After discussing puncture, or Cæsarian section, it was determined to try to lift the tumour out of the pelvis by distending one of Barnes' bags in the rectum. This was done, the patient being placed on her hands and knees. This succeeded, and a living

child was born. He relates another case in which the tumour did not get into the pelvis; the woman was delivered twice at term without accident.—*Dublin Q. J. of Med.*, May, 1870.

11. Dr. Olshausen relates *a case of dilatation of an osteomalacic pelvis during labour.* The woman had borne seven children. Osteomalacia became developed during the last pregnancy. After twenty-four hours' labour the child was found transverse, the os uteri still closed. By external manipulation a head-presentation was obtained. Next morning the waters escaped, the head was at brim. The pelvis was now more penetrable; two fingers would pass where only one would pass before. The spontaneous dilatation continued to make evident progress. Some hours later symptoms of collapse set in, and at the same time signs of death of child. By perforation and cephalotripsy labour was soon ended. The woman recovered.—*Berlin Klin. Wochenschr.*, 1869.

12. Dr. Braxton Hicks relates four cases of labour, in which, on account of disproportion, *turning was employed to deliver after the forceps had failed.* Three of the children were born alive. He refers to other cases in which version was employed as a primary operation with a view of saving the child, but dwells upon these four as proving by a crucial test the value of turning as a means of saving fœtal life where the forceps fails. No injury was caused to the mother.—*Guy's Reports*, 1870.

13. Dr. Oettler relates the history of a case in which he performed Cæsarian section four times in succession upon the same woman. She was extremely rachitic. The conjugate diameter measured scarcely 2 inches, the diagonal conjugate 2½ inches. In subsequent operations Oettler made his incision on the side of the pre-existing cicatrices. Slight adhesions were found between uterus and abdominal walls. He also avoided the cicatrix in the uterus. The woman perfectly recovered. All the children were saved. On the last occasion the patient declared that menstruation ceased at the end of July, that a single coitus took place on the 9th August. Labour set in on the 9th May, giving 273 days as the term of gestation. [Complete success justifies the repeated operations; but it may well be questioned whether, with a pelvis measuring 2 inches in the conjugate and 2½ inches in the diagonal diameters, it is not more in accordance with sound principle and science to perform embryotomy. The cephalotribe; the removal of the vault of the cranium, and extraction by the face; or sections of the skull by the wire-écraseur after the reporter's method might all have effected delivery.—R. B.]—*Mon. f. Geburtsk.*, Dec., 1869.

14. Dr. Pingler relates *two cases in which the Cæsarian section was performed on dead women, the children being saved.* Case 1. The woman had lived in extreme poverty, and the worst hygienic conditions. She was in her fourth pregnancy when she was seized with severe dyspnœa; she had œdema of the legs; induction of labour was contemplated. The woman died. The Cæsarian section was per-

formed, and the child was extracted; the cord pulsating: it was revived, and was living ten months afterwards. Careful calculation established that the mother had been dead twenty-three minutes when the child was extracted. The pregnancy had reached the thirty-fifth week.

Case 2. The patient died of apoplexy. Extraction of a living child was made about fifteen minutes after the mother's death. It died in thirty-two minutes.—*Ibid.*

15. Dr. Beckmann relates a case in which Cæsarian section was performed after the mother's death. The woman died rather suddenly after an attack of convulsions, for which she had been bled, and had subcutaneous injection of one-third grain of nitrate of morphia. Incision was made five minutes after the mother's last breath. A male child, weighing 4 lbs., was extracted; it exhibited feeble heart-contractions. After two and a half hours of persistent use of various resuscitating means, regular respiration was established; the child lived.—*Berlin Klin. Wochenschr.*, 1869.

16. Dr. Spiegelberg discusses the value of artificial premature labour. He inquires not only whether a child was born alive, but how long it survived, and thinks that tried by this test the operation is much overrated. He compares a number of cases in which labour was provoked on account of contracted pelvis with others in which labour took place at term, and affirms that the operation is far from being so conservative to the child or to the mother as is generally believed. He submits that it should never be undertaken when the pelvis is not less than three inches in conjugate diameter. If the first indication for the operation, namely, the saving of the child where there is pelvic contraction, call for great restriction: so also is the second, namely, that of saving the child where there is habitual death of the embryo during gestation. Since this he says happens from hereditary syphilis, it matters little whether it dies unborn, or shortly after birth. In any case it dies. There remains only the third common indication, namely, the saving of the mother, uncontested. Diseases depending upon, or aggravated by pregnancy, give the most rational indication.

[As might be expected these propositions were not unanimously accepted by the meeting before which they were propounded. It is enough to state that many of the cases were observed in hospitals, where the patients are subjected to frequent examinations by students, and to various other injurious influences which endanger the success of the operation to mother and child.—R. B.]

Dr. Hegar observed that he had seen inflammatory affections of the uterus, and even fatal issue, result from injections into the uterus. —*Mon. f. Geburtsk.*, 1869.

REPORT ON SURGERY.

By John Chatto, M.R.C.S.E.

On Arterial Transfusion.—Professor Hueter in this article describes the procedure of arterial transfusion, and exhibits its preferability to venous. He prepares the artery for the syringe, while the blood which has been taken by venesection is being defibrinated by an assistant. The radial artery just above the wrist, or the posterior tibial under the malleolus, should be selected, and should be sufficiently isolated to allow of a probe being freely passed under it for 2 or 3 centimètres of its length. Four well-waxed and strong silk threads are passed around the vessel, one of them being used as a reserve ligature in case of accident to the others. The thread nearest the heart is first tied, just like an ordinary ligature. The syringe now having been filled with blood, the thread nearest the periphery is somewhat tightened, so as to prevent, for an instant, any of the collateral blood gaining access to the artery. During this instant the artery is opened by a transverse cut made by the scissors, divided through about half its tube, giving a gaping wound much easier to pass the syringe into than is the wound of a vein. The point of the syringe is directed towards the periphery, and by means of the third thread the vessel is secured to it, about a centimetre of its canula penetrating into the artery. The traction on the second ligature must now cease, and the piston of the syringe is put into action. When the syringe has to be refilled, the second thread must again be temporarily tightened; and when the injection is completed this thread is to be permanently tied as a ligature. The artery itself is divided close to the first and second ligatures by means of scissors, and the portion attached to the canula of the syringe removed with it.

Professor Hueter states that he has performed the operation eight times in this way, and all of the numerous persons present could testify to the great ease with which he did so. He hopes that this will induce others to try it, and thus spread farther a valuable means of treating exhaustion consequent on disease. His cases show that in a few minutes a quantity of blood, equal to 1 civil pound or 0·5 kilogramme, may be thus forced into the general circulation, without causing extravasation or any inflammation in the hand or foot operated upon. Although extravasation has not been observed, yet the finest vessels undergo great expansion, the papillary bodies being filled with more blood than even in a condition of inflammation. The skin becomes swollen and of a purple colour, especially on the dorsal surface of the hand and foot. The colour is even seen through the thick epidermis of the heel. At the end of the transfusion all swelling and coloration disappear, a profuse sweat covering the foot or hand. It is found that in arterial transfusion a considerably greater amount of pressure has to be used than in venous transfusion. Thus in anæmia, that exerted by one hand will not suffice. This greater amount of pressure required is one of the objections to arterial transfusion, which may have to be overcome either by a simultaneous venesection, or the employment of a special syringe constructed for making more pressure.

The most essential advantage of arterial transfusion would seem to consist in the fact that the blood is conveyed to the heart more slowly and equably than in venous transfusion. As the operation is usually resorted to only when life is fast ebbing away, and the heart's action at the lowest, it cannot be a matter of indifference to throw a large quantity of blood suddenly into the right heart. A perverse distribution will take place if the artificial heart is brought thus in too near propinquity. That this is not a mere fanciful observation, Prof. Hueter's experience, extending to 12 cases of transfusion, has convinced him. In some cases he feels convinced that the disturbance of the circulation, caused by venous transfusion, has only hastened death. It is true that in some of the cases so little blood is thrown in that no disturbance of the circulation can result; but it is very questionable whether such transfusion is of any service. Prof. Hueter is convinced that in anæmia or fever, which would have proved fatal, nothing less than eight ounces can be of any avail; and that in most of them double that quantity is required. In individuals who are not anæmic, but are in a normal condition as far as the quantity of blood is concerned, its condition having undergone alteration, as in poisoning, septicæmiæ, &c., transfusion should be accompanied by venesection, rather less blood being drawn than is injected. The circulation thus does not become overcharged with blood, and a portion of that which is poisoned or diseased is removed. Another advantage attendant upon arterial transfusion is, that if a small quantity of air gains admission this does no mischief, but becomes absorbed; while during the short course between the vein and the right heart it might act very mischievously. Again, the security from phlebitis has to be taken into consideration, as patients have been saved by transfusion, afterwards to die of phlebitis and its consequences. A wound of the radial artery is less dangerous than one of the cephalic vein; for secondary hæmorrhage from a properly secured artery is a very rare occurrence, and it is less dangerous than the suppuration of venous thrombi. Having now performed venous transfusion four times, and arterial eight times, Prof. Hueter is in a condition to compare the practical results of the two modes; but while he awards a decided preference to the arterial, he is far from saying that venous should never be performed. He wishes the profession to give his method a fair trial. In fact, whether venous or arterial infusion is employed is a subordinate matter, as compared with the extension of the practice of the operation, as a most powerful weapon in circumstances often regarded as hopeless. —*Archiv für Klin. Chirurgie,* Band xii, H. i.

On Subaqueous Operations.—Professor Gritti concludes his essay upon this subject in the following words:—" The primary idea is to prevent the access of air to closed cavities and bleeding surfaces, and water is the means by which this may be realised. No other methods with this scope attain better results, none are so suited to the hand of the surgeon, are so economical or so devoid of inconvenience, as this one of performing operations under water. The surgical applications of which the method is capable are numerous, and chief among these is thoracentesis, which has already received practical application, and represents a new step in surgical practice. Next in importance comes the extraction of foreign bodies from the knee-joint, which I recommend, under the belief that we have in it a sure means of rendering safe an operation, which has been so much dreaded and avoided. Next we have arthrocentesis, employed by so few, and yet such a valuable means of diagnosis and treatment. Then comes the important series of operations—tenotomy, aponeurotomy and myotomy, as well as phlebotomy—for the treatment of varix. The former will furnish the surgeon with the means of remedying many deformities of the limbs without the fear of suppuration, and by an easy procedure, which is relatively prompt and safe. The division of the veins for the cure of varix may prove more questionable in its results, but at all events it is logically indicated."

As Professor Gritti intimates, the only case in which the plan has been tried is one in which thoracentesis was required for pleuritic purulent effusion. The woman was forty years of age, and had already had thoracentesis performed in the ordinary way. The pus having collected again, she was placed sitting in a bath, and an assistant raising her arm, easy access was gained to the intercostal space, between the sixth and seventh rib. Abundance of fluid was uninterruptedly discharged, and the wound healed by first intention.—*Annali Universali di Medicina*, Feb.

Effects of Shortness of the Frænum of the Prepuce.—M. Jansen observes that some years since his attention was called to the frequency with which shortness of the frænum of the prepuce occurs, and to the relation which it bears to pihmosis. Having under his charge a considerable body of troops, he availed himself of the examinations, which are officially made in the Belgian army, to ascertain the proportion in which the defect prevailed. Of 3700 soldiers examined, he found that 3153 (85·2 per cent.) had the prepuce of normal conformation; 458 (12·3 per cent.) had shortness of the frænum; and 89 (2·5 per cent) great narrowness of the prepuce. Thus 547 out of 3700 soldiers had this vicious formation in a greater or less degree. He examines in detail the various consequences that may ensue on this state of things, and concludes as follows: "1. This shortness of the prepuce, which has hitherto been so little noticed, is deserving the attention of practitioners. 2. In all these cases it is more or less difficult to expose the glans, and a certain degree of phimosis consequently always exists. When this is highly marked, the prepuce also exhibits at the same time a manifest narrowness. 3. This narrowness of the prepuce may give rise to accidents due to the absence of cleanliness; accidents dependent upon the traction exerted by the frænum on the meatus, and interfering with erection, ejaculation, and micturition: and to numerous accidents due to the phimosis itself. 4. Whatever treatment the patient may be submitted to, as long as the vice of conformation in question is not removed, the above accidents will not be remedied. 5. When there is no narrowness of the extremity of the prepuce, the division of the frænum suffices. Otherwise, circumcision must be performed, taking care to leave the prepuce as long as possible, or to effect its dilatation.—*Journ. de Méd. de Bruxelles*, Jan., Fev., Mars.

On Puncturing the Bladder above the Pubis.—Professor Dittel observes that notwithstanding the ease with which this operation may be performed, except in very fat persons, it is yet generally avoided as far as possible. This arises from the subsequent difficulty of dealing with the catheter. Where the urethral passage can be speedily reestablished, everything goes on well. But this is not always so when the urine has to be discharged by the artificial aperture, owing to the great difficulty of retaining the catheter without giving rise to various complications, such as irritation of the mucous membrane of the bladder, cystitis, pyelitis, &c. The end of the rigid catheter prevents all movements on the part of the patient, while its retention causes it to be covered with incrustations, which irritate the mucous membrane. The idea occurred to Professor Dittel of substituting for the rigid metallic catheter one of vulcanized caoutchouc, and the advantage he has derived from this is the cause of the present communication. He narrates the case of a patient who suffered from retention of urine in consequence of a tumour of the prostate. Between the 17th November and the 19th August he underwent the operation of puncture three times, the catheter on two occasions having slipped out without (he being a fat man) being able to be reintroduced. On the last occasion, after remaining in two or three weeks, it caused great pain and induced cystitis, and the following simple apparatus was substituted. This consisted of a catheter of vulcanized caoutchouc, having at its end a round somewhat arched disc. From the other side of the disc another caoutchouc tube branched off, which communicated with the catheter, and hung freely down. The catheter was passed down as far as the disc, and secured. All irritation now subsided, and the patient was able to move about without inconvenience, the urine being discharged from time to time through the depending tube. When the catheter becomes incrustated, which it does far less rapidly than is the case with metallic catheters, it is replaced by another. It will probably be best to employ a metallic catheter for the first fortnight, before resorting to the caoutchouc one, as in that time a canal will have been formed from the wound, which will allow it to be introduced and replaced without difficulty. This procedure does away with the complications which usually ensue on this operation, and by rendering it less dangerous, it will probably render its performance more frequent.—*Allg. Wien. Med. Zeit.*, Jan. 4th.

On the Reduction of Dislocations.—Professor Warren Greene, in this paper, protests against the common doctrine that the great impediment to the reduction of dislocations consists in the tonic contraction of muscles; and maintains, on the contrary—1. That the main opposing force is the untorn portion of the capsular ligament. 2. That in all efforts at reduction the primary object should be the relaxation of the untorn portion of this ligament. 3. That occasionally the small size or peculiar shape of the rent in the capsule, peculiar conditions of the nervous system, or muscular contractions, may constitute the major forces with which we have to deal: but that these cases are so extremely rare as not at all to invalidate the general rule. In estimating the relative importance of ligamentous and muscular resistance to reduction, we have to bear in mind that —1. The shafts of long bones are, as a rule, surrounded by groups of muscles quite as powerful in their combined action as are those which envelope their articulations. 2. In fractures of the shaft, with displacement of its mobile fragments, whose sharp and rugged ends are constantly provoking muscular spasm, which increases its own cause, the amount of "contraction" is at least equal to that resulting from displacement of its smooth and rounded articular extremity, which occupies a fixed position. 3. In the various (not impacted) fractures, the cases are very rare in which the surgeon is not able to make sufficient extension to overcome muscular contraction, even without anæsthetics—his main difficulty being to retain the fragments in proper apposition; while in dislocations effective extension is the ordinary mode—often requiring the force of several men, or of the compound pulleys. 4. Profound anæsthesia annuls muscular resistance; but while it allows the fragment of a fractured bone to be replaced with the utmost facility, it oftentimes fails to diminish in any appreciable degree the difficulty of reducing dislocations, the most powerful extending force, if applied in the ordinary manner, still being required. 5. It frequently happens that dislocations occurring in strong men, where there is no evidence of extraordinary muscular injury, are reduced with great ease by the rules laid down in the books, without anæsthetics; and when the muscles are seen and felt to be in a state of positive resistance.

Professor Greene gives in detail an account of his investigations as to the production of the various dislocations on the corpse when denuded of its muscles.—*Boston Med. and Surg. Journ.*, March 3.

Removal of a Polypus of the Larynx by Ablation of the Thyroid Cartilage.—M. Krishaber read a very interesting case of this description to the Paris Société de Chirurgie, and which was subsequently reported favorably upon by M. Guyon. The following are the author's conclusions:—1. There are cases of polypus of the larynx, the destruction and extirpation of which cannot be accomplished by the natural passages. In such cases we can open the larynx directly. 2. The aperture may be made according to the nature and position of the tumour either in the membranes or the cartilages. 3. When the polypus is implanted in Morgagni's ventricle, the incision should be made in the thyroid cartilage. The space thus obtainable is sufficient for the extraction of even a large polypus, without division of the thyro-hyoidean or thyro-cricoidean membranes. The section of the cartilage may be so made as to avoid injury to the vocal cords, and so leave the voice intact. The presumed ossification need not present a contra-indication, although it delays the healing. 4. Laryngotomy, consisting in dividing *en masse* the whole body of the larynx, membranes, and cartilages, should be rejected. When, by means of the laryngoscope, we have ascertained the exact site of the tumour, it suffices to open the larynx at such site. 5. Of all procedures hitherto employed for the extinction of polypi, the operation here described is that in which incision of the larynx is least extensive. It is to this I attribute the recovery of my patients. I propose to name this operation "Restricted Thyroidcal Laryngotomy."—*Revue Photographique des Hôp. de Paris*, Sept.

On Debridement of the Testicle in Gonorrheal Orchitis.—M. Salleron publishes this paper as a caution against accepting the statements of so well-known an authority as Vidal. That author, in his 'Pathologie Externe,' observes that as orchitis is an inflammation accompanied by strangulation, owing to the dense fibrous tunic which envelopes the testis, the best treatment is to make a small incision into the tunica albuginea by means of a lancet or bistoury. The operation, he says, causes little pain, and is quite harmless, for among 400 patients so treated, he never met with any accidents whatever, while the patients very speedily obtained relief. This little operation had likewise promptly succeeded where other most energetic means had failed to be of any use. That the practice has not proved so successful in the hands of others is shown by the fact that it has not become generalized; and M. Salleron, in this paper, relates two cases to show that it may do much eventual mischief. In both of these prompt relief followed the puncture, but the whole of the substance of the testicle was also, in both, discharged through the wound in spite of every care taken to prevent it.

M. Salleron shows that, from the anatomical structure of the parts, this result must be expected, and he does not accept in explanation of Vidal's not having met with such effects, the suggestion advanced by Gosselin and others, that he did not penetrate the tunica albuginia at all. It is highly probable that among the 400 cases operated upon by himself and pupils this was sometimes the case, and, as in one half the cases of orchitis, there is accompanying effusion into the tunica vaginalis, this might even not unfrequently happen; but there can be no doubt that in many of his cases Vidal penetrated the tunica albuginea, and probably contented himself with noting the immediate relief obtained from the operation. At all events, it is an operation to be avoided, either as unnecessary when not dangerous, and very dangerous when immediately useful.—*Archives Générales*, Feb.

Tubercular Ulcer of the Tongue.—M. Trélat, believing this form of ulcer of the tongue to be very imperfectly known, presented a paper upon the subject to the Académie de Médicine relating an interesting case that came under his care at St. Louis. We have not space for the details, and it will suffice to say that a young man in good health applied concerning an ulcer of the tongue, which had arisen from some unknown cause. For six months it resisted the various modes of treatment, and at the end of this time signs of pulmonary tubercle began to manifest themselves. Another four months passed away; and when the ulcer, which had become much larger, and then occupied both sides of the tongue, showed signs of great amelioration under the use of the actual cautery and phenic acid, the patient was rapidly carried off by a "galloping" consumption. The autopsy exhibited the lungs loaded with tubercles in different stages, and the tissue of the tongue contained numerous granular bodies, which proved on examination to be tubercular. The older authors, when observing ulcerations of the mouth in the subjects of phthisis, attributed their existence to the exhausted and cachectic condition of the patients, and looked upon it as a sign of approaching death. It was supposed never to be met with except in advanced, or at least, confirmed, phthisis. If well-marked phthisis always preceded ulcers of the tongue in these cases, the diagnosis would become much easier than it is. But although this is the general rule, there are exceptions to it, as shown by the present case, and in another occurring in the practice of M. Ricord. In that, seven months after the appearance of the ulcer, the patient, though enfeebled, exhibited no signs of phthisis, which did not present themselves until some weeks later. Another remarkable point illustrated by this case is the occurrence of tubercles, or rather, gray granules, in the tongue itself, these becoming ulcerated. Hitherto such ulcers have been regarded as only the result of cachexia and defective nutrition. In this case microscopical examination determined that not only were they ulcers occurring in a tubercular subject, but ulcers of tubercles. It is very possible that both varieties of ulcers, cachectic and tubercular, may be met with in the mouths of the subjects of phthisis; but M. Trélat recollects other cases having the same appearances as observed in the present one, and he suspects that a more exact examination would have shown them to be tubercular. The diagnosis of these cases, as well as their treatment, is very embarassing, as no sign of the disease which gives the key to their nature is present. The ulcers may occupy any portion of the cavity of the mouth as well as the tongue, and indolent at first, they afterwards become painful, but are accompanied by little or no ganglionary enlargement. They are superficial, never having the deep, irregular, indurated appearance of cancroid; but the portion of the tongue which constitutes their base becomes more or less indurated and voluminous, so as to resemble a true tumour. An appearance precedes the ulcers which M. Trélat is disposed to regard as pathognomonic, viz., a round spot from one to four millimètres in size, and of a yellowish colour, resembling phlegmonous pus, on the surface of the mucous membrane, one or more follicular orifices being observed in the epithelium. In a few days the epithelium is destroyed, and ulceration is set up. Sometimes several of these spots in different stages of evolution may be observed. Of course the prognosis is very unfavorable in these cases, and they are very rebellious to treatment. The actual cautery would seem to be the best agent for producing, at least, great temporary amelioration, and its employment should not be too long delayed.—*Archives Générales*, January.

SUMMARY.

Air-passages. — Langenbeck. On Subhyoidean Pharyngotomy. (Berlin Klin. Woch., Jan. 10 and 17).—Trendelenburg on the same. (Arch. f. Klin. Chir., xii, H. 1).—Uhde. Tracheotomy in the Duchy of Brunswick. (Arch. f. Klin. Chir., xi, H. 3. A statistical account of 100 cases.)

Amputation.—Mac Cormac. A. through the Kneejoint. (Dublin Journal, May. Photograph).—Stephen Smith. A. at the Knee-joint, by Modified Lateral Flaps. (Amer. Journ. Med. Science, Jan. Woodcuts.)

Aneurism.—Watson. Case of Popliteal A. treated by compression. (Ed. Med. J. Woodcut of the author's Compressor). —Buck. Case of Femoral A. successfully treated by Flexion. (Amer. Journ. Med. Sc. Occurred in a case of relapse after cure by compression.) Decristoferis. A. of the Extra-Pericardial Portion of the Ascending Aorta, successfully treated by Electro-Puncture (Gaz. Med. Lombardia, Feb. 5 and 12. The report comes down to the 80th day, when the patient had resumed his ordinary habits of life.)

Blood-vessels.—Fischer. Wounds and Aneurisms of the Gluteal and Ischiatic Arteries. (Archiv f. Klin. Chir., xi, H. 3.)—Kocher. On the Process of Arrest of Hæmorrhage in Acupressure, Ligature and Torsion. (Ibid.)—Roser. On Arrest of Hæmorrhage and on Secondary Bleeding. (Ibid., xii, H. 1.)

Dislocations. — Kocher. New mode of reducing D. of the Shoulder. (Berlin Klin. Woch., Feb. 28. A modification of Schinzinger's forcible rotation outwards. Woodcuts.)—Barth. On Separation of the Epiphyses. (Arch. der Heilk., H. 3. Reunion took place in two cases.)

Ear.—Bouisson. On Amputation of the External E. (Gaz. Méd., March 19—June 11.) —Bonnafont. New Apparatus for Injection and Aspiration of the Tympanum. (Union Méd., April 30.)—Jolly. Ulceration of the Internal Carotid in Caries of the Petrous Bone. (Arch. Gén., March. Supplement to an essay of 1866.)—Jacoby. Perforation of the Mastoid Process. (Arch. f. Ohrenheilk, B. v, H. 3. A case given in minute detail).—Lucæ. Treatment of Chronic Purulent Otitis. (Berlin Klin. Woch., Feb. 7. Woodcut of a simple apparatus for irrigating the ear.)

Excision.—West. Cases of E. of the Wrist-joint. (Dublin Journ., Feb. Illustrations.)—Murney. Case of E. of the Ankle. (Ibid. Illustrations.)—Rochet. Case of E. of the Shoulder-joint. (Rev. Photographique des Hôp., Jan. Photographs.)—Leisrink.

Statistics of E. of the Hip. (Arch. f. Klin. Chir., B. xii, H. 1. An elaborate statistical examination of recorded cases.)

Eye.—Wilson. On Extraction of Cataract by Von Graefe's Peripheral Linear Section. (Dublin Journ., May).—Arlt on the Operative Procedure in Separation of the Retina (Allg. Wien Med. Zeit., No. 30).—Hairion. On Palpebral Granulations (Annales d'Oculistique, Jan.)—Von Graefe. Pathology and Treatment of Glaucoma. (Ibid., Jan. and March. Translation of Von Graefe's important memoir.)—Sichel. On Treatment of Diseases of the Lachrymal Passages (Bull. de Thérap., April 15 and May 15).—Eastlander. On Choroiditis in Recurrent Typhus (Arch. f. Ophthal., B. xv, Ab. 2).—Adamiak. On Intraocular Pressure in Glaucoma (Monatsblatt f. Aug., Dec., with a discussion at the Ophthalmological Congress).—Emmert. A New Exophthalmometer (Ibid., Feb.)—Talko. On Section of the Supra-orbital Nerve in Clonic Spasm of Eyelids. (Ibid., May.)

Fractures.—Ollier. On Treatment of F. by Metallic Points (Lyon Méd., Jan. 30. Following out Malgaigne's Practice with Improved Apparatus. Woodcuts).—Volkmann. A "Railway" Apparatus, suitable for F. of the Thigh on the Field of Battle. (Berlin Klin. Woch., May 16. By a system of rollers it allows of transport being made with great ease to the patient.)

Heart.—Buridani. Case of Penetrating Wound of the Heart. (Gaz. Med. di Torino, Feb. 7 and 14. The patient lived four days.)

Hernia.—Annandale on Fatty Hernia (Edin. Med. Journ., March).—Rezzonico. On the Importance of Ice in the Reduction of H. (Annali Univ. di Med., April).—Ravoth. On the Regulation of the Action of the Spring in Trusses (Berlin Klin. Woch., Dec. 13. Woodcuts).—Wernherr. Statistics of H. (Arch. f. Klin. Chir., B. xi, H. 3. The etiological portion of an elaborate Essay).—Pauli. Pathological Anatomy of Hernia and Strangulation. (Ibid., xii., H. 1.)

Hip-Joint.—Blasius. On Hip-joint Disease. (Arch. f. Klin. Chir., xii, H. 1. Description of Preparations in the Author's Museum, with Plates.)

Intestinal Canal. — Frantzell. On Enterotomy in Ileus (Virchow's Archiv, January.)—Berenger-Feraud. On the Different Modes of Treating Wounds of the I. (Bull. de Thérap., Jan. 15 and Feb. 15).—Tillaux. On the Treatment of Internal Strangulation of the I. (Ibid., March 15 and April 15.)

Laryngoscopy.—Oliver. Aphonia from Paralysis of Intrinsic Muscles of Larynx treated by External Manipulation. (Amer. Journ. Med. Sci., April. Woodcuts.)—Hamilton. Encysted Bursal Tumours in front of the Larynx. (New York Med. Journ., March.)—Ruppaner. Contributions to Practical Laryngoscopy. (Ibid., Jan. Cases with illustrations.)—Krishaber. Case of Polypus of the Larynx, removed by the Natural Passages. (Union Méd., April 28.)

Nævus.—Duncan. On Galvano-Puncture in N. (Edinb. Med. Journ., March. A very favorable account of its employment.)

Neuroma.—Genersich. On Multiple N. (Virchow's Archiv, Dec. Founded on a case given in minute detail.)

Œsophagus.—Trélat. On Internal Œsophagotomy in Stricture consecutive to Inflammation. (Bull. de Thérap., March 30. Illustrations.)

Orchitis. Mauriac. On Reflex Neuralgia sympathetic of Blennorrhagic O. (Gaz. Méd., May 7.)

Ovariotomy.—Atlee. Cases of O. (Amer. Journ. Med. Sci., Jan. and April. This is the commencement of an account of a third hundred of cases; the mortality of the first 200 having been about 30 per cent.)—Bergmann. Four cases. (Petersb. Med. Zeit., 1869, No. 5. Detailed at length to the Dorpat Medical Society.)—Hoenig. Four cases. Berlin Klin. Woch., Jan. 24—31. A figure given of a new trocar.)—Jouon. On the Treatment of Ovarian Cysts by Incision and Suppuration. (Union Méd., June 7. A case related in recommendation of the practice when large adhesions, &c., render the extraction impracticable.)

Parotid.—Laugier. On Serous Cysts of the Parotid Region. (Archives Gén., May.)

Penis.—Demarquay. On Gangrene of the P. (Archiv. Gén., May.)

Rectum.—Miller. Polypus of the Rectum. (Edinb. Med. Journ., Jan. Relates a case, and refers to thirty-seven others on record.)—Berenger-Féraud. On the Therapeutical Applications of the Anal. Obturator. (Gaz. Hebdom., March 11. Woodcut of the elastic obturator employed by the author.)

Syphilis.—Zeissl. Pathology and Treatment of Chancre. (Allg. Wien. Med. Zeit., Feb. 8—March 8.)

Tetanus.—Letievant. Neurotomy in Traumatic T. (Lyon Méd., May 8 and 22.)

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Varicocele.—Paturban. On the Operative Treatment of V. (Allg. Wien. Med. Zeit., March 1—April 12.)

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1870.

Analytical and Critical Reviews.

I.—Sanitary Organization in England.

1. *Administrative Authorities.*

FROM an admirable report drawn up by Dr. A. P. Stewart, one of the most laborious sanitary reformers of the day, we learn that the Joint Committee appointed by the British Medical and the Social Science Associations, "to promote a better administration of the laws relating to registration, medico-legal inquiries, and the improvement of the public health," owes its existence to Mr. Hastings, the founder and now chairman of the council of the one association, "himself the worthy son of a father whose name is held in reverent and affectionate remembrance by thousands as the founder of the other."[1]

The measures taken by that committee, and especially the impressive statements made at its instance by a large and influential deputation to the Duke of Marlborough, Lord President of the Council, the Earl of Devon, President of the Poor-law Board, and Mr. Gathorne Hardy, the Home Secretary, at the Privy Council Office, on May 22, 1868, were probably the main causes of the appointment of the Royal Sanitary Commission.

The first memorial of the Joint Committee, presented on that occasion, and now fortunately reprinted in the appendix to the evidence just published by the commission,[2] deserves careful attention at the present time, when so many are waiting to see what may be the effect of the earlier stages of the movement upon the final report of the commission.

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[1] "Report of Joint Committee on State Medicine of the British Medical and the Social Science Associations." Reprinted from the 'British Medical Journal,' August, 1868.

[2] 'First Report of the Royal Sanitary Commission, with the Minutes of Evidence up to 5th August, 1869.' Presented to both Houses of Parliament by command of Her Majesty.

The history of the formation of this commission and of its reconstruction owing to a change of Government is sufficiently well known. Yet it may be as well to mention that against the exclusion of the metropolis and the omission of Scotland and Ireland from the inquiry, a vigorous protest was made in August last, in a second memorial to Government, by the authors of the first, supported by some active spirits belonging to the metropolitan branch of the British Association. But the principal and most reasonable ground for regret among the original promoters was that no power had been conferred on the commissioners to visit or to send itinerant commissioners to the large towns and other districts, in order to obtain information, and to report on specified matters, as prayed for by the memorialists. The want of adequate machinery for a thorough examination into the state of sanitary administration in those very districts where flagrant abuses may be the subject of most conflicting testimony, or may never be heard of without local investigation, if not felt by the commissioners, is fully understood by experienced observers outside, who ground their hopes upon the general effect of the inquiry on the public mind, rather than upon any conclusions which may be formally drawn from a perplexing mass of evidence elicited in the palace of Westminster. Many of these replies are in substance mere iterations; not a few show an utter misconception of the real point at issue; others rest on some hypothetical fallacy, or obscure the main question by a sort of bye-play of secondary and unimportant considerations.

In the absence of powers of local visitation, the conclusions of the ultimate report may depend somewhat on the extent to which the Circular Questions issued by the commissioners will be answered definitely, correctly, and intelligently, by the several local authorities and officials to whom they are addressed.

We cannot hope for much reliable information from this source. We have heard officers of local authorities confess that they had neither time nor inclination to fill up gratuitously, except in the most perfunctory manner, voluminous returns, which to answer fully would require their undivided attention for many days. There are several forms of returns. The two first, addressed to chairmen or clerks of local boards and officers of other local authorities, might have been advantageously abridged and consolidated, as by far the greater portion of their contents is common to both, and exceptional questions might have been marked for particular authorities. One cannot see why town councils under the Municipal Corporations Act are not specified. The third form appears to be well adapted for rural districts; while the fourth and fifth, general and medical, suffice at all events to prove the breadth of view and comprehensiveness of purpose which actuated the compilers. But we venture to think that far too much has been demanded. Many of the questions involve matters of opinion, requiring, for satisfactory reply, considerable study and thought. Information on mere matters of fact might have been more summarily called for and more satisfactorily obtained.

But there are schedules without end. More than a year ago a most comprehensive category of questions, skilfully arranged under fourteen heads, was prepared by authority of the Joint Committee, which requested in vain that it might be included in the inquiry of the Royal Commission. These really good forms, we are told, still remain unused; but had there been anything like medical organization in the country, the information they asked for might have been fully and accurately reported.

Recently, also, a long series of questions has been circulated by the committee of the British Association on sewage utilization; and we learn from Mr. Bailey Denton (Ev. 4892) that the proportion of answers then received was scarcely more than one to five of the circulars issued, while the replies from local authorities show such complete ignorance that he feels justified in saying—"they are incompetent to act in the sanitary control of their districts."

To revert to the Royal Commission. Notwithstanding certain defects of power and plan, to which we have called attention, it is, we believe, doing its great work energetically. The many intricate ramifications of this inquiry show that its limitation, for the present, to the provincial districts of England is not without decided advantages. "The metropolis," said Mr. Thring (352), "from its size, is entirely different from any other town; and the Irish and Scotch towns are subject to different laws from those in force in England." But much may be learned, for the service of the English counties, from London, Scotland, and Ireland, and these will, in turn, benefit by the work done by this commission. For instance, Professor Gairdner has advanced some startling facts respecting the fearful condensation and degraded condition of the masses in Glasgow, and has suggested measures of the boldest nature for their relief—reforms equally necessary for all very large and excessively crowded towns.

Dr. Burke also has given valuable information relative to mortuary registration and sickness returns in Ireland; and those Nestors of medicine, Dr. Christison, of Edinburgh, and Dr. Stokes, of Dublin, have done much to promote a truly national sanitary reformation by submitting themselves to examination, as did Dr. Acland and other members of the commission. The metropolitan officers of health have also been ably represented by Dr. Druitt, whose evidence will appear in the second report.

Beside these extra-provincial contributions to the evidence, and contrary to the wish expressed at first by a few interested and prejudiced persons to confine the commission to purely sanitary matters,—its interrogations have often extended to medico-legal subjects, such, for example, as inquiries into the cause of death, the value of certificates of physical capability for labour, &c. It was doubtless found practically impossible to separate these questions from the subjects of sanitary work.

As far as it has hitherto proceeded, the inquiry might be considered under the heads of sanitary law, organization, results and defects of present administrative arrangements, and indications for future legislation. But to discuss, even summarily, all the matters brought before the commission would be to write a volume on the principles and practice of public hygiene. All that we can accomplish within the limits of a review is to consider the many questions of organization which affect the working of the health laws in the provinces, with especial reference to the position and agency of the medical profession in any general system. Facts and results of sanitary administration will not here be handled, except incidentally.

With regard to the laws of public health, we may refer to an original communication by Dr. Rumsey in our last number; but even that comprehensive view of the numerous special, social, and medical statutes which regulate or influence sanitary administration in England, conveys but an imperfect notion of the multiplicity and intricacy of legal details forced on the attention of the commission. Systematic writers on the principles of state medicine appear sometimes to ignore the forms and methods of procedure under a variety of enactments, local regulations, traditions, and customs. On the other hand, eminent authorities in the law seem only to recognise the importance of simplifying and consolidating legal *formulæ*, with the view of attaining, as effectively as possible, the commonly admitted objects of existing legislation. Thus, in answer to a question which, in one shape or other, continually meets the eye in this report—"What should be the subjects of a sanitary code?" Mr. Thring, the Parliamentary counsel, replies in effect that its provisions should be limited to water-supply, drainage, and removal of nuisances, &c. &c. (362, &c.). On the other hand, Surgeon Hewlett, of the Bombay Army, proposes nineteen heads of a sanitary code, which he thinks should be of universal application (5740). The legal view may also be contrasted with that taken by the author of the 'Memorandum on State Medicine,' reprinted in the appendix. Now, although it may be unnecessary, and therefore undesirable, to call for immediate legislation on all the topics mentioned in that document, it would be inexpedient, if not unsafe, to endeavour to amend the law and its administration, even in the more obvious respects, without a distinct precognition of probable requirements concerning the rest.

As regards the principles of sanitary law, it may suffice to say that there seems to be a very general agreement of opinion that existing enactments affirm all, or nearly all, necessary principles. Even certain legal defects seem to be due more to the confusion and variety and non-existence of jurisdictions (1886), and to the want of simplicity and definiteness of action, than to any remarkable omission of personal or communal responsibilities. As Mr. Simon says, "There are faults of incompleteness, but they are small in comparison with the faults of confusion. The state of the law, in relation especially to local authorities, is chaotic" (1809).

Again, as to two objects of paramount importance, the provision of healthy homes for the lower classes, and the conservancy of rivers and sources of water supply, the more obvious defects of law are attributed generally to the absence of properly constituted authorities, by whom well-advised regulations, having the force of law, might be carried into effect.

Of course in this evidence the question continually arises whether such and such an enactment should be compulsory or permissive—whether the local sanitary authorities *may* or *must* enforce a certain legal declaration; but the extent to which compulsion should be carried is admitted to depend chiefly on the kind of authority to which action is or ought to be committed; for while an intelligent, well-informed, and willing board—if such could be constituted—might be simply empowered to do this or that act, a naturally obstructive or ignorant body would have to be compelled, at least if it were probable that such compulsion would ensure the proper execution of the sanitary measure; and, indeed, the proofs of inefficiency and inertness which some kinds of local authorities so abundantly afford have led to very strong assertions of their utter unfitness to be entrusted with any sanitary duties, even under the powerful pressure of a superior authority or a central department. So that proposals of amendment in sanitary law to a great extent resolve themselves into projects of reform in the constitution of authorities.

In the first place, many who entertain a wholesome Anglo-Saxon preference for local self-government by small groups of population, on the old tything principle of mutual responsibility, recommend that parishes should still constitute "units" of sanitary administration. Some continue to favour a comparatively recent extension of that principle to any part of a parish, to any township, or other place separately rated for the relief of the poor, nay more, to any small district formed under the Local Government Act, or under the Sanitary Acts for special drainage. They argue that greater personal interest is felt in the well-being of each small community, and probably more accurate knowledge of its wants and circumstances possessed by its own more active members, than by a board representing a number of such communities and acting over a wide area. There is something to be said for the old parochial notion, and a few instances have been adduced of successful efforts within a very limited boundary.

But a large majority of witnesses tell a very different tale. Parish authorities in general cannot or will not act unless compelled, or about to be compelled, by summary legal process. Besides, vestries are of an indefinite and variable composition. They are mostly without responsible or competent officers. There is no one on whom to fix responsibility. "They slip through your fingers; you can never catch them;" said Mr. Simon (1824). The incumbent of the parish, being *ex-officio* chairman of vestry, is often the only person with whom the central or superior authorities can communicate.

Even the appointment by vestry of a committee for health purposes seems to have rarely accelerated effective action, while it is reported to have provoked many bitter feuds, as at Tonbridge, Hatfield, and Stow-on-the-Wold. The evidence of Mr. Pember, of the Parliamentary bar, is very strong on this point. A sanitary committee has not seldom turned round upon its parental vestry and defied it, the latter being legally responsible for the doings, defaults, and expenditure of the former.

The creation, by the Acts of 1865 and 1866, of a new class of local bodies, called "sewer authorities," must now be looked upon as a legislative blunder. It has led to a very objectionable duplication of local authority, and practically to the repudiation by boards of guardians of their functions as "nuisance authorities." No one has shown more explicitly than Mr. Hutchins[1] the complicated difficulties arising, or liable to arise, out of the Sanitary Act, 1866, "which will in certain places and under certain circumstances establish two different governing bodies, having actually the same designation and acting for the same purposes within the same area," each endowed with compulsory

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[1] 'On Difficulties which exist in administering some of the Sanitary Acts of Parliament. By James B. Hutchins, of the Medical Department of the Privy Council.' 1869.

power. The probable results may be given in Mr. Hutchins's own words:

"It is certain that the opponents of sanitary works, wishing to do nothing, and yet not to confess their inaction, would profess their anxiety to execute all necessary works, having all the time a full conviction that their duties could not be brought home to them. The local board would in all probability say the vestry was the proper authority . . . . as that body was in existence before the board, and the vestry would assert that the local board was the proper authority, because it had been formed at a more recent date, and had, therefore, superseded the vestry.

"On the other hand, each body might assert its right to execute works independently of the other—a course of proceeding equally objectionable; and so a place might have two sewers running side by side, like the lines of opposition railways."—pp. 22, 23.

Again, in creating a new sort of parishes, called "special drainage districts," carved by vestries out of the old parishes, the evils of multiplication of small governing bodies have been curiously aggravated. Mr. A. Taylor, one of the inspectors of the Local Government Act Office, thinks that this carving out of special drainage districts is carried on to a very mischievous extent. Mr. Tom Taylor is of the same opinion. The former instances the small parish of Merton, near Wimbledon, which has made itself into two special drainage districts, each with its committee (1291). He adds, "there can be no earthly reason why Merton should have two local authorities within it, though we have no power to prevent it."

The same absurd misuse of a permission granted by a too confiding legislature to incompetent bodies is reported to have occurred, among many other places, at Berkhampstead (313), at Hatfield (653), and at Epping (730, &c.), where a new district was formed by combining bits of three adjacent parishes; but the farce ended there by the extinction of all local authority, the resignation of the board, and the substituted action of the Home Secretary as master of the works. The case of Hitchin is equally curious. The local board having drained its filth into the little river, and a Chancery injunction having been obtained to prevent the abomination, the board resigned *en masse*, so that, as Mr. Tom Taylor told a Parliamentary Committee in 1864, "the only change is, not that the river has been freed from the sewage, but that the town has been freed from the local board."

Certainly, no district of the kind ought to be constituted without the approval, after due investigation, of a superior authority, nor is there any justification for committing the sanitary management of a small district to an authority elected by its inhabitants. It is quite possible that, in some places, this measure may have led to more effectual drainage; but in many others it has confessedly roused a succession of acrimonious debates, either respecting the definition of boundaries or about the infliction of sewer nuisances upon neighbouring tracts of country. Although the voluntary combination of adjacent parishes or districts, for more effectual sewerage, is permitted by law, we have discovered only *one* unquestionable instance of its adoption. A notable example of its rejection deserves to be mentioned. The refusal of eight parishes, in a condition "so gross and so foul that one is scarcely willing to place on the notes the details" (4246), to unite in the execution of measures which—on the sewer theory—were quite necessary, led to the introduction of the Cray Valley Bill during the last session of Parliament. The very novelty of this ingenious project—the want of precedent—is said to have been the cause of its defeat by red tape.

Once more, as to local board districts. Although, happily, their formation has been discouraged by the Amendment Act of 1863 in places with less than 3000 inhabitants, there is abundant evidence to show that they have been arbitrarily and needlessly multiplied, especially in the northern counties. Here, two or more have been cut out of a single parish or township. There, one has been made up of patches of territory taken from adjoining parishes. If variety is pleasing, there is no end of such amusement in the operation of the Local Government Act. Rarely, indeed, has any regard been shown, either to old and well-known boundaries or to the probable future requirements of growing populations. It has been repeatedly stated, on good authority, that a large proportion of the smaller local board districts have been created with the single object of escaping inclusion in some highway district.

Mr. Morris (898, 999) attributes the passion for these formations in Yorkshire partly to the popular fancy for that caricature of administration which is misnamed local government, for generally it is government only by a small and noisy faction. The process appears to be going on more rapidly than ever. From a return made to the House of Commons, July 31st, 1868, we learn that there were then 549 places under the Local Government Act, 25 of which contained less than 1000 inhabitants. But in April, 1869, Mr. Tom Taylor reports 671 districts—an increase of 122 in nine months, if both statements be correct. One sixth of these places had at the last census a population not exceeding 3000. Notwithstanding the rapid extension of the system, the principle which it sanctions can hardly be maintained by the legislature in face of the counter-evidence, unless the minimum limit of population be considerably raised; but to this point we shall revert when remarking on towns.

The chiefs of the office under which these acts are administered evidently disapprove of the reckless and uncontrolled formation of petty spheres of administration, subverting all existing divisions, all established limits of communal action. That which is truly valuable in local effort—that which is something more than "porochial" bluster—may be promoted, with greater benefit, by special arrangements under larger areas of management.

Before taking leave of these minuter spheres of sanitary action, it may be observed that the political dogma of the necessary concurrence of representation with taxation has been improperly pleaded as the ground for constituting elective bodies in districts of the smallest extent. Without questioning the validity of the principle in matters of imperial concern, or even in certain affairs of county or municipal administration, we demur to its universal application. For, if carried out with logical severity, it might make every householder the administrative authority in the expenditure of his own health-tax. We think, however, that, under an improved sanitary organization, the axiom might be accepted, virtually though indirectly, for all districts—partly by equalizing, on Mr. Goschen's plan, certain public charges over larger areas, and partly by securing the appointment of small parochial committees for purely local purposes. There are abundant instances of the successful adoption of the latter method in the municipal government of large towns. The prevalent notion that the inhabitants of every little place, which might very well be included in a larger scheme of local improvement, must of necessity possess a "body" of its own to carry into effect any given measure, rests, we believe, on a popular delusion, an administrative quicksand, which underlies the whole machinery of both the Local Government Act and the "special-drainage district" scheme of the later sanitary Acts.

II. We may now safely proceed to the consideration of larger districts as units of administration; for an extension of area may be taken as an admitted principle of sanitary reform.

First, there is the poor-law grouping of parishes; and of all existing divisions this seems to be the favorite with the majority of the examinees. The reasons stated are, we think, in the main incontrovertible. The intimate connection between pauperism and a miserably low condition of public health, and the dependence of the former upon the latter—originally shown by Dr. Southwood Smith, Mr. Robert Baker, Dr. Arnott, and other able contributors to Mr. Chadwick's famous report of

1842—has been repeatedly urged by a succession of official and public writers, and is forcibly put in one of the latest pamphlets before us.[1] But for the stolid yet suspicious apathy with which it is received by that mob of respectable Philistines, the well-to-do public, one hardly need repeat the truism, that the real remedy for pauperism consists in maintaining the health and effectiveness of the lower stratum of the labouring classes—always supposing that work may be found or provided, and that the labour market is free. *A priori*, therefore, the most serious duty of "destitution authorities"—as Mr. Simon calls them—would be to look well into the sanitary condition of the masses who are ever quivering on the brink of the pauper gulf.

This, then, is the primary plea for using parochial unions as ordinary areas of health-management. Mr. Simon has put the case well in his eleventh report; thus,

"The poor-law division of the country is a long-accomplished and locally well-known division, which furnishes limits to many local relations and, not least, fixes the Registrar-General's 'districts' for statistics of population and births and deaths. Every union has its administrative board, presumably of the best sort which the area can be expected to give for any purpose of local government, and carefully constituted on the double bases of rate-paying suffrage and *ex-officio* qualification, and, moreover, so constituted that each parish of the union is represented in it; and this authority has its fixed meeting-place and meeting times; it has its permanent clerk, qualified in law, and it has, always acting in detail over the whole union-area, as visitors of the poor and their dwellings, a staff of other permanent officers, medical and non-medical. No approach to such organization as this exists for any other purpose in the rural districts of the country, and it would seem to me a simpler and safer course to bring the common health-service of the country within scope of that existing organization than to attempt a differently planned organization for objects exclusively of health."

The enormous advantage of a machinery which might bring all the local facts of population, occupation, mortality, and sickness, to bear, promptly and directly, upon local administration, is too obvious to need one tenth of the corroborative statements which swell this first report of the commission. We would, however, call attention to the evidence of Mr. Darby, the Inclosure Commissioner, on this point (7352-6).

The question in Ireland is greatly simplified by the fact that there, not only are all the registration districts conterminous with the poor-law unions—which are the areas for sanitary administration outside of towns—but the sub-districts also are

[1] 'Address to the Poor Law Medical Officers' Association,' by the President, Dr. Rogers, April, 1870.

conterminous with the dispensary (or medical relief and vaccination) districts. Our greatest difficulties in England arise from the absence of this desirable identity of area for many purposes of local management.

Two main objections have, however, been made to poor-law unions as health-areas, and these deserve particular attention.

(1) A large proportion of the unions are of very inconvenient shape and extent for local sanitary government. They were not formed with any view to that object. The assistant-commissioners who planned them looked merely to parish boundaries, neither regarding those of boroughs and counties, nor yet taking into consideration the physical features of the district. Different commissioners seem to have acted on very different principles of grouping, for the extent and population of unions and the arrangement of parishes vary greatly in different parts of the kingdom, even under the same conditions of population density. Compare, for instance, the two agricultural counties of Norfolk and Devon, and it will be found that the average population of unions in Devon is nearly 50 per cent. above that in Norfolk.

Frequently, no doubt, unions were compactly formed. But in other cases the object really seems to have been to fix the seat of union management inconveniently and inaccessibly, as regards the inhabitants of outlying parishes, whether ratepayers or rate-receivers. In many cases the interests, the feelings, even the very prejudices, of influential people in the neighbourhood, were consulted in preference to the real objects of the measure. A casual inspection of a map of seventy contiguous unions in the midland counties showed us at least twenty-six in which the place of management either lay on the border or was within four miles of the nearest point of boundary—more than half of these so-called "centres" were within two miles of the border—while the distance from the furthest point varied from seven to twelve miles. Aylesbury and High Wycombe are curious, and probably not extreme, instances of this perverse method of grouping parishes. Other singular irregularities were apparent. Yet, according to Mr. Lambert and Dr. Farr, "convenience" was the leading official idea. If that were indeed the theory of poor-law organization, the exceptions are too numerous to allow of its being taken as the rule of practice. Mr. Lambert acknowledges (4673) "that in the formation of unions there were other things that had to be taken into account." And among these were local Acts. For example, the old parochial corporation of Shrewsbury seems to have caused the surrounding union of Atcham to assume the singular form of a circular belt of territory completely surrounding that incorporation. To the interference of unions with county boundaries we shall revert.

Notwithstanding these irregularities, and probably errors, in the definition of unions, it is certain that similar objections apply generally to every existing division of the country. It is also undeniable that the objection may be removed or reduced to a *minimum* by a general revision of union boundaries, as advocated by many official persons, on grounds altogether distinct from sanitary reasons. Mr. Lambert himself shows that the Poor Law Board has absolute power to alter the parochial composition of unions, to subtract or add as may be found advisable for administrative purposes (4678—4683), and that this sort of redistribution had been put in practice in some places with much advantage.

The second objection to poor-law unions as sanitary districts is—that their present governing bodies are generally unfit for the execution of measures of a scientific or ameliorative character—measures which, though unquestionably prudent and ultimately economical, are not immediately remunerative. But this objection applies to every other body founded on the rate-paying element, and manifestly less to boards of guardians than to bodies which contain no *ex-officio* element. It may also be neutralized in three ways, by adopting Mr. Simon's advice (1825)—first, that the guardians be elected, *ab initio*, to per-perform sanitary functions, as an integral part of their original appointment; next, that these boards be made, as to health, directly responsible to a superior sanitary authority; and thirdly, that their power of acting by committees be largely developed. Such committees might be of two kinds—the one for *places*, as parishes, small townships, or "special drainage districts" (2006); the other for *objects* of a special character appertaining to the whole union, as roads, labourers' dwellings and lodgings, dispensaries and vaccination, epidemic visitations, and other matters to which we may refer hereafter. But for the latter purpose it would be necessary that persons of superior intelligence, not members of the original board, and having special aptitude for health questions, should be added to the committee (2010—2116). This should not be a merely permissive enactment, as in the Sanitary Act, § 4. It is not enough that the board or the committee itself should be empowered to add persons specially qualified. A superior authority should be made responsible for the addition, and should take care that the persons appointed render effective aid in the execution of sanitary measures. By such a measure alone, as far as we can see, might the constitution of the administrative body be so radically improved as to render it unnecessary to press a suggestion, renewed during this inquiry (1930, 2139), and repeatedly made before Parliamentary Committees, especially that of 1844, namely, that the provision of medical aid for the *out-door* poor be wholly separated from the general relief of destitution, and be combined with other measures for the improvement of the public health under local authorities to be constituted expressly for these purposes. The really important object of that suggestion might be secured, without the creation of entirely new machinery, by the appointment of well-constituted sanitary committees of boards of guardians.

Secondly. Several witnesses, of whom Mr. Rayner, of Liverpool, may be taken as a good example (2291-9), recommend the employment of highway districts, as groups of parishes for sanitary management. These are formed by justices in quarter sessions after full hearing of parochial suggestions or objections. They are said to coincide commonly with the petty sessional divisions of counties. And they might for these reasons facilitate magisterial enforcement of sanitary regulations. It is possible also that they may sometimes be of more convenient extent than unions. But the insurmountable objection to their adoption for health purposes is that they do not exist everywhere. It appears from Glen's 'Law of Highways' that there are, in all, not more than 308, or thereabouts, of these districts in thirty-five or thirty-six counties; several counties showing only a single highway district, several others only two or three such districts. It must also be remembered that their formation depends wholly upon county or local sympathies and antipathies, and that they offer none of the facilities for scientific inquiry and statistical record which are supplied by unions. The want of co-operation, reported of other local boards, seems to be equally common between road-surveyors or way-wardens and sewer or nuisance-authorities. Uncertainty of jurisdiction and mutual repudiation of responsibility are the necessary consequences (110, 119). There is no reason, however, why highway districts should not be made conterminous with unions, and their respective jurisdictions combined under one board of management.

The remarks of several examinees on this point remind us of the measure proposed years ago by one of the most far-sighted statesmen of modern times, the late Sir James Graham. The main principles of his Bill were—identity of area and authority for highway and poor-law purposes, and the employment of a scientific surveyor in each union. That excellent measure was unfortunately abandoned in favour of our happy-go-lucky custom, and the shape and size—even the very existence—of highway districts were left to the whims of squires, churchwardens, and overseers. Of course, a new grouping of parishes has resulted, and thus another obstacle has been raised to the assimilation of local areas of administration.

It is noticeable that, by the Highway Act of 1835, "persons of superior skill and ability" were to be appointed as surveyors. But there is small proof of the fulfilment of that intention. There is no real test of competency for road-making except a civil-engineering qualification. If that were made indispensable, the very officers who are so urgently required for every health-area would be provided.

Here is another cogent reason for combining highway management with nuisance-removal and other sanitary measures. [As Mr. Rawlinson said (716) "the maintenance of the roads and the construction of the sewers and drains should go together." Or, according to Mr. Bircham, there might be "a compulsory formation of districts throughout the country for roads and nuisances and health." No reform of sanitary administration can be satisfactory without a combination of "all modern purposes of local government"; nor, in our opinion, need this be "a work of immense duration" (1969). Two or three industrious men, endowed with the organizing faculty, might complete and lay before Parliament a scheme of county and district divisions within two years.

Thirdly. It has been suggested that the country might be remapped afresh, on principles of physical topography, solely for objects of public health.

But to add to the present chaos by establishing another description of local government in new districts—irreconcileable as these would be with any existing divisions—would surely be unjustifiable, unless it could be shown that in no other way is it possible to carry into effect a class of very necessary measures. As, however, the question has been raised with respect to river conservancy by persons no less eminent than Mr. Rawlinson and Sir W. Denison, chairmen of the late and present River Commissions, it cannot well be ignored. In their view, drainage, whether natural or artificial, is the one thing needful. On this theory, natural features of land-surface and watershed, marked by the levels and contour lines of engineers, are to determine the outlines of provincial administration. All existing limits are to be set aside. Or, if not wholly set aside, they are to be made subservient to the scheme of river basins, as marked by the Fisheries Act map.

This fixed idea of civil engineers is well described by Mr. Bailey Denton thus:

"I look upon sanitary requirements as consisting of water supply and drainage; drainage extending beyond towns to lands in the neighbourhood of towns, and to the proper removal of nuisances, of course including the proper discharge of the sewage or the effluent water from sewage by the rivers of the country, which must ever remain the drains of their watersheds. I regard the functions of rivers as being identical with sanitary arrangements. The sources of rivers are the sources of water supply, and the discharge of rivers into the sea is the only (?) means of freeing the country of the liquid refuse of towns which the land cannot (?) retain" (4849).

On this system, the units would be either special-drainage or local-board districts. These would be grouped or represented in sub-conservancy districts, each having a board for the management of a tributary stream or for a stage of the main stream. And these boards, again, would finally appear by delegates in a supreme conservancy board, governing the whole basin, which would be bounded by a long and devious line of watershed. The principal river basins in this country, be is remembered, vary from two or three hundreds of square miles to four or five thousands.[1] All these great river conservanciet would be subject to control by a national board (532).

The idea is grand but utopian. We need not discuss the constitution of these conservancy boards, for Mr. Rawlinson and Sir W. Denison appear not to agree on this point, nor indeed are the members of the present River Commission thoroughly in accord as to the relative powers and jurisdictions of the central and the local authorities. We wait to hear what the Commission has to say on this subject.

III. River conservancy is only one of those matters which have to be dealt with in areas much larger than those of unions or municipalities. Beside the drainage of lands, the repair of water-courses, the protection of springs, the hill storage of water for the needs of an increasing population, and the prevention of river deposits from towns, factories, and mineral works,—it is of great importance to provide for the prevention of certain influential trade-nuisances, as smoke, which are never likely to be effectually repressed, still less cured, by town councils or district boards, representing "those who most offend" (539).

Again, the masses for whom decent and safe house accommodation cannot be found within municipal precincts may fairly claim legislative protection against unscrupulous building speculators outside those limits. Healthy dwelling-places must be provided elsewhere. This may be effected, as has often been shown, without any waste of capital or interference with industry. An uncontrolled and hitherto destructive town-influx of people must be succeeded by a regulated and salutary efflux,

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[1] A very useful and interesting "Table of Rivers" is given by Mr. Bailey Denton, 'Rep.,' p. 271.

if the ancient vigour of the English race is to be maintained. The necessary measures, wisely planned and boldly executed, could be directed only by authorities acting over very extensive areas. Again, there are works of sewage utilization in wide districts to be initiated and controlled. There would, moreover, be scientific agencies—medical chemical, engineering—to be provided for those extensive tracts.

Above all, there is needed some intelligent and powerful intermediate authority between the "unit" of sanitary management and the central department. Now, should this intermediate or superior authority be a River Conservancy Board or a County Administrative Board? Believing that the objects of river conservancy can be attained under systematic county management, we do not hesitate to choose the latter. We stand by the old well-known lines. For in cases where more than one county is concerned in a single river basin, no better elements for the *one* river authority could be devised than those which might be selected by and from county boards. The method and details of combination would very properly be left to the approval of a Government department. In other cases, where a county includes two or more "gathering grounds," the requisite number of river committees might be appointed by the county, with sanction of the central authority. The county would be the moving power, the government the controlling.

But against any kind of *intermediate* authority we find in this evidence some strong protests—as from Mr. Simon and from officers of municipalities—to whose objections we shall revert. Yet the principle of supervision by counties or subdivisions of the larger counties or unions of the smaller, is supported not only by leaders of the river conservancy movement, but also by men in high official position, as Mr. Thring of the Home Office, Mr. Tom Taylor, Mr. Arnold Taylor, Mr. Menzies, and others. It is very well advocated by Earl Fortescue, who (when Lord Ebrington) acquired considerable experience in public health and poor-law administration, and whose opinions are entitled to great weight.

"I wish very much," he said, "to see county administration extended as far as possible, as something intermediate between the central government superintendence in London and merely local action in the different unions, or highway districts or small market towns or populous village administrations and the sewer authorities (5510). . . . My conviction is that the interference of a county board, consisting of well-known county persons, would be received in a much more friendly spirit than the interference of an inspector sent down from London sometimes is (5512). . . . I attach very great value to county administration (5498)."

Lord Fortescue would have the county financial boards made largely elective (5831*a*), in order that the several communities might feel that they were reasonably represented. He believes "that there is a very strong county feeling in England." We suspect, indeed, that in this metropolis but a faint idea exists of the force of the old county spirit, derived as it is from historical associations and distinctions of race, custom, and character. We are too apt to regard all provincialisms as relics of barbarism, and to forget their inherent vitality.

The failure, hitherto, of attempts to consolidate union and highway management is attributed by Lord Fortescue to the difficulty of infringing on the principle of county administration; the boundaries of highway districts depending wholly on the old counties, those of unions determining the extent of the new or registration counties. He thinks it "most unfortunate (5496) that the first Poor-Law Commissioners established their unions without the slightest attention to county boundaries;" and he would, therefore, hardly admit the force of the following excuse for that reckless invasion:

"It is an inconvenience," said Dr. Farr, "but it appears almost necessary, because you are obliged, in forming a poor-law union of an aggregate of parishes, to get some point at which the guardians can conveniently meet; and it happens that that point is on the county boundary, where counties are divided by a river" (4454).

If, indeed, the convenience of "farmers and other persons" had really been the guiding principle of union formation, how is it that there exist so many remarkable instances of neglect of that principle, even where there was no landmark to violate, no river to cross—instances to which we have referred, where the border of a union is close upon the place of meeting in the centre of a county?

In a recent paper with a somewhat misleading title,[1] Dr. Rumsey advocates first the revision and then the grouping of unions under county authorities. After asking how the limits of old county government are to be reconciled with the boundaries of unions in those cases, said to be 180 in number, where they extend into more than one county, he proceeds:

"The Bill for establishing administrative boards for the transaction of county business in England, introduced but not passed in the last session of Parliament, supplies an answer to this question by its 15th clause:—'Where a union is situate in more than one county' it would, by this simple and excellent provision, be divided for purposes of county administration; the separated parishes being disposed of according to their gross rental, and either formed into a

[1] "Some Population Statistics for Sanitary Organization." (Reprinted from the 'Transactions of the Manchester Statistical Society.') 1870.

separate union or annexed to another adjoining union in the same county, under the direction of the Poor-Law Board. Any single parish divided by a county boundary—and there are more than 500 such parishes in England—would belong, by the same clause, to that county which comprises the greater part in value of such parish. . . . There are also, in many instances, small peninsular portions of a county, almost surrounded by an adjacent county and identified with it in affairs of local business; and these might be treated, as were detached parts of counties by the Acts of 1843 and 1844 (relating to coroners' duties), which included all such detached portions in the shires, ridings, or other divisions, by which respectively they were surrounded.

"These or similar provisions would remove one of the oldest and most vexatious difficulties arising out of the complexity of the territorial divisions of England. They would abolish the ancient parochial unconformity with counties. They would also remedy for practical purposes the modern error of union intrusion upon counties."

The advantages of a revised and consistent system of territorial divisions for all objects of local management may doubtless be overrated; but it seems quite possible, by moderate changes, to reconcile the boundaries of counties with those of sanitary districts; the latter to be identical for purposes of destitution relief, registration, road maintenance, nuisance removal, medical relief, and vaccination; and aggregates of the minor to constitute the major areas of administration. Nor would it be a trifling gain thus to avoid the present perplexity of "a census in two parts, containing different and sometimes irreconcileable methods of enumeration."

The adaptation of union to county boundaries has been, we are aware, carefully considered by the Poor-Law Board, and the opinions of several inspectors ascertained on the point. The difficulties are said to be rather those of finance than of organization.[1] It appears that the promising measure on which Dr. Rumsey relies has been postponed by the Government until a Parliamentary Committee shall have reported on the areas and incidence of local taxation. All the bearings of the proposed county reform are, therefore, hardly known at present. But it is certain that as the late beneficial change from parochial to union chargeability has already removed some obstacles to an improved distribution of parishes, so a further extension of uniform chargeability to county areas would remove others of a more formidable kind.

If the principle, which Mr. Goschen is applying to the metropolis, were extended to the counties, and if the burden of the inmates of workhouses were equally distributed among the

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[1] See Parliamentary Paper, No. 122, Session 1870.

county ratepayers, not only would the main financial difficulty be at an end, but the administration of the poor law in general and that of medical relief and sanitary inspection in particular, might be immeasurably improved under the superintendence of a county board.

To the medical philanthropist this becomes a most interesting question. The many varieties of destitution, the widely different character of the several classes for whom the poor law provides a shelter, an infirmary, an asylum, a maternity, an orphanage, and a school, besides a place of labour for the able-bodied, seem to point clearly to some classification of inmates, and their distribution in different buildings. It is obvious that, under county direction, this separation might be judiciously effected, and then the objection based on the local use of each workhouse by a single union would vanish. When the homes of the labouring classes in rural districts are brought, by county authority, under systematic inspection, and their sanitary condition properly regulated, the accommodation of some classes of workhouse inmates—as orphans, idiots, and imbeciles—might be provided for in cottages, with much saving of expense to the ratepayers.

But we are not merely concerned with what counties might do. We see that they already undertake many duties and responsibilities connected with state medicine. The justices, aided by their "visiting physicians," inspect asylums. They regulate and appoint medical officers to prisons. They exercise some control over coroner's inquests, for which the county pays. They direct preventive measures during epizootic visitations. In general, these affairs are managed with ability and efficiency. It is not unreasonable, therefore, to assume that a well constituted county board in which the educated element predominates, would prove a more satisfactory court of appeal in the first instance from local and district boards, and a more available engine of compulsion than any central bureau.

It is true that, under alarming emergencies, a sort of central despotism might not only be tolerated, but even demanded by the people. But such intervention, however plausible, would require to be jealously watched, and should be adopted only as a last resort.

We are by no means satisfied that freedom of scientific inquiry and independence of medical judgment would be as safe under a single department of public health in the metropolis, as under improved county management. And some objections, which we have already noticed, to the proposal for an intermediate authority may help us to a conclusion on this point.

Sir Joseph Heron may be taken as a specimen of one class of objectors; Mr. Simon of the other. The former, representing one of the most populous cities in the kingdom, conceives, not unnaturally, that any authority, except the Secretary of State, greater than his own, would be intolerable and 'unworkable'—an obstruction, leading to constant and very inconvenient collisions. Much of this, of course, is sentimental. That which is probably real in the objection might be prevented by conferring on communities, like Manchester, co-ordinate authority with a County Board,—that is, by providing for their joint action, on something like equal terms. First-class towns ought not to decide the question against county management, as we shall see when remarking on the case of other towns.

Mr. Simon's objections are more important, because of their more general bearing. While proposing (1971) "that for certain special health objects, there should be a second set of local authorities—authorities of larger areas"—he says that any intermediate body between the nuisance authority and the central government would "intercept communication, and diminish the force of stimulation." Here he discloses the central idea: that the motive power of local action must be governmental dictation. The "communication" must be direct between the one central and the many local boards. The "stimulation" must be applied from Whitehall. If this were an established postulate, our prospects of social progress and local effort would be melancholy.

It is not surprising that a skilful administrator at the seat of government, conscious of the excellence of the work which he is accomplishing, should be tempted to look upon communities much as a general does upon his brigades and regiments, to whom it might be fatal to entrust too much voluntary action. The larger and the more influential those local authorities, the less might be his direct influence. A great central officer may even see some advantages in reducing the weight and prestige of local authorities. Small areas and petty officers are often more manageable. "*Divide et impera*" might be the motto. But this principle of government presupposes a permanence of absolute wisdom at the centre, and of absolute dependence at the periphery. Is this a desirable, a hopeful result?

It was generally admitted that duties are now thrown upon the Secretary of State by the 49th section of the Sanitary Act, which are beyond his power. In the event of recusancy by local bodies, he is expected to find and to appoint proper persons to do the works; he may have to manage and control their execution, to borrow money for their cost, and in the last resort to levy and collect the rates, perhaps for a long term of years.

Mr. Rawlinson and others very rightly object to the State undertaking any such responsibility and risk of loss. It is in these cases that a powerful and intelligent county authority, well acquainted with the localities, would act with the greatest facility and efficiency.

If, under Mr. Eykyn's Bill for Public Prosecutors, after it emerges from the parliamentary crucible, these important officers are to be appointed for each county separately, the efficiency of administration will be immensely increased.

On the whole, the reasons for a county organisation, securing a high order of administrators, and properly qualified and well paid officers, seem to us overpowering. We commit the question, with confidence, to the judgment of the medical profession.

IV. In ascending from the smaller to the greater areas of provincial administration, we have endeavoured to keep tolerably clear of the question of towns, as presenting the chief difficulty in sanitary organization, and one which could not be correctly treated until the chief territorial divisions of the kingdom had been considered.

In affairs of general polity, there has been, and probably still is, too great a tendency to make very marked distinctions between town populations and country districts. But, in matters of public health, such distinctions are becoming less reasonable, more inconvenient, and sometimes really obstructive.

In the progress of civilisation, as the physical sciences and social economy are more skilfully applied to the many occupations of the people, as "agriculture itself becomes more and more a kind of manufacture," it is less desirable to maintain mediæval barriers and to enforce municipal boundaries, as limits to sanitary administration.

Whoever made the town or the country, in the poet's idea, any one of our great centres of industry might say with a certain Topsy, "I 'spect I growed." But how do these towns grow now-a-days? Like monstrous cephalopods, with their long and powerful tentacula of commerce and manufacture and art, they are continually stretching over the surrounding surface, and voraciously sucking in and assimilating the vital force of the country.

This self growth, which, as to area, is progressing even more rapidly and remarkably than as to population, is a fact which renders any hard and fast line between municipal and rural administration next to impossible. The great borough perplexity seems to be, what are we to do with the suburbs?

"For here it is that building speculators of the lowest order, and hordes of wanderers, who escape from populous places for want of room and from country parishes for want of employment, combine to make the border territory between town and country the worst regulated and in some respects the most dangerous of all districts."[1]

Sir Joseph Heron assured the commission that builders have thrown up contracts within the city jurisdiction, in order "to buy land in an adjoining district where they were not to be bothered by building bye-laws" (2376).

The object of a general buildings-law is, as the same witness said, "to prevent, in the very early history of a town, the bringing into existence those objectionable constructions" which are afterwards so difficult to remove.

No one showed more plainly than Mr. Carr, of Newcastle, the importance of applying preventive regulations of a most stringent kind to outlying districts and small townships, "while they are in growth," because, as he said, in course of time, some local authority, if not the town council, will have to incur the responsibility of their management, and to bear, at an enormous cost, the burden of extensive structural reforms, in consequence of the present abandonment of these places to chance. He and others would extend the same regulations to rural districts, for in many parts of England, the north especially, new towns are being formed with amazing celerity. It is said that, in the seven years 1861-8, about thirty places, not reckoned as towns in the last census (although each place had then more than 10,000 inhabitants) have adopted the Local Government Act, some of them in districts which, in our grandfathers' times, were agricultural or waste land.

Facts like these show the unwisdom of official proposals to enact two distinct codes of sanitary law—a town code and a village code—to be administered by different kinds of local authority and even worked by different classes of officers. Mr. Thring indeed acknowledged (344) that the real difficulty is "how to distinguish the laws you ought to apply to very large places from the laws you ought to apply to country places." We think, with Sir Joseph Heron, that no sufficient reason has been shown for two codes, although there are some special enactments which are indispensable only in a certain amount and density of population; those, for instance, which relate to particular trade nuisances, insalubrious occupations, slaughter-houses, &c., and which might remain in abeyance for all other districts, ready for application to any place as it might become a town, at the discretion of a superior authority.

If the main features of a good buildings-act were generally

[1] "Dwellings for the Labouring Classes" (Dr. Lankester's 'Journal of Social Science,' May, 1866).

enforced—such, *e. g.*, as those suggested by Mr. Menzies and Mr. Bailey Denton—not only would health and decency be promoted in neglected rural districts, where disease and mortality are said to be generally on the increase, but adequate preparation would also be made for any further aggregation of people or local change of industry.

Strongly convinced by the evidence before us, as well as by the history of public hygiene, that sanitary regulations—whether in the form of statutes or of bye-laws having statutory force, which the latter have not at present—should be virtually the same everywhere, without reference to town boundaries, we regret to be compelled to differ from Mr. Simon as to his distinction between "special health areas" and "common health areas"—at all events, as to the extent to which that administrative separation should be carried. Not that we would suggest anything so preposterous as that municipal corporations and local boards generally should be deprived of powers of internal government. Nothing of the sort will be listened to by Parliament, even were it desirable. But there are many towns so small and yet so obstructive to general measures of improvement, that, as Lord Fortescue recommends, they had better be treated as parochial units and included—though by special representation—in wider districts, or under county organization (5515*b*). It might be expedient to fix a certain limit of population varying perhaps with its density, or to name other local conditions, which should entitle a town to be regarded as a separate "sanitary district," so as to possess the same degree of autonomy as that already suggested for revised parochial unions. Parliament having, by the Artizans Dwellings Act of 1868, limited certain powers of demolition and reconstruction to towns having 10,000 inhabitants, this—though a rough method of estimating town capabilities—might be accepted, subject to certain conditions. A limitation of this kind was proposed by several examinees. Mr. Thring named 20,000 as the *minimum* population of "large towns." Mr. A. Taylor, Mr. Norris, and Mr. Rayner, thought that 10,000, or perhaps 12,000, inhabitants should constitute a town, in legal sense. The same amount of population is recognised by Dr. Farr as a limit for the higher class of towns. There were, he informs us from the census, 153 English towns with a population of 10,000 and upwards. Deducting London and the ten largest provincial cities and boroughs, and adding—from Dr. Rumsey's "Population Statistics"—thirty-four other townships and local board districts, also containing in 1861 more than 10,000 persons, it appears that there were in 1868 about 176 places to which special town regulations might be applied. The census of next year will probably report a greater number of such towns, containing just about half the population of England.

The system of borough government authorised by the Municipal Corporations Act was advocated by Dr. Farr for all large towns. But it appears that among the 208 municipal boroughs then existing only ninety-eight contained that amount of population which has been named as qualifying a place for local government independently of unions. Among the rest, we suppose, are those "troublesome" little places which are said by him to interfere with highway management and with the police of the county.

If all large towns were to be converted into municipal boroughs, it would be necessary to confer charters of incorporation upon seventy or eighty other places, that is to say, if they applied for charters; for it would be hardly justifiable to force a particular kind of incorporation upon any town which might be satisfied with its existing system of Government.

Now, we cannot avoid asking what real superiority, as regards practical sanitary management, the borough organization has over that prescribed by the Local Government Act? Would double the number of mayors and aldermen, and a corresponding addition to the paraphernalia of civic pomposities, improve either the health or the respectability of town populations? We do not wish to tread on any national corns, and so shall not anticipate an answer to these questions. But it may be as well to say that continental municipalities are no more efficient than our own. The Health of Towns Association told the following story some time ago:—"An Austrian Minister was remonstrated with by a member of the English Parliament on the ill condition of one town. The Minister acknowledged the fact, but said in despair, that nothing could be done to improve the condition of the population. *C'est un mauvais trou ; mais que voulez vous, c'est une municipalité !*"

We have lived in a provincial city, in a town under commissioners, in a local-board district, also under parochial government in a market town, and in a country house; and it has always appeared to us that the motives of those in power were much the same under the various descriptions of bumbledom. One thing is clear, that the most serious difficulties, the most obstinate resistance to measures of improvement, are to be found in those towns where the masses of working people are most densely crowded, where destitution and squalor, obscenity and intemperance, disease and deformity, insubordination and trade outrages, follow upon neglect of the first principles of public hygiene. No sort of town government, unless it be made directly responsible to a superior authority, can be expected to cure this social cancer. The question to be decided by the sanitary commissioners and by Parliament is, how best to secure that responsibility.

Obviously, it would be right to put an end to the absurdity of the co-existence of two governing bodies, under different acts, in the same place, as at Stafford and elsewhere (7235). Places with less than 10,000 inhabitants, whether boroughs or not, might be included, except under peculiar conditions, in district combinations; these in turn being represented, as we have before explained, in county boards. Places containing from 10,000 to 100,000 inhabitants, without surrendering their power of self-government in matters purely internal, might be represented individually and proportionately to their population in the superior board. In a large proportion of cases, the authorities of the town and of the union which contains it might be comprehended in a single board of health. A reform of this kind would probably do away with the plea for a separate town code and for limiting special rights and obligations within artificial boundaries, which must be for ever liable to change or else always in the way of good government. If all towns of (what might be called) the second class, were to be fairly represented in the county authority, compulsory interference with local defaults would rarely, we believe, be found necessary.

A few words only as to the position which cities and towns of the first magnitude might take in a national organization—for we have no intention of touching upon the details of sanitary regulation in towns. Those containing (say) more than 100,000 inhabitants, or for any other sufficient reason retaining a complete autonomy, might stand on the same legal footing as is proposed for county authorities; because their great size, wealth, and other capabilities, enable them to secure all the skill and counsel with which any single governing body could be assisted. Yet even the largest towns—Liverpool, Manchester, Birmingham, &c.—should, like counties, be responsible for gross neglect or mismanagement to the central authority. They should also be legally authorised, perhaps compelled, to co-operate with the adjacent county authorities in all matters of more general and extensive concern, such as water supply, sewage-utilization, suburban or rural settlements.

V. With regard to central departments, there seems to be a general consent among their chief advisers—even a sort of admission by the interrogators on the commission—that something like consolidation should be effected; at all events, that, so long as it may be found convenient that different departments of sanitary control should be conducted in separate offices, there should be some systematic division of labour, together with facilities for prompt co-operation in cases requiring the joint action of more than one office.

The suggestions of the earlier examinees, and the tone of the inquiry, evidently leant at first towards the Home Office, as the one seat of central authority. Mr. Simon and his staff of inspectors were to be transferred to it from the Privy Council. The Board of Trade was also to be relieved of its responsibilities under the Alkali Act, as well as in the investigation of complaints against water companies, gas works, and other commercial interests (37, &c.). Again, the Inclosure Commissioners, the modern representatives of the old Commissioners of Sewers, who deal with sparsely populated districts much on the same principle as the Local Government Act Office deals with towns and villages, would be connected with the latter office; so that the whole sanitary district question might come under one central authority.[1]

It was suggested also that the Poor Law Board, if not finally relieved, as it probably might be, of all responsibility in the administration of out-door medical relief, vaccination arrangements, registration appointments, ought to be brought more closely into connection with other health departments under the Home Secretary, as regards the medico-sanitary and nuisance-removal duties, now performed by officers under that board. The same principle of co-operation, if not of inclusion, was to be applied to the "arithmetical" department—the General Register Office.

In tracing the inquiry onwards, however, many difficulties presented themselves about making the Home Secretary of State the one Minister of Health. Memories revived of prompt and effective action by "Orders in Council." The old, and now more trustworthy, source of authority in times of danger to the public health, whether from human pestilence or cattle plague,

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1 Mr. Hutchins has thus described the relations of the Inclosure Commissioners to a certain commercial company (p. 48):

"There is another department of the Government which may, in some cases, exercise a sort of supervision over the doings of local authorities; this department is the Inclosure Commissioners. By a private Act, passed in 1867, certain local authorities are authorised to delegate to the "Town's Drainage and Sewage Utilization Company" any of the powers which such authorities possess for sanitary purposes; but the delegation must be made with the consent of the Inclosure Commissioners. It is thus possible that one set of works in a place will be sanctioned and supervised by the Local Government Act Office, while another set of works of a similar kind will be sanctioned and supervised by the Inclosure Commissioners. In short, if a local board itself executes any works, the consent of the Local Government Act Office will have to be obtained; whereas, if a local board delegates the execution of any works to this company, the consent of the Inclosure Commissioners will have to be obtained. Here is another complication of jurisdiction which it is very desirable should be done away with as soon as possible."

the Privy Council, appeared to be indispensably necessary to central action. Its limited control over medical education and qualification through an affiliated body, the General Medical Council, has given it a hold upon state medicine, which, in our opinion, it would be neither safe, nor easy, nor desirable, to shake off. The influence of "my lords" has been enormously increased, to the great benefit of the people, under Mr. Simon's direction of scientific inquiries and sanitary inspections—a series of efforts conceived and executed, under the seemingly limited powers conferred by the Public Health Act of 1858, with remarkable and unlooked for success.

Here and there, also, the notion, almost amounting to a conviction, is expressed that the Home Office is already overburthened with affairs of general, political, and judicial moment. It is also felt by many that were a single department of health to be constituted under the Home Secretary, it would not be a *principal* department of Government, with its own special chief sitting in one of the Houses of Parliament.

Some believe that the time is ripe for the creation of such a department, the Minister of Health being empowered to call for information from other departments, to institute inquiries by inspectors, as well into matters of abstract science as into local conditions; to indicate the nature and extent of measures required under varying circumstances of public health; to receive and adjudicate appeals on disputed points, unless these are referred to courts of law; and in extreme cases to enforce the law at the cost of the recusant authority.

Instead of drawing any definite conclusions from what has been published of the evidence, as to the form which the central direction of sanitary administration may take, we prefer the attitude of indecision, waiting to hear what more has to be said, especially by the Commission itself.

In the following number of this Review we propose to continue the subject of sanitary organization under the head of OFFICERS.

II.—Le Gros Clark on Surgical Diagnosis.[1]

THE mantle of the Hunterian professorship of surgery and pathology of the Royal College of Surgeons of London has usually

[1] *Lectures on the Principles of Surgical Diagnosis, especially in Relation to Shock and Visceral Lesions, delivered before the Royal College of Surgeons of England.* By F. LE GROS CLARK, F.R.C.S., Member of Council, and late Hunterian Professor of Surgery and Pathology in the College, Surgeon to, and Lecturer on Surgery at, St. Thomas's Hospital, and Examiner in Surgery in the University of London.

fallen upon one of the Members of Council. This arrangement, worthy and proper in many respects, has generally the disadvantage of rendering both the principles and practice set forth a shade behind the time.

There have been exceptions to this effect notably on one or two very distinguished occasions. On the other hand, at a time of life when the inducement to novelty and progress are less strong, the warmth of earlier hopes is chilled, the expectations of great results chastened, and the judgment modified by the lessons of experience; the utterances of the surgical oracle are more likely to be of the *safe* kind, and to compromise less both his own character for wisdom and discretion and that of his colleagues. If we were asked by one word to characterise the manner of book under present consideration, we should be tempted to select the word which we have emphasized in the last sentence. It is essentially a *safe* book, and the lectures prudent reading. The intensest orthodoxy is found in every page, and when principles are deduced they are usually so well within the record that they somehow often leave the impression of having been recorded before.

The lectures, twelve in number, were delivered in two courses in the theatre in Lincoln's Inn Fields. The first is introductory; the second and third are devoted to the general principles of surgical diagnosis; the fourth and fifth to fractures of the skull and lesions of the encephalon; the sixth and seventh to injuries of the spine and lesions of the cord; the eighth to fractures of the chest and lesions of the lungs; the ninth to lesions of the neck, throat, and heart; the tenth and eleventh to lesions of the abdomen and viscera; and the last to the same subject, with a few remarks upon injuries of the pelvis and its viscera. This wide range of subjects is treated throughout in the practical and well-disciplined manner which we should expect from the position of the teacher in one of our largest London hospitals, with abundant opportunities for the selection of cases of the most interesting kind; and though somewhat loosely thrown together, and the understanding thereof rendered occasionally rather ambiguous by a somewhat prudish phraseology, no professional man can peruse them without gleaning a good deal of solid information.

In the first or introductory lecture the prevailing characteristic shows itself in a caution against the abuse of statistics as applied to medicine. The learned professor thinks that hasty generalisation is still a prevalent fault of the impatient student "allured and beguiled" by "the attractiveness of discovery."

Again, an essentially critical mind is evinced in the remark that many of the so-called discoveries of the present day are merely rediscoveries, and that "the progress of generalisation in pathology and therapeutics is necessarily impeded by the obscurity which envelopes the operations of the living organism." Yet to the attentive reader, looking for aids to diagnosis (p. 7), crumbs of comfort are held out in the opinion that "some steps in advance have been made. The development of caloric generally in varying states of disease, and even topically in local affections, is a subject fraught with much interest, and promises to throw light on many obscure conditions" treated by the surgeon.

After a brief sentence, conveying the usual tribute to the microscope, the author objects to the complexity caused by the use of expressions liable to misconstruction, as exemplified in the description, under the several heads of adhesive, suppurative, ulcerative, phagædenic and gangrenous inflammation, of what are, in his opinion, different phases of the same state, modified by accidental circumstances.

We should prefer rather to designate them as different conditions of the general system, undergoing locally a similar dynamic derangement.

In one of the many digressions which, like those of Swift, we frequently prefer to the author's argumentative clue, we find the following wise and timely remarks:

"The popular success which so often attends an exclusive pursuit is a temptation to the production of crude compilations, containing premature generalisations and authoritative expressions of opinion, which the experience of the writers in no way justifies. And I would venture to add that early success in practice is not the boon which many young men are disposed to regard it, and I am sure it is in the aggregate detrimental to the science of the profession which their natural endowments *calculate* them to illustrate and adorn . . . . Sure I am that the fruit of many a promising worker's time has been lost to the profession in consequence of the premature engagements of a successful practice—a misfortune which rarely befals him whose ambition it is to acquire a reputation as a general surgeon; whereas the history of our profession supplies many striking instances among our distinguished surgeons of the fruitful cultivation of science in their youth, as well as of the permanent influence such early devotion of their time and talents has had on their own future career."

At the present time, when books on special subjects are published daily with the view of obtaining that practice and experience of the results of which they ought to be the exponents, a gentle deprecation, whose evident truth and the benevolent phraseology gives it almost a humorous turn, comes fitly from an authority on surgical ethics, speaking *ex cathedrâ*. From this flows naturally the digression to the subject of preliminary education, ending in a tribute to the efforts of the College of Surgeons to secure a higher standard in its *alumni*, and an apology, often heard in the college precincts, for conforming to the necessities of supply and demand on the part of the public and their professional attendants.

The author, at p. 18, is

"disposed to regard the discernment popularly attributed to sagacity or an intuitive perception of that which escapes common notice, as really the fruit of patient study and accurate observation, conducted by a well-balanced and educated intellect, of which a conscientious love of truth is a predominant characteristic. A bold and shrewd conjecture of the charlatan may be occasionally correct, and may dazzle the ignorant, whilst the many failures are kept out of sight, or accounted for by explanations or excuses which impose too readily on the uninitiated, who are singularly incompetent to appreciate the long and tedious process by which the scientific practitioner attains that tact and aptitude in the diagnosis of disease which expresses itself with the modesty and misgiving often appropriate, even in the most experienced."

Some illustrations of the sources of obscurity in diagnosis follow. One is drawn from a private case, unique in the author's experience, which he saw with a fellow-councillor, and the particulars of which are briefly condensed as follows.

A large tumour in the popliteal space, bulging laterally, tense in some parts, soft in others, generally somewhat elastic, with the skin thin in many places. A distinct thrill was audible, as of an artery pressed upon; but no variation in size when the femoral artery was compressed, and no distinct pulsation or expansive enlargement. The patient was emaciated, and with health greatly deteriorated; and the history of the case threw but little light upon the important question which presented itself to the attendants, whether an aneurism or an encephaloid tumour was what they had to deal with? To them the preponderance of evidence was in favour of the latter, and with the *doubt still resting* upon their minds they advised amputation as the *safest* course!

The disease proved to be diffused aneurism of the popliteal artery, the proof, of course, coming too late to save the limb. There seems to us to have been at the disposal of the surgeons in this case an important means of certitude, which does appear to have occurred to them. Before proceeding to the serious mutilation which was practised, and while all things were in readiness either to tie the artery or, as a last resource, to amputate, would it not have been as well to make a puncture or two with the small trocar and canula so efficiently provided by the instrument-maker, even before the contrivance of the "Aspirator," and so used as to bring out a specimen of the contents of the tumour, as by the instrument used in tasting cheese, and then and there to have submitted it to the microscope. If blood-cells only were found, then the artery might have been tied; if cells of a suspicious character, then amputation would have followed with the certainty of a more consolatory after examination of the disease. In a case of somewhat similar doubtful external characters this proceeding, in our hands, rendered the nature of the case perfectly clear before operating. In those of the author it would have been a much more powerful approval of the aid rendered to scientific modern surgery by the microscope than the merely passing allusion made to that instrument in these lectures.

We are glad to see an allusion in the lectures to an inflnence very powerful for good or evil to patients subjected to operative proceedings, the time and season for which lie at the choice of the operator, but which is, in our experience, but rarely referred to by writers on surgery; and that is, the atmospheric states and variations which may prevail, independently of the positive endemic and epidemic influences which seem to be beyond the ordinary means of indication.

It is, perhaps, not remarkable that writers on the chirurgical art who do not shine particularly in the manipulative part should incline to that disparagement with which highly scientific and literary votaries affect to regard it. The Hunterian professor condemns such disparagement equally with the high commendation sometimes given to "mere facility of execution, which though, after all, only a question of mechanical dexterity, it is often regarded by the thoughtless with an admiration which is as exaggerated in degree as it is humiliating to the operator." He subsequently says, however, that

"the merit of a well-conducted operation consists less in its rapid and skilful performance than in its fitness and the care with which it has been planned; the arrangement of each step with anxious forethought, and the anticipation of obstacles and preparations to meet them, demand qualifications which test the resources of the surgeon, and the success of an operation depends at least as much on the possession of these qualifications as on the exercise of manipulative skill."

We may safely say that it has fallen to the lot of most who have been in the habit of witnessing the operations at our different metropolitan hospitals to see the most elaborate preparations for the most impossible and usually unforeseen contingencies, and the most anxious pre-arrangement of details, to miscarry wholly, and to embarrass both the operator and his assistants, for want of the coolness, self-possession, and dexterity which is the gift of the surgeon, "born, not made." That which the author calls the "peculiarity of surgery, that the mind which plans and the hand which executes belong to the same individual," is mostly found as a combination very highly developed in those who have risen to *real* eminence in the art and science of surgery. To philosophise and generalise upon, and to criticise the failings of the healing art, belongs to one class of minds, doubtless a very high class; to design and carry out manipulative or other plans for the relief of the sufferer belongs to another class, of at least equal importance to the public weal. Let each be content with the Homeric estimate that their value is *more than armies* when wisdom is united to skill. It is more rare to find these mental and physical characteristics combined in one and the same person; and when a good and dexterous operator and a surgeon successful in his cases embodies also philosophic ability and a brilliant literary and oral power of stating and enforcing his opinions,—in such a man we have the highest development of which the surgical profession is capable.

In the second lecture the professor thinks it necessary even at this day to explain carefully the difference between the technical meanings of the words "symptoms" and "signs," as a sort of afterthought upon his own definition, viz. "Any deviation from a normal condition, whether functional or organic, is called a symptom or sign." In the newest schools of both medicine and surgery it is, we believe, well understood that the word "symptoms" denotes functional departures from the normal phenomena which are *subjective;* while the word "signs" refer to those evident changes which are *objective* to the observer, and it is often preceded by the adjective "physical;" and also that printed tables have been drawn up by some societies for the better observation of cases, in which this distinction is carefully made.

In the "subjective and objective indications" (at page 35) it is stated that "direct violence suggests fracture, and force indirectly applied points to dislocation. So uniformly almost is this the case, so rare the exceptions, in the shoulder-joint, that a clear and well-defined description of the manner in which the injury was inflicted is alone sufficient, in nearly all cases, to determine the nature of the lesion, whether it be dislocation or fracture." Now, although we admit that the joint selected for illustration of this somewhat sweeping doctrine is the very best for the purpose, yet even here the exceptions to such a rule are so numerous that the surgeon who should be ill-advised enough to trust only or mainly to it would soon discover it to be a specimen of a generalisation somewhat vague and certainly overdrawn.

At page 42, on the value of the indications of the tongue, we have a glow of ardour almost impulsive in praise of the mute testimony to disease given by this organ:

"I would trust the tongue," says the professor, "when almost all else beside might seem to forbid hope; and I should know that its glazed and brown surface tell more plainly than the parched member could articulate that food is useless, the chemistry of organic life is at fault, nourishment has ceased, and that dissolution is at hand."

The specimen just given is, perhaps, the nearest approach to eloquence that we find in the calm utterances of the philosophic professor, and it illustrates that tendency of enthusiasm to overrate the object of its worship, against which, in the commencement of the course, the lecturer warned his hearers. That the tongue indications, taken with the other symptoms, are of great value no one will deny, but the appearances specified may certainly result from causes more or less local to the mouth, nose, and throat, without the constitution participating in the derangement to any serious extent.

The observations upon delirium, facial expression, colour, excito-motory phenomena, rigor, sensibility, temperature, and specific sensation, are short, but evince occasionally much shrewdness of observation; *e.g.*—

"It is said that there is a peculiar tint of skin allied to malignant disease, but its frequent absence deprives it of worth. Not infrequently disease of unequivocally malignant type is associated with a healthy complexion and well-nourished frame."

There is no doubt that surgical writers have usually overestimated this symptom. It is one which is well marked only in the later stages of malignant disease, and is frequently only the expression of a condition which is already made too evident by the other signs and symptoms.

Speaking of *pyæmia*, we find—

"Rigor is so usual—I may say so constant—an event as ushering in this fatal malady that although its presence may not be accepted as conclusively diagnostic, in combination with other indications of the disease, yet the absence of this symptom may induce the surgeon to be hopeful, even when the otherwise condition of the patient might excite great anxiety. Rigor is marked by a rise in temperature, and in the subsequent stage of sweating it falls, in some instances, below the natural standard. What may be the proximate cause of these changes, so entirely at variance with the feelings of the patient, is matter of conjecture."

"Dr. Johnson, in some interesting observations on this subject, remarks that probably the sympathetic plays an important part, by its influence on the muscular contractility of the capillaries; and that there seems reason for believing that in rigor there is an anæmic condition of the cord, and, in epilepsy, also an anæmic condition of the brain" (p. 49).

That the vaso-motor nerves of the vessels of the nerve-centres play a very important part in temporary and functional derangements the experiments of Brown-Sequard and others have clearly established; and this doctrine, *pro tanto,* has passed from the realms of mere conjecture. Dr. Johnson's investigations into the permanent changes induced by disease in the coats of the blood-vessels in these parts have also a very weighty bearing upon their pathology.

In Lecture III, upon *pain,* we find a *subject* of adverse criticism (p. 75):

"Pain, if long continued or severe, is exhausting, but, *per se,* is very rarely fatal. I think the shock of pain is much over-estimated. It is difficult to separate from the suffering the effects which are due to concurrent causes in operations; but it is certain that great and almost continued pain is compatible with protracted life. I have known some remarkable instances of this fact; and although I appreciate to the full the value of chloroform, I am not aware that operations are rendered less fatal by its agency in securing the patient from suffering."

Now, we think that herein the professor does not discriminate enough between the different *kinds* of pain. There are pain *and* pains—pain wearing, crushing, exhausting, and, in the end, fatal; and pains, exciting, stimulating, and even tending to counteract the tendency to death. Of the former kind is the pain which has been too often inflicted purposely by man on his fellow for punishment and torture, causing ultimately death from exhaustion, although vital parts may have been carefully avoided; and of the latter kind are usually the acute neuralgic pains, the pains of labour, and those of toothache.

With regard to the other question of the value of chloroform raised by the lecturer, we would fain ask—If the good effects of this agent are not due to its anæsthetic properties, to what, then, are they due? A little further on (at p. 67) we find, further, the opinion that "chloroform is a friendly agent in modifying the depressing effect of our handiwork, in that the mind of the patient is tranquil in the anticipation of passing through the ordeal without pain." It is, then, the opinion of the author that the beneficial effect of chloroform is owing to its taking away the *fear* of pain rather than the *pain* itself! How, then, as to the effect upon very young children and animals, who do not anticipate or dread operation? And to what is the supporting effects of opium, chloral, and other sedatives due, if not to the anæsthetic effect upon the nerve-centres, and to the quietude and absence of restlessness and irritation induced by those remedies? To deny that the beneficial effect of chloroform is due to its plain and evident power of removing pain, and to attribute it to some other more indirect influence, is surely deplorable departure from the simple law of induction which has been so safe a guide in scientific experiments!

"*Crepitus* as a diagnostic sign of fracture is generally much valued by surgeons. I am not disposed to attach so much importance to it by itself, except in experienced hands and under peculiar circumstances. I think it is subordinate in significance to many other signs and is often obscure, and liable to be confused with a similar sensation encountered in synovial and other effusions" (p. 62).

If disposed to severity a reviewer might characterise the passage just quoted as an evidence of affectation of singularity which simulates, with but imperfect success, originality and shrewdness in observation. Granted that, in the hands of a tyro, this sign may be mistaken for synovial or effusion crepitus, as much may at least be said of every sign to which the professional judgment attaches value. The requisite experience being at hand, *crepitus* is the most certain and characteristic mark of fracture, at any rate, in the bones of the extremities. In the head, pelvis, and thorax, there are, of course, causes which interfere to prevent it or mask its presence, but these are the only circumstances which are usually effective in robbing the sign of the peculiar and just value given to it by surgeons. In this respect it is scarcely inferior in importance even to distortion and deformity, and when the two are combined the evidence of fracture becomes irresistible.

In the paragraph upon *treatment* as a means of *diagnosis*, we find the author coinciding with the valuable doctrines previously advocated from the same chair by Mr. Hilton as to the great value of *rest*. He might with advantage to his hearers have also alluded to the administration of chloroform and other drugs, such as the iodide of potassium and the mercurial preparations; the former as a means of diagnosis in hysterical joint affections, and the last named in syphilitic diseases, so often obscure in the history, and the true origin of which may be pertinaciously denied by the patient. For our own part, in a disease so universally disseminated, and contracted either ignorantly or under circumstances branded with the powerful stigma of shame and so conducive to concealment, we prefer to believe the silent evidence of the effect of these drugs in preference to the history given by any patient whatsoever.

The extensive meaning given by the author to the word *shock* (p. 67), which is made to include the reflex phenomena which ensue on the passage of the catheter, as well as the terrible *rigor* which ushers in *pyæmia,* to our mind illustrates a similar confusion of ideas as that upon which we have before animadverted in respect to *pain.* There is a stimulus to the nerve-centres which produces the *shudder* of a comparatively slight irritation, and which may even be allied to a pleasurable sensation; and there is the stunning effect of a violent impression on the same parts, by which is indicated a very serious mischief, speedily followed by collapse, alternating with a reaction, which gradually becomes feebler as the insidious and searching poison penetrates closer to the citadel of life.

Instead of the "advancing step in generalisation" desiderated by the author in this definition being "attained thereby," we seem more likely to accomplish by this road a general chaotic vagueness. In proportion to the irritability of the excito-motory and ganglionic centres, and the impressibility or energy of the functional organic processes, a patient may, indeed, almost "die of a rose in aromatic pain;" or he may survive the most terrible mutilations, or the slow work of the frightful pest which makes him "by inchmeal a disease." But the word *shock* owes the most of its value, as will be acknowledged by most practical surgeons, to its conveying conventionally an idea of *danger* to the economy of the system. It has the same relation to the symptoms with which the professor would confound it as, in military parlance, a *feint* or sham attack has to a real attack. The one may, indeed, be converted into the other, but a careful distinction between the two is useful to the defending force as well as to the student of the campaign. With the subsequent remarks of the professor at "degeneration of the liver or kidneys, and, *à fortiori,* of the heart," accelerating the fatal effects of a shock which in a healthy subject would be comparatively trivial, all practical surgeons will agree, inasmuch as it has become almost an *axiom* (not to say a *platitude*) among them.

The effects of shock at different ages, the rapidity of reaction (when possible) in the young, its retardation in the aged, and the less speedily evident, but often more fatal, symptoms of shock in the less impressible and resilient structures of the latter, are observations for which a larger amount of originality and importance may justly be conceded to the author.

In the section on *railway injuries* (p. 71) we find many pertinent though, once more, digressive remarks respecting the "almost uniform diversity of opinion which these cases elicit in medical witnesses in courts of justice." Much of this the author attributes to the deception practised by witnesses. In proportion to the disposition of the medical man to be impressed with "confidence" or "suspicion," in cases where so much depends upon the statements of the patient, is the character of the testimony of the skilled witness. We fear that something must also be attributed to the *bias* induced by the retaining fee, and to the feeling, encouraged usually by the sister profession always engaged in the matter, as to "our side."

Beneath the author's remarks there seems to be also a feeling of the same kind, if we may judge by the nature of the remedy which he proposes:

"It is that when medical men are engaged in the prosecution or defence of such claims, they should make it conditional that they be permitted to confer together, and, at the least, attempt to arrive, by consultation, at a joint opinion as to the true nature of the case and validity of the claim, leaving, of course, the amount of compensation, if any, to be assessed by the jury."

This seems to be an equivalent to the system of medical juries which has been advocated by many writers on medical ethics. The compensation of the medical skilled witnesses is a point which can only be properly determined by a special agreement between the contending parties and the witnesses called by them. The legal compensation for the injury, if any, can only in the nature of things be determined by the common or special jury. No one, we fancy, would think of proposing to place the onus of the latter upon the very insufficient shoulders of the medical witnesses, thus to usurp the functions of both judge and jury.

The retained urine and involuntary evacuation of the bowels in *paraplegia* is (p. 72) referred by the author entirely to the cyclo-ganglionic influence, which remains unimpaired in compression of the spinal cord. But there are also differences in the mechanical conditions. The retention of fæces against the involuntary peristaltic action of the bowels is accomplished by a muscle, the *sphincter ani*, which is almost entirely voluntary or spinally reflex in its action. The retention of urine is due in great measure to the elastic fibres of the neck of the bladder, aided by the circular involuntary muscular fibres in that situation and in the prostate; while its evacuation depends in great measure upon the voluntary compressing force of the abdominal muscles as well as upon the preponderance of the muscular fibres of the middle and fundus of that organ. A case illustrating this point well is found in a subsequent lecture (Lecture VI, p. 157), in an elderly man with defective co-ordinating power who was habitually unable to evacuate the bladder without the aid of a catheter, but who could control the evacuation of the fæces under ordinary circumstances, but lost that power when the bowels were relaxed and the peristaltic movement increased.

In the next page "the rhythmic action of the heart after its removal from the body, and the peristaltic action of the intestines," is explained by the limited generation of nerve force in the ganglia which pervade their structure. This explanation, first given by Remak, which may be found in any of our text-books on physiology, seems to be urged as an original argument against the "essential independence" of the action of the heart "assumed by physiologists," and against the assumption that the rhythmic contractility of its muscular tissue after isolation is of the same kind as that of muscles of an amputated limb. So far as we know the doctrine first mentioned, originally taught by Galen, is now admitted by all physiologists, while the latter assumption is solely the property of the learned professor himself.

In the remarks upon *temperature* (p. 73) we arrive at some valuable and original observations obtained from cases in St. Thomas's Hospital, with the assistance of Mr. Wagstaffe, the surgical registrar, of which the results may be briefly stated as follows:

In simple *shock* and *reaction* the fall in temperature averaged from one to two degrees about half an hour to an hour after accidents, the reaction being marked by a rise to from 100° to 103° Fahr. in the next thirty-six or forty-eight hours. *Hæmorrhage* appeared still further to diminish the temperature after shock. In a case of cut-throat which recovered, the thermometer was at 91·2° one hour afterwards, and never reached beyond 100·1° during reaction. This unusual depression of temperature is attributed partly to mental causes.

In *rigor* the temperature always *rose* before the attack of shivering, and remained high (from 100° to 106°) until generally about half an hour afterwards, varying with the acuteness of the disease.

After *operations* there was usually a fall of about half a degree (rarely below 97°) in cases which recovered. No fall took place in the fatal cases. In the succeeding *reaction* an unfavorable termination was expected if the temperature exceeded 104°, except where the other signs were satisfactory. In cases which recovered the highest temperature was reached in twenty-four to forty-eight hours after operation.

Before *death* in acute diseases the temperature rises till the time of death. The highest death temperature recorded was 106° in a case of *pyæmia*, of which an interesting chart is given in the volume. The highest temperature seen by the author in cases which recovered was 105° after *compound* *fracture.* After *head injury,* where no reaction took place, the temperature before death was as low as 89·6°.

In the paragraph on the *functions of the pneumogastric* and its influence upon the heart (p. 81), the idea suggests itself irrepressibly that the profession is "*supra crepidam.*"

"Indeed (says he) I must admit that I cannot understand how this negative influence—for as such the inhibitory property ascribed to this name must be regarded—is exercised, and what it may be. Probably further investigation may throw light upon the subject."

To No. IV of the 'Journal of Anatomy and Physiology' (May, 1869) we are able to refer the professor for the investigations which he desiderates, in a paper upon the "Influence of the Vagus upon the Vascular System," by Dr. W. Rutherford. The chief result of 120 experiments therein described was, that "the inferior cardiac branches of the vagi are *inhibitory* nerves of the heart, and their functions cannot in any sense be regarded as motor;" and they seem to show conclusively "that the contractile elements of the blood-vessels are, like those of the heart, presided over by two systems of nerves, one motor, the other inhibitory." Dilatation of the vessels is effected by these latter nerves, but this dilatation is passive, and is simply due to the elasticity and blood pressure being no longer opposed by the contraction of the vessel. The inhibitory nerves seem "always to end in ganglia," "in vaso motor nerve cells whose evolution of energy they are capable of diminishing."

Ignoring the facts upon which these conclusions are founded, our Hunterian professor considers that

"The pneumogastric is probably *almost exclusively* a centripetal nerve, conveying impressions between the important organs to which it is distributed and the brain. Thus, it communicates the sensations of hunger, thirst, and repletion from the stomach; it gives warning of the presence of carbonic acid in the lungs; and that it possesses an analogous function in relation to the heart, we conclude from the negative rather than positive evidence."—(p. 80.)

We fear that among the array of physiologists who have investigated this subject there is not one whose conclusions support the negative evidence which has weighed with the author. Willis, Lower, Valsalva, Moleschott, Schiff, and Lister, believe the inferior cardiac branch of the vagus to be the *motor* nerve of the heart, while Volkmann, Pfluger, the Webers, and Von Bezold, consider with Dr. Rutherford, that its function upon that organ is *inhibitory,* and restraining upon the rhythmical movement carried on by its inherent ganglia, while its superior cardiac branch is proved by Cyon and Ludwig ('Journal de l'Anatomie,' November 5th, 1867), to whose accuracy of observation Rutherford bears testimony, to be the "depressor" nerve because of its power of lowering the blood pressure by dilating the blood-vessels of the heart, and thus diminishing the resistance to its contractions.

In the *use of the trephine* considered in the fourth lecture, the lecturer agrees with the opinion now generally entertained by surgeons, that the employment of this instrument should be much more limited than was the custom among the older practitioners. That it should not be used until symptoms of compression have shown themselves in cases of depressed fracture without a communicating wound of the scalp, all modern surgeons will agree; but the conditions are very different in cases of *compound* fracture. When this occurs with depression of both tables, the use of Hey's saw and the elevator, or of the latter alone, will generally suffice to remove the pressure from the brain, without the employment of the trephine. But in those cases of compound fracture by a pointed instrument alluded to at page 89, in which the scalp and outer table are both perforated, and the inner table, as is usually the case, driven in upon the brain in a spicular and stellate way, we are at a loss to conceive how the judicious and careful use of the trephine can increase or complicate the damage already received by the brain or its membranes; nor how the fragments can do anything else than irritate the brain and give rise, in the future, if not immediately, to serious symptoms; nor how they can be removed by any other means than by this instrument. After saying that in such cases the immediate symptoms of compression are rarely absent, but that even when they are, the lecturer very cautiously adds:

"I cannot say I am disposed to subscribe to the doctrine that so serious and in itself so dangerous an operation as trephining should, as a rule, be resorted to."

And a little further on,

"I must, however, admit that fatal mischief does sometimes supervene from inflammation and suppuration between the dura mater and bone, or even in the substance of the brain; and then the use of the trephine rarely affords more than temporary relief."

But it may be said, if the trephine were used in the first instance, fatal mischief occurring in the bone or dura mater, and, perhaps, in the brain itself, might be prevented; while its skilful use could certainly not make matters worse, any more than a free opening in the skin and fascia would make worse any other deep-seated and confined suppuration already communicating externally through a small sinus. This is one of those cases in which, awaiting development in the expectant attitude which throughout the lectures is a favorite one with the professor, is too frequently watching the tide of destruction rising to a fatal termination "with folded arms and listless hands."

In one of the cases given to sustain the *expectant* doctrine in these injuries, the situation of the depression about the middle of the frontal bone, without symptoms, suggests strongly a large frontal sinus with depression of the outer table only; or if the inner also, a lodgment found for it between the cerebral lobes. The other case given to illustrate this important point seems from the description to have been a fissure only without depression, and that in a youth of fourteen.

Upon this subject of injuries to the head many cases are shortly described, which, though somewhat disjointed, are very interesting, and we regret that want of space prevents further allusion to them. We may say in passing that we quite agree with the author in discarding the doctrine of "contre-coup" as a cause of fracture, *i. e.* violence applied to one side of the cephalic globe, producing a fracture at the other side only, without any external cause of such as counter pressure or resistance; while, on the other hand, we dissent from his somewhat inconsistent position that rupture of a meningeal artery of any considerable size at a point distant from that of fracture can be caused by the simple *vibrations* of the skull (p. 104). These arteries lie mainly in grooves or channels in the bone, and any fissure running across them in the bones may sever them by its sharp edges, while the fissure itself, like a recent one in glass, is apt to be so indistinct as to escape observation.

In the definition of *concussion* and its distinction from *shock* (p. 120), we have some valuable remarks. The author says, "I have never made or witnessed a *post-mortem* after speedy death from a blow on the head where there was not some palpable lesion of the brain," though he would not deny the possibility of such an occurrence in which "concussion *plus* shock and *plus* other predisposing conditions or concomitant injuries" which may be overlooked (p. 121). We believe that a very careful and microscopic examination of whole of the nerve tissue in such cases would usually find damage, or, as the author prefers to call it, *lesion* in some vital part.

The temperatures in two instances of simple concussion which recovered, taken half an hour and an hour respectively after the accident, were found to be 93·5° and 96·2°. In a fatal case of fractured base with laceration of the brain the temperature fell to the unprecedentedly low degree of 87·4° one hour and a half after the accident, scarcely reaching 90° just before death. In relation to this subject, the professor remarks that the temperature has been found at St. Thomas's Hospital more uniformly low in spinal and other visceral injuries than in head injuries, together with a steadier fall and longer duration.

One case challenges attention from its peculiar character. An excavator was buried under some falling earth. When brought in he was insensible, with contracted and sluggish pupils, stertorious breathing, and convulsions. His respirations became intermittent, and he died comatose in six hours. No injury was found in the brain or spinal cord, but some of the contents of the stomach, indentified by comparison, had got into the trachea and blocked up many of the bronchial tubes. In this case apnœa was complicated with concussion.

Pyæmia following upon head injuries have been very rare in the author's experience. Not more than two cases have occurred in St. Thomas's Hospital during the last five or six years. In several instances of brain injury (not particularised) he has noticed difficulty in articulation and perplexity in the choice of words, either without hemiplegia, or coexistent with it, on either side indifferently.

In one case of fracture of the ethmoid bone given at a previous page (97), complicated with laceration of the inferior surface of both anterior lobes of the brain, an escape of cerebral matter into the nose and mouth, the presence or absence of this important symptom of *aphasia* is not noted. "Yet the patient survived till the fifth day, and was rational till the morning of the third." We are left to infer that the reason of the patient was discoverable by speech.

In the lecture devoted to injuries of the spine (No. VI), we have many interesting cases. In one followed by paraplegia and ultimately fatal by asphyxia, the theca of the cord was distended with fluid blood derived from a ruptured spinal artery. In another, the two upper dorsal spines were moveable and crepitated; no paraplegia; but a good recovery. A third patient fell on some spikes, one of which entered near the umbilicus, indenting the body of the second lumbar vertebra, the point of which was found, broken off, lying across the rectum.

He gives also two cases in which, after accidents not very severe, *paraplegia* more or less complete was present for some months before the patients slowly recovered (p. 146). The symptoms are attributed, we think justly, to something beyond simple concussion, probably extravasation of blood into the spinal theca or canal. In another case, from a railway collision, a man walked four or five miles after the accident; then, in twenty-four hours, became quite paraplegic, from which he only entirely recovered after the lapse of two years (p. 148). A young sailor struck by a carriage wheel on the back complained at first only of a little tenderness over the dorsal region, with peculiar breathing. In the night, however, he had vomiting, followed by a succession of epileptiform fits with violent *emprosthotonos* and unconsciousness. He had never before had fits. This case recovered in a few days.

The symptoms and pathology of railway concussions of the spine are well and compendiously given in the work. The emotional excitability which accompanies the form of injury is almost a characteristic, but the author is "disposed to regard the cases of railway spinal concussion generally as instances of universal nervous shock, rather than of special injury to the cord" (p. 152). The most useful of the few illustrations in the work are those representing sections of the spinal cord in a case of railway injury examined microscopically by Dr. Lockhart Clarke (published in the 'Transactions of the Pathological Society' for 1866), contrasting the shrunken and diseased condition with the appearance of the healthy cord.

At page 161 we find the autopsy of an interesting case of caries of the odontoid process of the *axis* and destruction of its ligaments, resulting primarily from a fall on the head. The symptoms were in abeyance for five months afterwards, and then showed themselves in occasional inability to rotate the head, and a sudden jerk or slipping as of the bone of the neck, into its place. This was followed by comparative freedom of movement usually for several weeks. He could walk when admitted into the hospital, and sensibility and command over the bladder were natural. He died suddenly from dyspnœa and paralysis. The transverse ligament of the atlas had disappeared, and the upper end of the spinal cord was crushed and pulpy, and much cheesy matter was found in the dorsal region in front of the dura mater. The lower dorsal spines during life were prominent and tender. Respecting the important distinction between the symptoms of shock proper and the effects of injuries of the spinal cord we have (p. 163) the following remarks:

"In shock the appeal is specially and directly made to the cyclo-ganglionic system of nerves and organic life is primarily affected; the functions of animal life are involved secondarily, and by sympathy. In lesions of the brain and spinal cord, this order of phenomena is inverted. I have often been struck with the limited amount of shock which as a rule accompanies even severe lesions of the cerebro-spinal centre; whereas, in structural injury of other viscera, especially of the membranous organs of the abdomen, the shock is immediate and fully pronounced."

He goes on to explain the evident cause as found in the very large development of the cyclo-ganglionic (sympathetic) nerves which are exposed to the injury in the latter case.

In Lecture VII the author treats of *tetanus* and its varieties, contrasting them with hydrophobia, epilepsy, chorea, and hysteria. He considers that the acute form of tetanus, though almost always mortal, may yet, in some instances, and these not *subacute* in character, be followed by recovery. An interesting fatal case is recorded to show that unilateral spasm (*pleurosthotonos*) affecting the side of the injured arm may occasionally precede the usual characteristic condition of the symmetrical spasm (p. 173); and another of very severe hysterico-tetanic spasm of the legs, arising from spinal irritation in a young lady, cured by counter irritants and sedatives. He quotes the opinion of the late Professor Sewell of the Veterinary College as to the importance of perfect quietude, rest, and freedom from excitement in the tetanus of horses (best treated by carrying the stable key in the pocket), and mentions a remarkable recovery from traumatic tetanus in a young girl following an injection of tobacco infusion into the rectum, whether *post hoc* or *propter hoc* he does not pretend to decide. An illustration after a drawing by Dr. Lockhart Clarke, showing the distortion and shrinking of the cord in tetanus from fluid disintegration of the grey matter enlivens this part of the book. Dr. Clarke's conclusion is that the symptoms are due to a morbid state of the blood-vessels and not to excessive functional activity of the cord, seems to be based chiefly upon the dilated and often disintegrated state of the blood-vessels found in these cases. The lecturer, however, considers that, as a consequence of this, an exalted activity of the cord must exist, and that vascular disturbance does not necessarily imply structural lesion of the nerve tissue of the cord itself. The eminent physiologist above quoted would probably consider that the one condition is the primary cause of the other, and that the disease has its different stages of morbid change. The lecturer throws out subsequently the valuable suggestion that while rapid evolution of these organic changes may produce *tetanic* convulsions, their slow development may cause *paralysis*, as chronic affections of the grey matter of the hemispheres produce *dementia*, and acute affections *delirium*.

After the brief review of the symptoms of the foregoing convulsive diseases, the professor gives as his conclusions: 1st, That they may all be dependent on blood poisoning. 2nd, That the phenomena of rabies are distinctly due to this cause. 3rd, That even in traumatic tetanus the pathological changes in the cord are probably influenced by a morbid condition of the blood, but that this progressive disintegration would appear to be the cause, and not the consequence, of the muscular spasm.

The operation proposed for the relief of the compression of the cord in *fractured spine* is then considered, and the conclusion arrived at by the author is unfavorable to attempts to raise or remove the depressed bone. He states "of the many cases of fractured spine which I have on record, and which I have examined *post-mortem*, I cannot recall an instance in which depression of the arch alone sufficed to account for the symptoms;" urges that direct violence sufficient to drive in the arch of spinous process, the body of the vertebra remaining unbroken, must always inevitably prove hopelessly destructive to the nerve tissue, and considers, further, that the activity of the reflex phenomena observed in the legs in these cases is *not* (as has been considered) inversely *proportionate* to the amount of damage done to or division of the compressed cord; but are often almost in abeyance. We fail consequently to have in this a measure of the amount of good likely to be derived from relieving the cord from mere pressure. He further is of opinion that the only supposable form of spinal injury which might be benefited by operation is a fracture of the vertebral arch alone, with limited depression, or the recent intrusion of a spiculum of bone within the theca. But he adds, where are these cases to be met with? and how are they to be recognised?

Lecture VIII contains much useful matter in relation to emphysema, pneumothorax, hernia of the lung, hydrothorax, and empyema.

In answering the question, how is extravasated gas removed? he considers the solubility of the gas of the confined air present in the pleura, viz., oxygen, nitrogen, and carburetted or sulphuretted hydrogen (the latter as the result of decomposition), and believes that the immensely superior solubility of the last-named gas (viz. 300 cubic inches in 100 cubic inches of water) over that of nitrogen (viz. 2 cubic inches) in the fluids around it, may throw some light upon the obscure subject of blood poisoning, where the conditions favorable for its development are present. Into the great question of the germ theory now exciting the attention of the pathological and physiological public, and illustrated by the supposed innocuous effects of air which has been already respired when present in the pleura, the professor does not enter, having in these rapidly progressing days been a little left behind since the delivery of his lectures.

But the bearing of the fact of the absence or paucity of oxygen in air which has passed through the lung, upon the development of decomposition and putrefaction of the effused fluids is evident, and may be taken by the opponents of the theory of Professors Lister and Tyndall as an explanation of the innocuousness (if that quality be real) of air in the pleura which has passed through the lungs.

The author considers that *concussion* and *contusion* may occur in the lungs without puncture, external wound, or fracture; and adduces the not unfrequent result of pugilistic encounters in proof of it, as well as two cases in his own practice. In one, a lad of twelve years, the entire left lung was the seat of suspended respiration, caused, as he believes, by an impression upon the vaso-motor nerves. An extremely excited action of the heart was considered to show obstruction to the pulmonary circulation. The boy recovered from the symptoms in forty-eight hours. This condition may be observed also sometimes coexistent in the opposite lung, which is explained by the author as either from an increased circulation through it causing congestion, from sympathy, or from comminuted concussion.

In *cut throat*, the lecturer remarks upon the frequency with which the large vascular trunks of the neck escape injury, attributing it justly, in great measure, to the prominence of the larynx, and the sterno-mastoid muscles, contracted under the excitement of the suicidal act, and to the popular idea that cutting the windpipe is the fatal thing to be accomplished. A good deal must also be attributed to a cause not mentioned by the author, viz., the readiness with which the great vessels of the neck slip aside under pressure, from the looseness of their areolar sheath, and also to the toughness and elasticity of the vascular walls, which renders it a difficult matter to cut them except with a very sharp instrument in a very determined hand. It has been several times observed by the writer that the internal jugular vein has been laid open, while the carotid artery lying to the more exposed side of it has escaped injury by slipping from under the knife. When the wound is low in the neck the author remarks that chest complications are more apt to arise, and gives a case in point where death ensued from pleurisy caused by the infiltration of pus in the contiguous areolar tissue after a throat cut about the cricoid cartilage (p. 225). In eighteen cases of cut throat in St. Thomas's Hospital fifteen were male and three only female, eleven recovered and seven died. In five the wound was superficial, one only of these died from the casual complication of pericarditis. In six the wound was above the hyoid bone, four of which were fatal, one from shock and delirium, two from bleeding (from the left lingual artery in one of these), and the other from the admission of air into the open mouth of a partially divided vein. In five cases the thyro-hyoid membrane was opened and all these recovered.

The professor is sceptical as to the benefit of *tracheotomy* in cases of scalding of the mouth and throat, judging from the result of his own cases, and details one case of this kind in which the parents of the boy refused the offered aid of this operation to save him from impending suffocation, the favorable result of palliative treatment justifying that refusal. Another case in which it might have been justified, also recovered without it. But it may be urged that *laryngotomy* was the operation which might have been preferred by many in such a case, and that it, or the high operation of tracheotomy or cricotomy (when performed early enough), is not subject to many of those consequences which render tracheotomy so often fatal.

The danger to which the author attributes most importance is that of broncho-pneumonia, ensuing, he thinks, from shock to the vaso-motor nerves or pneumogastric filaments of the lungs.

The characteristic caution of the author is again exemplified in the remark (p. 235) that "it is a dangerous practice to permit patients to inhale chloroform in operations involving the interior of the mouth, although the risk is diminished if the sitting posture can be maintained." The danger of intrusion of blood into the larynx even in small quantities is the reason adduced for this gloomy warning against a practice habitually carried out even in severe operations at most of the London hospitals, and by dentists, both with this and a similar agent, the nitrous oxide, without the result so dreaded by the author, except in one or two isolated cases.

A more merited caution is given (p. 240) as to the danger incurred in wearing the ordinary tracheal tube after the operation of tracheotomy. Two instances had come to the knowledge of the lecturer "where fatal hæmorrhage in children was consequent on ulceration extending through the trachea into the innominate artery." We have also met with one case of this kind, caused by the point of a tube too sharply curved, and we join with the professor in recommending variations or some alteration in the shape of this instrument. As found in the shops, it is almost always too short above the curve, too abrupt in the curve, and too long below the curve.

At page 246 the author gives the outlines of a curious case of death resulting, in a boy of seven, from a needle having passed through the fifth rib cartilage into the heart. About an inch had broken off, and was found lying in the pericardium, having caused hæmorrhage into it from the apex of the heart to the amount of six or seven ounces.

Another case of supposed wound of the heart by a penknife which recovered is also given, from the practice of a country surgeon.

In Lecture IX (p. 250) are the details of a remarkable but *not* unique case of wound of the heart, of which the preparation shown at the lecture is illustrated by a woodcut. It occurred in the practice of Mr. Fuge of Plymouth. The patient was a soldier wounded by a ball in the chest at Corunna, who lived fourteen days after an opening one inch long into the cavity of the right ventricle, near the origin of the pulmonary artery. The tricuspid valve was lacerated, and the ball was found in the pericardium. A case of wound of the right ventricle by a lance at the battle of Waterloo is recorded by Mr. Guthrie. The man lived four months after the injury, and died from pneumonia (quoted in Williamson's 'Military Surgery,' p. 99). In the same volume is also given an instance of rupture of the *septum cordis* by impact of a cannon shot. The man lived for four days afterwards.

In Lecture X, p. 258, the author records a case of *ruptured diaphragm* with protrusion of the bowels into the left pleural cavity, illustrated by a woodcut of rather an artificial and diagrammatic character, and showing very little of the relative anatomy of the parts. A railway porter died in the hospital from fever with pulmonary complication, in whom nothing more peculiar than dulness on percussion was observed during life in the left dorsal and lateral regions, with bronchial respiration. There was the history of a squeeze between the buffers of two railway carriages, two and a half years before. The transverse and descending colon, an enlarged and softened spleen in the stomach, and a large proportion of the small intestines, had escaped through an aperture in the left portion of the central tendon, while the left lung was collapsed and bound down by adhesions.

In cases of severe abdominal contusion with shock the author considers that the consequent tympanitis and constipation is referable to injury inflicted on the ganglionic nerves, in correspondence with the opinion which he advocates respecting paralysis of the lung after injuries of the chest.

We suppose, however, that in most of these cases there is sufficient *local peritonitis* to account for these symptoms without the assumption contained in such a theory.

In connection with the subject of tympanitis the author (at p. 287) remarks, "The rapid generation and speedy subsidence of tympanitis under some circumstances are remarkable facts, and are suggestive of the gas being derived from the circulation and accumulating in the peritoneum, containing probably in solution a considerable quantity of watery vapour produced at a heightened temperature; and the fact that a rigid contraction of the abdominal muscles sometimes noticed in these cases controls or prevents tympanitis, would seem to favour this conjecture. I have certainly observed the subsidence of tympanitis without, so far as the patient's testimony could be relied on, the escape of gas from the bowel." We fail to see herein any real proof of the somewhat startling assertion that the gas is contained in the peritoneal sac, both the rapid generation and speedy subsidence, and the preventing or controlling power of the abdominal muscles are circumstances which would equally affect the presence of gas within the bowel, and the author himself implies a very just and significant doubt as to the reliance to be placed upon the patient's evidence as to the escape of flatus in the ordinary manner, which might occur unconsciously and during sleep.

In contrasting two of his cases of operation upon ovarian cysts, one speedily fatal after opening of the peritoneum only and placing in the opening a piece of lint to promote adhesion of the serous surfaces previously to opening the cyst; and the other, recovering well after a prolonged operation for entire removal of the cysts with much difficulty from extensive adhesions; the author concludes that "those operations which involve the protracted exposure of the peritoneum are successful in proportion to the changed condition of the serous membrane—its permanent degradation, below its healthy standard," illustrating it by the action of nitrate of silver in stopping the progress of erythema on the skin by setting up a previous change in the tissues.

He considers that both the liver and kidney may frequently be ruptured to some extent without fatal results, as proved by puckerings and linear cicatrices often found on them, especially on the liver, in *post-mortems*, and further, he believes that organic injury of the solid abdominal viscera is by no means so fatal as that of the membranous. By the latter somewhat old fashioned and unscientific term the lecturer, we presume, means the stomach and intestines, injuries of which give rise to the fatal consequences of extravasation of their contents, which are of course more irritating to the peritoneum than the blood which may be effused in injuries of the solid viscera. But when these latter are so much injured that the secretions of bile or urine pass in any quantity into the peritoneum, we presume that the consequences are as fatal as in extravasation from the tubular viscera.

The last lecture contains remarks extending over so wide and important a range of subjects that we cannot fairly find fault with their somewhat disconnected and compressed character.

When we mention internal strangulation; hernial and other obstructions, and the operations for the same; foreign bodies in the stomach and intestines; fractures of the pelvis, and their complications; rupture of the bladder and urethra; lesions of the rectum; and shock in pelvic injuries and in certain morbid conditions of the uterus, with recapitulation and conclusion,—we have said enough to show that necessity for superficial and fragmentary treatment, which in great measure disarms criticism.

The author's conclusion that abstinence from interference by operation as a *general* rule in *internal obstruction of the bowel* is, perhaps, judicious; but we would by no means go so far with him as to make the rule universal; and moreover, we think that some weight is due to an argument for operation which the author, in common with many other surgical writers, does not consider, viz., that even if not successful in saving life, an operation under chloroform usually relieves the patient from much suffering and distress, and promotes that "*euthanasia*" which the ancient physicians considered to be an essential part of their art.

In relation to *hernia* the author alludes to the very high average *death rate* from this cause as derived from the Registrar General's report. This very common disease, quite remedial as it is if detected early and properly treated, causes upwards of 800 deaths annually—a striking fact, and one which reveals a very widespread ignorance or carelessness on the part of the public or their professional advisers.

The lecturer inculcates, in *strangulated hernia*, early operative interference, and advocates the opening of the sac as a rule, believing the supposed dangers of exposure of its contents to be in great measure imaginary. In this point he is at issue with some of the most eminent of his colleagues.

In respect to *rupture of the bladder* we find the author expressing an opinion upon the effect of urine upon the peritoneum, which makes us reconsider a little our previously expressed criticism as to the extreme prudence of his notions generally. At page 341 he states himself to be committed to the opinion (amongst others more commonly entertained by surgeons) "that rupture of the bladder, with extravasation of urine into the peritoneum, is not usually accompanied by collapse so profound as that which marks rupture of the stomach or intestine, and the escape of their contents: that in some instances the presence of urine seems to be tolerated almost passively by the serous membrane; and that, as a rule, it is not resented so actively as is the presence of fæculent matter from the bowel," and "that it seems not improbable that urine may be *absorbed* by the peritoneum."

We should have wished that this very important and interesting question had been more fully discussed and illustrated by cases than the lecturer was able to do. As it stands, it is fairly liable to the verdict of "not proven." Indeed, almost the only proof offered in support of the opinion is found in the answer to the question put by the author of what becomes of extravasated urine which finds its way into the peritoneal cavity? and the answer is, *probably* it becomes absorbed.

For the maintenance of this position clear evidence is required —1, that a case of ruptured bladder with effusion of urine into the peritoneum is capable of recovery; 2, that urea and other constituents of the urine should be found in the peritoneal effusion (which amounted, in one case adduced by the author, to four pints of straw-coloured fluid); 3, that there is clearly no *suppression of urine* in the kidneys in those cases in which no urine has been passed or can be found in the bladder or peritoneum, in consequence of the shock produced by injury. No evidence of this kind is offered by the author, and therefore, though disposed from our own experience to share to a certain extent in his opinions, we are bound to say that the proof is not on the record.

In the extended field of observation included in the lectures of the Hunterian professor, there are many more interesting points for discussion than we have been able to allude to, and many pregnant observations crop out in his somewhat fragmentary and discursive method of cultivating it. The style is in many places involved, and the manner of expression sufficiently professional, not to say pedantic, in its affectation of precision in the use of technicalities.

We are the more surprised, therefore, when we find (at p. 100) the *Aqueductus Fallopii* in the periotic bone called the Fallopian tube! and (at p. 130) the contents of a brain abscess described as dark-coloured unhealthy *pus*, when just afterwards it is stated that these abscesses "are found to consist of nerve matter in a state of degeneration and decomposition; pus-globules and *other reproductive or reparative* elements are almost, or entirely absent."

III.—Gardiner Hill on Non-restraint.[1]

On the covers of the book before us are printed in large gilt letters the words 'Dr. Gardiner Hill on Lunacy.' The title of the book is fallacious, and Mr. Gardiner Hill is *not* a Doctor of Medicine of any British University, as the assumption of the title of Doctor might lead the public to surmise.

The book is not a treatise on lunacy, but a thrice-told tale of the writer's connection with the early growth of what has been termed the "non-restraint system" in the treatment of the insane. This question has been fully investigated again and again, and the reopening of it in the fulsome and empirical style which characterises this volume can serve no other purpose than an advertisement. The egotism and self-laudation which characterise almost every page bring to our mind a statement made and published by Dr. Dickson, now the proprietor of a licensed house at Buxton, to the effect that "In this enlightened age there are nearly 3000 persons confined in upwards of 150 establishments called private licensed houses, the *proprietors of which have as great an objection to an empty house as a publican could have to a similar predicament*."[2]

We do not profess to know the antipathies of "publicans," or the "objections to an empty house" entertained by the proprietors of "establishments called private licensed houses;" but certainly no more seductive phrases could be employed in eulogy of his own hostel by the enterprising host than are to be found in these pages; nay, more, we regret to observe that the writer even so far loses sight of ordinary propriety as to travesty the phrases of Holy Scripture in describing the progress of non-restraint. Thus our attention is invited "to that brighter era when the day-star from on high, as it were, visited us;" and we are told that, "that era may be unquestionably assigned to that period when we proclaimed, as it were, with one voice the abolition of mechanical restraint." Again, "All were alike smitten by one demon or another until the old Adam was thrown off, and the reign of liberty for all sorts and conditions of the insane installed in its place" (p. 59). We are reminded of "the balm of Gilead," and "of the lost sheep," and are treated with the usual stock phrases of those who bid for popular applause.

It is a sorrowful duty to have to relate this of one who was once honorably associated in a great work with so distinguished

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[1] *Lunacy: its Past and its Present.* By Robert Gardiner Hill, F.S.A., &c. Pp. 85, and Appendix. 1870.

[2] 'On Public Asylums for the Insane of the Middle and Higher Classes,' p. 25. Churchill.

and noble-minded a physician as Dr. Charlesworth; but fealty to truth, and to the dignity of the medical profession, has compelled us to do so.

The following self-laudatory paragraph is selected at random from the pages of the volume under review. A case of recovery is recorded. It is one of those romantic cases so often cured by the marvel-mongers of the age—that of "a country clergyman," who with the "poor governess" appears to be the special subject of their sympathy and success. But let the author speak:

"In *eight months* afterwards a more tranquil frame of mind supervened; he became more sociable, and was prevailed on to write some poetical effusions, which possessed sufficient merit to be printed; having the pen of a ready writer, his versification was considerable, the upshot of it all being to restore him to the performance of his clerical duties, that had thus been grievously interfered with. This happy termination has never been interrupted; he is now (1870) fulfilling all the duties of a country clergyman in the most effective manner, esteemed and appreciated by all who know him. *After this may I not say 'Nil desperandum.'*"—P. 74.

The quotation is accurate, but the italics are our own, as we desired to emphasise those portions of which the author appeared to be most proud. And so in the following quotations, which we subjoin illustrative of overweening vanity and self-conceit:

"Whatever may be the advantages of these remedies, they are comparatively insignificant by the side of a new compound which *I have recently most successfully employed* under the designation of the hydrate of chloral. . . . . . If of *other remedies I have to speak, be it known that that splendid medicine from America designated under the name of podophyllin* is capable of producing most energetic action on the functions of the liver, and as a cholagogue its virtues are not to be surpassed. This especially applies to cases of melancholia. . . . . . *Nor would I disguise* from my readers the fact that atropine can be successfully brought into action in cases of mania arising from uterine irritation, more particularly those of an intermittent character. *As to epilepsy*, that *opprobrium medicorum*, *I do not hesitate to affirm* that most material assistance may be derived from the use of ozonic ether. . . . . *I would also enumerate, amongst other remedies*, the advantages that may be derived from the salts of iron in combination with strychnine. A word to the wise: those only half know their lesson who cannot appreciate the benefits of the last-named alkaloid."—Pp. 80, 81.

"By these various agencies, and with proper moral control, *I have had no difficulty* in encountering *all cases af acute disease. I have had patients who have been under treatment at other asylums, even for long periods,* and with them the treatment, continuous and prolonged though it be, has generally been successful."—P. 81.

The puerile vanity which impelled the writer to assume a distinction to which he has shown no legal claim has prompted him to ignore the labours of those who have preceded him in the great work of ameliorating the condition of the insane. Not content with the high distinction of placing the topmost stone on the edifice which had been built up by the labours of Pinel, Tuke, Ellis, and Charlesworth, he wishes to arrogate to himself the honour of the entire structure. He thus ends his preface:

"Divide et impera" may be a safe political maxim, but it is no rule in his code; he would rather exclaim on such a vital and historical point, "Aut Cæsar, aut nullus."

And in harmony with this maxim he does not hesitate to write thus of the illustrious dead, to whom he was under deep obligations:

"As to Dr. Charlesworth, I think it necessary to state that he *never* made any experiment on any patient for the abolition of mechanical restraint. He *never* suggested the possibility of doing without it; he *never*, more than any other governor or physician, seconded the slightest approach to any experiment of mine; he never eulogised me, except if saying what is simply true be eulogy."—P. 49.

The italics are the author's. We will make no remark on the style, but will appeal to the earlier and purer writings of the author, for another description of the labours and merits of Dr. Charlesworth, and of the aids which he brought to bear in furtherance of the non-restraint system. In 1839 Mr. Hill published the lecture on which his true fame rests; and in the preface thereto he writes, not in Cambyses' vein as now, but in a more sedate style, as follows:

"The principle of the mitigation of restraint *to the utmost extent* that was deemed consistent with safety was *ever the principle* pressed upon the attention of the boards of the Lincoln Asylum by its able and humane physician, Dr. Charlesworth. *At his suggestion* many of the more cruel instruments of restraint were *long since* destroyed, *very many valuable* improvements and facilities gradually adopted, *and machinery set in motion which has led to the unhoped-for result of actual abolition, under a firm determination to work out the system* to its utmost applicable limits. *To his steady support*, under many difficulties, *I owe chiefly the success* which has attended my pains and labours. He *originated* the *requisite alterations* and adaptations in the building, and threw every other facility in the way of accomplishing the object."

The sentences in italics have been so marked by ourselves, to afford a contrast with those which the author has emphasised in the work under review, published some thirty years afterwards. The contrast is, indeed, a painful one; and it is all the more deplorable after full and complete justice had been done to the writer by Charlesworth, by Conolly, and by nearly all who have written upon the treatment of the insane. We ourselves, thirty years ago, reviewed his labours with praise, and declared that "Of all the works that have appeared on the subject of lunatic houses since the publication of Mr. Tuke's account of the Retreat there is none which contains matter more deserving of attention than that recently published by Mr. Hill."[1] Fealty to truth then, as now, however, compelled us to state his "lecture was little more than a simple commentary on resolutions of the board of management of the Lincoln Asylum for twenty years past, during which period, under the superintendence of Dr. Charlesworth, and latterly with the vigilant co-operation of Mr. Hill himself as house surgeon, almost every kind of bodily restraint is stated to have gradually fallen into disuse as superfluous, or worse than superfluous—a mere substitute for want of watchful care." This contains the true history of the beneficent work. The total abolition of restraint during the house-surgeonship of the author was a mere accident of time. It emanated from no original idea of his, but was the necessary and culminating result of years of previous labour and forethought of others. It was the outcome of the axioms laid down by the board under the guidance of Dr. Charlesworth some years before Mr. Hill entered on his duties at Lincoln, and expressed as follows in the reports of the Lincoln Asylum:

"In 1829—'The governors have particularly directed their views to the subject of coercion and *restraints, well aware of their injurious consequences* to the patients.'"

"In 1831—'The *fair measure of a superintendent's ability* in the treatment of such patients *will be found in the small number of restraints which are imposed.*'"—*Seventh Annual Report*, p. 2.

"In 1833—'*It is unceasingly an object in this institution*, and should form a prominent point in the 'Annual Reports,' *to dispense with* or improve as much as possible the instruments of restraint.'"—*Ninth Annual Report*, p. 4.

The greatest possible encouragement was always given to the house surgeons who restrained the fewest patients, and these 'Reports' of 1829, 1831, and 1832 show that total abolition was the wish of the Governors. "*To dispense with* the instruments of restraint" was their earnest desire, but failing this, "to improve as much as possible." Mr. Marston and Mr. Hadwen, predecessors of the author in the house-surgeonship of the Lincoln Asylum, delighted the board when they could report that no patient was under restraint, and it is not sur-

[1] 'Brit. and For. Med.-Chir. Rev.,' vol. x, 1840.

prising, therefore, that a young and energetic surgeon, on succeeding these gentlemen, should aim to outvie them, and to realise in practice what had been so anxiously yearned for and so clearly indicated by the governors of the hospital. It was an honorable ambition on the part of the author, and we have no desire to detract from his merits; but we cannot allow him the supreme fame which is due to the discoverer of any great law or fact in science or arts. The result which our author announced was, as already stated, simply the evolution of principles and practices which had been carried on for years, and which result was expedited, not created or discovered, by his intelligence and zeal. Neither was it evoked by him from the suggestions and facts recorded by others, in the same manner as the sun's attractive force was demonstrated by Newton, after it had been surmised by others, or foreshadowed in their researches, or as the circulation of the blood was demonstrated by Harvey, after the facts and speculations of previous physiologists. It was not, like the above discoveries, a problem solved by the inductive power of one mind, from the facts and intimations of others superadded to its own, but was simply the practical issue of a daily special work, carried on under prescribed regulations. Given the principles and instructions in force at the Lincoln Lunatic Asylum, as embodied in the Annual Reports of the Directors, and it required only courage, zeal, perseverance, and honesty on the part of the house surgeon ultimately to arrive at the conclusion, and "to express his belief, *founded on experience in the house*, that it may be possible to conduct an institution for the insane without having recourse to the employment of any instrument of restraint whatever" ('Thirteenth Annual Report,' 1837). The problem has, as yet, reached no further than that it is the "belief of many practical men that lunatic asylums may be so conducted." Dr. Charlesworth expressed the belief, and most enthusiastically advocated the practice, as he had ever regarded manacles as a most pernicious evil in the treatment of the insane. It is to the discredit of the author that, seventeen years after the death of that great and good man, he should, by implication, convey the idea that Charlesworth loved the old system, and delighted in the invention of mechanical restraints. To no other end could *he have printed in capitals* every word of an entry, made August 25th, 1833, to the effect that in the case of a most violent patient "the boot hobbles, invented by Dr. Charlesworth for confining the feet to the bedstead, were applied this evening" (p. 26), without adding, that these "boot hobbles" were invented to *displace* more formidable and painful manacles.

To revert to the non-restraint theory, the problem has, as yet, extended no further than the utterance of the above "belief," because no person can possibly prove that a case may not arise in which some kind of mechanical control may not be beneficial. It is, after all, a speculative statement rather than a demonstrative fact "that restraint is never necessary, never justifiable, and always injurious in all cases of lunacy whatever." Even our author, after having published this dogma—after having adduced the Lincoln Lunatic Asylum as proof of its practicability—*after* having proclaimed—

"*There*, such a system is in actual and successful operation—*the theory verified by the practice.*"—*Preface to Lecture*, 1839—

had to resort to strong mechanical restraints in this very hospital, which was singled out as the place in which "the theory" was verified. His journal records "*I directed* Corston to place C. A— under restraint." "The moment I attempted to secure the wrists a struggle commenced, the wrist-locks being seized by the patient. . . . At length she was thrown down and overpowered." In the 'Medical Circular,' November 23rd, 1853, the author published a long statement explanatory of this "restraint," which lasted for eighteen hours, ascribing it to a disorganised state of the staff of the attendants; but how can this be always guarded against? May not a "disorganised state" of the attendants spring out of the fury and violence of the special patient to whom they are called? The good Dr. Conolly, profiting, possibly, by the above remarkable experience, was too wise a physician to speculate on the future, or to state dogmatically that restraint could never, under any circumstances whatever, be justifiable. Indeed, in more than one public establishment of repute in England the superintendent abstains wholly from the use of any mechanical appliance, not because in some very rare and isolated case it could not be useful, but because he feels that the infringement of the beneficent rule of "non-restraint" in any case would form a plea for its repetition in cases not so special, urgent, or exceptionable, and thus lead to greater practical evils to the insane as a class than the good to be effected in a most rare and exceptional case could compensate for. Let us not be misunderstood. We regard the practice of the "non-restraint" system, as published by Mr. Hill in 1839, and subsequently so eloquently enforced for a long series of years in the practice and writings of Dr. Conolly, as one of the greatest achievements of modern times, fraught with untold blessings to thousands, and rendering possible the scientific treatment by physicians of all forms of mental derangement.

"Suum cuique" has been, so to write, our motto in penning these observations. We have no wish to cloud the fact so honestly reported in the 'Thirteenth Annual Report of the Lincoln Lunatic Asylum,' 1837, that Mr. Hill thus early "expressed his own belief, founded on experience in this house, that it may be possible to conduct an institution for the insane without having recourse to the employment of any instrument of restraint whatsoever;" but we deplore his present unfair reticence of the labours and opinions of others, and in the interest of the medical profession we condemn the empirical tone and style of this his latest production. If the author does not mar his own reputation, he may rest assured that his name will be ever associated with the great benefactors of mankind; but the medical reputations are few which can afford to offend alike the taste and the dignity of the profession by such bathos and egotism as the following:

"My case is parallel with that of the captain of a ship of war who suddenly finds himself in front of a hostile fleet, securely anchored, it may be, in the Bay of Restraint. My mission, according to my views, was not to flinch, but to break the centre of that force. And so it was, that though I was afterwards joined by many consorts bearing friendly colours, my flag, hoisted on board the good ship 'City of Lincoln,' was the first to proclaim the success of the achievement. Another seeks to deprive me of the honour of the victory, either by claiming it for himself or sharing it with me. I can assent to neither course. I was the responsible captain in command, and lived on board. The sleeping partner, Dr. Charlesworth, an ornament in his way, lived ashore, and could only give a sanction to my proceedings. Whilst we applaud and reward the efforts of those who successfully pierce Alpine passes, who gauge the depths of the ocean, and connect distant continents by the hidden mysteries of electricity, be it borne in mind that other agencies were at work—that work which was brought to its triumphant issue when the last link of the chain was removed from the body of the downtrodden and neglected lunatic" (p. 85).

IV.—Physiology of Respiration.[1]

The book which forms the subject of the following article is one of those which may well make us envious of the advantages enjoyed by the cultivators of physiology in France and Germany.

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[1] *Leçons sur la Physiologie Comparée de la Respiration professées au Muséum d'Histoire Naturelle.* Par Paul Bert.

Lectures on the Comparative Physiology of Respiration delivered at the Museum of Natural History. By Paul Bert.

It is not desirable, however, to bewail ourselves whenever the occasion presents itself. We hope the time is not far off when this country will again take the place in physiology it has held and ought now to hold, as in other branches of natural science; meanwhile we may accept thankfully the contributions to our knowledge which come to us in the teeming scientific literature of the Continent.

The name of the author has not hitherto been so prominent in the scientific world that we may assume our readers to require no sort of personal introduction. He may be allowed, therefore, to say a few words for himself:

"Disciple of two masters, Gratiolet and Cl. Bernard, I have had [he says] the happy fortune to be habituated by them to consider successively natural phenomena from two different points of view—one speculative, the other experimental. The first of these masters has shown me in action, in unequal degrees of perfection, but always with absolute sufficiency, the different types of the animal kingdom; with the second I began to learn what instructive derangements in the complex expression of a living harmony can be brought about by man, disposing of certain conditions internal or external to a given animal, and varying them at pleasure; both teaching that under infinite variations of form the problem of life is one and the same; but one seeking the solution in the analysis of specific diversities, experiments ready made by nature; the other in the investigation of the consequences in the functional equilibrium of a single animal, which result from an experiment made in the laboratory; one tracing the facts which he ascertains to conditions which he cannot rule, and which he seeks simply to know; the other introducing at will different conditions, in order to determine their influence and follow their effects. In a word, the one naturalist, the other experimentalist—physiologists both."

The book yields ample evidence of this double training. It does not consist, as might have been supposed from the title, mainly of descriptions of the respiratory apparatus in the various classes of animals, and of detailed accounts of the different ways in which the great function of respiration is carried on; the great purpose of the author has been to follow out to the utmost the train of changes which begins and ends in the interchange of gases in the lungs, to complete and perfect the general theory of respiration by means of experiments on different animals. The lectures are thirty-one in number, aud the principal subjects discussed are the respiration of the tissues, the gases of the blood, the respiratory mechanisms of the vertebrate class, the relations of certain nerves, especially the vagus, with the respiratory movements, asphyxia in a confined atmosphere, and death by drowning. On all these points some original information is contributed, and a philosophic largeness of view pervades the entire book. The experiments are vividly described; there are instructive digressions into matters on which the author has previously worked, and now and then most interesting displays of his individuality. We make no apology for taking up this work alone, without collating with it other recent works on the same subject, or for departing somewhat from the order in which the different points are considered.

We may first glance at the history of the physiology of respiration, and trace the gradual establishment of the theory of this process, now universally accepted. Among the hypotheses which have successively prevailed some have been mechanical. Respiration was for the purpose of dilating the lungs, so as to permit of the circulation through them of the blood, or it was to redden the blood by friction, in the same way as it is reddened by agitation with air. With others, again, the object of respiration was physical—to cool down the blood overheated in the heart. Other opinions were of a less tangible and some of an extravagant character, and need not be enumerated. The first hypothesis which could be called chemical was propounded by J. Mayow, Fellow of All Souls', Oxford, who, dying in 1679, at the age of thirty-three, had already demonstrated that there was in air some principle "spiritus nitro-æreus," or igno-æreus, which supported equally life and combustion, which combined with metals in the process of rusting or calcination, adding to their weight, and which was given up to blood exposed to air out of the body, as well as during its passage though the lungs. He even compared the placenta to the lung in its influence on the blood of the fœtus. It was nearly 100 years after this that Lavoisier arrived at anything like a complete and consistent theory of respiration, asserting that the oxygen (*air pur, air éminemment respirable*) only of air was concerned in respiration, the nitrogen playing merely a passive part, that the death of animals in a confined atmosphere occurs when they have converted the greater part of the oxygen into carbonic acid (*acide crayeux*), and that to restore air spoiled by respiration, carbonic acid must be removed and oxygen added. He identified respiration with combustion, and the theory as it left his hands survives at the present day, developed, it is true, but not essentially modified. The three regulators of the human machine, according to Lavoisier, were—1. Respiration effecting in the lungs or system the combustion of carbon and hydrogen. 2. Transpiration, keeping down excessive temperature. 3. Digestion, supplying to the blood, water, carbon, and hydrogen, lost or consumed by transpiration and respiration. It is worthy of note that Lavoisier discussed the question whether the carbonic acid was formed in the lungs or in the system, but without coming to a decision. He conjectured, also, that carbonic acid was formed in the alimentary canal and absorbed into the blood.

It is not necessary here to trace the gradual establishment of the fact that the seat of the combustive process is in the system, and that the carbonic acid is brought to the lungs in the venous blood ready made. It is now definitely proved that there are two orders of events in respiration. 1. The interchange of oxygen and carbonic acid in the lungs or other respiratory apparatus. 2. The consumption of oxygen and production of carbonic acid in the blood and tissues.

The first phase of the process has been the subject of repeated investigation, and is, comparatively speaking, well understood; but something remains to be ascertained as to the law under which the exchange of gases between the blood and air takes place, or rather as to the conditions modifying the operation of the law. According to the law of the diffusion of gases, the volumes exchanged are in inverse ratio to the square root of the density, which would give a constant proportion of 85 of carbonic acid given off to 100 of oxygen absorbed; whereas, on the contrary, the proportion is very variable. Vierordt's modification of the simple law which refers the rate of interchange to the relative solubility of gases under varying virtual external pressure would be more nearly applicable to the conditions which obtain in respiration; but many complications interfere with the operation of the physical law of the diffusion of gases, even when stated in the most general form. In the first place the air actually present in the air-cells of the lungs, the medium between which and the blood the interchange immediately takes place, has by no means the composition of pure atmospheric air, but contains a considerable and, no doubt, variable admixture of carbonic acid. If successive portions of an expiration are examined, it is found that the proportion of CO_2 is much greater and of O much less in the air last given out, and in an experiment in which the reserve or residual air was actually withdrawn from the air-cells by connecting the trachea of a dog by a tube with a large exhausted receiver, and establishing the communication just at the end of expiration, Bert found it to contain only 12 per cent. of O, and no less than 8 per cent. of CO_2. The gases, moreover, which change places are not in similar conditions. The oxygen is in the gaseous state, the CO_2 dissolved in the blood, and, indeed, partially held in combination, the blood again having a strong chemical affinity for O, and fixing it immediately on its passage through the respiratory membrane. Finally, this membrane is not to be considered merely as a porous septum, permeable indifferently by all gases. The interchange between the air and blood is effected by solution of the gases in the substance of the moist respiratory membrane; O and CO_2 being soluble in colloid animal matter, traverse it readily. Nitrogen having only a slight solubility in animal membrane, takes little part in the process, and the rate of diffusion of the O and CO_2 may be affected by a greater relative affinity in the membrane for one or the other.

One of the facts most accessible to experiment is the change in composition of the air which has been breathed. On this part of the subject, as it is not treated of in the work of Bert nnder review, we shall say but little. Most investigations have been directed simply to the estimation of the quantity of CO_2 and water exhaled from the lungs under various conditions, fasting and after food, at work and at rest, and at different periods of the day or night. Very rarely has the concomitant consumption of O been ascertained. It is known that the amount of O abstracted from the air is more than sufficient for the formation of the CO_2 exhaled, leaving a margin for other oxidations; but the ratio $\frac{CO_2}{O}$, which varies considerably, has not been determined under all the different conditions named, still less have the variations in the amount of O lost and CO_2 gained by air which has served in respiration been connected experimentally with the correlative variations in the proportion of these gases in arterial and venous blood. When this has been done it throws light, not merely on the interchange of gases in the lungs, but on the nutrition and disintegration or oxidation of the tissues. For example, a man at work consumes more oxygen and exhales more CO_2 than when at rest. The proportion of O to CO_2, however, is not the same. While at rest and during sleep a larger amount of O is taken up into the system in proportion to the CO_2 given off than during exertion, and it is inferred that during repose an oxidized product is formed in the organism, and especially in the muscles, which is broken up and yields CO_2 as the muscles are thrown into contraction.

The gases of the blood have been the subject of repeated and careful investigations. Mayow was the first to observe that, under the air-pump, fine bubbles of gas could be seen to issue from arterial blood; Vogel, Collard de Martigny, Enschutt, and Bischoff extracted CO_2 by exhaustion; Magnus was the first to obtain O by this method; Humphry Davy employed heat, and obtained both O and CO_2; Priestley, displacement by H,N, but especially by CO. All these methods—exhaustion, heat, displacement, singly or in combination—find their use in estimating the gases of the blood.

An ordinary air-pump will extract from blood N and CO_2, but none of its O. By means of the torricellian vacuum all the N is withdrawn, all or nearly all the CO_2, and some, but not all, the O, unless the blood is raised to a temperature of 40° C. A part of the CO_2 is retained much more firmly than the rest, and most experimenters have found it necessary to add some acid, in order to extricate the last portion. Pflüger, however, has succeeded in extracting the whole of the CO_2 from arterial blood by unaided exhaustion by the barometric pump. The amount of gases yielded by blood varies greatly. Ludwig has given a table of ten analyses of the gases of arterial and venous blood, the average result of which is as follows for 100 cubic centimètres:

	Oxygen.	CO_2.	N.	Total.
Arterial	15·03 c.c.	29·15 c.c.	1·6 c.c.	45·76 c.c.
Venous	8·17 c.c.	33·65 c.c.	1·37 c.c.	43·12 c.c.

Oxygen, as is to be expected, predominating in arterial blood, CO_2 in venous blood; the excess of CO_2 4·5 c. c. in the latter, however, not corresponding with the deficiency of O 6·86 c. c. We shall return to this subject and to the consideration of the variations in the amount of O and CO_2, and in their relative proportion, after following the reasoning and the more important experiments by which it has been ascertained how far these gases are held in solution merely in the blood, or are in combination with some of its constituents.

First as to the CO_2. It is at once suggested by the fact that a part of this gas is extracted by exhaustion with comparative facility, while the remainder is held with great tenacity that it exists in the blood partly in solution and partly combined. This was demonstrated by Fernet in the following way:—All the gases of the blood were expelled by a current of hydrogen; the blood was then exposed in a suitable apparatus to carbonic acid at different pressures till saturated, and the amount taken up ascertained. If the CO_2 were simply dissolved, the proportion absorbed ought, according to the law of Dalton, to vary with the pressure, and there should be a constant ratio between the increase of pressure and the increase of volume taken up. But this is not the case. Comparing subsequent increase of absorption on increase of pressure with the volume first taken up, it is always less than it should be, if the gas first absorbed had been simply dissolved. A part, then, has entered into combination. By a further application of the same method, the proportion between the CO_2 in combination and that in solution is ascertained. While the ratio of absorption to pressure is less for increased pressure than on the initial exposure, it is constant for subsequent augmentations of pressure, and thus gives a coefficient of solubility of the gas in blood, which affords a basis for calculation, the details of which need not be gone into here. In this way it has been ascertained that of the carbonic acid in the blood three fifths exist in a state of solution, two fifths combined with some of its constituents. But the investigation has been carried much further than this, and it has been shown that the particular constituent with which the CO_2 is chiefly combined is the carbonate and phosphate of soda of the serum. By comparing serum with blood, it was seen that while the CO_2 in solution is diffused equally through the blood, that combined is almost exclusively held by the serum, and comparing again the behaviour of serum with that of a solution of its salts, it was evident that to the carbonate and phosphate of soda the serum owed its property of appropriating the CO_2.

Oxygen is held by the blood much more powerfully than carbonic acid, and cannot be entirely extracted by exhaustion without the aid of heat or expelled by N,H or CO_2. CO, however, entirely displaces it. The simple fact that more oxygen is absorbed by blood when warm than cold, proves that it is not merely dissolved, and by a line of investigation such as that already described it was shown by Fernet to exist in the blood mainly in combination with the corpuscles. More recently it has been demonstrated that hæmoglobin or hæmato-crystallin is the constituent of the corpuscles with which the O combines, forming with it a definite chemical compound, the so-called oxyhæmoglobin, which can be isolated, and which gives the characteristic spectroscopic bands of arterial blood.

The oxygen, then, of the blood is in combination with the hæmoglobin of the corpuscles (partly in the state of ozone); the carbonic acid is contained almost entirely in the serum, partly in solution, but chiefly in combination with carbonate and phosphate of soda; the small proportion of nitrogen is simply dissolved in the liquor sanguinis.

A question here arises with respect to the carbonic acid. How is it that this gas, not being free, but for the most part in chemical combination, is given off from the blood in its passage through the lungs? Is it not necessary that some acid should be present which may displace it from its union with the alkaline salts, and so facilitate its escape? It might be said that a sufficient proportion of the CO_2 is simply in solution, and that it is not necessary to suppose that the combined CO_2 is set free; this, however, would land us in other difficulties, and, moreover, Claude Bernard has shown that when a solution of bicarbonate of soda is injected into the jugular vein of a rabbit, its carbonic acid is liberated in the pulmonary circulation. Mitscherlich, Tiedemann, and Gmelin supposed that lactic or acetic acid was generated in the blood on its exposure to oxygen in the lungs, and that this displaced the carbonic acid. Robin states that in the lung-tissue there is present an acid,—pneumic acid, which decomposes the carbonates, but Robin's discovery remains unconfirmed, and no acid has been found in the blood,—which is alkaline in the lungs, as elsewhere. More recent investigations by Schöffer, Preyer, and Pflüger, prove that the blood-corpuscles, or rather their hæmoglobin especially in the oxygenated state, facilitates the extrication of carbonic acid in a remarkable degree, and is capable of taking the place of an acid in respect to this action, thus rendering unnecessary any other acid, whether generated in the blood or formed by the lung substance.

We have thus examined the conditions relating to the first phase of respiration—the interchange between the air and blood; there remains to be considered the second phase—the consumption of O and production of CO_2 in the blood and tissues. In this the starting-point is again the proportion of O and CO_2 in arterial and venous blood respectively, looked at, however, from a different point of view, viz. how much CO_2 has been produced, and how much O lost during the passage of the blood through the systemic capillaries. But, first, where is the oxygen of the arterial blood used up and the carbonic acid of venous blood formed? Unquestionably, where the circulation is delayed in the capillaries; at one side of the capillary network the blood is arterial, at the other venous, the oxygen being consumed partly by the blood itself, but mainly by the tissue. Whatever, then, detains the blood in the capillaries, or intensifies the chemical changes in the tissues, increases the difference between arterial and venous blood. Thus muscle in action uses up more O than muscle in repose. In an experiment of Claude Bernard the arterial blood going to a muscle gave 7·31 c.c. O, 0·81 c.c. CO_2; venous blood returning from it while in repose, 5·0 c.c. O, 2·5 c.c. CO_2; during contraction, 4·28 c.c. O, and 4·20 c.c. CO_2; when paralysed by section of its nerve, 7·2 c.c. O, and 0·5 c.c. CO_2. The venous blood of animals during hybernation, of animals frozen to death, or after section of the spinal cord (Cl. Bernard), or of man during syncope (Hunter), is red, because in all these conditions there is muscular relaxation and diminished consumption of oxygen. The blood issuing from glands (kidney, spleen, salivary glands) in a functionally active state is red (Cl. Bernard), but for a different reason,—the rate of circulation through them is accelerated.

Ludwig's average of ten estimations of the gases of the blood has been already given, but when the individual analyses are compared the diversity between them is little less than astonishing. For instance, one specimen of arterial blood yielded 11·39 c.c. of O, and 32·78 c.c. of CO_2, another 16·95 c.c. of O, 27·47 c.c. of CO_2. One specimen of venous blood gave 4·15 c.c. of O, and 35·31 c.c. of CO_2, another 12·64 c.c. O, and 29·5 c.c. CO_2.

A difference such as is seen in these examples must have a meaning, and this is lost when the variations are merged in an average. It is here that Bert takes up the investigation. In previous examinations little or no account has been taken of external conditions, which may have great influence on the results, or of the state of the animal, such as whether he is fasting or full, and the venous blood seems to have been taken indifferently from any of the veins or from the heart.

The problems Bert proposes to himself for solution are the following, to determine the proportion of oxygen:—1. In blood taken at the same time from different parts of the same animal, *e.g.* arterial, venous of head, viscera, extremities. 2. In blood of similar animals in different conditions. 3. In blood of different animals in similar conditions.

The method he employs is that devised by Claude Bernard, namely, displacement of the oxygen by carbonic oxide, the amount of the former set free being estimated by means of an alkaline solution of pyrogallic acid. Unfortunately, this does not give the amount of CO_2, since this gas is not displaced by CO, and as the ratio $\frac{CO_2}{O}$ varies, an important element of knowledge is still wanting; but the number of comparative experiments demanded by the plan laid down renders a complete estimation of all the gases by the barometric process impracticable.

1. As to blood from different parts of the same animal, Claude Bernard had already found that 100 c.c. of

	O.	CO_2.
Arterial blood of a dog gave...............	18·93 c.c.	0·00 c.c.
Venous blood from heart gave	9·93 c.c.	2·81 c.c.
„ „ hepatic vein gave ...	2·80 c.c.	6·53 c.c.

Bert obtained from the same quantity of

	O.	CO_2.
Arterial blood of a dog	20·5 c.c.	3·2 c.c.
Venous blood from heart	14·4 c.c.	3·8 c.c.

The complete consumption of the O in the hepatic blood is remarkable. Bert says that, together with the excess of CO_2, it justifies the ideas of Lavoisier and Spallanzani as to the origin of a part of the CO_2 in the intestinal canal, whence it is absorbed into the blood. But surely, if the CO_2 had been formed in the intestine by decomposition of the food, there would be no reason for the complete disappearance of O. A more probable explanation is, that the blood emerging from the liver has been twice over subjected to the deoxygenating influence of the structures.

2. The different conditions under which the blood of similar animals has been examined are different temperatures and barometric pressures, during digestion and abstinence, in the chloroform sleep, in asphyxia, &c.

Blood absorbs more oxygen when warm than cold, a fact which alone shows that the O must combine with some constituent. On the contrary, CO_2 escapes more freely when the temperature is higher. These are both facts of importance in relation to the greater vital activity of warm-blooded as compared with cold-blooded animals.

The effects of varying barometric pressure have not been directly investigated, but, from experiments, in which the virtual external pressure of oxygen has been varied by admixture with different proportions of nitrogen, Bert considers the conclusions of Jourdanet on the effects of living at considerable elevations to be justified, and that on rising above the sea level there is, at moderate elevations, an increased escape of carbonic acid from the serum, without separation of oxygen from the hæmoglobin, a phase of decarbonization of the blood which is beneficial; at very great heights a lessened absorption of oxygen, a phase of deoxygenation which is injurious.

The different amounts of O and CO_2 contained in arterial and venous blood during abstinence and during digestion is a subject of great interest. Claude Bernard had found in the blood of the same dog, on successive days, in 100 c.c.—

	O.	CO_2
Arterial, fasting	21·06 c.c.	0·00 c.c.
„ full	18·93 c.c.	0·00 c.c.
Venous, fasting	12·66 c.c.	1·5 c.c.
„ full	9·93 c.c.	2·8 c.c.

But in the carotid blood of two dogs of the same size, on the same day—

	O.	CO_2.
Fasting	15·50 c.c.	3·7 c.c.
Full	9·2 c.c.	4·6 c.c.

In the blood of a rabbit after twenty-four hours' fasting very little difference was found. "Résultât contradictoire avec les précédents dira l'un—résultât qu'il est bon de noyer prudemment dans une moyenne dira l'autre. Pas du tout—résultât exact, et l'apparente contradiction s'explique aisément." The rabbit, in effect, was not truly in a fasting condition, its urine was alkaline and turbid, and it had still vegetable acid salts in its blood. After three days' starvation the blood gave 12·32 c.c. O and 2·6 c.c. CO_2, as compared with 15·04 c.c. O and 3·6 c.c. C_2O, after food.

The question arises—How is it that with the increased absorption of oxygen known to take place during digestion the proportion of O in *arterial* blood should be diminished? One reason is, that from the amount of new material taken up during digestion the proportion of corpuscles to liquor sanguinis is relatively smaller, and the corpuscles alone conveying oxygen a given quantity of blood will yield less of this gas. Bert concludes also that oxidation of the newly absorbed substances must go on in the arterial blood. If so, it must be very rapid to take effect by the time the blood has arrived in the carotid artery. But this conclusion is not inevitable, increased oxidation in the systemic capillaries bringing down the oxygen in the venous blood returned to the heart to a very low point, and a quickened rate of circulation of this through the lungs from the accelerated action of the heart during digestion, affording less time for the increased oxygenation required will fully account for a diminished proportion of O in arterial blood.

The chloroform sleep has been a subject of careful investigation by Bert, but his results are not given in detail in the present work. He reconciles the conflicting statements of different observers as to the colour of the blood during the administration of chloroform by saying that it is dark during the state of excitement and struggling, red during the quiet sleep, when, also, the amount of oxygen it contains is very large, 12·4 per cent., as against 7 3 per cent. previous to the chloroform inhalation. This he attributes to muscular relaxation, as in syncope and other conditions previously referred to. The idea that the action of chloroform is due to arrest of oxidation, which would explain the increase of oxygen in the blood, he dismisses as pure hypothesis ; it is, nevertheless, an hypothesis around which a vast mass of consistent and corroborative evidence arranges itself.

In asphyxia all the oxygen of the blood is exhausted, and a large excess of carbonic acid is present. In asphyxia from shutting up an animal in a confined portion of oxygen the blood is red or black, according to the volume of oxygen as compared with the size of the animal, the cause of death being accumulation of CO_2 in the blood. When a bird is on the point of death from this cause rarefaction of its atmosphere is followed by instant relief, due to the consequent escape of CO_2 from its blood.

The results of the examination of the blood of different animals under similar conditions are, that the arterial blood of carnivora is found to contain more O than that of herbivora, the blood of a fowl more than that of a duck, the blood of a newborn animal less than that of an adult of the same species. The two last facts will be found to have great interest when the question of comparative resistance to asphyxia by different animals comes to be considered.

A further investigation still remains. The rate of consumption of oxygen and of production of carbonic acid has been more or less accurately determined under various conditions; the agency of the different structures in this process has to be ascertained. This constitutes the problem of the respiration of the tissues so called. The term is convenient if it is borne in mind that it is not a mere interchange of gases which is taking place between the blood and tissues, as in the lungs, but that the process is essentially different, oxidation as contrasted with oxygenation. Bert aptly compares the action of the tissues on the blood with the way in which vibriones, as described by Pasteur, live in fluids containing no free oxygen. The anatomical elements of the structures like the vibriones decompose the oxygenated substance, abstracting the oxygen they require for the performance of their function from its combination with the hæmoglobin.

Spallanzani was the first to show that animal tissues absorb oxygen and exhale carbonic acid, not only anticipating J. Liebig, who usually has the credit of establishing this fact, but going beyond him in the completeness of the demonstration. Matteucci ascertained that during contraction muscle absorbed more O and gave off more CO_2 than in repose; Valentin that exhalation of nitrogen marked the commencement of cadaveric decomposition. Claude Bernard was the first to examine the effects of other gases than air.

Bert examines this complex problem in all its bearings. It presents three variable elements. 1. The atmosphere; 2, the tissue; 3, the animal; and he studies the phenomena presented by—

1. Different tissues of the same animal in the same atmosphere.

2. The same tissue in atmospheres of different composition, at different temperatures.

3. Corresponding tissues of different kinds of animals in the same atmosphere.

The method of examination is the same in all cases. The animal is killed by bloodletting; a given weight of the tissue to be examined is immediately cut in fragments and distributed in a thin layer on gratings, one above another, in a given volume of the atmosphere to which it is to be exposed, the time of exposure of course being the same. The amount of CO_2 exhaled, and of O absorbed, is estimated in the usual way by a solution of potash and of pyrogallic acid.

The following table gives the result of an exposure of 100 grammes of the various tissues of a carnivorous animal, a cross between a jackal and dog, exposed to air for twenty-four hours at a temperature varying in the time from 10° C. to zero:

		c.c.			c.c.	
Muscle	absorbed	50·8	of O,	exhaled	56·8	of CO_2.
Brain	„	45·8		„	42·8	„
Kidney	„	37·0		„	15·6	„
Spleen	„	27·3		„	15·4	„
Testicle	„	18·3		„	27·5	„
Bone crushed		17·2		„	8·1	„

Numerous other experiments yielded similar but not identical results. The same order was always observed, but the proportion between the O absorbed and the CO_2 given off was not constant for the same tissue, and it will be seen from the table that a tissue absorbing more O than another does not necessarily exhale more CO_2.

In another series of experiments the conditions in which the structures are placed during life are more nearly imitated; the tissues are immersed in defibrinated and oxygenated arterial blood. Blood in which muscle has been immersed for a time becomes dark in colour, and is found to have lost much of its oxygen; that in which spleen substance has been placed is less dark in colour, and has lost less oxygen, but is darker and contains a smaller proportion of oxygen than a part of the same blood retained for comparison.

In the second branch of the investigation, in which similar tissues are submitted to the action of different atmospheres, the most important results are as follows:

The richer the atmosphere in oxygen the greater the absorption of this gas, but the amount of carbonic acid given off bears no proportion to the increased absorption of O.

Carbonic acid is exhaled in an atmosphere of nitrogen or hydrogen, a fact of great interest.

The absorption of oxygen is not prevented by the presence of carbonic oxide. This gas, which by its affinity for hæmoglobin prevents the oxygenation of blood, does not impede the oxidation of tissues.

When a tissue is submitted to the action of a vacuum before exposure to air it absorbs more oxygen, but does not yield more carbonic acid.

By submitting similar tissues of different animals to identical atmospheres it has been ascertained that the flesh of warm-blooded animals absorbs more O and exhales more CO_2 than that of reptiles, fishes, and invertebrates; that in brown flesh (*e.g.* of dog or duck) oxidation is more active than in white flesh (*e.g.* of rabbit or fowl); that it is much less active in the structure of new-born animals than in those of adults.

The conclusions from the facts obtained are very important. First, the muscles must consume most of the oxygen taken into the blood in respiration. The expenditure of O in the various organs will obviously depend on two factors—(1) the affinity of the tissue for oxygen, (2) on the quantity of oxygen brought to them by the blood. Muscles have the greatest affinity for oxygen, are freely supplied with blood, and they form fully half the weight of the body. Even in repose, therefore, they must consume the greater part of the oxygen, and still more when in action.

Another important conclusion is that the nature of the tissue-change is not direct oxidation, but a splitting up of larger highly oxygenated molecules. This is evident from the great variations in the ratio $\frac{CO_2}{O}$, from the fact that CO_2 is exhaled in an atmosphere of N when no O can be absorbed; that after removal of all the gases from a portion of tissue by exhaustion CO_2 is still given off, the tissue being kept under mercury. The exhalation of CO_2 is only the last of a long chain of phenomena of which the absorption of oxygen is one of the first links.

The entire circle of respiration has thus been followed. Oxygen is taken up by the blood in the lungs in exchange for outgoing carbonic acid; it enters into combination with the hæmoglobin of the corpuscles, and is thus carried to every part of the body. The nutritional energy of the anatomical elements abstracts the oxygen from the corpuscles, and builds up with the pabulum, also supplied by the blood large oxygenated molecules, which sooner or later split up with evolution of force into simple molecules, among which are carbonic acid and water. The carbonic acid is conveyed by the liquor sanguinis, partly in solution, partly in combination with its salts the carbonate and phosphate of soda, to the lungs. When set free from its combination, probably by the influence of the corpuscles, the liberated CO_2 makes its escape.

After considering the general theory of respiration, Bert takes up the subject of the respiratory mechanism in the different classes of animals. It would be impracticable as it is unnecessary to follow him throughout, but there are many points of great interest which may be noticed. He has for the first time applied the graphic method systematically to this branch of research, displaying great perseverance and ingenuity in adapting it to the various animals.

In fishes, air-bulbs connected with a registering apparatus similar to that with which Marey has made every one familiar have been introduced into the mouth and pharynx, and under the gill covers, and the traces show what is indeed obvious, only it has been differently described, that mouth, pharynx, and gill covers, all open and close simultaneously and not alternately, the current of water being directed in at the mouth and out of the bronchial apparatus by a valve-like arragement in the mouth and at the edge of the gill covers. It is not a kind of deglutition, the mouth may be stitched up or the opercula tied down, and respiration though embarrassed will go on. It is interesting to note that certain general features of the respiratory rhythm more or less constant throughout the vertebrate lines appear in the traces obtained from fishes.

The death of fishes removed from water is not due to the drying of the gills, as has been supposed, but to diminution of the respiratory surface by their collapse and mutual adhesion. This is shown by the following experiments:—1. A gudgeon lives longer when suspended with mouth and gills open which would favour desiccation; 2, when the opercula are removed; 3, in dry oxygen than in dry air; 4, does not live longer in moist air than in dry.

Again, the greater duration of life out of water say in an eel, is not due simply to any mere mechanical arrangement for keeping the gills moist. An eel will live longer than a gudgeon—1, in water deprived of all air by boiling; 2, in water saturated with carbonic acid; 3, in water under the exhausted receiver of an air pump. In these circumstances, the protection of the branchia would afford no advantage. The eel, it is evident, offers greater resistance to asphyxiating agencies generally; and not only so, but if the two fishes are beheaded, and circulation and respiration are consequently entirely suspended, reflex phenomena persist in one much longer than the other. We have here a grouping together of facts highly characteristic of the author. Instead of a small anatomical detail we are compelled to seek for some general cause underlying all the phenomena, and this is found in the properties of the tissues of the eel, exemplified in the fact that their "respiration" or rate of oxidation is much less active. In harmony with this dominating quality are the various anatomical peculiarities which in a subordinate degree contribute to the prolonged survival of this fish when removed from water.

Some very curious and interesting details are given as to the respiratory movements of reptiles. In the frog, while the mouth and glottis are closed, the pharynx is filled with air through the nose; then while the floor of the mouth is quiet, the glottis is suddenly opened, and the air is expelled from the lungs through the nostrils by the contraction of the chest walls, immediately after which, while the glottis is still open, the floor of the mouth is raised, and the air contained in the mouth and pharynx is driven mainly to the lungs, but partly out by the nostrils which are narrowed to prevent undue escape. In the turtle and tortoise and in lizards a deep inspiration (1·7″) is at once followed by a half expiration (1·3″); when the glottis is closed and a long pause ensues (23·8″), the act of respiration is then completed and inspiration is repeated. In the crocodile two or three rapid inspirations and expirations are made, and then there is a long pause in full inspiration, the air being kept in the lungs by closure of the glottis. The devices by means of which these facts are ascertained are really amusing. Masks and muzzles are fitted on lizards, alligators, and snakes, and a crocodile even submits to have one nostril plugged while the other is put in communication by the tube with the registering apparatus.

The uses of the air sacs of birds and their play in respiration form the subject of another interesting inquiry. It is not as an increase of respiratory surface that they act, since, as was pointed out by Hunter, their walls are not vascular nor are they clothed with cilia. They diminish the specific gravity which will be of service in flight, but especially in floating on water. In aquatic birds, again, this may be useful in shifting the centre of gravity when the head is plunged deeply in search of food. Probably the most general and important function is to warm and moisten the air going to the lungs. Two pairs of these air sacs are within the thorax, outside the thorax are two other pairs, abdominal and cervical, and a single median sac, clavicular, anteriorly. All, both intra- and extra-thoracic, communicate with the divisions of the bronchi. In inspiration, air is drawn into the lungs and into the intra-thoracic air sacs, both from the trachea and from the extra-thoracic sacs; in expiration it passes not only out by the nostrils but into the extra-thoracic air sacs. The air residing in the sacs must become warm and moist, and mixing with the air from without as it passes to the lungs must *pro tanto* prevent desiccation and chilling of these organs. Another effect of the arrangement of the air sacs in birds is, that the lungs are twice traversed by air that is on its way both to and from the extra-thoracic cavities. The thorax of birds expands both laterally and antero-posteriorly, but the expansion ceases in the antero-posterior diameter, and the return movement begins before respiration is quite over in the transverse diameter.

In man, Vierordt and Ludwig describe an inspiratory and expiratory pause, and it may be added that many of the traces given by Dr. Sanderson in his Croomian Lecture before the Royal Society, indicate an expiratory pause, one as described in the last making the pause to occupy three fifths of the entire respiratory period.

Marey, on the other hand, and with him Bert, give traces which seem to show that in man and in mammalia generally there is no actual pause either at the end of inspiration or of expiration, but only a diminished rate of movement. In Dr. Sanderson's experiments, a source of error is evidently the open end of the T-shaped tube which connects the trachea of the animal with the registering apparatus, a large tube exaggerating the pause while obstruction of the open end obliterates it.[1] Bert registers both the movement of the chest walls and the current of air obtaining traces corroborative of each other; the movement of air is observed by causing the animal to breathe into a large vessel in communication with the registering apparatus which marks the varying pressure in the vessel.

Inspiration is, of course, effected by the descent of the diaphragm and the elevation of the ribs, and a curious point demonstrated by Bert is, that the diaphragm taking the abdominal viscera as a fulcrum raises somewhat the six lower ribs. The great expiratory force is the elasticity of the lungs themselves which is so great that when the chest is opened immediately after death, not only do the lungs collapse, but the lower ribs spring outwards, showing that they have actually been drawn in by the elasticity of the lungs. This action of the lungs must give rise to a continuous intra-thoracic negative pressure which will act on the heart and great vessels. In inspiration this is increased by the further stretching of the lungs; and as Dr. Sanderson has shown, the result is a more complete distension of the auricles and great veins, and, consequently, in better filled ventricles an increase of arterial tension. In inspiration, again, the air contained in the lungs is momentarily rarefied, and in expiration momentarily condensed in consequence of the resistance offered by the narrow glottis to the entry and exit of air. This has been experimentally demonstrated, and one effect will be that during inspiration the carbonic acid will more readily escape from the blood. Another effect which Bert says will follow cannot be accepted, namely, that the rarefaction will tend to draw the air from the air cells into the large tubes; the very contrary is the case,

[1] Dr. Sanderson's experiments were for the purpose of determining the influence of the respiratory movement on the blood pressure, and his apparatus, though ceasing to register a current of air as it became feebler, would mark changes in the direction of the current, and thus would indicate accurately the beginning of inspiration and expiration—all that his investigation absolutely required. The error is noted because the authority of the lecturer will probably be considered a guarantee not only of the soundness of his main conclusions, but of the accuracy of his details.

there is a rush of air along the large tubes to the lobules, but were this not the case, were the trachea tied, since it is the lobules and their air cells which dilate under the influence of the thoracic movements, while the larger tubes undergo little increase in capacity, the displacement of air would be from the tubes to the air cells.

The principal facts in this department of comparative respiration are, that the respiratory movements are more frequent in mammals than in birds, which would scarcely have been expected; more frequent in herbivora than in carnivora, although the latter consume more oxygen; more frequent in small than in large animals of the same natural group, the consumption of oxygen also being much greater. An explanation of the last of these statements, very generally accepted, has been that small animals exposing a larger surface in proportion to their bulk to the cold external medium, lose more heat and thus require a greater amount of combustion to keep up the temperature. The fact holds, however, with regard to fishes, and as their temperature varies with that of the water in which they live, the explanation does not apply to them. The increased consumption of oxygen in small animals is also observed when the external temperature is raised so that the loss of heat does not occur. A more general law is thus seen to be in operation, which is further exemplified in the fact that the respiration of the tissues is more active in small animals.

The effect of obstruction to the entry and exit of air in respiration has been shown by Marey to be to render the movements slower and more simple, the rhythm also being altered by prolongation of inspiration. This is when the obstruction is moderate only; when it is considerable, giving rise to dyspnœa, but not to such a degree as to excite struggling, Bert finds the effect to be first a storing of the respiration, then a quickening with diminished amplitude. When the obstruction is carried so far as to excite intense dyspnœa and struggling, the respiratory movements are irregular, slow, and deficient in amplitude at first, then as the animal becomes more quiet and insensible very regular and more ample. If the obstruction to the passage of air is in one direction only, the corresponding period is prolonged; when it is to expiration the chest gradually becomes distended and the anguish is extreme.

In immediate relation with the respiratory movements stands the question of the influence of the vagus and other nerves on their rate, rhythm, and amplitude. This has been anew investigated by Bert, who has brought to bear upon it ingenious applications of the graphic method. Unfortunately, however, the concomitant effects on the frequency and force of the heart's action and on the arterial blood-pressure are not noted. And although the problem is simpler in the case of the respiratory movements, and the dependence of these on variations in the circulation is less direct and immediate than the dependence of the heart's action, and the blood pressure on the respiration and on each other, we can never be assured that secondary causes have not intervened when some of the phenomena have been disregarded. With this reserve the conclusions of Bert may be given, clearly stated as they always are, and supported by abundant and cogent experimental evidence.

First, as to the section of the vagi, numerous traces are given, showing the effects on respiration in various classes of animals. Among considerable variations two characters are constant: 1, the number of inspirations is diminished; 2, the amplitude is increased. Expiration is most affected, a true expiratory pause appearing. Division of one nerve has a similar effect to division of both, but in a very inferior degree. Section of one is seldom followed by serious results; section of both is usually fatal in about ten days from consecutive changes in the lungs. These are not directly due to the loss of the motor influence of the nerve on the muscular fibres in the walls of the tubes or vessels, since division of the vagus on one side is not followed by congestion in the corresponding lung or by increased secretion from the bronchial mucous membrane, but to the imperfect respiratory movements.

The results of galvanization or irritation of the vagus and its branches and of other nerves are formulated as follows:

1. Respiration may be arrested by stimulation of the vagus (Traube); of the larynx (Cl. Bernard); of the nostrils (Schiff); of most nerves of sensation (Schiff); this last not yet confirmed. 2. The arrest may take place either in inspiration or in expiration. 3. A feeble stimulus accelerates, a stronger slackens, a very powerful stimulation arrests respiration. 4. When the respiratory movements are thus arrested, so also are all the movements of the body. 5. Respiration recommences in spite of the stimulus which has stopped it, if it be continuously applied, and when it is withdrawn, is almost always accelerated. 6. Arrest is more easily obtained during expiration than in inspiration. In some animals arrest in inspiration never occurs. 7. A stimulus of sufficient strength to arrest the respiratory movements in inspiration will arrest them instantaneously at whatever phase of the action it is applied, and this whether applied to the vagus or to its superior laryngeal branch.

Rosenthal concluded from his experiments that the superior laryngeal and vagus nerves were antagonistic, one, the vagus, being the inspiratory, the other the expiratory nerve, but Bert does not accept this theory in its elegant simplicity, agreeing with Schiff that there is nothing special in the superior laryngeal nerve. On the other hand, he places side by side with his own results just given, the conclusions of Mantegazza as to the influence of pain on the respiratory movements, that slight pain accelerated respiration, severe pain rendered the respiratory movements less frequent. But pain is only one manifestation of the centripetal action of nerves, and is not essential to the effect on respiration, the vagus itself is but slightly sensitive to painful impressions, and the results of stimulation of this nerve and of the superior laryngeal and nasal nerves are manifested when the animal is under chloroform. A formula of the most general character is thus reached. All feeble excitement of centripetal nerves accelerates the respiratory movements; all powerful excitement slackens them.

An important question which remains to be considered is the cause of death in asphyxia in a confined atmosphere, whether it is deficiency of oxygen or excess of carbonic acid. The phenomena of death by asphyxia need not be described, but an interesting fact is that when the volume of air in which an animal is confined is relatively small and the asphyxia is rapid there are convulsions, whereas when the volume is large and death slow, no convulsions occur; just as in death from hæmorrhage convulsions do or do not come on according as the fatal loss of blood is rapid or slow. An animal in the sleep of hybernation dies without waking and without any agitation; this same animal if awake would not pass through a stage of stupor unless the carbonic acid were removed as it was formed, in which case it goes to sleep.

The first point to be determined is the composition of the air in which an animal has died asphyxiated, this has been the subject of investigation by Cl. Bernard, W. Müller, Valentin, and others, and of numerous experiments by Bert. Mammals and birds die after having exhausted the oxygen to about the same degree, the former, however, consuming the oxygen rather more completely, and producing more carbonic acid. Birds leave about 2 per cent. of oxygen and 14 per cent. of carbonic acid, mammals usually less than 2 per cent., sometimes less than 1 per cent. of oxygen, and 16, 17, or 18 per cent. of carbonic acid. The composition of the air left is not materially different when the animal asphyxiated is in a state of hybernation or newly born, but herbivora use up the oxygen rather more completely than carnivora, and death seems to be much more rapid when the temperature of the air to which the animal is confined is maintained at or near blood heat. Reptiles appear to perish in an atmosphere which would scarcely cause inconvenience to mammals or birds, a fact which is remarkable when their respiratory inferiority is considered.

In warm-blooded animals the cause of death in a confined atmosphere is clearly the privation of oxygen, seeing that only 2 or 3 per cent. of this gas is found in the air in which the animal has died. When, however, oxygen is substituted for air in the experiment, the animal being placed in a limited volume of this gas, death occurs when the proportion of carbonic acid has reached a certain height, 23 to 25 per cent. on the average; although the proportion of oxygen remaining is greater than is contained in atmospheric air; the result is the same, whether the carbonic acid and oxygen have been mixed in the required proportion or have been brought to this proportion by the respiration of the animal. Here the blood and tissues are found after death of a bright scarlet colour, showing that oxygen has not been prevented from entering the blood, the cause of death, then, must be the carbonic acid. As soon, in fact, as the tension of the carbonic acid in the surrounding medium equals its tension in the blood, the gas accumulates in the blood and acts as a poison. We thus see how it is that reptiles die in an atmosphere in which the proportion of carbonic acid is comparatively small—they have little internal heat to drive off the gas; and why rats perish in air maintained at a temperature of 30° to 38° C., leaving 12 to 14 per cent. of oxygen and only 6 per cent. of carbonic acid—the internal tension due to heat is neutralised by a corresponding external tension; why, again, an animal dying in a confined volume of oxygen is relieved and revives for a time when its atmosphere is rarefied by the air-pump.

Carbonic acid is usually regarded as having qualities positively poisonous as compared with hydrogen and nitrogen, which are considered to be merely irrespirable, and Bert relates experiments which he believes to be confirmatory of this view, especially experiments made on new-born animals, which die much more rapidly in an atmosphere of carbonic acid than in hydrogen or nitrogen. But it is surely inconsistent with the idea we attach to the term a "veritable poison" employed by Bert, that carbonic acid can be safely used as an anesthetic, and that an animal will live for hours in an atmosphere in which it is present in the proportion of 10 per cent. Death by asphyxia, in whatever way it may be induced, is immediately due to the arrest of the oxidations which evolve the nerve force by which the great functions of the animal machine are kept in play, and this may occur from want of oxygen or from obstruction to the combination of oxygen with the oxidizable material. It is by the latter method that carbonic acid acts. When the external and internal tensions of the carbonic acid are equalised it accumulates in the blood, and accumulating in the blood it is not removed from the tissues which in time become saturated with it, and thus the changes which result in its formation are impeded or arrested. Carbonic acid is not a poison in the same sense as carbonic oxide; the latter enters into a combination with the hæmoglobin of the corpuscles from which oxygen cannot displace it, whereas the former at once gives place to oxygen when the physical obstacle to its escape is removed.

Some of Bert's experiments on death by submersion are particularly interesting. It is well known that the fatal fact in drowning is the entry of water into the air passages, where it is churned into a bloody froth, which even when the sufferer is withdrawn from the water prevents the entry of air and destroys life. This water finds entrance in large quantity only when consciousness is lost and the instinctive exclusion of the liquid is replaced by deep involuntary inspirations. The lungs will absorb and transfer to the blood a prodigious amount of water. In the course of 3½ hours 21 litres of warm water gradually flowed into the trachea of a horse, after which it was killed; no water was found in its air passages. A dog was made to breathe water from a vessel of known capacity by means of a tube in the trachea; 375 grammes of water were inspired, and the entire lungs removed immediately weighed only 200 grammes. At least half of the fluid must thus have been carried away in the blood. If, therefore, after apparent death by drowning the blood can be kept moving through the lungs, however feebly, there is a possibility that the water which has gained entrance will be absorbed.

Some aquatic animals can remain under water for a very long time, the hippopotamus fifteen minutes, the whale half an hour. In the seal, hippopotamus, &c., there are certain anatomical peculiarities manifestly in relation with this power:—1, a sphincter surrounding the vena cava inferior at the level of the diaphragm; 2, enormous abdominal venous plexuses and sinuses; and 3, means of compressing the carotids (as in the hippopotamus) or retia mirabilia (as in the whale). Gratiolet's theory of the operation of these provisions is as follows:—"While the animal is under water the vena cava inferior is closed by the sphincter, which thus shuts off the blood of the greater part of the body from the heart so that only that returning from the head and anterior extremities passes through the lungs and is distributed to the body generally. In this way the blood is gradually withdrawn from the pulmonary circulation and accumulated in the abdominal venous sinuses. The presumed advantage is physical in its nature, namely, that cerebral congestion is in this way obviated." This theory is probably true so far as it goes, but it is insufficient; it does not apply, for example, to animals in which no such apparatus exists. In order to elucidate the question the duck and fowl are selected for comparative experiment. The duck bears submersion for an average period of 11¼ minutes, while a fowl dies in 3 minutes. The tranquillity of the duck when submerged, due to its familiarity with water, accounts for a part, but a small part only, of its superiority over the fowl, which struggles and loses air. That it resists asphyxia of all kinds better is shown by the fact that when the trachea is tied a duck will show signs of life for 8½ minutes, a fowl for 3½ minutes only. This difference between the fowl and duck is not explained by any venous dilatations, or by larger air sacs in the duck, since the existence of these conditions is negatived by anatomical investigation; it is not due to slower consumption of oxygen by the tissues of the duck, or to a larger proportion of oxygen in its blood, since in both these respects the advantage is on the side of the fowl, as has been shown by experiments already mentioned. The probable cause of the difference is the larger amount of blood found in the duck, and the larger proportion of its solid constituents which will afford to the tissues a greater supply of oxygen. This hypothesis is supported, if not established, by the fact that if a duck is bled so as to bring down the proportion of its blood to body-weight to the level of the fowl it dies in 5 minutes after submersion instead of living some 12 minutes.

Another problem, similar to the last, is that originally propounded by Harvey—"Why new-born animals resist asphyxia so much longer than adults." First as to the precise facts. It is found by experiment that new-born puppies, kittens, rats, &c., placed in tepid water will show signs of life at the end of half an hour or more, an adult animal dying in 3 or 4 minutes. There is a similar resistance to asphyxia in newly-hatched birds if naked and helpless, but not if they are active and strong, like the Gallinaceous chick. The duration of life varies with the temperature of the water, the warmer it is the sooner is life extinct. A kitten lived 27½ minutes at 14° C., another only 11½ minutes at 36° C.

The usual explanation of the prolonged survival of new-born animals when deprived of air is, that the foramen ovale and ductus arteriosus are still open and allow the blood to pass from the pulmonary to the systemic circulation without going through the lungs, in which the absence of air brings on a block in the circulation of the blood. But young animals resist equally well gradual asphyxia in an atmosphere of nitrogen and hydrogen, in which the fœtal orifices would not be of the same service;

and a rat, 10 days old, in which they were found closed, only died after $11\frac{1}{2}$ minutes' submersion, while an adult would have been dead in $2\frac{1}{2}$ minutes. On the other hand, young fowls and ducks, having the fœtal openings patent, die in a minute and a half. It is not, then, to the peculiarities of the fœtal circulation that the prolonged survival of new-born animals is to be attributed. Again, the resistance to asphyxia is not in this case due to any larger amount of blood in the new-born animal. A young rat will show signs of sensibility 15 minutes after the removal of the heart and loss of all the blood; in an adult they cease in a few seconds. In an adult rat all reflex phenomena cease in a few seconds after decapitation, they persist in a rat newly born for a quarter of an hour, and a similar difference in the manifestation of vital properties is seen in a detached portion of muscle or nerve. It thus becomes evident that the increased resistance resides in the tissues themselves, that is, in the anatomical elements. But not only is life more slowly extinguished by asphyxia and hæmorrhage, but by some poisons. A new-born animal requires a dose of strychnine in proportion to its body-weight ten-fold that which kills an adult, and with this death is much slower; at the same time convulsions are induced by as small a dose in the young as in the adult, so that there is no want of susceptibility to its influence. This is exactly comparable to what is seen in reptiles; they are easily thrown into convulsions by strychnine, but they do not soon die, and often recover. Prussic acid, again, is less fatal to young warm-blooded animals and to reptiles.

It has been seen that in new-born animals the tenacity of life is equally manifested in asphyxia and in loss of blood, and that it resides in the anatomical elements. It is in some property of the tissues themselves, then, that we should seek the explanation, and this is found in the fact already stated, that in the new-born animal the respiration of the tissues, the consumption of oxygen and formation of carbonic acid is much less active than in the adult. This carries us as far back towards a comprehension of the ultimate cause as is possible in the present state of our knowledge; the tissue-changes being slower, function is more slowly brought to a stand-still by want of oxygen or accumulation of carbonic acid. We can even see how it is that tissue-respiration or oxidation is comparatively so small in the new-born animal; during intra-uterine life of the two antagonistic processes, nutrition and oxidation, going on throughout the existence of an animal, the former predominates, the fœtus derives its warmth from the mother, and has no need, therefore, of oxidation for the maintenance of its temperature, nervo-muscular action is also at a minimum, and there being little expenditure of force little oxidation will be needed. Experiment and deduction thus give each other mutual confirmation, and our comprehension of the intimate changes taking place in the tissues of animals is enlarged.

Many instructive experiments and interesting discussions have received no mention in this brief analysis of a volume pregnant with new facts and large views. If our readers are driven to the work itself in search of them so much the better. We may conclude with a statement of two general considerations frequently exemplified in the work, and again insisted on in the final chapter. The first is that physiology is not necessarily deducible from anatomy, or its office simply to explain facts ascertained by anatomy. The erroneous ideas that the survival of certain fishes in air is due to the protection of the gills, that the endurance of prolonged submersion by certain birds or mammals is in virtue of peculiarities in their blood-vessels, have their origin in the presumed necessity to seek in the structure of organs the explanation of their functions. New-born animals resist asphyxia: an anatomical explanation is sought, and a persistent communication between the left and right hearts is found: circulation can thus be carried on, and the required explanation is attained. What! the same resistance is observed when the heart is removed!

Anatomical inductions must take the second rank; experiment must be our guide, and this not only in the difficult questions and fundamental mysteries of the problem of life, but in simple matters of mechanism.

The second of these considerations is a question of method. In the study of certain of the problems two methods presented themselves: one seeking by complicated apparatus, by minute estimation of physical conditions, by vigorous calculation, the numerical value which expresses certain facts with an exactitude approaching the absolute; the other taking less account of mathematical and physical exactitude than of the physiological conditions introduced often unknown to the experimenter which render illusory the pretended accuracy of the calculations. The first seeking refuge from error in numerous observations, and hiding in averages enormous divergences in results; the other working always by comparison of simultaneous experiments, in which all unknown influences will act with equal force, and by rigorous observation of individual facts; rejecting the system of averages as useless or dangerous—useless if the average only expresses the mean result of experiment, dangerous if it conceals differences which ought to be explained, not effaced. It is not necessary to state which method Bert has followed; we shall look with great interest for further developments of it in the more important chair in which he has recently succeeded Claude Bernard. W. H. BROADBENT.

V.—Swain on Diseases of the Knee-joint.[1]

IN 1865 the subject for the Jacksonian Prize Essay of the College of Surgeons was, "The Injuries and Diseases of the Knee-joint, and their Treatment by Amputation and Excision Contrasted," and Mr. Swain, of Devonport, was the successful competitor. In 1866 his essay was published in the 'British Medical Journal,' and now he has made some slight additions to it, and brought it out in a separate form.

The subjects for the Jacksonian Prize Essays have always been of the most practical character, and the treatises that they have called forth are among the most valuable in recent surgical literature.

We need not mention examples, for several will immediately occur to the minds of our readers. Many of the best of our modern works in different departments of surgery were originally written as Jacksonian Prize Essays, and not a few of those who have gained this honorable distinction now stand high in professional reputation and public favour.

The subject which forms the theme of this volume is an eminently practical one, and the College of Surgeons acted wisely when they proposed it. More than twenty years have elapsed since the operation of excision of the knee-joint was revived by Sir William Fergusson, and during that time it has been extensively practised by some surgeons, while others have set their faces against it. This state of things has now gone on so long that it is very desirable that we should come to something like an agreement about the value of the operation. Surely such agreement is by no means impossible. Old prejudices are passing away with the generation who entertained them, and when once the case can be argued on its merits, and after a sufficient number of trials to give us a safe basis for deduction, it cannot be very long before we arrive at something like a settled opinion on the subject. Even now the question is discussed in a more temperate spirit than formerly. There is not such a storm of controversy as once there was. We do not hear so much of some hospitals in which the operation is pushed to excess, and of others where it is never practised at all. Ex-

[1] *Injuries and Diseases of the Knee-joint, and their Treatment by Amputation and Excision Contrasted.* By WILLIAM PAUL SWAIN, F.R.C.S., Surgeon to the Royal Albert Hospital, Devonport. London, 1869. Pp. 252.

aggerated statements on both sides have been dropped, and a fair and impartial spirit prevails, and no doubt Mr. Swain's work will do something to help us to arrive at a clear and sound opinion.

There is, perhaps, no surgical proceeding which has given rise to so much discussion in this generation as excision of the knee-joint. It has been a veritable battle-field for surgeons, and we cannot wonder at it, for hitherto the results of the operation have been extremely variable. Sometimes it has turned out admirably; at other times the limb which has been preserved has been nothing but an encumbrance to the patient, and he has been glad to get rid of it by subsequent amputation. Each party has had plenty of instances that they could point to in support of their views, and thus they have given weight and force to their opinions. But now there have been a sufficient number of excisions to enable us to say in what instances the operation is likely to turn out well, and what conditions are unpromising, so that we may hope to arrive at something like a true estimate of the value of the operation. Regarding it as a proceeding which has won for itself an established place in surgery, we have now to ascertain by experience and observation what are the conditions under which it ought to be practised; and the volume before us is a valuable contribution to this end.

Mr. Swain's essay opens with a brief account of the anatomy of the knee-joint, and then he passes on to review its morbid conditions, the diseases to which it is liable, and which may necessitate the interference of the surgeon. The fourth chapter is devoted to the wounds, injuries, and deformities of the joint; and then we are furnished with the data for considering the relief which may be afforded by an operation. The chapter which relates to the present position of the operation of excision of the knee is a very interesting one. Our author has brought together a mass of statistics of cases which had occurred before 1865, and now he has added some more which have been treated since that date, and which had not previously been tabulated. The total number of cases that he has collated is 472. Of these the mortality equalled 24·57 per cent. Fifty-four underwent subsequent amputation, and 302 were left with useful limbs. After he has explained the position which the operation now holds, and the average success which has attended it, the author devotes his sixth chapter to a minute and careful account of the way in which it ought to be performed, and the precautions which ought to be taken in splinting and dressing the limb afterwards. This is a valuable chapter, and will be found an admirable guide for any one who is undertaking the excision for the first time. We cannot too strongly enforce Mr. Swain's advice, for we are sure that the due selection of cases, and the minute care in the after treatment, are two points upon which the success of the operation mainly depends. He then passes on to indicate those cases to which the operation is applicable, and this leads him to consider its value in reference to the various diseased conditions of the knee, and in reference to injuries and deformities. But what is to be done if the operation has been tried and has failed? Suppose the patient is left at the end of a year with a leg riddled with sinuses, or with a sound but flail-like limb, what is to be done? To this question our author devotes considerable space, for it goes to the very root of the matter; and he dwells at length upon re-excision and amputation as the alternative which presents itself in such cases. The volume is brought to a close by an appendix in which some details and statistics are given which could not very well be introduced in the text.

Mr. Swain has had some considerable experience of his own in excision of the knee-joint at the Royal Albert Hospital, Devonport; and as a former pupil and house surgeon of King's College Hospital, he had the opportunity for many years of watching the results of the operation in the hands of others. It is curious to observe how few hospitals furnish any considerable number of statistics in reference to this operation. At one or two of our largest metropolitan institutions the proceeding seems to be almost unknown; at two or three others it is occasionally practised; but it is King's College Hospital and King's College men who have the credit of having given it the fullest trial. It is to the distinguished professor of clinical surgery in King's College, and to the pupils whom he has sent forth, that we are mainly indebted for having revived this operation, and established it as one of the recognised methods of dealing with certain affections of the knee-joint.

The view which our author takes of the operation may be shown by quoting part of the paragraph with which his work concludes, and in which he sums up his opinions:

"I have contended [he says] that whereas it is a glory to the surgeon to save both limb and life to his patient, he has no right unduly to risk the latter in attempting to save the former. Much as I admire, and desire to practise in every fair case, excision of the knee, I have no wish to strike amputation of the thigh out of the roll of surgical procedures. It is a painful operation for the surgeon to undertake, and a still more painful and distressing circumstance for the patient to go forth maimed to so fearful an extent; but the prolongation of life is the surgeon's great triumph; and if the loss of a limb to his patient is the cost at which this is procured, he must not hesitate to perform his duty. On the other hand, it behoves us as a profession to be more careful how we deal out such a full measure to our patients. A great and important addition has been made to our operative resources by the revival of excision of the knee-joint; and we must be careful how we allow either prejudice against, or ignorance of, the operation to prevent us from giving those who are under our care, and who may require it, the benefits of its use."

These words represent very well what is the prevailing opinion among surgeons at the present time with regard to this operation. We might formulate it by saying, "In suitable cases excise the knee-joint rather than amputate in the thigh, and the chances are three to one that your patient will do well." How strongly this contrasts with what Liston said. In the latest edition of his 'Practical Surgery' the only mention that we find of the operation is this:—"It [resection] has been attempted on the knee, but with no encouraging result." Since Liston's time, however, great progress has been made, and we have no doubt that, had he lived a few years longer, he would have been as willing as any other surgeon to practise the operation in appropriate cases.

Before we take leave of Mr. Swain, it may be of interest to our readers if we quote one or two passages from his work, and thus allow him to speak for himself.

In treating of certain affections of the knee-joint, he says:

"I believe that these acutely painful cases of articular disease are eminently marked out for treatment by excision of the joint. As an example of this class of disease I would refer to the following case, further particulars of which are given in the appendix.

"The patient was a young girl, æt. 16, who was admitted under my care into the Royal Albert Hospital, in February, 1864. The condition of the joint on admission is correctly portrayed in the adjoining woodcut, the knee-joint being firmly fixed in the position seen. For five years she had suffered from repeated attacks of inflammation in the joint. As an out-patient she had been treated with tonics, and a gutta-percha splint had been adjusted to the limb. Upon her admission the limb was placed on a McIntyre splint and swung. It was also blistered, and various other applications were made. Extension in any shape or form she could not endure, and night after night her pain was so intense that no sedative procured her rest.

"On May 28th I excised the joint, finding the bones more extensively involved than I had expected.

"On November 19th she was made an out-patient, the limb being firmly anchylosed and in perfect position.

"The case in many points illustrates the propriety of excision. It came under my notice at an advanced stage, when considerable contraction of the knee had taken place. 'The influence of rest' was fully tested for many months; but the disease steadily progressed, and the excessive pain and broken rest told much on my patient's constitution. After the operation she experienced little or no pain, except at an after stage, arising from exceptional circumstances, viz., the unfortunate galling by the splint in the upper part of the thigh, which produced a large diffuse abscess. The great point to which I would direct attention is this: that whereas before the operation no sedative procured her rest at night, no sooner was the diseased articulation removed, than she slept night after night with comparative ease and comfort. A singular, and I believe unprecedented, accident befel her. Soon after she left the hospital cured, with perfect bony anchylosis, she fell over some stone steps, and fractured the femur two inches above the excised knee, the osseous union remaining intact."—P. 99.

It is sometimes a question whether excision ought to be practised after extensive injuries to the knee-joint. The statistics of amputation in the thigh under such circumstances are so unfavorable, that our author is of opinion that the shock is less when only the knee-joint is taken away, and that, therefore, excision ought not to be excluded from our means of dealing with such cases. To show how well a primary excision after injury may sometimes turn out, Mr. Swain quotes the following case from the practice of Mr. Kempe of Exeter:

"June 10th, 1862. John Fewings, Exwick, æt. 13, was admitted under the care of Mr. Kempe, with a lacerated wound of the right knee, freely communicating with the joint, the result of accident. Excision of the joint was performed about one hour after the receipt of the injury. Great constitutional disturbance and some delirium followed the operation, which were subdued by opium, &c. A large abscess formed on the right hip, apparently arising from some extravasated blood, which took place at the time of the accident.

"He was made an out-patient in September, with a very firm union of the bones, but with one or two small sinuses.

"Mr. Kempe saw this lad, about twelve months after his discharge, loading a railway van, apparently with as little inconvenience as if he had suffered no loss of the joint."—P. 113.

These extracts will serve to show our readers how interesting are the different classes of cases with which our author is concerned, and how important is the method of treatment which forms the subject of this essay.

Mr. Swain's work can hardly fail to have a wide circulation, and to command the attention of surgeons. The temperate and judicious tone in which it is written will do much to recommend excision of the knee, and to help it to take its proper place—and neither more nor less than its proper place—among the operative procedures of surgery.

The volume is well illustrated with a number of woodcuts which are chiefly taken from photographs. If some of them are not very artistic, they have at least the merit of being faithful, which is a point of much greater importance. We cannot make surgery picturesque; but true representations of morbid conditions, whether taken by the sun's rays or by the artist's pencil, are most valuable additions to any work upon a surgical subject.

VI.—Inman's Restoration of Health.[1]

In arranging this book on our library shelves we have put it next to '*Heberdeni Commentarii de Morborum Historiâ et Curatione*' on account of the resemblances in essential character of the two works. Like that classical volume, the present consists of short essays on the diseases which, from their frequency, are most likely to afford a common ground of interest between author and reader; each essay being complete in itself, and having no expressed bond of union with its neighbours. Dr. Heberden marks strongly that intentional disconnection by an alphabetical arrangement, Dr. Inman by taking the simplest of all pathological classifications, namely, the anatomical position of the seat of the most obvious symptom, proceeding from top to toe. Both works have a decided clinical character, without at the same time taking the form of the regular clinical lecture on cases known to both lecturer and audience. We learn from Dr. Heberden's own statement that he habitually made short notes at the bedsides of his patients of what he observed and heard, and once a month looked these over and extracted whatever he found graphically illustrative of pathology or therapeutics. At these monthly revisions he would seem to have interwoven in his text the racy anecdotes and pithy observations which distinguish the writer. We do not glean from Dr. Inman's own pen how his materials grew together, but we so often mentally accompany him to the sick-room, and what passes there is so vividly described, that either a sketch has been made on the spot, or the author possesses one of those stereotyping memories which enabled Turner to depict all the details of a sunset sky, viewed in company three months before. Incidentally these sketches give evidences of social manners, many now passing away, which might otherwise be lost. Thus we learn

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[1] *On the Restoration of Health; being Essays on the Principles upon which the Treatment of many Diseases is to be Conducted.* By Thomas Inman, M.D. Lond., Physician to the Royal Infirmary, Liverpool. 1870.

that in the north-west provinces of England it is usual to employ the vocative "doctor" in addressing a physician, even among the upper middle classes. And we are sorry to find also that the custom of beguiling freshmen into drunkenness is not quite yet extinct among medical students.

In both of the authors we are comparing there is an almost pedantic avoidance of citing any authority beyond personal experience; not apparently from any contemptuous distrust of its value, but because it is not the business immediately on hand.

The style of both is easy and straightforward. Easy to read, we mean, for traces can be seen in both of considerable labour being given to modelling the sentences into that form of facile fluency for which readers are so grateful. The medium of speech indeed is different: the dead languages were in Heberden's days the readiest means of communication between European readers; and the adoption of Latin has doubtless been the reason for his wide popularity and classical position on the continent. English in the last century beyond the channel was a much deader tongue than Latin. But the ponderous Ciceronian march, so fashionable among scholars, is rejected; and the neat short sentences remind one rather of the 'Commentarii' of the brilliant statesman who first adopted that word on his title page. With a similar kind thoughtfulness for his readers, Dr. Inman has taken Addison for his model rather than Johnson. Indeed we are, at the beginning of each essay, constantly reminded of the 'Spectator' by the neat crisp way he has of, not plunging, but "*sloping*," so to speak, *in medias res.* There is no time lost in introducing the subject, yet we are never either startled with irrelevant matter, as in newspaper articles, or repelled by platitudes, as in systematic lectures.

From the first two chapters one may gather the general principles which guide Dr. Inman in his treatment of disease. His heroes, as he tells us in page 16, are "such men as Bennett, Chambers, Paget, Hilton, Brinton, Johnson," whom he looks upon as the inaugurators "of the rational medication of the present day." It is true he has not here attempted to point out the thread of harmony which strings his varied gems, apparently considering himself exempt from the task by the publication, some years ago, of his '*Foundation for a new Theory of Medicine.*'[1] We cannot of course do here more than allude cursorily to that work, which excited in its time a good deal of criticism. The '*Foundation*' is in fact the thesis that "disease is a deficiency of vital force;" and, whether we are

[1] Second edition, 1861.

prepared to go the whole length with the author or not, we must allow that all changes of therapeutical practice in our own day are, consciously or unconsciously, veering in the direction indicated by that compass. That the predominating idea is really new, and not merely a putting into a more scientific form the results of ancestral observations, has been so fully shown in a recent number of the 'Quarterly Review,'[1] on the "*Aims of Modern Medicine*," that we may be excused from stating more than our general consent to the argument; while the maintenance of the thesis we must leave to its propounders. What we purpose here is to exhibit the restorative physician in action, bearing witness of himself.

The opening subject is headache, for which the most important therapeutical positives are rest and appropriate food, and the most important therapeutical negative is the avoidance of purgatives, and of every other thing likely to produce or foster debility. In chronic cases oil-rubbing to the skin and iron to the digestive organs, in acute cases stimulants and anæsthetics to the skin of the head are commended; and the rectification of the cerebral circulation by gentle compression of the scalp is held of much value. Before adopting any of these means, however, the physician is urged to think no labour wasted which is spent in making minute inquiry into the possible foreign causes, such as organic tumefactions, and the changes produced by certain drugs, such as quinine, alcohol, and especially purgatives, or by morbid poisons in the blood, such as those of influenza, urea, uric acid, &c. Excluding these, which form a small minority of the cases coming under the physician's notice, headache is viewed as a cerebral debility, and to be so treated.

The next matter handled is infantile convulsions. The immediate cause the author considers to be an imperfect supply of blood to the brain from "irritation"—that is, impediment to the normal vitality—of the cerebral capillaries. The primary stage is one of anæmia, as shown by the pallor of countenance preceeding and at the beginning of the attack. The stagnation or congestion of blood is secondary, and indeed is often wanting, as shown by the absence of flushed face in many cases. According to the restorative school it is always deficiency, and never excess, of the sanguineous pressure and supply which induces loss of consciousness; and into the same category as hæmorrhages, which lessen supply by lessening the whole bulk of blood, they would refer distant congestions, which withdraw a portion from general circulation, and distant irritations, which derange the action of the sympathetic nerve upon the capillaries

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[1] April, 1869.

throughout the body, and most especially the delicate set of capillaries in the brain. Tumours, adhesions, superficial abscesses, ossifications, &c., within the cranium all rank as wounds of the brain, which (as Dr. Inman might have told us was first observed by Schröder van der Kolk) produce acute anæmia in the first instance. This bold generalisation of the cause of convulsion wonderfully simplifies the principles of treatment. It is scarcely necessary to say that these consist in giving tonics and as much suitable nutriment as the patient can digest. The reader is, however, warned that an animal may be equally starved by not being able to digest food, or by being not able to get it; and a very frequent cause of infantile fits is pointed out to be the feeding babies with hard muscular fibre earlier in life than the gastric solvent fitted to reduce it is formed.

The advantageous use of chloroform inhalation in alleviating the spasm of fits is noticed with much enthusiasm, but we cannot think it prudent to recommend mothers to make the drug a permanent denizen of the nursery cupboard, and to administer it themselves on a handkerchief.

During the composition of the chapter on palsy the author seems to have been suffering from an attack of scepticism. He falls foul of a number of old-fashioned remedies which he denounces as murderous, and a good many objects of contemporary faith which he placards as useless, but he gives us nothing in their stead. It would seem, however, that he uses some, but superciliously attributes their benefit to a most remote contingency. Thus, for example, he thinks that blisters may run a chance of doing good by some of the cantharidin being absorbed through the skin into the deep-seated tissues. If so, why does not he give cantharidin by the mouth? Again, he will allow no credit to galvanism beyond that of keeping the muscles pliable and ready to act as soon as the nervous energy is replaced to them, just as we keep a well-aired bed to receive a drowning man. But the unkindest cut of all is at hysterical paralysis, which he simply says he does not believe in, and at syphilitic paralysis, of which he has never read a fully authenticated case.

Now, his so-called disbelief in hysterical paralysis must be purely a question of words, for he himself cites a case of paralysis cured by mental causes, and, therefore, dependent on mental causes, and between this and hysterical paralysis we know no distinction but an etymological one. No doubt syphilitic paralysis is a somewhat rare disease, but it is surely mere superciliousness not to credit the recorded cases of it.

In the title to the next chapter, (On "Pervigilio," &c.), for no reason that we can guess, there is broken a very excellent rule laid down for himself by the author in the preface, namely, that when he had to choose between a technical and a common word he has selected the latter. In what part of the world has been, is, or ever will be, "pervigilio" a common word? In fact it is so uncommon that we have doubts if it ever did exist, except as the ablative of "pervigilium," which pseudo-Catullus and Justinian use to imply a voluntary vigil or wake. The context shows that here it is the nominative case, and means "wakefulness," a common, expressive, and sufficiently melodious English word, which we hereby commend to Dr. Inman for employment in his second edition. Our reason for noticing the breach of the rule is that it is such a very good one for medical authors above all others.

Dr. Inman seems inclined to favour the recent theory of the process of going to sleep being, partially at any rate, of an active rather than entirely of a passive nature. He recognises the necessity for a supply of psychical and physical energy to effect it. He points out how exhaustion of mind or body, in consequence of previous excessive use, render it difficult. Hence he treats ordinary sleeplessness by digestible food taken shortly before the convenient period of repose, and indeed would recommend that also as an adjunct to other treatment required in more complicated cases.

Insanity is looked upon by our author as a more permanent deficiency of the nervous apparatus, of which wakefulness, sleep-walking, and the like are minor manifestations. It is with him not a disease of the mind, but of the mind's tools. The only disease of the mind which he would recognise is wickedness. Here we are at issue with him; for if the mind can be perverted by the acknowledged inclination to do wrong, what contradiction is there in supposing it capable of perversion into thinking wrong? No bodily derangement is present to make a man covetous or profane; why should it be presupposed necessary when he fancies himself a tea-pot? The material doctrine would lead us to neglect the educational treatment which is so important in the slighter and less obvious examples that come before the general practitioner—the treatment of those who are their own keepers, and anxious to avoid the pain and disgrace of foreign restraint. Many of these cases we call euphemistically hypochondriasis and hysteria, but a great step in the direction of their real cure is made in recognising the mental element in their nature and treating it. We are glad to see this is done in the newest systematic treatise on medicine.[1] But we must concede to Dr. Inman that the corporeal medication has an

[1] Reynolds's 'System of Medicine,' art. "Hysteria," "Hypochondriasis," and "Ecstasy," by Dr. Reynolds, Dr. Gull, and Dr. Chambers respectively.

equal importance, especially in the more confirmed patients who come under the charge of specialists; and we fully agree with him that the most valuable part of that medication is a liberal diet. We are glad to find that these high-farming principles are becoming daily wider spread among the proprietors of asylums in spite of the serious drain upon their profits which they must involve. An old friend of ours, whom we were obliged to send into retreat not far from London, had no complaint to make of his quarters, except the constant smell of eating, which was going on morning, noon, and night, "as bad as the Crystal Palace on a general holiday." He allowed the error was on the right side.

In the succeeding chapter, "On Cold or Catarrh," is fairly stated the difficulty of deciding the question whether these diseases of the air passages are the expressions of a morbid poison absorbed into the general system, or whether the general affection is the result of the injury to the mucous surface. The immediate excitant of catarrh Dr. Inman thinks is not cold, as people assume from its name, but the transition into heat; a very practical question for the sufferers, as it indicates the true place and time for exercising caution. The process produced by the action of heat succeeding to cold on the part affected he views as akin to chilblain, that is to say, a loss of elasticity and arrest of circulation in the capillaries, where the blood stagnates and gets dark. Consequently the relief of the local symptoms should include the removal of the congestion by small evacuations of blood, as, *e. g.*, catarrhal sore throat by a leech or two over each tonsil, conjunctivitis by leeching the temples, or earache by the same beneath the external meatus. The same principle directs us in the early stage of coryza to divert and recirculate the blood congested in the frontal sinuses, and causing the characteristic headache, by the application of very hot water to the brows.

Dr. Inman believes in the possibility of the temporary morbid process of catarrh being converted by artificial aggravation into a disorganising process, and that in the case of the lungs a cold may become a consumption. He points out that this takes place during the period which should be that of convalescence, and that the providing for this period is a most important part of the treatment of those subject to catarrhs. Now, as the illness itself considerably depresses the powers of life, care should be taken that the patient does not assist the enemy by starving, by taking antimony, ipecacuanha, colocynth, and such like; but that, on the contrary, he fosters the failing strength by rest, by dilute nutriment and warmth. The direct application of opium as a tonic to the relaxed blood-vessels in the nose and throat is also of great use. We think there is a slip of the pen where sesquichloride of iron is proposed as a gargle. We have always found it aggravate the distress in catarrh. When the abdominal canal is affected the pleasant remedy recommended is warm port wine negus.

In the next essay, "On Sore Throat," attention is drawn to the neuralgic condition of the pharyngeal muscles which accompanies it, the soothing of which is rightly directed not to be lost sight of in the treatment. Not only on this score, but as a direct subduer of inflammation, opium is recommended for a local application in cynanche.

"In the preparations of opium—and I almost exclusively [writes the author] "employ the tincture mixed with water alone in strength varying from one part to three and one to eight—we find a material which will promote cicatrization of ulcers, abatement of inflammation of mucous membrane, &c., and relieve muscular irritability."

Dr. Inman, in the next chapter, insists strongly on the value of the same principle, viz., of stopping muscular spasm by local anæsthetics, in the treatment of true and false croup. The next, "On Bronchitis," had better have been united with that on catarrh.

The discourses following, "On Consumption," and "On Oil-rubbing" (to which should be added "&c."), have for their chief point the importance of supplying to the body the means of forming its essential constituent—adipose tissue. We believe people pretty generally have left off doctoring the lungs in consumption, and acknowledge "the chest as the battle-field of past conflict, the stomach the ripening ground for new series of life." But they have been somewhat lazy in devising variety of means of attaining the object. Their first prescription has been cod-liver oil, their second prescription cod-liver oil, and their third prescription cod-liver oil; and in the numerous cases where this disagrees they administer a placebo for a season, and then, if the patient still is under their thumb, cod-liver oil again. Now, Dr. Inman points out that for internal use there are several esculents, which, though not so good, perhaps, generally as fish oil, in special cases suit better. He is particularly fond of rum and milk, cream and almonds, separately, mixed, alternated, and in various forms which taste may suggest. But experience of its advantageous employment has convinced him that externally oleaginous matter deserves more extensive use than has hitherto been accorded to it.

"It is very useful in bronchitis, phthisis, mesenteric disease, marasmus, diabetes, and myalgia. Appropriately used it becomes an aphrodisiac. The plan may be varied by medicating the oil with such stimulants as turpentine or other essential oil, or with narcotics such as belladonna, opium, or morphia. In conclusion, let me repeat [says our author] once more my favorite formula for cases of phthisis or general debility—'Keep the stomach for food, the rectum for physic, and the skin for oil.' It is a homely saying, but not the less true on that account."

While on the subject of homeliness we would remark that Dr. Inman too often falls into the popular fallacy of attributing to an unscientific source valuable additions to technical knowledge, when in fact the merit is really due to the scientifically educated mind, which alone could have taken the hint. For example, he attributes to "ancient families" his knowledge of the value of rum and milk as an article of medicinal diet, to an "old woman" his favorite linctus of honey, lemon-juice, and rum, and to a medical writer, whose precepts he certainly would not ordinarily follow, his appreciation of oil-rubbing—as if they were not to be learned by the student from his regular appointed teachers. Now, it happens that we can recollect, in spite of the lapse of time, the sources whence we enriched our minds with the above-named furniture; the first was learned from our professor of materia medica; the second from a court physician, who was our senior medical officer; and the third from the lecturer on the practice of medicine; so that we might almost infer, as a counter-balance to Dr. Inman, that the highest dignitaries alone communicated such choice experience.

In the management of pneumonia, which is next, it is needless to say, after such a context, we are taken the whole length and breadth of the restorative treatment. We think he somewhat underrates the value of an even temperature and moist warmth, as given by a poultice in the treatment of inflammation of the lungs.

Pleurisy occupies a chapter. Here a great point is made of the true source of pain in that disease. Our author contends that the sharpness of this symptom depends, not on the pleura, but on the neighbouring muscles being inflamed. In confirmation of his doctrine, he points out that the degree of agony is not at all in proportion to the real danger, and that many of the worst cases of pleurisy cause no pain at all, and, indeed, are sometimes undiscovered by both practitioner and patient; while an agonising stitch may lead to the detection of a very insignificant amount of serous inflammation.

Of "Heart Disease" we read that the real danger of death and pain lies in the exhaustion of the cardiac muscular fibres by an undue amount and a more continuous kind of work being thrown upon them. They get paralysed by strain, just as the muscles of the limbs do after abnormal exertion. The remedies are rest and ether.

After two sensible chapters on "Vomiting" and "Dyspepsia" (of which the main drift seems to advise us to cure those conditions by ascertaining their external cause, and removing that), we come upon one where the author again exhibits his destructive powers without attempting reconstruction. This is on "Jaundice," in which it is boldly asserted that the presumed connection between that discoloration of the surface and disorder of the liver is entirely baseless.

"To the ordinary observer it might seem a silly thing for a physician to insist upon his own ignorance in certain cases, and to try and persuade others that they are equally in the dark. But further consideration will lead such an one to change his opinion. He will see that the theory of jaundice being an hepatic disease, involves the belief that it can be influenced by such drugs as are believed to act upon the liver. Mercury, podophyllin, and aloes have the character of being medicines that do in some way influence the viscus in question, and consequently have been ever used in the treatment of jaundice. If, now, we cut away from such reasoners the belief that jaundice has a purely hepatic origin, we equally blast their faith in cholagogues, and then the routine practititioner is bound to prove that the medicines he uses are really more serviceable than any other. This he cannot do until he has suspended their employment."

The arguments upon which Dr. Inman rests his scepticism are the generally acknowledged facts that where the liver is found diseased, either by post-mortem examination or by indubitable physical signs during life, jaundice is exceptional; that where accumulation of bile is evident (as before bilious attacks) there is no jaundice; that, on the other hand, jaundice is found where there has been no opportunity for the accumulation of bile in the blood, as in new-born infants, or in adults after falls or operations, or suddenly from mental emotion. The writer here is surely a special pleader rather than a philosopher; for he has omitted all notice of the obstruction of the ductus communis by impacted calculi or catarrhal affections. The pressure of cancerous pylorus or duodenum, and other similar morbid conditions, which, limiting the area of the gall-passages, certainly are accompanied by jaundice. And we think that he runs a risk of leading his readers to forego, not only mercury and other so-called cholagogues, by which abstinence the patients probably have no loss, but other real deobstruents, which commend themselves to the rational mind on the best established chemical and physiological grounds. We will not, for instance, consent to shut our eyes to the advantages of ether, first noticed by Heberden only as a solvent to biliary calculi, but also remarkably efficacious in freeing the blood from the less concentrated materials of those calculi. We will not, we say, deny to a patient the chance of receiving good from such a remedy as this in jaundice, because it is at present impossible to demonstrate that the icteric tint is due to bile regurgitated into the circulation. Nor is the use of an excess of aqueous drinks (especially Heberden's "aqua marina"), as making the bile more fluid, and of animal food in increasing its amount, to be treated with contempt, because we cannot exactly trace the whole connection between the liver and cutaneous discoloration. We feel bound to raise our voice against all medical scepticism, not only because it is unphilosophical, but because there is a special temptation to it in the present generation.

The next two chapters, on "Constipation" and "Obstruction of the Bowels," form, in point of fact, one, and had better have been united. They give very sensible warnings against the ordinary routine practice of administering purgatives.

"Peritonitis" is the subject of a short but valuable essay, in which a great point is made of distinguishing those cases marked by severe pain as a prominent feature and those in which there is none. In the former it is contended that invariably the muscles in the neighbourhood will be found affected, usually in consequence of the immediate cause of the peritonitis. This makes a serious difference in the prognosis and treatment of the complaint. Into the management and possible cure of the excitant of the inflammation, whether it be cancer, tubercle, a recently emptied uterus, uræmia, mechanical injuries, or what not, the writer does not enter. What he proposes is a *processus integer* for the peritonitis itself, and that consists of opium and opium's physiological brother—rest. "Opium by the mouth, by the bowel, and by the skin, is, *par excellence*, the remedy for peritonitis. Warm applications are adjuncts to it." There is really great justice in favour of this sweeping proposition, and there does not strike us at the moment more than one exception; that is, the case of uræmia, in the peritonitis of which disease opium hastens the coma so often fatal. Here we should prefer the "warm adjuncts" without the drug.

The succeeding chapter, "On Diarrhœa, Dysentery, and Cholera," is pleasant reading; but it does not contribute much to our information, nor is it very explanatory of Dr. Inman's opinions. Perhaps he has not got any decided opinions on those heads.

In "Diabetes" the author thinks that the therapeutics of the disease have been rather retarded than advanced by separating from it what the new nomenclature now calls "Diuresis."

He would retain the ancient terms of "diabetes mellitus" and "diabetes insipidus." It is quite true that the old-established remedy, opium, is as useful in the one case as in the other; but still it strikes us that there is an essential difference in the nature of a disease where there is merely an increase of a physiological constituent of a secretion, and one in which there is added to the secretion somewhat completely foreign to its normal condition; and the recognition of this essential difference in nature must in the end lead to improved practice.

In the treatment of glycosuria Dr. Inman frequently questions the popular plan of diminishing the amount of sugar by cutting off from the diet table all its sources. He thinks it about as reasonable as cutting off the allowance of liquids from a dropsical patient. "Such a diet, instead of assisting the system to repair damages, prevents its doing so by depriving it of the materials it has been accustomed to assimilate." With the principles of action enunciated in these pages we are surprised that we hear nothing of the use of cod-liver oil in diabetes. Surely the author must have tried it, and, having tried it, he is singularly unfortunate if he has stumbled on cases which it has not suited.

In chapter xxv, "On Dropsy," we have again been surprised at being led up to the very door of the temple, and then left to find our own way in. Speaking of the chronic forms of the disease, Dr. Inman sums up the whole of our definite knowledge of the proximate cause as tracing it to an impoverished condition of blood; yet he never draws the obvious therapeutical inference that iron is the fitting remedy for it, though he must have used it hundreds of times. And though he points out what microscopists know so well, that the soaking of the tissues with water is quite consistent with a very emaciated and atrophied state, if, indeed, it be not a necessary concomitant, yet he does not lead us to the restorative employment of oleaginous remedies.

In a chapter on "Fever" there are some deservedly satirical remarks upon the various meanings attached to the word, and how unlikely it is that the profession will escape from its muddled condition till it is more philosophical in the use of terms. Might we suggest, in passing, that a step would be made in the right direction by defining "fever" to be "an increase in the temperature of the whole mass of the blood"? There is something definite in this, because one can measure the increase, and say how much and how little fever there is. Probably there would be thus brought under one class all the cases which pathologists would desire to group together, and there would be excluded some of an essentially different nature;

and if such a result follows, we shall blame the therapeutists if they do not make an equally good use of the distinction. So many physicians now habitually carry thermometers, that the world really seems ripe for the definition.

In acute rheumatism it is argued that the theoretical reason for the administration of alkaline remedies is fallacious, inasmuch as the sweat, which is described as acid, is not so when first passed, but only as a result of decomposition; while the amount of acid in the whole bulk of urine does not exceed that of health. If the theory were not a fallacy, the potash treatment of rheumatic fever would assuredly have had most striking and indubitable results; whereas, in the hands of others as well as of Dr. Inman, the best that can be said of it, in the statistics of the fortunate, is that it lessens the duration of the disease by two or three days, and the worst that can be said of it is that those so treated are ill two or three days above the average. In the Liverpool Northern Hospital Dr. Inman has had great reason to speak well of the employment of lime-juice to the extent of eight ounces a day. The patients get well quick and enjoy their medicine. Yet in some London hospitals it has proved a failure, and the poor usually detest it. Why is this? Probably because in Liverpool many of the patients were seafaring and amphibious characters, or with their blood brought into a scorbutic condition by a residence in the close alleys of an ill-built town, whereas in the metropolis such forms of acute rheumatism are exceptional at the western hospitals.

"Rheumatism," like "fever," is a word which troubles many persons by the variety of morbid states to which it is applied. Dr. Inman proposes to limit it to pain in the white fibrous tissues, commencing in the neighbourhood of joints, accompanied by a local diminution of temperature. Where red muscle is affected he would call the disease "myalgia." This appears to us too great a violence against the popular use of the term; it would not include several conditions which otherwise will be without a name; and, moreover, we doubt very much whether the strict line which its author supposes can be drawn between myalgia and rheumatism. The nomenclature adopted by the College of Physicians seems preferable. Implicitly defining rheumatism and gout to mean *pain in the organs of motion,* the College nomenclature makes the following classes distinguished by definitions which may be thus condensed:

Acute rheumatism = *Where the said pain is of a cyclical character—specific—febrile.*

Gonorrhœal rheumatism = *The same, with gonorrhœa.*

Synovial rheumatism = *The same, with excess of synovia in the joints.*

Muscular rheumatism = *Pain without fever in the red fibrous and adjacent tissues.*

Chronic rheumatism = *Pain and swelling of the joints, non-cyclical in character, without deposit of urate of soda or distortion.*

Acute gout = *Where the said pain is of a cyclical character and is externally distinguished by red swelling of hands or feet, internally by excess of uric acid in the blood.*

Chronic gout = *Pain and swelling of the joints, non-cyclical, with deposits of urate of soda.*

Gouty synovitis = *The same as the last, with excess of synovia in the joints.*

Chronic osteo-arthritis = *Non-cyclical pain, with deformity of joints.*

Anybody who is curious on points of classification will find that a complete dichotomous division can be made of these names.

Under "Myalgia" (chapter xxx) the author includes a vast number of pains which have been considered due to inflammation of internal organs or to neuralgia, to hysteria, to malingering, &c., and which he ascribes to an altered condition of muscles, fasciæ, or tendons, arising from direct injury or overwork. If his views are correct (and to a great extent certainly we go along with them), they are capable of improving much our therapeutics of these disorders.

The essay on "Neuralgia" is inferior to that in 'Reynolds's System of Medicine' by Dr. Anstie. Though advocating the same pathological and therapeutical views, it is much less forcible. The reason of this is declared to the attentive reader by internal evidence—the writer of the best essay is a victim to the disease. That is the way to make a physician think well, write well, and prescribe well, for any given complaint—let him have it himself. A most valuable collection might be made of monographical treatises written by medical sufferers. The same cause makes chapter xxxii of the volume we are reviewing an excellent one. The author has inherited gout from his father, and has himself suffered from it as soon as he became a parent. His experiences are amusing as well as instructive; he not only "grins and bears it," but he makes lookers-on grin as well, and finally comes to the same conclusion as another gouty physician (Sydenham), that patience and flannel are the best cures for the paroxysms. His practical suggestion of laying the affected foot on fur in a bandbox, as a substitute for a bed-guard, will be thankfully received. Directly the acute stage begins to abate, Dr. Inman finds that strapping the foot and leg tight enough to keep the ankle rigid is often of great service by enabling him to hobble about. He uses light white wines; but should any drink turn acid on the stomach, he takes it only warmed. Cold food of all kinds is to be avoided, especially uncooked vegetables, till the stomach has regained its powers.

The essay on "Sciatica" is a controversial one. Dr. Inman's view of the pathology of the disease is peculiar. He dissents from the theory which assigns to it a rheumatic nature, and from that which would attribute its origin to the great trunk-nerve after which it is named. He does not deny that either or both of these may conduce to the development of the diathesis; but its essential character he holds to be myalgic, that is to say, that it is situated in the muscles, tendons, and fasciæ, and not in the trunk-nerves of the affected part. We cannot say we think even the evidence he has cited points entirely in this direction, while he puts too much in the background the frequency with which pain on pressure may be traced home to the issue of the trunk-nerves from their bony covering. We cannot again follow him in the connection he sees between wasting palsy and this class of diseases. There are many cases of progressive muscular degeneration where pain is entirely absent in the early stages.

Chapter xxxiv explains the mystery before remarked upon, why the author does not believe in hysterical paralysis. He proclaims his belief "that there is no such disease as that which was designated hysteria by older writers, and is still spoken of as a complaint by many in the present day." So that all he really means to be sceptical about is the connection between the womb and that set of familiar symptoms to which it has given its Greek name. And herein we willingly go along with him, and would consent to expunge the word from our nomenclature if any workable substitute were proposed. In the meanwhile let us be content with ignoring its etymology; assuming the derivation to mean nothing, we can classify the pathology of the disease in its right connection; or, if we like, we may derive it from the Bacchanalial festivals called Hysteria (from the sacrifices of swine), where probably the nervous system of the votaries was morbidly affected. The College nomenclature has aided in this by placing hysteria between chorea and catalepsy among the diseases of the nervous system. We cannot, however, join Dr. Inman in treating hysteria by bodily repose, pity, and belief. Doubtless this plan answers when the pains, &c., are truly myalgic; but when such is the case we count it an instance of wrong diagnosis, and not to be called hysteria at all. To the emotional disease we feel sure that coddling is injurious; corporeal inactivity drives the mind to unwholesome thoughts, sympathy encourages them, and the finding a doctor to accept all their subjective sensations as evidence of a material cause confirms and nourishes those sensations. There is probably no course of conduct by which the physician can do more harm than by this, if we except medicating and manipulating the generative organs, in hysteria. We speak strongly, because the temptation to the exercise of fatal false kindness is great, and in practice we, in fact, find for one hysterical woman who has been treated harshly at least twenty whose disease is aggravated by injudicious sympathy.

We have taken the reader a somewhat lengthened course through the contents of this book, because we saw no other way of fairly conveying an idea of its homely common-sense, and its bold manner of saying out what it has to say without fearing to startle by novelty or to bore by staleness. Superficially it may seem egotistical; but at all events it is free from that self-conscious posturing to the reader, apologising for differing or agreeing, for stating something new or old, and the like, which renders nauseous much of the writing of the present day. The language has been called "vulgar" by some critics. If it be so, the same may be said of all middle-class daily speech, for we cannot see how it differs from that; and fault has especially been found with this plainness, as bringing in the public as judges on medical questions, and on this point we feel it a duty to make a few observations. All trades, professions, and employments, paid by the public, either in money or in honour, ought thoroughly to understand that by the opinion of the public alone they must stand or fall. If it is intelligent and acquainted with the subject-matter of the special art, it will appreciate what is good in the exercisers of the art, and reject the meretricious. If ignorant, it will honour the unworthy and neglect the true interests of itself and the profession. We therefore bid God-speed to every attempt made in good faith to render the technical knowledge of our art less repellant to the general public, whether by translating into the mother-tongue its scientific terms, or by adopting a lighter and more familiar style and a more anecdotal mode of illustration than is usual in medical books.

VII.—Diseases of Children.[1]

When the enormous mortality among children of all classes is considered, it seems somewhat strange that the subject of infantile disease should in past years have been comparatively neglected. Fifteen years ago no children's hospital of importance existed in London, while long before that Paris could boast of several such institutions on a magnificent scale, such as the Enfants Malades and Ste. Eugenie Hospitals. Within the last few years this indifference has passed away, and the good example set in Ormond Street has given rise to many imitators, and led to the establishment of several similar institutions in the metropolis, while only last year the munificence of a single individual has founded a new and magnificent children's hospital of larger dimensions than any at present existing in London. It is a matter for regret, however, that all this energy has not been better organized. As it is, we have a number of small hospitals, some containing only from ten to twenty beds, whereby we lose the splendid opportunities for clinical study and instruction which the larger children's hospitals in Paris so abundantly afford. For the sake of our students and younger practitioners this is much to be regretted, since, as it is, the vast majority of them conclude their studies without having acquired any practical acquaintance with a department of the profession of paramount importance in their future career, and one requiring much special knowledge only to be gained from the grouping together of large numbers of children.

It is satisfactory to find that the increased attention being paid to this subject is already bearing fruit in many excellent works and monographs, in the production of which our own countrymen have not been behindhand. The four works we have selected for review may be taken as typical of the views entertained in England, France, Germany, and America, and

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[1] 1. *Lectures on the Diseases of Infancy and Childhood.* By Charles West, M.D. Fifth edition. London, 1865.

2. *Traité Pratique des Maladies des Nouveau-nés des Enfants à la Mammelle et de la Second Enfance.* Par E. Bouchut, Médecin de l'Hôpital des Enfants Malades. 5me édition. Paris, 1867.

3. *A Treatise on the Diseases of Infancy and Childhood.* By J. Lewis Smith, M.D. Philadelphia, 1869.

4. *A Practical Treatise on the Diseases of Children.* By Alfred Vogel, M.D., Professor of Clinical Medicine in the University of Dorpat, Russia. Translated and edited by H. Raphael, M.D., from the 4th German edition. New York, 1870.

we have chosen them as enabling us to contrast the opinions of standard authorities in their respective countries.

All our readers are, doubtless, familiar with Dr. West's admirable volume, and will welcome the appearance of a new edition. No praise is needed of a book so well known, which has placed its author in the first rank of British physicians, and gained for him an enduring reputation as an authority on infantile disease.

Dr. Bouchut's work is also, doubtless, familiar to many of our readers. Like Dr. West's, it appears before us in a new edition, carefully and thoroughly revised. Much new matter has been incorporated, especially the author's views on the use of the ophthalmoscope in the diagnosis and prognosis of cerebral disease, a subject of great importance, which has as yet received but little attention in this country.

Dr. Smith's volume is as yet but little known, and we are pleased to have the opportunity of bringing it under the notice of our readers. It is in many respects one of the best works on infantile disease we have met with. The latest views are clearly discussed in it, and it has a special advantage in being one of the most reliable guides as regards practice with which we are acquainted. Indeed, we do not hesitate to place it first on the list in this respect. Admirable as are the descriptions of disease in Dr. West's work, we have always thought that he is somewhat too conservative in his views as to treatment, and too apt to recommend a severity of practice but little suited to the frail organisms with which we have to deal. In our opinion, in no class of cases is the so-called "restorative treatment of disease" so essential as in the young, and we know of no systematic work in which this fact is so thoroughly recognised as in Dr. Smith's.

Dr. Vogel's work has a high reputation on the Continent, has already reached a fourth edition, and has been translated into several foreign languages. The American translation by Dr. Raphael has but recently been published, and is a faithful and excellent rendering of the original. Although Dr. Vogel's work is unquestionably a learned and able treatise on infantile disease, we confess to a feeling of disappointment in its perusal. As is the case in many German works, the pathology of disease receives, if not an inordinate, at least a disproportionate share of attention, while the therapeutics are considered far too briefly. This renders the book more useful as a work of reference than as a guide to practice, and diminishes its value to the practitioner.

We propose to limit ourselves to the consideration of a few of the more prominent infantile maladies, and to contrast briefly the views entertained by our authors, which seems to us a better plan than a more discursive review of the books in their entirety.

There are few infantile diseases on which more has been written than croup, and yet we are far from having arrived at a unanimous opinion as to its nature. Is the true pseudo-membranous croup a disease *per se*, as most English writers have long taught, or is it, in fact, diphtheria chiefly affecting the larynx? One would think that this *quæstio vexata* should not be difficult of solution, and yet it seems by no means easy to give it a definite answer. If it be really only a form of diphtheria, as most foreign writers and some eminent physicians amongst ourselves teach, then the extremely active treatment habitually employed in this country is manifestly erroneous.

The weight of evidence seems undoubtedly to tend to the conclusion that the true croup, accompanied by the presence of distinct false membrane, is really diphtheritic.

The French writers, such as Bretonneau, Trousseau, and others, have done much to clear up the confusion which exists on this point. Bouchut, although he does not very distinctly admit the identity of membranous croup and diphtheria, and leaves us in doubt as to whether he acknowledges the existence of the former as a specific complaint, still seems, on the whole, to share the opinion of his countrymen as to the essential identity of the two, and his description of the disease, and of its pathology, includes much that is incompatible with any other theory.

Although Dr. West has greatly modified his views as to the treatment of the disease, and no longer recommends the exceedingly energetic measures he formerly advised, he still appears as the staunch champion of the classical doctrines on the subject. Croup is still with him "an inflammation of a highly acute character of the larynx or trachea, or of both, which terminates, in the majority of cases, in the exudation of false membrane, more or less abundantly, upon the affected surface."

To account for his change of treatment he has to resort to the stock explanation of change of type—a theory most convenient, but, in the opinion of many, not very tenable.

It seems to us that the explanation of the difference of opinion as to croup is really to be found in the frequency with which acute inflammatory affections of the larynx, accompanied by swelling of the mucous membrane and the characteristic spasmodic complications, but without membranous formation, are confounded with the true membranous croup. Nor is this confusion a matter of astonishment, since both have essentially the same class of symptoms, depending on the same cause—the obstruction of the air-passages.

It is certain that those children who are reported to be "subject to croup," who are described as having had several attacks of the disease, have really suffered from the first and less formidable affection.

We believe the mistake to be a most common one, and a recognition of the fact would, doubtless, explain much of the discrepant opinion on the subject. Even Dr. West's vast experience does not seem to have entirely guarded him against it, since he advises certain precautions to be "observed with especial care if the premonitory symptoms of croup appear in a child who has previously suffered from the disease, or in whose family a liability to it exists."

That the true croup is apt to recur in the same child, and be periodically recovered from, is, we believe, an assumption not supported by any carefully recorded facts. To our mind the only evidence of the existence of true croup is the actual observation of the false membranes, either on the tonsils and pharynx, or in the sputa. We have frequently observed cases diagnosed as croup in which no such evidence existed, and we do not believe that the diagnosis, at least in the earlier stages of the malady, can be made from symptoms only. When the disease has advanced so far as to cause loss of voice and symptoms of impending asphyxia there is less room for doubt, but then the presence of the false membranes can generally be made out. Still, we willingly admit that there are certain points of apparent dissimilarity between croup and diphtheria which are well worthy of careful consideration, and which, at first sight, appear to throw doubt on their identity. One of these is the frequency with which undoubted diphtheria attacks adults, and the rarity of anything in them approaching to the laryngeal symptoms of true croup. If, it is argued, diphtheria and croup are identical, how is it that the false membrane does not more often extend into the air-passages when adults are affected with the former disease? The answer to this is that children have a far greater tendency to laryngeal affections of all kinds than adults, and that the spread of the diphtheritic membrane into the larynx is favoured in the young by this tendency. It is no less difficult, indeed, to account for the absence of croup in adults on the theory of its specific distinctness from diphtheria. All that we can say is that the young have a tendency to it, which disappears after adolescence.

Then, as regards contagion and the epidemic spread of the disease, on which many would rely for a distinction, it is tolerably certain that diphtheria rarely spreads by direct contagion, while sporadic cases of the disease are constantly being met with. It is not, therefore, necessary to admit that if true croup is diphtheria, we must either have direct contagion or epidemic spread to account for its presence.

The supposed greater frequency of croup in northern and damp climates is, we think, susceptible of explanation by the frequency with which simple acute laryngeal affections are mistaken for it. We must have much more accurate diagnosis than is now common before we can allow this to weigh as an argument in favour of specific distinctness.

One would hope to gain some accurate information from post-mortem examinations. The presence of false membranes on the pharynx and tonsils may, we think, be taken as pretty conclusive evidence of the diphtheritic nature of the disease. At any rate, it is difficult to understand how they got there, and what they mean, if croup be strictly and only an acute inflammation of the larynx. The frequency with which this extended seat of the false membranes has been noticed, even by those who favour the usually received views, is worthy of attention. Not to go beyond Dr. West himself, we find that, out of ten fatal cases reported by him between 1840 and 1849, no less than seven had false membranes in the pharynx and œsophagus. In other respects Dr. West admits, and we apprehend that this is now a pretty generally received opinion, that no distinction between croup and diphtheria can be based on the anatomical character of the two diseases.

Vogel alone suggests that a distinction between the two diseases can be based on the microscopic character of the exudation. He tells us that in membranous croup "the exudation consists of band-like fibrous cords, between which pus-cells are deposited;" while in diphtheria "it consists of an amorphous detritus, in which *no bands of fibrine* and but few pus-cells can be found." From these extracts it is evident that Vogel considers the essential point of difference to be the absence of fibrous bands in the diphtheritic membrane. The distinction is at best a doubtful one, and seems to us to be entirely vitiated by his own description of the diphtheritic membrane in another part of the book (*vide* p. 92), where, under the head of diphtheria, he tells us that microscopically it consists "of granules and cells, solitary epithelium cells, and *striæ of fibrine.*"

There remains the supposed absence of albuminuria and secondary paralytic affections after croup. We say *supposed,* because we do not think sufficient attention has been paid to this point to admit of our giving a very positive opinion on it. It is only recently that the probable identity of the two diseases has been seriously considered in this country, and we are pretty certain that the presence or absence of albuminuria has as yet been ascertained in a very minute proportion of cases. For all we know to the contrary, it may be of very general occurrence, and it is certainly found in some cases. Indeed, Bouchut tells us that he has found albumen in the urine in two thirds of the cases that have come under his observation, although he does not consider that it is always necessarily dependent on croup, but thinks it may be caused by antecedent scarlatina, or may depend upon the presence of a state approaching to asphyxia.

From the absence of secondary paralytic affections not much can be inferred, since so large a proportion of cases are fatal. Still, the solution of the problem will probably depend upon the careful study of these points, and we strongly recommend them to the attention of our readers.

Dr. West makes an earnest appeal against the too exclusive use of a supporting and stimulating plan of treatment, to which his great experience necessarily gives much weight. He says, "I have met with not a few instances of idiopathic laryngeal croup which, in the hands of younger practitioners who thought of nothing but diphtheria, were being plied with stimulants and sesquichloride of iron, and were saved by antimony, by emetics, and the use of mercurials." We willingly admit the value of this warning without drawing from Dr. West experience the same conclusion as himself. The effects of the disease when it attacks the larynx are so rapid that it is essential to choose the remedies which will most rapidly obviate them, and our experience coincides with that of Dr. West in teaching that emetics are of the utmost value for this purpose. By frequent emesis the swelling of the mucous membrane is lessened, the spasm diminished, and the rejection of false membranes facilitated. Whether calomel in frequently repeated small doses aids materially by diminishing the exudation of false membrane is more open to question, but still we are not prepared to deny its utility. In place of it Dr. Smith recommends a mixture of chlorate of potash and chloride of ammonium in small doses frequently repeated, which, he tells us, is the favorite remedy employed in New York, and is, moreover, recommended by Dr. Jacobi, of the Children's Hospital in that city, who has great experience in the treatment of the disease. "The following formula, writes Dr. Smith, for these medicines is for a child from three to five years old:

℞. Potass. Chlorati, ʒj;
Ammon. Muriat., ℈ij;
Syrupi simplic., ℥j;
Aquæ, ℥ij. Misce.

"One teaspoonful every twenty minutes to half an hour, or in cases not severe every two hours. This should be continued regularly night and day, until the cough becomes looser, or until it is evident, if the case is unfavorable, that it can be of no service."

Locally, Dr. Smith advises the constant application of ice, a remedy little used in this country, and of which we have no practical experience. We may mention that Dr. Jacobi speaks of it in the following high terms:—"The only thing that I do not like to dispense with in the treatment of croup is ice, which, if anything, is the simplest, most unexhausting, and direct remedy possible. There is, in fact, no period of croup in which it has any contra-indication, although its effect is only to be considered as preventive of exudation."[1] This is high praise from a high authority, and will, we hope, lead to a fair trial of the remedy. It need in no way interfere with the more common treatment of causing the patient to breathe an atmosphere loaded with steam, the value of which is incontestable.

The advantages of tracheotomy in croup are now so universally admitted that we are not surprised to find all our authors recommending it in suitable cases. The difficulty is to know exactly when to perform it, so as neither to resort too soon to an operation which, if it does not increase the risk, is, at any rate, not one we would wish to perform unnecessarily, or to delay it so long that it ceases to be of any but the most temporary benefit.

The question is not easy to solve, and its answer must generally be left to the judgment of the practitioner, bearing in mind that the tendency in this country has often been to postpone it too long. The indication laid down by Bouchut is worthy of remembrance, as it may possibly aid us in coming to a conclusion. Like all authors, he tells us that the proper time for operating is not when the patient is in a state of semi-asphyxia, but when the first symptoms of impending asphyxia show themselves. The most reliable sign, he says, is commencing anæsthesia of the skin. As soon as this takes place, as shown by the insensibility of the child to impressions, such as pricking or pinching, then is the time to operate, even though grave symptoms seem entirely absent.

To those who see much of infantile practice there are few diseases of greater interest than tubercular meningitis. It is a complaint so insidious in its commencement, so deadly in its results, and so little amenable to treatment, that all will gladly welcome any new means of detecting it in its early stages. With

[1] 'American Journal of Obstetrics,' No. I.

regard to its essential nature, both Bouchut and Smith differ somewhat from West. The latter author, whose description of the pathology and symptoms of the disease is a model of clear and terse writing, and gives by far the best account of its protean forms with which we are acquainted, advocates the generally received view of the essentially tubercular nature of the malady. He considers, and we apprehend that the majority of our readers will agree with him, that simple meningitis, not dependent on the presence of tubercle in the meninges of the brain, and not occurring in children of the tubercular diathesis, is an event of comparative rarity. Bouchut, on the other hand, and his description is mainly followed by Smith, believes that simple meningitis is by no means a rare occurrence. Although he does not deny the frequent existence of tubercle in the membranes of the brain, he believes that in a considerable number of cases they are of secondary formation, produced by the irritation due to repeated and transitory congestions, of which the meninges have been the seats. He draws an analogy between tubercular meningitis and certain forms of scrofulous pneumonia, in which the presence of tubercular matter may be directly traced to changes undergone in the products of inflammatory exudation.

According to Smith, the minute granulations so constantly found in the pia mater, and the identity of which with tubercle is generally admitted, are by no means invariably correctly so described. "There are," he tells us, "two morbid products which may be mistaken for tubercle—one, pus which has been in great measure deprived of its liquid element; the other, plastic exudation, collected in little bodies, so as to resemble the ordinary forms of crude tubercles."

Now-a-days we are by no means so certain of the true anatomical characters of tubercle as we used to suppose ourselves, and perhaps the best criticism that could be passed on the elaborate microscopical description given by Dr. Smith would be to apply to it a very excellent observation made by Dr. Andrew Clark, that "in determining the nature of a pathological product its clinical history is of infinitely greater value than its anatomical character." Tried by this test the true tubercular nature of the disease cannot be seriously doubted, and to question it on the mere microscopical characters of certain pathological alterations seems to us a very retrograde step.

However this may be, all are agreed as to the importance of detecting, as early as possible, the incipient symptoms of this dread disease. In the premonitory stage treatment may possibly prove of service; as to its futility when the disease is thoroughly established, all are fully agreed. The early signs, however, are so vague and variable, so resemble the mere passing indispositions common to childhood, that any certain method of interpreting their meaning would be of the greatest possible value. This, Bouchut teaches, exists in the use of the ophthalmoscope. The merit of introducing what we trust may prove to be a really valuable aid to the diagnosis of many obscure forms of cerebral disease must undoubtedly be ascribed to this eminent physician, although other claimants to the honour are not wanting. We owe to his courtesy, in a recent visit to the wards of his hospital in Paris, a demonstration of the facility with which, in practised hands, the ophthalmoscope may be used in the diagnosis of infantile disease, and we see no reason why it should not be as much used and found as serviceable for this purpose as is the stethoscope in doubtful cases of phthisis.

As the subject has attracted comparatively little attention in this country, a brief résumé of the results of Bouchut's numerous researches may not be without interest.

In all forms of meningitis, whether simple or tubercular, acute or chronic, ophthalmoscopic examination will enable us to detect, in the vast majority of cases, certain well-marked changes in the optic nerve, retina, or choroid. Their precise character and amount vary considerably, and are in direct relation with the seat and extent of the intra-cranial disease. As a general rule, the amount of appreciable alteration will be proportioned to the intensity of the inflammation of the meninges, the engorgement of the sinuses, and the amount of serous effusion. As one might easily infer, inflammatory changes in those parts of the brain and its membranes more immediately in connection with the optic nerve, such as the anterior and inferior portion of the brain, are more certain to produce intra-ocular alterations.

The most ordinary ophthalmoscopic phenomena to be observed may be divided into three classes:—1. *Lesions of circulation,* such as congestion of the papilla, flexuosity, dilatation, and a varicose condition of the retinal veins. 2. *Lesions of secretion,* œdema of the papilla and retina. 3. *Lesions of nutrition,* comprising grey granulations and white spots upon the retina, atrophy of the papilla and choroid, and the formation of tubercles in the choroid. The amount and intensity of these changes vary greatly, and, we need hardly say, we have here an aid to diagnosis which requires considerable skill and practice on the part of the physician. The use of the ophthalmoscope, however, is now so universally taught, that the majority of our younger practitioners can have no difficulty in availing themselves of it. By its means many obscure cases can be explained, and the true prognosis of suspicious symptoms given. Nor can there be a doubt that many of these intra-ocular changes may be detected at a very early stage of the disease, long before undoubted general symptoms have manifested themselves, and at a time when treatment may prove of real value.

It is not asserted that ophthalmoscopic signs are invariably met with, but only that they exist in a very large proportion of cases. Out of eighty-six cases of meningitis in which Bouchut examined the eye, appreciable changes of the papilla and retina were only absent in three. Doubtless farther investigations will enable us to define more precisely the results to be expected from ophthalmoscopy in the diagnosis and prognosis of the disease, but in the mean time we are in possession of sufficient facts to render its study an object of importance to all who are interested in the matter.

It may be objected that the use of the ophthalmoscope is so difficult in children as to render it practically useless. We can only say that we observed Dr. Bouchut use it without special arrangements in the course of his ordinary visit, and that no elaborate preparations of any kind are required; a little *tactus eruditus* and practice only are necessary.

There are few infantile diseases so important and common as inflammatory affections of the chest, and it is much to be regretted that there seems to exist a good deal of confusion with regard to them. On opening one book on children's diseases we find conditions described as common which another author declares to be most rare. No one can doubt that the labours of Legendre on the subject of lung collapse have been of extreme value. They have shown us how apt this is to take place in the course of bronchitic affections, and how important it is to bear its probable occurrence in mind. It is unquestionable, however, that many pathologists have laid more stress upon it than it merits, and, in their anxiety to explain how many of the cases formerly attributed to lobular pneumonia are really due to collapse, have come nearly to overlook altogether, or at least greatly to under-estimate, the frequency of the most important class of consecutive pneumoniæ. From this error it seems to us that Dr. West is scarcely free, and in his chapter on pneumonia the distinction, both as to clinical history, fatality, and treatment, between primary or lobar, and secondary or lobular pneumonia is scarcely so sharply drawn as it might well be. The researches of Ziemzen and others have placed the matter in a clearer light, and the teaching of Bouchut appears to approximate more nearly to what seems to us to be the true view of the case. What he calls the "pneumonie d'emblée," that is, primitive, acute pneumonia, developing itself at once in a previously healthy child, after exposure to cold or some similar cause, he describes as being almost invariably lobar, generally limited to one lung, and comparatively rare in infants under a year old. By far the more common forms of pneumonia, however, are those which develop themselves out of an attack of bronchitis, or which come on in the course of hooping-cough, measles, croup, or other acute disease. These consecutive pneumoniæ, or broncho-pneumonias, as they are called by some, are of importance to understand and to differentiate, if possible, from acute lobar pneumoniæ, since they are far more fatal in their results. This is the form of disease with which lung collapse has been most often confounded, and the true explanation of the confusion so prevalent in the description of various authors will probably be found in the fact that the inflammatory affection is no doubt very frequently, if not generally, consecutive to and directly dependent on the collapse.

It seems that congestion and inflammation with exudation take place rapidly in a lobule which has collapsed, the changes in the exudation gradually producing the yellow puriform fluid, which may be observed exuding from the centre of each inflamed lobule on post-mortem examination. Hence, as Bouchut has well pointed out, too much reliance has been placed on the insufflation test as a means of distinguishing this state, since, as he states, he has frequently succeeded in inflating lung-tissue in which the evidence of inflammation was beyond any doubt.

When lobular pneumonia of this type has become pretty generalised, its distinction from lobar pneumonia by physical signs alone is almost a matter of impossibility, and we must rely on its chemical history for diagnosis. We will gain material aid from an accurate observation of the temperature by means of the thermometer. In acute lobar pneumonia there is a high persistent temperature of about 104°, lasting till the seventh or tenth day, when a *sudden* crisis occurs, and the temperature falls at once to its normal standard. In lobular or broncho-pneumonia, on the othor hand, the temperature, though elevated, scarcely reaches so high a point, and does not maintain it as steadily, while there is no *sudden* crisis and fall, but a gradual lowering of the temperature extending over several days.

That important variety of pneumonia which has recently attracted considerable attention, in which the inflammatory exudation undergoes a cheesy metamorphosis, gradually leading to the disintegration and breaking up of the affected lung, does not receive the attention it merits from any of our authors, and it is to be hoped that ere long we may be in possession of some accurate observations with regard to it.

Vogel, however, says that "in children lobar pneumonia never degenerates into tubercular, as sometimes occurs in adults, for tuberculous children generally succumb in the first few days after having acquired the croupous pneumonia."

As might be expected from Dr. West's conservative notions of treatment, he advocates a plan of management which is scarcely consonant with modern views on the subject. The conversion of so high an authority on infantile disease as Barthez from the antiphlogistic, which he at first practised, to an expectant and restorative treatment, is a fact of great importance to those who advocate the latter method, and from his conclusions Dr. West distinctly dissents. Our respect for Dr. West's opinion is so high that we are not prepared to question his assertion that in certain cases of acute idiopathic pneumonia, occurring in a robust child, the administration of antimony in the manner he recommends, "subdues the fever and abates the dyspnœa in a remarkable manner."

Dr. West would, however, probably admit that in cases of this kind the tendency is to recovery, and that the child would probably get well however treated. The danger of such a plan in the consecutive pneumoniæ, in which all our endeavours must tend towards helping the patient to tide over his illness, would be very great indeed; and we would require to feel much more certain than we do that the majority of practitioners possess Dr. West's diagnostic skill before we would like to see it generally adopted.

Vogel's objections to an antiphlogistic plan of treatment are particularly well expressed, and we strongly recommend this portion of his work to the attention of our readers.

The views held by Bouchut as to that interesting complaint which he calls "myogenic paralysis," more generally known as essential or infantile paralysis, differ somewhat, especially as regards its pathology, from those generally taught. Dr. West, although he gives an excellent description of the symptoms and treatment of the disease, does not enter into the consideration of its pathology, while by a strange and unexplained omission Dr. Lincter does not treat of it at all.

The generally received opinion of late, and one which has been gaining ground, is that the cause of the disease is seated in the spinal cord itself, and that it probably consists in a congestion of its substance, subsequently followed by atrophy. We can readily understand, therefore, why on post-mortem examination no appreciable structural alterations are met with. It must not be forgotten, however, that the opportunities for autopsies in such cases are comparatively rare, and the methods of preparing the cord recommended by Lockhart, Clarke, and others has only recently been known. It is by no means impossible, therefore, that with our improved means of observation we may yet detect changes in this complaint which have hitherto eluded observation. The whole analogy, indeed, of paralytic affections seems to point to the existence of some morbid state of the nervous centres. Bouchut, however, maintains that the "fons et origo mali" is in the muscles themselves, and points, in proof of this, to the granular and fatty changes which are always to be observed in the fibres of the atrophied muscles. We fail, however, to find in his work any proof that these changes are primary, and not secondary, as they are generally supposed to be. His views on this point necessitate his placing, in an entirely separate category, those by no means uncommon forms of infantile paralysis in which the loss of motion is merely temporary, and in which, after the lapse of a short time, perfect recovery takes place. These he designates "essential paralysis," as they can be referred to no recognisable morbid alteration. It seems to us that this is a most unnecessary division, and one which can have no other object than to support Bouchut's peculiar views as to the nature of the disease. It would not be easy to explain how these cases differ from the first mentioned, since their symptoms and causes are identical, and even in the former the majority of the muscles affected completely recover; atrophy occurring in a few of them only.

Bouchut denies that paralysis is commonly preceded by a febrile attack. The occurrence of this he looks upon as purely imaginative. Although it is by no means always present, it seems to us that its frequent occurrence is incontestable, although it need not be considered a necessary phenomenon.

The important researches of Duchenne are not regarded with any favour by M. Bouchut, who accuses him, indeed, of teaching only what he himself had formerly. Although we are not in a position to give an opinion on this claim to priority, still we think that no one who has read M. Duchenne's monograph can deny its great originality and value. He lays it down as an important prognostic indication that in those muscles in which atrophy will subsequently occur the electro-muscular contractility is lost after the first week or ten days, while in those which afterwards regain their motor power it remains unimpaired. Bouchut, on the contrary, is of opinion that the loss of contractility does not show itself until an advanced period of the disease, when atrophy has actually taken place. That Duchenne's statement is rather too sweeping and precise, all who have paid much attention to the subject will allow. We have often observed an important improvement on regular Faradisation in muscles apparently wasted, a very considerable time after the paralysis; but, on the whole, it must be admitted that Duchenne's views are borne out by experience, and that the electrical test gives us an important aid to prognosis which nothing else will supply.

Our space will not admit of any further consideration of the numerous subjects in these volumes, on which we might readily dwell. We would only, in conclusion, direct the attention of our readers to Dr. Smith's very interesting and exhaustive essay on cyanosis—a subject passed over with comparatively little attention in most works on infantile disease. They will find in it a very complete account of the structural alterations met with in this affection, as well as an able discussion of the various theories advanced to account for the peculiar coloration to which the disease owes its name.

VIII.—Dobell's Reports on Medicine.[1]

The 'Progress of Medicine' is a matter of such vast and all-absorbing interest to the profession that any attempt to "report" such progress, if ably and judiciously carried out, is sure to be received favorably, and in proof that such an undertaking is likely to be successful, it is only necessary to point to the long-continued and prosperous career of two well-known works which we noticed very briefly in our April number—'Braithwaite's Retrospect' and 'Ranking's Half-yearly Abstract of the Medical Sciences.'

As still further evidence of the estimation in which such works are held, we may refer to the approbation bestowed on the 'Year Book of the New Sydenham Society,' which, although now appearing in a biennial form, still holds a high place in the favour of the members of that very thriving Society, and indeed in the estimation of all the reading practitioners of the day.

It may be permitted to us, perhaps, to go even a little beyond the before-mentioned instances and to allude (as we think ourselves fairly entitled to do), with feelings of complacency and satisfaction, to our own 'Chronicle of Medical Science,' containing short "reports" on materia medica, pathology and principles of medicine, toxicology, micrology, physiology, surgery, midwifery, &c., these divisions being under the supervision of gentlemen well known to science, of the highest and most unquestionable ability, and but little

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[1] *Reports on the Progress of Practical and Scientific Medicine in different parts of the World.* (For the year beginning June 1, 1868, and ending June 1, 1869.) Edited by Horace Dobell, M.D., &c.; assisted by numerous and distinguished coadjutors. London, 1870. Pp. 616.

likely to let any points which "represent progress" to be overlooked, albeit that Dr. Dobell says in his *advertisement to his first year's reports,* "he is aware that in reading a work for the first time the points which represent progress are apt to be overlooked by a reviewer."

Now, it is true that the works we have mentioned, including our own 'Chronicle of Medical Science,' contain only comparatively short abstracts from the writings of men who are doing real service for the "progress of medicine," but, nevertheless, these abstracts are sufficiently extended, *when the importance of the subject requires it,* to give not only the pith of the writer's ideas and the fulness of his meaning, but even the very elaboration of the author's views, thoughts, and reasoning is reproduced; so that nothing may be missed that can be considered important for the reader to be made acquainted with.

Such being the case, we took up Dr. Dobell's comely, well-printed, 'Reports on the Progress of Medicine in different parts of the World,' with the feeling that he was about to initiate a new order of things, and that the profession would receive at his hands a series of original communications, or, as he says in his preface, "original and independent reports from all parts of the world, written by distinguished men resident in the countries which they represent," reports, moreover, asserted to differ "widely in scope and intention from the Half-yearly Abstracts—'Biennial Retrospects' and other works of the kind hitherto published, but a glance at the contents soon served to dissipate the illusion, and to show that although in a very few notable instances the volume does contain "original and independent reports," by far the greater part of it is occupied with extracts derived from the 'Medical Times and Gazette,' 'Lancet,' 'Medical Press and Circular,' 'British Medical Journal,' 'Guy's Hospital Reports,' 'London Hospital Reports,' 'St. Bartholomew's Hospital Reports,' &c., &c., *et hoc genus omne*: add to these some letters, which might not improperly be considered "private," from gentlemen who regret having neither the time nor the ability to furnish the reports asked for, and the general character of the book may be said to have been sufficiently described.

It is always so much more satisfactory to have to speak in terms of commendation of any worthy attempt at enlightening the bulk of the profession than it is to feel compelled to pronounce an unfavorable judgment, that we are almost bound, in justification of the adverse opinion we have arrived at, to give something like a running commentary on the contents of a volume heralded forth with the pretentious title of "Reports on the Progress of Medicine in different parts of the World."

A "review" of the work, in the usual and ordinary acceptation of the term, is quite out of the question, for whilst the apologetic strain of the editor's preface almost forbids criticism, the arrangement of the different articles, if not their very nature, defies it.

After introducing a comparative table of weights and measures—taken from Squire's 'Companion to the British Pharmacopœia'—as being useful "for the purpose of reference when reading the reports from foreign countries;" the first quarter of the globe we are introduced to is "Africa," and the first "original and independent report" is a transcript from Dr. Horton's work on the climate and meteorology of the west coast of Africa: the editor says, "the following tables are transcribed from Dr. Horton's work as the best and most recent observations obtainable upon the climate of the west coast of Africa," then the tables themselves follow. Of their accuracy and usefulness there can be but little doubt, but to dignify quotations, and tables "transcribed" from any work, however good, with the title of an "original and independent report," *i.e.* for this particular book, is simply preposterous.

We next have a letter, fourteen lines in length, from the Rev. H. Callaway, M.D., of Spring Vale, Natal, who in a straightforward plain way says that he cannot furnish any report, that he is living in a wilderness, has no time to read up even the 'Lancet,' and that there is no man in the colony who possesses any literary taste.

Again, we have a letter beginning "Dear sir" from C. W. Browne, M.R.C.S., &c., district surgeon, Seymour, Stockhamstown, who regrets being unable to furnish a "report," but wishing to say something writes a letter about the climate, although he has no meteorological instruments, and then in conclusion ventures to give an abstract of his "own case," as showing the influence of the climate on pulmonary disease. This letter occupies two pages, and the final sentence of it suggests the desirability of any one coming to his neighbourhood being provided with "an earth-closet."

These two letters, and the "transcribed tables" from Dr. Horton's work, constitute *the whole* of the "original and independent reports" from that small portion of our habitable globe called "Africa!"

The next heading is ALGERIA, and for this we are requested to see RIVIERA and ALGERIA, which we purpose doing when we arrive at the proper page.

ALTITUDE follows ALGERIA; we do not know the place, but the direction fixed below the word "altitude" enlightens our ignorance. (For calculations on the effect of altitude see "Experiments," &c.; see also index.)

AMERICA is now placed on its trial, and comes out of it certainly a little better than "Africa" did, and this we partly anticipated would be the case from the following announcement made by the editor: "I was particularly desirous that America should be well represented in this work, and therefore wrote to the leading medical men in New York, Boston, Philadelphia, and other places, explaining the objects and plan of my projected report, and asking for their co-operation."

As a result, America occupies about twenty-two pages out of the six hundred and sixteen constituting the book, and the aforesaid "twenty-two" contain a letter from "The Society for Reporting the Progress of Medicine;" a "Report on the Climatology of Consumption, with special reference to Minnesota, by Brewer Mattocks, M.D.;" and a "*Résumé* of the Progress of Laryngoscopy and Rhinoscopy, by A. Ruppaner, M.D., New York" (for further details see "Dr. Ruppaner's work on this subject"); this parenthetical addition being, we suppose, the editor's, although his initials are not attached to it.

We leave the letter from the Society for Reporting the Progress of Medicine, Dr. Brewer Mattock's report on a special subject, and Dr. A. Ruppaner's *résumé* of one equally special, to tell their own tales as to the progress of medicine in America.

We now arrive, alphabetically it may be imagined, at BIARRITZ, "Report by Chas. E. Chapman, M.D., of Biarritz." This report fills six pages, and its object is simply one of climatology.

We have here reached CHINA, and grieve to find that so little can be said for so large a place. Hankow furnishes a "Report from the Wesleyan Medical Mission, Hankow, by F. Porter Smith, M.B. London." As the whole "report" occupies the whole of one page, we must decline any attempt at epitomising it.

CIDER COLIC comes now before us. "Cider Colic, by James Buckman, Esq., F.R.G.S., late Professor at the Agricultural College, Cirencester." This report *extends!* over two pages, and explains pretty clearly how and where to get the complaint, and what it is like when you have got it.

CLIMATE stands next in order, and though not exactly a "country," yet as it assuredly belongs to *any* and *every country*, its position here is certainly a very legitimate one. "On Change of Climate in the Treatment of Chronic Diseases, by Thomas More Madden, M.D., &c., Assistant Physician, Rotunda Hospital, Dublin." It would appear that climatology stands some chance of being done to death in this work, but, notwithstanding this, it is only fair to Dr. Madden that we should say these 'observations' (for such he styles them, and *not* a 'report') will be read with pleasure by any one who has not already become surfeited with the various publications on the subject with which of late we have been inundated. This article reaches from page 38 to page 58, and at the bottom of this page we are referred for further information concerning "climate" see index.

CONSUMPTION and other WASTING DISEASES head page 59. "On Pulmonary Consumption and its Treatment by Ether and

Etherised Cod-Liver Oil, by Balthazar W. Foster, M.D. &c., Birmingham."

This communication by Dr. Foster is merely a reiteration of the principal points contained in a paper which he read before the British Medical Association at the annual meeting held at Oxford two or three years since.

"On the Use of Pancreatic Emulsion of Fat in the Treatment of Consumption and other Wasting Diseases in Ireland, by R. I. Kinkead, A.B.L.M., Dublin," comes next, and it is to be supposed represents *one* of the reports on the "progress of medicine in Ireland," although, taking it for granted that Dr. Dobell intended to carry out his alphabetical arrangement, we have not yet reached the letter I.

Dr. Kinkead says his report is "deficient in two respects," and when he defines the causes of these deficiencies it is tolerably evident that the report, however carefully drawn up, is neither more nor less than a very unreliable one, and this, indeed, appears to be the light in which Dr. Kinkead views it.

From Ireland we take just one little jump, and here we are in Portugal with another report "On the Use of Pancreatic Emulsion, &c., in Portugal, by E. H. Brandt, M.D." This "reporter" gives, *in half a page*, a very favorable account of the effects produced by "pancreatic emulsion."

Another jump, but this time home again to Scotland, from which place we have a report, *a third of a page in length*, "On the Use of Pancreatic Emulsion, &c., in Scotland, by Patrick Heron Watson, M.D., F.R.S.E.," and this gentleman also thinks well of the emulsion.

Now we hear from Prince Edward's Island, and we have a report from the pen of W. H. Hobkirk, M.D., &c., and again it is "On the Use of Pancreatic Emulsion." This contributor thinks so highly of the emulsion, and has had so many opportunities of giving it a fair trial, that *ten lines* are sufficient to exhaust all he has to say on the subject.

We scarcely know whether to consider the next article as a communication or as a sort of précis from some paper read by Dr. L. Down before the Clinical Society of London. However this may be, it is headed "On the Use of Pancreatine," and has Dr. J. Langdon Down's name attached to it. It covers a page and a half, and is the narrative of the cure of a man who had "fatty stools," and who appeared to have been much benefited by taking "pancreatine," but as the man continued to improve, even when *not* taking pancreatine, Dr. Down seems to have been not a little puzzled to account for his uninterrupted good health.

Cretinism of the Cottswold Hills.—This is a letter from Dr. Douglas Watson, Chalford, near Stroud, which may be considered as not saying *anything* beyond holding out a hope of *something to come in the future:* it is (*i.e.* the letter) at least half a page in length.

Denmark and Sweden.—"Report by Vald. Rasmussen, M.D., &c., Copenhagen (Translated by Thorlakur O. Johnson, Esq.), the Contagiousness and Inoculability of Pulmonary Consumption (Phthisis) and Tuberculous Disease." It is really quite a relief to come at last to something which merits the designation of an "original and independent report," and if space permitted we should gladly devote some time in drawing attention to this very interesting and admirably drawn-up paper, but although we have already expressed a general condemnation of the book, we specially stated that there were some few reports to be specially excepted, and this report of Dr. Rasmussen's is one of them. It will well repay perusal.

It will be observed that we have again entered on the "alphabetical arrangement"—that is to say, *for a time.*

Experiments.—"Projected Experiments and Investigations." Then follows an account of a projected therapeutic experiment which did *not* take place because it was not likely "to pay."

Next we have, from the pen of the editor, and under the head of our former friend Altitude, "Suggestions for Observations and Experiments at great depths below, and great heights above, the Level of the Sea." Another short paper follows, written by the editor, and this time *extracted* from the 'Chemical News' of Sept. 4th, 1868.

Minchinhampton Common as a Health Resort.—Seven short uninteresting letters on this subject serve to fill up three pages; and then we hear from the

Falkland Islands.—"Letter from Horner W. Watts, M.D., Colonial Surgeon, Falkland Islands." This letter states that the population is very small, the islands are salubrious, there is very little sickness and no poverty, and so on. This is the report from the Falkland Islands, and it occupies half a page.

A brief communication from Henry Smith, F.R.C.S., surgeon to King's College Hospital, is the next "report." It refers to fistula and tuberculosis and their presumed association. It fills a page.

Food, Heat, Motion.—Under this head we again hear from the editor. The paper is three pages in length, and whatever merit it may have it is so infinitely inferior to the communication read by Professor Haughton on the same subject before the British Medical Association, at Dublin, that as Dr. Dobell has made use of "extracts" pretty freely, we wonder he did not avail himself of Professor Haughton's paper, in its entirety, and acknowledge the source from which he obtained his material!

France.—"Report for the year, June, 1868, to June, 1869, by Professor Villemin, Val-de-Grâce, translated by George Gaskoin, Esq." We cannot resist again expressing our satisfaction with encountering, midst the heterogeneous mass of nothingness of which these reports principally consist, a veritable report from the pen of a well-known and distinguished man. Professor Villemin occupies eighty-six pages with reports on anatomy, physiology, general pathology, internal pathology, external pathology, ophthalmology, &c., &c. The paper is a valuable one, and the editor should rejoice, as no doubt he does, at having it in his power to present his readers with such a luminous and lucid exposition of the progress of medicine in France. This paper, with a *précis* obtained from different French publications, forms about one sixth of the whole volume.

Germany.—"Report by Julius Althaus, M.D., &c.; Paralysis." This report covers twenty-nine pages, and is all very well as far as it goes, consisting, as it does, of mere extracts from a long list of German publications. As a task Dr. Althaus has executed the "culling" and "collecting" process he has had recourse to in a very creditable manner, and our only exception to the report is, it is not "original and independent" *according to the act.*

Hospitals, &c.—"On Hospitals, Infirmaries, Dispensaries, &c., by Percy Leslie, M.D. Edinburgh (of Birmingham)." Some eight pages are occupied by Dr. Leslie in an attempt to describe a variety of medical (charitable) institutions, and to point out their excellencies and their defects.

"Abstracts of Reports from Hospitals and Infirmaries," by R. Shingleton Smith, M.D. London, &c. Here follows that long list of *abstracted* reports from the various journals, hospital and infirmary reports, &c., that we commented on when commencing this notice: the last hospital in the list that is laid under contribution being the Royal Hospital for Diseases of the Chest, London, "the birthplace," as the editor calls it, "of pancreatic emulsion." These abstracts, with which Braithwaite and Rankin abound, fill very nearly one twelfth of Dr. Dobell's volume—that is, something short of fifty pages.

Iceland.—Report by J. Hjaltelin, M.D., Chief Physician, Iceland. This is another of the "original and independent reports," and the reader who is interested in the country of Iceland and its special diseases cannot fail to derive both pleasure and profit from its perusal.

India (Hindostan).—The entire of the report from this important part of our dominions is collected in the shape of abstracts from the current Indian medical literature, viz., 'Indian Annals of Medical Science,' 'Indian Medical Gazette,' &c. &c. &c. Eight pages suffice for this purpose.

Italy.—Absolutely no report at all from Italy. There are two short extracts from two letters received by the editor from Dr. Sammut, dated from 'Napoli,' and from these we gather that at some future time "a report" may probably be received.

JAVA AND MADURA (forming the Netherlands—India).—Report by J. R. Wylie, M.B., &c., Batavia, &c. This important and large colony, containing a population of seventeen and a half millions, has its "medical progress" reported in four pages and a half. Dr. Wylie, in some concluding remarks, says, "I feel this attempt on my part to be too short to be creditable."

LEPROSY.—"Note on Leprosy, by Tilbury Fox, M.D.," &c. This "note" takes up half a page, and consists of, it appears to us, four brief paragraphs, disrupted, so to say, from a letter sent by Dr. T. Fox to the editor; it is addressed from 43, Sackville Street, Piccadilly, W., and is dated October 5th, 1869.

MECHANICAL APPLIANCES, INSTRUMENTS, AND INVENTIONS.—"Report on Mechanical Appliances, Instruments, and Inventions, in different parts of the World, by Heather Bigg." Mr. Bigg fills fifty pages with a long list and a short description of an infinite variety of instruments and appliances invented and made by various persons in different parts of the world. Mr. Bigg's reputation as a surgical mechanist is a high one, and his name figures very conspicuously as an inventor and maker of many of the most ingenious and useful appliances.

Amongst the instruments described as being useful for correcting spinal curvature, we were a little surprised—we might almost say alarmed—to find one suited for remedying "croquêt curvature." The youth of both sexes should be put upon their guard, for hear what Mr. Bigg says:—"One of the most remarkable features attached to the *furore* for "croquêt," which has animated the young of both sexes during the last few years, is the form of lateral curvature which its excess produces." Then follows a description of this particular curvature, and lastly there is given a description of the instrument used for correcting the evil that croquêt has occasioned. Might we venture to suggest that this description (with, perhaps, a picture of the instrument), if placed in each box of the croquêt mallets, rings, and balls, might have a very deterrent effect on "the young of both sexes," if at all disposed to push croquêt to an extreme. Fancy the young people reading as follows:

"The instrument employed in these cures consists of a pelvic band with two lateral crutches; also two vertebral levers with padded plates, the right one resting on the upper part of the thorax, just beneath the armpit, whilst the left embraces the dorso-lumbar ribs on the left side. A thigh lever, having a free hip-joint, is also attached to the left limb, for the purpose of making the dorso-lumbar plate act as a fulcrum for expanding the spine.

"This instrument acts most powerfully, and soon restores the spine to normal form."

We should think so, and the very idea of it, no doubt, would serve as "a fond farewell" to croquêt.

Natal.—See "Africa." This we have done, and we remember how we profited by it.

Newfoundland.—Letters by William Anderson, M.D., &c., of Brigus. Two short, but not altogether uninteresting letters from Dr. Anderson, comprise this report; they, *i. e.* the letters, occupy four pages.

New Zealand.—Report of New Zealand, by William G. Kemp, Surgeon to Nelson Hospital, Asylums, Gaol, &c. This report, twelve pages in length, is altogether devoted to meteorology and climatology. Dr. Kemp concludes it thus:

"I cannot lay claim to much originality in the foregoing report, as it is compiled from a pamphlet by Dr. Hector, F.R.S., Government Geologist, to whom I beg to accord my warmest thanks for much valuable information and assistance; but I have been induced to send it as my contribution to your "report on progress," as by this means it will become more widely circulated and read, and will, I trust, be found useful and interesting, in a practical as well as in a scientific point of view."

So that here again we have a *réchauffé* doing duty as an *original and independent report.*

Oporto.—See "Portugal."

Paraguay.—"A Traveller's notes on the more prevalent Diseases of Paraguay." By F. Masterman, late Assistant-Surgeon, Paraguay Military Service. Six pages and a half, relating principally the personal practical experience of the writer.

Portugal.—"Report by E. H. Brandt, M.D., Knight of the Tower and Sword (of Oporto)." There is much interesting matter in this report, and the twenty-six pages of which it consists seem to be as well entitled to be considered "original and independent" in their contents as anything we have met with in the book.

Prince Edward's Island.—See also "Consumption," &c. Report from Prince Edward's Island, from W. H. Hobkirk, M.D., Charlotte Town. Another short letter, and "a general statement" which is nearly as brief, constitute the report from Dr. Hobkirk. The two, that is, letter and general statement, are spread over two pages.

Riviera and Algeria.—"Report on the North Shore of the Mediterranean during the Winter of 1868-69, and on Algeria in Spring, by Henry Bennet, M.D.," &c. &c. "Progress of medicine" is again represented in this report by seven and a half pages on climatology, written by Dr. Henry Bennet, of "London and Mentone," and in his usual facile style.

Scarborough.—"Report from Scarborough, by Cornelius B. Fox, M.D., Scarborough." An admittedly imperfect report of the climatology of Scarborough, indicating once again the "progress of medicine;" and, sad to say, we are threatened with more of it.

Sulpho-carbolates.—Notes on the Sulpho-carbolates as Therapeutical Agents, by A. E. Sansom, M.D., &c. These "notes" by

Dr. Sansom, a colleague of Dr. Dobell, at the Royal Infirmary for Diseases of the Chest, refer especially to the introduction of the sulpho-carbolates by Dr. Sansom as remedies for certain zymotic diseases. In ulcerated throat, diphtheria, and scarlatina, Dr. Sansom seems to have made use of them, and with very satisfactory results; but at present they are on their trial, and our trial of them has not, so far, satisfied us of their usefulness.

TUBERCLE AND FISTULA.—See "Fistula."

As this is the last time we are requested to "see" some other article for the information that is to be given, we may take occasion to observe that it is tiresome to the last degree to be bandied about incessantly, as in these "reports," from "Consumption" to "Tubercle," from both of these to "Pancreatic Emulsion," and from all three to the "Index;" then again from "Prussia" to "Germany," and from "Germany" to "Prussia," from "Natal" to "Africa," and just the other way, &c. &c. &c. In fact, there is such a continuous system of "ringing the changes" in this respect, that the reader is irresistibly driven to the conclusion that the editor has been in excruciating difficulties for matter with which to fill his book; and this is only rendered the more unmistakably apparent if he should happen to be deluded into following the directions given to "*see*" such and such an article, as, in almost every instance, it will be found that the opinion expressed so repeatedly by Sir Charles Coldstream (in the play of '*Used Up*'), viz. "*there is nothing in it,*" is fully corroborated.

UNITED KINGDOM OF GREAT BRITAIN AND IRELAND.—The following report has been compiled from materials furnished by Dr. Brakenbridge, P. W. Squire, Esq., Dr. Stoker, Dr. Strange, Dr. Tidy, Dr. Trentler, Dr. Wahltuch, and others.

We have now arrived at the last division of the work; it commences at page 433, ending at page 616, and therefore goes a considerable way towards forming one third of the book.

As this report is admitted to be a "compilation," one is really disarmed, and *so far* prevented from further remark; but one cannot avoid again saying that with the very few exceptions we have already called attention to, this work consists *really* of "abstracts" from various medical publications issued either at home or abroad, and of some letters which, though not irrelevant to the subject matter of the work, might, nevertheless, find a more appropriate place in the privacy of the editor's desk, rather than in the publicity afforded in his published work.

The gentlemen (or is it the editor?) who have effected this compilation forming the report for the "United Kingdom" have introduced the various subjects with some prefatory remarks, taking up about half a page. They say, in commencing, "Science, of whatever department, advances by continuous progress." Why, of course it does; and surely nobody required to be told this, for immediately progress *ceases* to be *continuous* it ceases to be "progress," neither "a stand still" nor "retrogression" admitting of such an appellation. Further on they say—"Hence, in the following articles the subjects are not put forward as facts established beyond question, but as a collection of some of the most interesting and valuable of the materials of scientific medicine that have accumulated in one year."

We can only say that any one who confines himself simply to the relation of the "facts of medicine" will inevitably have his remarks restricted within a very narrow limit; but, even with the necessarily wider range taken by the compilers of these extracts, we are obliged to demur to the "fact" of their having drawn their information from "some of the most interesting and valuable of the materials of scientific medicine that have accumulated *in one year*."

Much, very much, contained in this "report" is *stale*, having appeared in various journals and other publications, not during last year *only*, but several years previously, so that our supposed new acquaintances are without difficulty recognised as "old friends" of much more than a year's growth; for instance, surely it is more than twelve months ago since we heard first of a certain preparation, of which Dr. Dobell is the inventor, the Royal Infirmary for Diseases of the Chest "the birthplace," and Messrs. Savory and Moore the manufacturers. Surely Dr. Dobell's little book on 'Tuberculosis,' and that on the 'True First Stage of Consumption,' were published, the former in 1866 and the latter in 1867. Are the extracts taken from the *third* volume of 'St. Bartholomew's Hospital Reports' only of last year's accumulation? But we might repeat these interrogatories *ad infinitum*.

The perplexities of an "editor" are always supposed to be many, and Dr. Dobell could scarcely expect to be more fortunate than "editors" are generally believed to be; but he appears to have had all manner of difficulties heaped upon him, as though the fates had conspired together to thwart the publication of his "reports." As we have had occasion to notice, some of his reports are even now *in nubibus;* others have been at one part of the world whilst their writers have been at another; and we are told in the preface that many important communications are *on* the sea, whilst others are *in* the sea; and it may be supposed that some are *under* it. Even the reporters themselves may be said to have proved recalcitrant, a few of them dying before giving up the reports they had promised to furnish.

With such a heavy list of obstacles we think Dr. Dobell would have best consulted his own reputation as an able editor and the credit of his "reports" by delaying the publication of these until such an amount of original and independent matter had gravitated into his possession as would have allowed of his carrying out the intention, we take it for granted he had, when first entering on his undertaking.

Presented to the profession in their present form, we fear that the 'Reports on the Progress of Medicine' will not prove an inducement to any one who has expended his money on the *first* volume to invest a like amount of capital in the purchase of the *second*—we mean, should a second volume appear.

IX.—Spectrum Analysis.[1]

In this series of lectures Professor Roscoe has delineated in an elementary manner the chief outlines of the vast subject of one of the greatest discoveries of modern times. In the appendices, which are of considerable length, he has entered into many refined details which it would have been unwise to have attempted to incorporate in the lectures themselves. The whole forms a handsome volume of high typographical excellence, and we regret that in attempting to give an analysis of the work we cannot set before our readers the woodcuts with which it is profusely illustrated, and which artistically are most meritorious.

We must here stop to compliment the Society of Apothecaries for having afforded Professor Roscoe the opportunity of delivering these lectures. It is, perhaps, not novel, at all events not unprecedented, that the body which has elevated and formed public opinion should be swept away by the public opinion it has itself created. Should the present disposition of public opinion upon the subject of medical education lead to the superseding of the Apothecaries' Company, we shall not the less willingly admit the great advantage it has been to our profession. Indeed, it is our opinion that but for its exertions in raising the standard of medical education the present position of that question would have been impossible. And it is not a little singular that it, and not any of the great universities or other purely medical corporations, should have been the foster-mother of a great discovery, which even now has some, and may come to have great, bearings on scientific medicine.

The greater portion of Professor Roscoe's first lecture is devoted to an exposition of the great discoveries of Newton in the composition of light, especially with regard to the different refrangibilities of the various elements of the solar spectrum. In these experiments

[1] *Spectrum Analysis. Six Lectures delivered in* 1868 *before the Society of Apothecaries of London.* By Henry E. Roscoe, B.A., Ph.D., F.R.S., Professor of Chemistry in Owen's College, Manchester. With Appendices, Coloured Plates, and Illustrations. London, 1869.

Newton allowed the light to fall on the prism from a *round* hole in the shutter. He did not, therefore, obtain a pure spectrum, but a series of spectra, an overlapping the other, owing to the light coming through different parts of the round hole. Had he allowed the light to pass through a fine vertical slit he would have observed that the solar spectrum was not all coloured, as it appears to us in the rainbow, but that at intervals the colour is interrupted by a number of dark lines, which we may regard as shadows in the sunlight, spaces where certain rays are absent. This fact was first observed by Dr. Wollaston, who, making use of a fine slit of light, discovered these dark lines, and described them in the 'Philosophical Transactions' for 1802. These lines are the roots of the science of solar analysis. They are always found in the same position in the analysed sunbeam, whether we take this direct from the sun or after it has been reflected by clouds, or moon, or planets, or otherwise diffused. The fixity of these lines and the exact mapping of their relative positions is of the highest importance, because their existence proves conclusively, by a chain of argument which we shall presently understand, the existence of iron, sodium, and other well-known substances, in the solar atmosphere. These lines of Wollaston were in 1814 more minutely examined by the German optician Frauenhofer, who mapped no less than 576 of them. He further ascertained that the light proceeding from the self-luminous fixed stars also contains dark lines, but different ones from those of sunlight. This fact led him to this remarkable conclusion: that whatever produced these dark lines—and he had no idea of the cause—was something acting beyond and outside our atmosphere, and not anything produced by the sunlight passing through the air. All subsequent investigation having justified these his conclusions, it is not to be wondered at that these lines, though first noticed by our countryman Wollaston, are now known to philosophers as the lines of Frauenhofer.

We now come to consider (Lecture II) more precisely the rationale of spectrum analysis.

If any solid body, such, for example, as platinum wire, is heated gradually up to a white heat, we find that at first no light rays at all are given off. As it becomes red hot it emits red rays only. Carrying the heating process further, we see orange, then yellow, and in succession green, blue, until at white heat it emits all the rays of light. But when we come to deal with gases instead of solids all this is changed. To whatever temperature a gas is raised, whether by the bluish flame of burning sulphur, one of the coldest flames we can obtain, or by the hydrogen flame which has a temperature of 3259° C., or even by the oxyhydrogen flame with its eight thousand degrees of heat (Centigrade), the colour of the light it emits is still the same. The proof of this is simple. Certain metals and their salts, when burnt, produce coloured fires; thus, sodium produces yellow fire, strontium and lithium red fires, and so on. Now this colour is produced by the incandescence of the vapour of the metal, that is by the heating of the metal in a gaseous state. When by burning a strontium salt we produce red fire we do so by heating the strontium till it is vaporised, and by then heating this vapour or gas. Whether this is done by the flame of burning sulphur, or by the oxyhydrogen flame, which is seven or eight times hotter, we still get red fire. The same is true of the yellow sodium flame. We may put this fact in another form and say that whatever temperature you employ you cannot make strontium vapour more than red hot, or sodium vapour more than yellow hot. You cannot raise either of them to a white heat. Not even by an electric spark, the temperature of which is so high that it has never been measured.

The methods by which we can obtain bodies in the state of luminous gas vary with their nature. But what is true of sodium and strontium is true of matter in general. It belongs to every chemical element, and if we can by any method get the vapour of a chemical element so hot as to become luminous, we find that the light emitted by it is peculiar to itself, and is distinctive of that special body, whether under ordinary temperatures and pressures the element be gaseous, solid, or liquid.

Here then we have reached the fundamental fact upon which the science of spectrum analysis is based, and by means of which we can detect the presence of any elementary body which can be obtained in the state of glowing gas. It is, perhaps, scarcely necessary to point out that it is only certain bodies which can be thus treated. Organic bodies are, of course, destroyed by such temperatures, and we cannot in this way ascertain their presence.

It may be well here to describe briefly the mechanism of a spectrum analyser, though it is difficult to do so without the assistance of illustrations.

The simple instrument first used by Bunsen (the great founder of the science), consists merely of a common hollow prism, filled with carbon bisulphide, placed in a box; a telescope at one end of the box, whilst the other is perforated by a tube carrying a lens; this admits the light produced by the incandescent body under examination. The requisite heat is obtained from a Bunsen burner, which produces a colourless flame from a mixture of air and coal gas. The light falling on the prism is split up into its constituent parts and viewed through the teleseope.

The accuracy of the analysis is greatly increased by the use of an instrument made by Steinheil of Munich. The principle is the same as that of the one just described, but we can see at the same time two flames. In one we burn some of the substance which we suppose to be contained in the body which we are investigating. One flame being immediately above the other we can see at a glance whether the lines in one spectrum coincide or not with those of the other. For example, we have a body which we suppose to contain sodium. We place it in one flame, and in the other we place some undoubted sodium. We see at once whether the spectrum of the supposed sodium compound is the same as that of the known one.

The great advantage of the spectrum as a means of analysis consists in its extreme delicacy.

$\frac{1}{180,000,000}$th part of a grain of soda can easily be detected. Soda is always present in the air; all bodies exposed to the air, show, when heated, the yellow sodium line. If a book be dusted near the flame the sodium reaction will be seen.

$\frac{1}{6,000,000}$th part of a grain of lithium can be easily detected. This, which was formerly supposed to be one of the rarest of metals, now turns out to be almost omnipresent. Almost all rocks, sea and river waters, the ashes of most plants, milk, human blood and muscular tissue, contain it.

$\frac{1}{1,000,000}$th part of a grain of lime can be easily detected. The practical character of this method is well illustrated by Bunsen's discovery of the new metals cæsium and rubidium. Shortly after his first experiments with spectrum analysis, he happened to be examining the alkalies left from the evaporation of a large quantity of mineral water from Dürckheim, in the Palatinate. Haviug separated out all other bodies, he took some of these alkalies, and found, on examining spectroscopically the flame given off by these mixed salts, that some bright lines were visible which he had never before seen, and which he knew were not emitted by either soda or potash. So much reliance did he place on this new method of analysis, that he at once set to work to evaporate so large a quantity as forty-four tons of this water. From this evaporation he obtained about 400 grains of the mixed metals.

In the hands of our countryman Mr. Crookes, the spectroscope has been the means of discovering another new metal, the now well-known thallium, the vapour of which yields one bright green line in the spectrum. This peculiarity has supplied the metal with its name, derived from *θαλλος*, a young shoot. Indium, so called from its producing two indigo-coloured lines, also owes its discovery to the spectroscope, in the hands of Messrs. Reich and Richter, of the celebrated Mining School of Freiburg.

By means of the spectroscope we can distinguish differences in ordinary gases; that is to say, in those substances which are gaseous at ordinary temperatures and pressures.

When we pass the electric spark through a gas, it becomes heated up to a temperature far higher than anything we can obtain by means of flames, and when thus heated the gas gives off the light which is peculiar to itself. Thus we find that, according to the nature of the atmosphere which surrounds the spark, the colour of that spark varies. Thus, if we experiment with hermetically sealed tubes containing each a small quantity of gas (known as Geissler's tubes), and pass an electric spark through them, we shall find that a hydrogen tube emits a red, a nitrogen one a yellow, a carbonic acid one a bluish, light. All this is visible to the naked eye. When we proceed to examine them through a spectroscope, we find that these naked eye appearances admit of decomposition. When we decompose the peculiar red colour which the hydrogen exhibits, we find three distinct bright lines. One bright line so intense as almost to overpower the others, thus accounting for the red colour which alone is presented to the unaided eye. In the same way strontium vapour which appears to the naked eye to be merely red is found by the spectral decomposition to contain also a blue element. The second line of the hydrogen spectrum is bright greenish blue, and the third one dark blue or indigo. Indirectly, the hydrogen spectrum furnishes a delicate hygroscopic test. If an electric spark be passed through moist air, the aqueous vapour is decomposed and the bright red hydrogen line appears. This does not occur if the air have been previously thoroughly dried.

The nitrogen spectrum is more complicated than that of hydrogen but perfectly definite and characteristic.

From certain observations of Plücker, Hittorf, and Frankland, it is found that some variations may be produced in the spectrum of some gaseous bodies (including nitrogen and hydrogen), so that they can be made continuous. This, however, in no way interferes with the fixity of position of the bright lines, nor can it influence the deductions derived from that fixity.

Each of the non-metallic elements yields its own characteristic spectrum, but in the case of some, such as silica, the difficulty of heating them sufficiently is very great.

Carbon and its compounds furnish a variety of curious spectra, and one of these has been usefully applied to manufacturing purposes. Steel differs from cast iron in containing less carbon. By the Bessemer process the superfluous carbon is burnt out of the iron with such rapidity that in twenty minutes five tons of cast iron are converted into cast steel. If the blast of air which effects this combustion be continued for only ten seconds after the proper time, the steel becomes so viscid that it cannot be poured from the ladle; if, on the other hand, it be discontinued ten seconds too soon, the iron contains so much carbon as to crumble under the hammer.

Skilled persons accustomed to work this process are able by inspection of the flame to tell with more or less exactitude when the air-blast has to be turned off. By the uninstructed no difference at all can be detected in the appearance of the flame; but by the aid of the spectroscope this point can at once be ascertained beyond the shadow of a doubt, and that which previously depended upon the quickness of vision of a trained eye has become a simple matter of exact scientific observation, for at a certain moment all the carbon lines disappear. And in some large works this method is habitually employed.

We have now to consider the means by which we obtain the spectra of heavy metals. Here again we are indebted to electricity.

The electric spark was first examined by Wollaston, but it was left for Faraday to determine its nature. He it was who first declared that the electric spark consists solely of the material particles of the poles and of the material through which it passes. By an elaborate series of experiments he proved that electricity has no existence apart from matter. When the electricity passes from the brass knob of a machine to the experimenter's knuckle, a quantity of matter passes too, consisting partly of particles of brass and partly of the air and moisture existing between the knuckle and the knob. Just as a voltaic current heats and volatilizes a metallic wire, so it heats the particles of brass and air and aqueous vapour, and it is the light produced by this heating that we call the "spark."

To obtain, then, the spectrum of a metal, all that we have to do is to introduce a portion of it into the electric spark. It is important to know that when we examine the spark thus produced we get something besides the spectrum of the metal. We find that there are two superimposed spectra. One produced by the very bright points of light lying close to the pole, and the other by the less luminous portion of the arc lying farther from the poles. The spectrum of the bright spots is that of the metal present: the light from the less luminous portion in the centre exhibits the spectrum of the incandescent air, and shows the particular lines produced by the gases present in the atmosphere, viz. nitrogen, oxygen, hydrogen (from decomposition of ever present aqueous vapour), and even of carbon when much carbonic acid is present. The co-existence of these two spectra was discovered in 1855 by the Swedish philosopher Ångström. Sir Charles Wheatstone in 1835 first pointed out that the sparks of different metals give dissimilar spectra, and that they were produced by the volatilisation and not by the combustion of the metallic particles, and he foretold that this method of discrimination might thereafter be applied to useful purposes. He also published a diagram of the lines which he found. These lines are correct as far as they go, but more recent researches with apparatus of increased delicacy and power have shown an immense number of lines, beyond those he delineated, in the metals examined by Wheatstone. And since then Ångström, Huggins, Kirchhoff and Thalén, have examined a great number of metals and mapped their spectral lines.

Temperature has something to do with development of the characteristic spectra both of metals and of compound bodies. Thus lithium at a certain heat furnishes only red and orange rays, whilst heated in the electric spark a splendid blue one is added. No increase of intensity of the spark produces any further development.

If a compound body such as calcium chloride be heated in a Bunsen burner a certain spectrum is produced which may be termed the flame-spectrum. But if by means of electricity we obtain what may be termed a spark-spectrum, we find between the two striking differences. This can be readily explained. It is a well known fact that certain chemical compounds, when they are heated up above a given temperature decompose into their constituent elements; but that below that temperature, these compounds are capable of existing in a permanent state. Hence at different temperatures differences in the spectra of compound bodies. It is not till we reach a high temperature that we get the true metallic spectrum.

The total number of metallic lines which have been mapped is something prodigious. Iron alone furnishes 460.

Proceeding onwards we now find ourselves in the presence of an entirely new class of facts, at present but imperfectly worked out but of the highest importance.

We find that under certain circumstances portions of light are stolen in transitu, and that the spectrum exhibits gaps in the shape of fine lines or broad bands showing by their blackness where the rays have been absorbed. Thus, if we decompose with a prism a ray of light which has passed through the violet vapour of iodine or one which has passed through red nitrous fumes, a number of black bands appear in the spectrum. Just as different metals yield each their own characteristic lines of light, so these vapours yield each their own characteristic bands of darkness. Some coloured gases do not possess these absorptive powers. Chlorine is one which yields no "absorption bands."

The solutions of many coloured metallic salts have this selective faculty, and Gladstone has found that with very few exceptions all the compounds of the same base or acid exhibit the same "absorption spectrum." For example, the green chromium salt has the same effect as the purple one.

Curiously enough a solution of didymium salt, which absorbs so little light that to the naked eye it is quite colourless, produces such an effect upon the light which passes through it, that the presence of the metal may be reliably diagnosed by its broad black band.

Chlorophyll, chloride of uranium, magenta, are other bodies which affect light in its passage through them. Blood diluted with water sufficiently to allow enough light to pass through it, shows two very distinctive absorption bands. So delicate is this test that a thousandth part of a grain of red colouring matter may be recognised.

Deoxidised blood gives only one black band similar to, but not identical in position with, the single band of magenta. Professor Stokes, in the 'Proceedings of the Royal Society' for 1864, tells us that the colouring matter of the blood, like that of indigo, is capable of existing in two states of oxidation, distinguished by a difference in colour, and by a fundamental difference in their action on the spectrum. These two forms may be made to pass one into the other by suitable oxidising and reducing agents, and have been termed red and purple cruorine. By the action of an acid on the blood, the cruorine is converted into hæmatine yielding a different absorption spectrum, and this hæmatine is capable of reduction and oxidation like cruorine. There is another very interesting point connected with blood, namely, that when it contains even small quantities of carbonic oxide gas in solution it yields a very peculiar set of bands. So that in charcoal vapour poisoning these may be diagnostically relied on by the toxicologist.

Dr. Thudichum has examined the spectra of a large number of coloured organic substances. Amongst these are the colouring matters of urine and bile and their derivatives,[1] ovario-luteine (the characteristic yellow ingredient of the corpora lutea of mammals, ovo-luteine (the characteristic yellow ingredient of the yelk of hen's eggs), and the alcohol, chloroform, and ether extracts of each of these substances. From a comparison of these observations, Dr. Thudichum believes that we are justified in assuming that—1. The spectra of the various extracts from the corpora lutea are identical with those of the several extracts from hen's eggs. 2. *A fortiori* the body producing the spectra in the extracts from the corpora lutea is chemically identical with that producing the spectra in the extracts from yelks. 3. This body is a yellow crystallizable substance which has not hitherto been defined, and which may be called "luteine.' This substance was isolated and examined as to all but its spectroscopical qualities by Holm (under the guidance of Städeler), who believed it to be hæmatoidine. Spectrum analysis, however, shows that it not only is not identical with this substance, but further, that it has no chemical or physical relation either to it or to any other derivative of hæmato-crystalline.[2]

But Dr. Thudichum's contributions to this department of science are too numerous and important to be noticed incidentally in an attempt to epitomize the general principles and conclusions of spectroscopy. They require to be handled specially. At the same time, we cannot allude to them in the most cursory manner without

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[1] 'Ninth Report of the Medical Officer of the Privy Council, 1866,' Appendix No. 10.

[2] 'Eleventh Report of the Medical Officer of the Privy Council, 1868,' Appendix No. 6.

remembering that these researches have been conducted under the auspices of Mr. John Simon the Medical Officer to her Majesty's Privy Council, honorably distinguished both by his own contributions to pathology, and by being the only fountain from which any public money flows for the promotion or advancement of exact researches in medical science.

In his fifth lecture, Professor Roscoe encounters what he calls the somewhat formidable task of endeavouring to explain the grounds upon which Professor Kirchhoff concludes with certainty, that in the solar atmosphere, at a distance of about 91,000,000 of miles, substances which we well know on this earth, such as iron, sodium, magnesium, and hydrogen, are present in the state of luminous gases.

In beginning to consider this matter, we shall do well to remember that the subject is still in its infancy; that it is only within the last few years that we have been at all acquainted with the chemistry of these distant bodies. We must not be surprised to find that some of our questions cannot be satisfactorily answered, and we may expect in several instances to meet with facts to which an explanation is still wanting.

We do not think that the author need have felt any difficulty in approaching the explanation of these miraculous discoveries, for this branch of his subject he has treated in the most lucid manner.

We now recall the reader's attention to Frauenhofer's lines already alluded to. Kirchhoff mapped an immense number of these as existent in the solar spectrum, and with such accuracy that an actual photograph of them made by Rutherford presented scarcely any differences. He then determined to test by experiment a frequently asserted coincidence of two of Frauenhofer's lines with the two yellow lines of sodium. This experiment is the starting-point of the discovery that the luminous envelope of the sun contains many of our terrestrial metals. We follow Professor Roscoe in giving Kirchhoff's own description of this experiment. He says, "In order to test in the most direct manner possible the frequently asserted fact of the coincidence of the sodium lines with the lines D" (as these particular black lines have been termed), "I obtained a tolerably bright solar spectrum, and brought a flame coloured by sodium vapour in front of the slit. I then saw the dark lines D change into bright ones. The flame of a Bunsen's lamp threw the bright sodium lines upon the solar spectrum with unexpected brilliancy. In order to find out the extent to which the intensity of the solar spectrum could be increased without impairing the distinctness of the sodium lines, I allowed the full sunlight to shine through the sodium flame, and to my astonishment I saw that the dark lines D appeared with an extraordinary degree of clearness.

"I then exchanged the sunlight for the Drummond's or oxy-hydrogen lime-light, which like that of all incandescent solid or liquid bodies, gives a spectrum containing no dark lines.

"When this light was allowed to fall through a suitable flame coloured by common salt, *dark* lines were seen in the spectrum, in the position of the sodium lines.

"The same phenomenon was observed, if instead of the incandescent lime, a platinum wire was used, which being heated in a flame, was brought to a temperature near its melting point by passing an electric current through it. The phenomenon in question is easily explained upon the supposition that the sodium flame absorbs rays of the same degree of refrangibility as those it emits, whilst it is perfectly transparent for all other rays."

This idea was further confirmed by substituting for the sodium flame a flame coloured by potassium, when *dark* lines appeared in the exact position of the characteristic *bright* lines of this metal. Bunsen and Kirchhoff have likewise succeeded in reversing the flames of lithium, calcium, strontium, and barium; and Miller partially that of copper. Kirchhoff found, further, that each of the 460 bright lines of the iron spectrum, a dark one was to be found in sunlight; and not only so, but the breadth and degree of shade of the two sets of lines were seen to agree in the most perfect manner, the brightest iron lines corresponding to the darkest solar ones.

From these facts is deduced the law or "theory of exchanges."

Every substance which emits at a *given temperature* certain kinds of light, possesses the power at that *same temperature* of absorbing from another luminous body the same kinds of light.

If, then, the sun is surrounded by an atmosphere in which iron and sodium and magnesium, and other metals, are in a state of incandescent vapour; and if, further, the light from the sun itself has to pass through this incandescent atmosphere before it reaches us, Frauenhofer's lines would be completely accounted for.

The chances arithmetically stated are as 1,000,000,000,000,000,000 to 1 that the dark lines of the solar spectrum which correspond to the bright ones of the iron spectrum are produced by the passage of the sunlight through iron vapour. This iron vapour might be contained either in the atmosphere of the sun, or in that of the earth. But it is not easy to understand how our atmosphere can contain such a quantity of iron vapour as would produce the very distinct absorption lines which we see in the solar spectrum; and this supposition is rendered still less probable, since these lines do not appreciably alter when the sun approaches the horizon. It does not on the other hand seem unlikely, owing to the high temperature which we must suppose the sun's atmosphere to possess, that such vapours should be present in it. Hence the observation of the solar spectrum appear to prove the presence of iron vapour in the solar atmosphere with as great a degree of certainty as we can attain in any question of natural science.

The metals which have been thus distinguished in the solar spectrum are sodium, calcium, barium, magnesium, iron, chromium, nickel, copper, zinc, strontium, cadmium, cobalt, hydrogen, manganese, aluminium, titanium.

Our own atmosphere does however produce dark bands as was first pointed out by Brewster in 1833. Some very interesting experiments by Janssen illustrate this part of the subject. He observed, that if the light from sixteen jets of coal gas be passed through a column of steam thirty-seven metres in length at a pressure of seven atmospheres, the steam exerts a strong absorptive power; groups of dark lines appeared in the spectrum between the extreme red and the line D. These lines are found to coincide with lines in the solar spectrum, which become intense when the sun is near the horizon, and are therefore due to absorption in the aqueous vapour of our own atmosphere.

In his sixth and last lecture, Professor Roscoe passes from the subject of solar to that of stellar chemistry. And here we are glad to find that two of our own countrymen are distinguished for their researches; indeed, to Mr. Huggins and Professor W. A. Miller we are indebted for almost all our knowledge of stellar chemistry.

Although the moon and the planets, shining by reflected light, do not reveal to the spectroscope the nature of their constituent elements, yet something may be learnt from the examination of their spectra. It will be remembered that some of the dark lines in the solar spectrum are caused by absorption in our atmosphere; now if an atmosphere of a similar kind existed round the moon or the planets the atmospheric absorption lines would, it is reasonable to suppose, appear more intense in the light reflected from those bodies than they do in the light which has passed through our air alone. But with regard to the moon, the observations of Huggins and Miller have been negative. No signs of a lunar atmosphere have presented themselves. A still more delicate and, as it appears to us, an infinitely more reliable test was employed by Huggins. He observed the spectrum of a star at the moment the dark edge of the moon passed over it. If an atmosphere existed round the moon the observer would see the starlight by refraction after the occultation had occurred—to use a not very exact phrase. The variously coloured rays of decomposed light are, however, unequally refrangible, and if any atmosphere surrounded the moon, the red rays being least refrangible would disappear soonest, and the other colours in rotation. As the whole of the different rays of the stellar spectrum disappeared simultaneously the non-existence of a lunar atmosphere necessarily follows. In Jupiter, Saturn, Venus, and Mars there are indications of some kind of atmosphere, that possessed by the two former probably containing aqueous vapour. From one peculiar band in the spectrum of Jupiter we are compelled to infer that his atmosphere contains a gas or vapour different to any yet discovered in our own planet.

In order to obtain a knowledge of the chemical composition of the stars it is necessary to use instruments of extreme delicacy, and when we remember that the light of a star emanates from a point, —that is to say that a star has no sensible magnitude, that the image of the star has to be kept steady upon a slit only $\frac{1}{300}$ of an inch in breadth, and, moreover, that the effect of the earth's motion has to be counteracted; when we add to this that the amount of light which even the brightest stars give is excessively feeble, that this light must be still further weakened by being spread out by a cylindrical lens into a band, and further that in our climate it is only on a few even of those nights when the stars shine their brightest that the air is sufficiently motionless to prevent flickering and consequent confusion of the spectra, we may imagine how extraordinarily difficult such researches as those of Huggins and Miller must have been. All the above mentioned obstacles have to be overcome before we can get the spectrum of a star, and even then in order to analyse its peculiarities we have to compare it with the spectra of the bodies whose presence or absence in the stellar atmosphere we desire to ascertain. This requires the contemporaneous examination of the two spectra, so that the coincidence or the reverse of the lines of each may be noted. The spectra of different stars differ both from each other, and from the solar spectrum. There is this general agreement amongst them. Like the sunlight, starlight is found to emanate from an intensely hot mass, and to pass through a gaseous envelope, which removes some parts of the light, so that all stars show some of Frauenhofer's lines.

Without giving a catalogue of stars and their components, we may say briefly that they contain a large number of elements, including some of the commonest and some of the rarest constituents of this earth.

A very curious revelation has been made by the spectroscope upon the cause of different stars being different colours, for it is well-known, though perhaps not to ordinary observers, that some stars are purplish, some reddish, and some of an orange tint. This depends upon the number of Frauenhofer's lines congregated in certain parts of the spectrum. In other words upon the amount of light stolen from different portions of the spectrum by the constituents of a stellar atmosphere. Thus, if the black lines are unusually numerous in the region of the green and blue colours of the spectrum, there will be a comparative preponderance of orange light, and consequently the star will have an orange hue. If, on the contrary, the black lines preponderate in the red and green sections of the spectrum the star will be of a blue cast.

We come now to one of the most singular of the many remarkable revelations of the spectroscope.

In May, 1866, a star, which had been previously almost unknown, and which was at all events of very insignificant magnitude, suddenly blazed out, and attained a magnitude almost equal to that of the largest stars seen in the heavens. Examination of its spectrum by Huggins and Miller showed that it differed altogether in its character from the ordinary stellar spectra, which consist of dark lines on a bright ground. There were in this particular star bright lines. Now what do bright lines indicate? They indicate the presence of certain gaseous bodies in such a state of luminosity that the prism receives the light directly from them, and not light which has been filtered through them. The bright lines in this instance evidently proceeded from incandescent hydrogen. By the 20th of August it had become reduced to its pristine insignificance, and it was calculated that its light was only $\frac{1}{700}$ as bright as it had been when at its maximum. We are told by Mephistopheles that "a fellow who speculates is like a brute driven in a circle on a barren heath by an evil spirit, whilst fair green meadow lies everywhere around." In spite of this Professor Roscoe here indulges in the following speculation. From recent observations by Lockyer and Janssen we learn that the red prominences in the sun are also caused by glowing hydrogen, so that we have a new reason for believing that the sun may belong to the family of variable stars. The question at once suggests itself to the mind, could a similar conflagration burst out in our system? Of the effects there can be no doubt. The intensity of the sun's rays being increased nearly eight hundredfold our solid globe would be dissipated in vapour almost as soon as a drop of water in a furnace. The temperature in the sunlight would rise at once to that attainable in the focus of the largest burning glass, and all life on our planet would instantly cease. Since the flames which ordinarily dart out from the sun are often eighty or ninety thousand miles high, and we have no reason why they should not become ten or a hundred times as great, we have no certainty that such an unpleasant occurrence may not happen. We can only hope that the sun, which has lasted so long as a comfortable luminary, may continue to restrain himself within reasonable limits during our own time.

Spectroscopic examination of nebulæ proves that they really are luminous gases yielding bright lines, which in some instances those of nitrogen and hydrogen, though in consequence of the low intensity of the light some of the usual lines are wanting. By this test, therefore, it has been proved that some telescopic appearances are fallacious, and would lead to false inferences as to the real nature of nebulæ. Comets have not escaped the researches of the spectroscopist, and very curious results have been obtained. Thus in Brorsens three bright lines were seen which do not coincide with those of any hitherto discovered body. In the comet No. II of 1868 were found lines which exactly coincide with those of the electric spark taken in olefiant gas.

There can be little doubt that, extraordinary as are the discoveries hitherto made by this new method, years will extend immensely their number and importance, and we sincerely hope that the number of those who are investigating spectroscopic phenomena more distinctly bearing upon medicine may daily increase.[1]

W. F. Wade.

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[1] Since the above was put into the printer's hands, Professor Roscoe has, we are glad to find, been called upon for a second edition.

Bibliographical Record.

Huxley's Lay Sermons.[1]—Professor Huxley has conferred a great benefit on a large class of the reading public by the collection and republication of the essays which are included in this volume. Although many of them, and notably that "On the Physical Basis of Life," with what may be termed its supplement on "The Scientific Aspect of Positivism," are doubtless familiar to most of our readers, every one will rejoice to possess them in their present permanent shape. We shall for the most part confine our attention to the first six essays which, under various forms of lay sermons, addresses, after-dinner speeches, and lectures, constitute a special group bearing more or less on educational topics. The lay sermon "On the Advisableness of Improving Natural Knowledge," is singularly eloquent and forcible in its style, and some passages in it are almost as trenchant as the author's well known reply to a high dignitary of the Church at the Oxford Meeting of the British Association. He begins by giving a sketch of the early history of the Royal Society and indicating some of the results due to the labours of the founders. For those who regard natural knowledge as "a sort of fairy godmother, ready to furnish her pets with shoes of swiftness, swords of sharpness, and omnipotent Aladdin's lamps, so that they may have telegraphs to Saturn and see the other side of the moon, and thank God they are better than their benighted ancestors" he has no sympathy. "I think I would just as soon be quietly chipping my own flint axe after the manner of my forefathers a few thousand years back, as be troubled with the endless malady of thought which now infests us all, for such reward." For his lucid exposition of the manner in which the improvement of natural knowledge has not only conferred practical benefits on men, but in so doing has effected a revolution in their conceptions of the universe and of themselves, and have profoundly altered their modes of thinking and their views of right and wrong, we must refer to his own pages.

The second essay, on "Emancipation—Black and White," is mainly devoted to an exposure of the errors of the present system of female education in this country, and hints for an improved

[1] *Lay Sermons, Addresses, and Reviews.* By Thomas Henry Huxley, LL.D., F.R.S. London, 1870.

system. Recognising and accepting the fact that the mind of the average girl is less different from that of the average boy than the mind of one boy is from that of another, he argues that whatever education is best for boys is also best for girls; and so far from imposing restrictions upon the acquirement of knowledge by women, he maintains that every facility should be thrown in their way; not only would he admit "girl graduates" to our *ad eundem* degrees, but "if obvious practical difficulties can be overcome," by which he apparently means the troubles and duties of maternity, he would allow them to contend with men in the busy turmoil of life, and, if they please, become merchants, barristers, and politicians. He would, doubtless, have added physicians to the above list, if the article had been written in 1870 instead of in 1865. And what would be the result? He believes that with a fair field and no favour it will be that of other emancipations. "Women will find their place, and it will neither be that in which they have been held, nor that to which some of them aspire. Nature's old salique law will not be repealed."

In studying the essay on "A Liberal Education; and Where to find it," the reader must bear in mind that it was addressed to working men. It contains the celebrated parable of the chess-board, which for simplicity of style and fitness in its application is almost unsurpassed in the wide range of English literature. The attention of every clergyman and schoolmaster should be directed to this section of Professor Huxley's volume. With regard to the former class we hardly know whether we should recommend them to avoid the next essay, or whether we should suggest that in studying it they might find something to their advantage. The paragraph that excites these conflicting doubts is one which runs as follows:—

"The clergy are at present divisible into three sections: an immense body who are ignorant and speak out; a small proportion who know and are silent; and a minute minority who know and speak according to their knowledge." And this is succeeded, some pages further on, by a suggestion almost as appalling in its way as the above assertion.. Why should scientific teaching be limited to week days? "Would there," the Professor asks, "really be anything wrong in using part of Sunday for the purpose of instructing those who have no other leisure, in a knowledge of the phenomena of nature, and of man's relation to nature? I should like to see a scientific Sunday school in every parish, not for the purpose of superseding any existing means of teaching the people the things that are for their good, but side by side with them. I cannot but think that there is room for all of us to work in helping to bridge over the great abyss of ignorance that lies at our feet."

Passing over the address on "The Educational Value of the Natural History Sciences," in which he forcibly inculcates the necessity of including physiological science in the curriculum of general education, we come to the last lecture of the educational group, "On the Study of Zoology." Our author here employs the term zoology in its most general sense, and consequently as including the subordinate sciences of morphology (embracing anatomy, development, and classification), the distribution of animals, both extinct and existing, and physiology, whose first object is "to deduce the facts of morphology on the one hand, and those of distribution on the other, from the laws of the molecular forces of matter." Taking a lobster as his text he employs it to elucidate all these truths. By dividing it into its various segments, and comparing and contrasting their appendages, he shows that a unity of plan, of the same kind as is discoverable in the tail or abdomen, pervades the whole organisation of the skeleton. By an appeal to morphology he shows that this doctrine of the unity of plan is no mere anatomist's fancy, but that it is the expression of deep-seated natural facts; for careful investigations of the ovum at different stages show that there is a period when by transverse constrictions it is divided into segments, from the ventral surface of which bud-like prominences shoot out, which are the rudiments of the various appendages. The lobster and certain allied animals are thus used to illustrate the terms *species, genus, family, order, class, province,* and *sub-kingdom.*

The geographical distribution of lobsters of different species is then noticed, after which the extinct animals constructed, on the same plan, but totally different from the existing lobster, are briefly referred to. In the last place our author shows how a living or just killed lobster may serve to illustrate many of the most important laws of physiology.

Everyone must, we think, agree with the author that the student who has once seen for himself the facts described in this lecture, who has had their relations explained to him, and has clearly comprehended them, has so far a knowledge of zoology which is real and genuine, and which is worth more than all the mere reading knowledge of the subject he could ever acquire.

This lecture, which was delivered to an audience of science-teachers, contains a very plain-spoken exposition of the prevalent errors of the present system. The following concluding remarks have, we fear, a very wide application: "Addressing myself to you, as teachers, I would say, mere book-learning in physical science is a sham and a delusion. What you teach, unless you wish to be impostors, that you must first know; and real knowledge in science means personal acquaintance with the facts, be they few or many."

Three of the subsequent essays are on geological subjects, one being his lecture to the working-men of Norwich "On a Piece of Chalk," and the two others being anniversary addresses to the Geological Society for 1862 and 1869. We regret that his latest anniversary address — perhaps the most valuable of all—is not included in this volume, which concludes with two review-articles on "The Origin of Species," and an address recently delivered to the Cambridge Young Men's Christian Society on "*Descartes' Discours de la Méthode pour bien conduire la Raison et chercher la Vérité dans les Sciences.*" We think that, without wounding the susceptibilities of our readers, we may assume that comparatively few of them have read this celebrated discourse. We would advise those to whom it is still "untrodden ground" to study it forthwith, taking Professor Huxley's address as an explanatory commentary.

Nicholson's Zoology.[1]—This little work, while laying no claim to originality, is likely to prove of far greater general utility than many more pretentious volumes. It contains in a remarkably convenient form the classification of the invertebrates with a description of each sub-kingdom, and of the manner in which it is divided, followed by an account of its divisions down to sub-orders.

In many cases where a received arrangement of the families of an order exists a synopsis of it is added.

The primary divisions of Professor Huxley have been adopted, except that the Molluscoida rank as a division of the Mollusca. A somewhat greater divergence from Professor Huxley's arrangement might have been better, as his sub-kingdoms Annulosa and Annuloida form groups neither homogeneous in themselves nor distinct from each other. In dividing the sub-kingdoms while retaining Professor Huxley's classification as a basis, the author has slightly modified it in those parts in which it does not agree with the arrangement most widely followed.

The introduction, which is the only really original part of the book, is a well condensed categorical statement of the general truths and definitions of zoology, with some mention of the principal theories which have been framed to explain them.

Woodcuts of many characteristic species, and rough but clear diagrams to illustrate the structure or mode of development of those groups that most require it, are inserted in the text.

When they are of sufficient importance or interest to warrant it, a short account of the habits of the animals described is added. This is especially done in the description of the Insecta. But in other respects the author's description of this class is the most meagre in the book. This is unfortunate, considering the surpassing interest and importance of the group, but no doubt it would have

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[1] *A Manual of Zoology for the use of Students, with a General Introduction on the Principles of Zoology.* By HENRY A. NICHOLSON, M.D. Hardwicke, 1870. Vol. I, "Invertebrate Animals," pp. xvi and 322.

been almost impossible to have condensed a more satisfactory account into the space available for it in so small a manual.

In other respects, we have but few faults to find. The discovery of Haltenia and other immediate forms, and of better specimens, leaves no room to doubt that the proper place for Hyalonema is among the Spongidæ and not in the Zoanthuria Sclerobasica, where Dr. Nicholson places it in accordance with the views of Dr. Gray. Dr. Nicholson's own opinion as to the position of the Graptolites carries weight, but in a work like the present, which ought to be strictly impartial, it would have been well to have given a sketch of the arguments of those who hold them to be Polyzoa, not Hydrozoa. The author has been judicious in the choice of the classifications quoted, but some of them, of course, are not free from defects. In his style the author is clear and simple; more than this can scarcely be expected in a work of this kind. His accounts convey distinct ideas of the animals of the groups described, and while he does not confuse the beginner by too minute details, the more important facts are all stated; thus the book is useful alike as a text-book and as a means of refreshing the memory of a more advanced student. We shall be glad to see the second volume of this valuable little book.

Cauvet on Medical Natural History.[1]—The idea of this work might have been suggested by the medical zoology of Moquin-Tandon. Professor Cauvet, however, has enlarged the field taken by his predecessor, and has aimed at producing a history of animals, vegetables, and minerals, useful or harmful to man, arranged in order according to natural series and generally accepted classification. The book seems to us a very good attempt to unite with the somewhat dry details of treatises on materia medica the more interesting—at least to the ordinary student—lore of zoology, botany, and mineralogy. The zoological portion occupies more than three hundred pages, and is, on the whole, a creditable compilation, affording a considerable amount of information, not merely in zoology, but in comparative anatomy and physiology. The author is eclectic in the sources whence he has drawn matter and illustrations. Professor Owen's well-known ideal plan of a typical vertebra is reproduced and explained in the context, although M. Cauvet accepts with hesitation the professor's vertebral theory of the cranium. A few pages further on we light on Mr. Huxley's grand tableau of the skeletons of the gibbon, orang outan, chimpanzee, gorilla, and man, grimly tripping after each other in single file—near relations in a *todtentanz*—the

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[1] *Nouveaux Eléments d'Histoire Naturelle Médicale.* Par D. CAUVET. Paris, 1869. Tomes 1er et 2nd.

A New Treatise on the Elements of Medical Natural History. By D. CAUVET. Paris, 1869. Vols. 1 and 2.

little gibbon, to suit the learned professor's purpose, being represented well-nigh as big as the giant gorilla, and full as large as man himself. But M. Cauvet is not to be charged with the eccentricities of our countryman, albeit he seems to have adopted in full Mr. Huxley's estimate of man's place in nature. From a zoological point of view, he writes, man is a mammifer of the order primates, constructed for the erect position, and separated from the anthropomorphic apes by characters of often less importance than those which separate the anthropomorphic from the inferior apes (p. 46). Neither does M. Cauvet admit the high development of the thinking power in man as a ground for elevating him in a kingdom apart, far above the rest of the so-called order primates. The attempt of Geoffrey Saint-Hilaire to erect a human kingdom on psychical qualities he smiles at with M. Vulpian as "le dernier terme de l'admiration de l'homme pour l'homme." It would be foreign to our purpose to discuss the subject with M. Cauvet, but we would only remark that the school of philosophers who believe that they can persuade mankind that they are nothing more than developed monkeys must have an "*admiration*" for their own abilities which cannot possibly be surpassed, although the fact of their holding the belief testifies to the very low estimate they must have formed of the thinking faculties of other people. Leaving such debatable ground, we may notice that the book contains a good outline of what is known of poisonous serpents and serpent-venom, a subject which has acquired fresh interest from the contradictory results lately obtained by Professor Halford in Australia and Mr. Fayrer in India, when investigating the power of ammonia as an antidote. Prince Lucien Bonaparte's observations on the nitrogenous principle which he found in the venom of the viper, and to which he gave the name echidnine, quoted by the author, should be supplemented by some careful and extended comparative investigations as to the chemical nature of the poisons of the more destructive crotali and cobras. No more promising field of investigation seems to be open in the whole range of animal poisons.

The entozoa infesting the human body are treated of at considerable length, and the subject, as indeed the whole of the book, is fully illustrated with good woodcuts. The chapter on tæniæ and bothriocephali contains a good account of the anatomy, habitat, and distribution of these unwelcome guests.

The botanical portion constitutes the largest part of the work. Vegetable physiology is treated of quite as fully as in ordinary botanical text-books, and systematic botany occupies nearly the whole of the second volume. The descriptions seem carefully done, and the illustrations, which appear to be mostly original, are clear and artistic. Any student who wishes to keep up his French, and at the same time to increase his acquaintance with the details of medical natural history, cannot do better than obtain and study M. Cauvet's volumes.

Crookes on Beet-root Sugar.[1]—The importance of sugar as an article of food and the greatly increased consumption of it per head of the population during late years have not escaped public attention. The beet-root sugar industry of the Continent has rapidly advanced to gigantic proportions, and there seems no reason why England should not be added to the list of sugar-producing countries. If all the proper agricultural conditions be fulfilled, sugar may be produced year after year without any injury to the permanent fertility of the soil. The sugar, which should constitute one tenth of the weight of the roots, may either be extracted or converted at once by fermentation into spirit; the residual may be used as food for cattle, the excreta of these being, of course, put upon the land; then, finally, the waste liquors, containing potassium salts, must be made into manure or allowed to flow back upon the fields in which the beet has been grown. Mr. Crookes's book gives us a complete history of the sugar beet, its varieties, composition, and culture, and includes an elaborate account of the manufacture of beet-root sugar and spirit. The last chapters relate to the accessory subject of the production of potassium salts from beets, and to the excise regulations. Considering the growing extent and the improvements continually introduced, this work may be regarded as satisfactorily representing the present condition of the industry of which it treats. It is essentially a manufacturer's manual.

Tyndall's Electrical Phenomena.[2]—A little book, compact, interesting, accurate. Hardly an unnecessary word nor a doubtful statement can be found in its pages. Lecturers in schools as well as students will hail the appearance of Dr. Tyndall's notes with satisfaction. Voltaic electricity, electro-magnetism, magnetism, diamagnetism, frictional electricity, electric telegraphy, and magneto-electricity, with notices of particular discoveries and constructions, are described briefly but clearly in these forty pages.

Tyndall on Light.[3]—By the publication in a cheap form of these notes Dr. Tyndall has earned the gratitude of those who have to teach as well as of those who have to learn something of the

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[1] *Manufacture of Beet-root Sugar in England and Ireland.* By W. CROOKES, F.R.S. London, 1870. Pp. xvi, 288.

[2] *Notes of Seven Lectures on Electrical Phenomena.* By J. TYNDALL, LL.D., F.R.S. Pp. viii, 40.

[3] *Notes of a Course of Nine Lectures on Light.* By JOHN TYNDALL, LL.D., F.R.S. Pp. 74.

nature and the phenomena of light. The way in which the most complex as well as the simplest facts and laws are explained with the aid of quite homely language, the gradual development of the treatment of the subject, the constant recurrence to first principles, that the student may feel his advance to be sure, the introduction of theories and laws *after* the facts they serve to explain and connect, the historical notices of the progress of special discoveries, the illustrations of recondite facts by references to everyday appearances and effects, and, finally, the completeness of the method adopted so far as the limits of this little book allowed, all these are points of conspicuous merit. We may be permitted, however, to breathe a word of doubt whether it is advisable for a teacher to speak so unhesitatingly of the existence of the luminiferous ether (p. 32) as Dr. Tyndall does. The chemical philosopher finds this ether, so welcome to the physicist, a terribly indigestible kind of matter. For matter it is affirmed to be, and though it is said even to possess particles, it cannot be excluded by any other kind of matter from a given space, nor can it be weighed. What we object to, is not the admirable hypothesis of the existence of an "ether," but the speaking of this imaginary, and only possibly real, ether, as if it were one of the elements—hydrogen, oxygen, sixty-two other elements—and ether.

Memoirs of the Anthropological Society.[1]—The Anthropological Society has now acquired a status and stability among the scientific associations of the country, a circumstance which this record of work done during two sessions will abundantly explain.

It has had a somewhat turbulent infancy, and was regarded as a fractious and somewhat objectionable child, treating with some disrespect the approved principles and cherished conclusions of certain anthropologists and biologists disposed to regard their views as final. There might possibly be charged against a few members of the newly born society a measure of impetuosity and an indiscretion in the manner of attacking beloved hypotheses, but this period of antagonism and rebellion has passed by, and every devotee of science will now wish success to the Anthropological Society.

The present volume contains twenty-six papers, some of which are of very considerable length and accompanied by illustrations. And we are glad to see so fair a proportion of contributors from the ranks of the profession.

No question occupies at the present day a higher interest in the scientific world than that of the antiquity, the progressive development, and the distribution of the human race on the surface of the earth, and it is one which has much light thrown upon it by the

[1] ***Memoirs read before the Anthropological Society of London,*** **1867-8-9. Vol. iii. London, 1870. Pp. 579.**

labours of the members of the Anthropological Society. We should be glad to see more work done in the direction of Dr. Beddoe's inquiry relative to the stature and bulk of man in the British Isles, an inquiry which it appears originated from a scheme put forth by that very indefatigable worker in ethnological science, Dr. Barnard Davis. A more accurate knowledge than at present possessed of the physical characteristics of our own people, including an examination of the effects of external conditions of life and employment, is a great desideratum, and we hope Dr. Beddoe will find many others to work with him in the field of labour he has so well opened up in the paper referred to.

The volume is admirably got up, and we commend it to the careful study of our readers and the society to their support.

St.-Preux on Nervous Functions.[1]—The author's cogitations have led him to the following conclusions, viz.—That the nervous system consists of several intelligent personal individualities forming two societies, and holding communication one with another by the means of electricity. That the brain, cerebellum, medulla oblongata, olivary bodies, pyramids, spinal cord, and ganglia, are each distinct personalities; that each is an independent self or *moi*. That impressions made upon the brain become latent until required for intellectual operations, sensations being impressed upon the anterior cerebral lobes, the images of impressions to be transformed into ideas upon the middle lobe, affirmations upon the posterior lobe. That the cerebellum is the intelligent executive of the mandates of the brain, while at the same time it regulates muscular action and maintains the bodily equilibrium, and occasionally supplements the functions of the brain, as in the movements of somnambulism.

We need not follow the author through similar fanciful explanations of the functions of other parts of the nervous centres, since our perusal of this pamphlet, although it has given us occasion to admire the author's ingenuity and courage, has not, we regret to say, advanced our knowledge of the obscurer parts of nerve-physiology.

Hammond on Sleep.[2]—Although we know from past experience that everything that issues from the pen of Dr. Hammond is sure to be well deserving of the attention of his professional brethren, we

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[1] *Reflexions sur les Facultés et Fonctions encore mal connus de certaines parties du Systeme Nerveux.* Par DOQUIN DE SAINT-PREUX. Pp. 84. Paris, 1869.

[2] *Sleep and its Derangements.* By WILLIAM D. HAMMOND, M.D., Professor of Diseases of the Mind and Nervous System and of Clinical Medicine to the Bellevue Hospital Medical College, New York; late Surgeon-General, U. S. Army, &c. Philadelphia, 1869.

must confess that we took up the present volume with the feeling that it was hardly likely to contain anything that we had not already learnt regarding "sleep, and its derangements," from the researches and writings of Dr. Abercrombie, Lord Brougham, and Sir William Hamilton (who have studied the subject from metaphysical, or rather perhaps a physiological point of view), and Sir Benjamin Brodie, Dr. Carpenter, Sir Henry Holland, Dr. Macnish, Mr. Moore, and Dr. Symonds, who have treated it as physiologists or physicians. We put it down with a very different impression, and freely admit that the author has added very materially to our knowledge of the subject. The subjects to which Dr. Hammond's book are specially devoted are sleep, dreams, and wakefulness. In his observations on the causes of sleep we are glad to see that he gives due prominence to the experiments on this subject by Dr. Fleming, which were published in the 'Review' in 1855, five years previously to the appearance of 'The Physiology of Sleep' by Mr. Durham, who is commonly regarded as the discoverer of the theory, now universally admitted, that sleep is due to a diminution of the quantity of blood in the brain. By a singular coincidence it so happened that in 1860, before he had heard of Mr. Durham's investigations, Dr. Hammond made a precisely similar set of observations on dogs and rabbits, and, as might be anticipated, arrived at precisely identical results. He has recently devised the following very ingenious method of illustrating the relative conditions of the cerebral circulation in the sleeping and waking states. A brass tube, open at both ends, is screwed into a round hole made in the skull with a trephine. Into the upper end he screws another brass tube, whose lower end is closed by a piece of very thick sheet india rubber, while the upper end is closed with a brass cap into which a glass tube is fastened. The last-named tube contains coloured water, and a scale is affixed to it. The second brass tube is screwed into the first tube, the india-rubber presses on the *dura mater*, and the level of the coloured water stands at 0, which is in the middle of the scale. When the animal goes to sleep the liquid falls in the tube, showing that the cerebral pressure has been diminished, and as soon as it awakes the liquid at once rises. Nothing can be more conclusive than this experiment, which was frequently repeated and always with the same result.

Dreams, according to Dr. Hammond, must have a foundation, "and this is either impressions made upon the mind at some previous period or produced during sleep by bodily sensations." He gives a number of original cases, in addition to those collected by previous writers, illustrating the origin of dreams from these two sources. As it is not often that the sense of taste is productive of dreams, we may especially notice a case in which this mental process was excited in a young lady who, having a habit of going to sleep with her thumb in her mouth, painted the former organ with extract of aloes, hoping that if her bad habit recurred the bitter taste would at once awaken her. She slept well, but in the morning found her thumb in her mouth and all the aloes sucked off. But during the night she dreamt that she was in a steamer made of wormwood, and in which all the plates, dishes, chairs, &c., were composed of the same material; that when she asked for a glass of water an infusion of wormwood was given her; that when she applied to a physician, and asked him to extract this bitter matter from her body, he prescribed ox-gall to be taken in pound doses; that to get rid of the ox-gall she was advised to consult the Pope, who sent her on a pilgrimage to the plains of Zoar to search for Lot's wife, with directions that she was to eat a piece of the pillar of salt as large as her thumb; that on reaching the object of her journey she resolved, after much deliberation, to break off and suck the saline thumb of the statue, and that she put the piece of salt into her mouth. She then awoke, and found, as we have already observed, that she was sucking her own thumb.

In the chapter on somnambulism Dr. Hammond describes several cases that have fallen under his own notice. On one of these cases —that of a young lady belonging to a somnambulant family—he was able to make a series of extremely interesting observations, which will be found in pp. 205—209 of his book.

In a practical point of view the chapter on wakefulness forms, perhaps, the most important part of Dr. Hammond's volume, but every portion of it may be studied with advantage. It is no slight praise on the part of a reviewer to be able to state, as we can do in this case, that the book is equally adapted for general and professional readers.

Catlin—Shut your Mouth.[1]—The author, who has acquired a certain degree of fame by his illustrations and descriptions of the North American Indians, now challenges our recognition as a philanthropist, as the discoverer of the most potent and prevalent causes of disease and misery among his fellow-mortals, and withal of the remedy. Indeed, the detection of the cause implies the remedy, and whether to be more astonished at the blindness of past generations in not recognising the cause of most of their fleshly ills, or at the simplicity of the remedy against them, must, so far as we are concerned, continue an open question.

The title of the book is striking, if not alarming, like that of old which caused some stir in its day—'There's Death in the Pot.' The author writes with amusing confidence in the value of his dis-

[1] *Shut your Mouth and Save your Life.* By GEORGE CATLIN. With 29 illustrations from drawings by the Author. Fourth Edition, considerably enlarged. London, 1870. Pp. 102.

covery and with charming *naïveté.* "It is (he tells us in his brief preface) only a question how many *millions* may look through it and benefit themselves by adopting its precepts." Not being the work of a medical man, we need not submit its medico-physiological assertions to criticism, but would satisfy the inquisitiveness of our readers respecting the teachings of the book by stating that he holds a gaping mouth, whether during waking or sleeping, but particularly in the latter condition, as the essential cause of the deplorable mortality among the children of civilised nations, of consumption and of most diseases and deformities of the body, and of disorders and defects of the mind, and, finally, of much moral evil in the world. He would even disallow opening our mouths to laugh, but we may be thankful that he enters no protest against opening them for the purpose of eating.

He writes as an enthusiast, and presses every sort of fact and statement into his service, often, however, with considerable violence and distortion. The intensity of his conviction respecting his topic is exemplified in the following quotation:—"If I were to endeavour to bequeath to posterity the most important motto which human language can convey, it should be in three words—Shut your mouth." Many grotesque figures are interspersed through the text, but the greatest oddity of the work is that the author appears to be serious.

Child's Physiological Essays.[1]—We had intended to have reviewed at some length this interesting volume of essays, but the demands on our space are so great as to oblige us to forego our purpose.

The essays collected in this volume, indeed, will commend themselves by the intrinsic importance of their subjects, and also by the manner in which they are treated by the writer, to attentive study. It is only necessary to give their titles to make good this assertion. They severally discuss "Marriages of Consanguinity;" "Recent Researches on the Production of the Lowest Forms of Animal Life;" "On the Production of Organisms in Closed Vessels;" "Some Aspects of the Theory of Evolution;" "Physiological Experiments;" and "Physiological Psychology." Were further apology needed for our cursory notice of this volume, it might readily be found in the fact that all the essays, except the fourth, on evolution, have been previously published; one, indeed, on "Recent Researches on the Production of the Lowest Forms of Animal Life," in the pages of this Review (July, 1864), and most of the remaining in the 'Westminster Review.'

In his essay on marriages of consanguinity he argues against

[1] *Essays on Physiological Subjects.* By GILBERT W. CHILD, M.A., &c. Second Edition, with additions. London, 1869. Pp. 300.

the popular belief in their degenerating effect on the species. The questions relating to the production of the lowest organisms and spontaneous generation have been more fully discussed since Mr. Child's essays were written, and the doctrine of heterogeny which he favours has gained ground.

Of the other essays we may say, that the author accepts the theory of evolution, that he wrote the one on physiological experiments in reply to the popular outcry against vivisection, and that the last essay, on physiological psychology, is in the main a review of Dr. Maudsley's work on the 'Physiology and Pathology of the Mind.'

We strongly commend the volume to our readers.

The Half-yearly Abstract of the Medical Sciences, and Braithwaite's Retrospect of Medicine have put in their customary appearance, and recount in their well-known manner most of the facts and opinions advanced during the first six months of the year, and published in the various medical journals. They well merit all the success they obtain.

Medico-Chirurgical Transactions.[1]—As the 'Transactions' of this principal medical society of the metropolis find their way into the hands of very many of our readers, and are likewise to be met with in the libraries of hospitals and of societies throughout the kingdom, no adequate reason presents itself for doing more than announcing the appearance of each succeeding volume. This course is likewise dictated, on the one hand, by the impossibility of presenting in the pages of this Review anything like an effective analysis of volumes of such magnitude, and on the other by the general recognition accorded to the value of the papers constituting the 'Transactions.' Moreover, particular papers setting forth new facts, or new hypotheses, or novel methods of treatment, will assuredly soon make their way to the public, and secure to themselves discussion, as well in the periodical press as in the productions of the laborious and large band of compilers who lay wait for novelties to give the requisite piquancy to their wares. For such and other reasons that might be enumerated, we content ourselves with noting the publication of this fifty-second volume, its appearance in the usual form and size, with illustrations where required, and with recommending those medical men who have not yet seen it to take care to do so, and thereby to make themselves acquainted with the opinions and practice of some among the leading physicians and surgeons of London on several important points of practice. We wish indeed that the 'Transactions' reflected

[1] *Medico-Chirurgical Transactions.* Published by the Royal Medical and Chirurgical Society of London. Vol. lii. London, 1869. Pp. 399.

in some moderate degree the experience of provincial practitioners; but this is far from the case, for of the twenty-one papers printed, one only—that by Dr. Humphry, of Cambridge, on the results of excision of the knee, is from the pen of a medical man in practice out of London.

Guy's Hospital Reports.[1]—The volumes of Guy's Hospital Reports have become an institution in medical literature, and are looked forward to with interest, not only by the alumni past and present of Guy's, but also by a fair portion of the reading members of the profession at large. They may always be safely referred to as containing contributions of original character and of practical importance. The present volume is more remarkable for practical papers, and these vary much in subject-matter, ranging from matters of general pathology to those which more particularly fall within the province of specialists.

In all there are twenty contributions, the whole of them by members of the hospital staff. Some of them are accompanied by engraved illustrations. It is singular to find the entire work devolving on those holding office in the hospital, and that the names of none of the multitude of past students, many of whom occupy important professional positions, occur among the contributors.

It is a delicate matter to single out particular papers in a collection of this sort, but in so doing we must express our object as simply being to indicate such as, by the extent of original research and by their subject matter, are likely to challenge most interest.

Mr. Poland has furnished a most elaborate and exhaustive series of tables of the statistics of subclavian aneurism, whilst Mr. Towne largely contributes to our knowledge of binocular vision, and Dr. Alfred Taylor gives important information relative to the processes for "detecting blood in medico-legal cases." But, perhaps, owing to the attention now directed to those subjects, the thermometric observations carried out by Mr. Goodhart, the house-physician, and the cases recorded by Dr. Parry as illustrative of "the influence of opium and some of its constituent principles in controlling the elimination of sugar in diabetes," will, on the whole, be read with more interest. To lovers of statistics Dr. Steele's "Numerical Analysis of the Patients treated in Guy's Hospital from 1861 to 1868" will furnish a satisfactory *pièce de résistance.*

St. George's Hospital Reports.[2]—This last-published volume of reports from the great west-end hospital merits the same commenda-

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[1] *Guy's Hospital Reports.* Edited by C. Hilton Fagge, M.D., and Arthur E. Durham. Third Series, vol. xv. London, 1870. Pp. 652.

[2] *St. George's Hospital Reports.* Edited by George W. Ogle, M.D., and Timothy Holmes, F.R.C.S. Vol. iv. London, 1870. Pp. 354.

tion we have accorded to the volumes of previous years. It contains twenty-one contributions on as many different subjects, and all referable to the class of practical papers. We are glad also to notice an increasing number of contributors from among former pupils, a circumstance that augurs well for the increasing success of the publication. It would be invidious to single out papers as the most valuable, and, indeed, their relative value would be differently estimated by different readers, according to their varying predilections for the several subjects dealt with, yet it is admissible to point to those bearing upon the more interesting and debatable topics of the day, among which we would class the "Notes on the Subcutaneous Injection of Morphia" by Dr. Edward T. Wilson, the essay "On Aphasia" by Dr. Wadham, and the "Remarks on a case of Locomotor Ataxy with Hydrarthrosis," by Dr. T. C. Allbutt.

As usual, several of the papers are accompanied by illustrations.

Leared on Imperfect Digestion.[1]—Dyspeptics abound, and no class of invalids are more disposed to make themselves acquainted with the malady they suffer, and with the remedies recommended for its cure. Hence it is so many books are addressed to these valetudinarians, and that so much success attends their publication. The appearance of a fifth edition of a book on imperfect digestion shows how much it is in favour. Indeed, Dr. Leared has taken pains to make his book so readable, and to help his readers to understand something of what the stomach is and what digestion and indigestion signify, that his non-professional readers must have advanced considerably in their ability or presumed ability to doctor themselves.

The changes wrought in this new edition are the introduction of a chapter on mineral waters, and of one also on the relations between dyspepsia and the function of sleep. The former appears to us to be just sufficient to point to the value of mineral waters, but too shallow to be of use to those who might seek practical directions from it.

On Pathological Families.[2]—To all who feel interested, and surely every medical man should feel interested, in the affinities and methodical nosology of diseases, the perusal of this pamphlet cannot fail to afford gratification. It is one other addition to the long list of attempts at classification, and affords evidence both of thoughtfulness and of independent observation.

M. Gaillard commences his essay by remarking that from the commencement of his practice his first attention was directed to the notices of particular cases, but he very soon found these to

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[1] *The Causes and Treatment of Imperfect Digestion.* By Arthur Leared, M.D., &c. Fifth Edition, revised and enlarged. London, 1870. Pp. 276.

[2] *Essai sur les Familles Pathologiques.* Par L. Gaillard, Chirurgien de l'Hôtel Dieu de Poitiers, &c. Paris, 1869.

become so numerous as to oblige him to arrange them according to their "natural affinities." He says—

"En réalité nous ne connaissons pas grand'chose des causes et influences primitives, ni de leur mode d'action, cela nous échappe; pour nous, le phénomène initial est une lésion de fonction, ou une lésion d'organe; commençons donc ici."

He then supposes the disease—that is, any disease—to have commenced, and asks, "What will be its course of progress?" answering this question by saying that it will be determined by "the *first impulse,*" and goes on to observe—

"On ne peut ni distinguer, ni séparer la marche de la maladie elle-meme. En établissant une première division des familles fondée sur la marche aiguë ou chronique de la maladie, nous sommes donc le plus possible rapproché de son origine."

It will thus be perceived that in the present instance the foundation of M. Gaillard's system is based on a simple division of diseases into acute and chronic, and under these heads he arranges his subdivisions or families—

"en mettant ensemble celles qui nous présenteront de veritables affinités par leur prognostic, leur traitement; ce sera bien établir le diagnostic. Le diagnostic doit être, comme l'indique son nom, la distinction des unités pathologiques principalement fondées sur les éléments qui nous conduisent au *prognostic* et au *traitement*, deux choses sans lesquelles la médecine n'existerait pas, et la science ne serait qu'une curiosité d'histoire naturelle."

Our author next gives his definition of acute and chronic diseases, with their distinguishing characteristics, and descants on the general features of hereditary diseases. He then proceeds with his "Classification des familles Pathologiques." With a prefatory remark or two he pleads for indulgence in the attempt he is about to make at a classification, and expresses a wish that the science of medicine should have, as well as the other sciences, its natural families. After remarking that he finds the principal types of disease to be already sufficiently well known, and that in collecting them together and classifying them he has made use of the same principles as are so employed in the natural sciences, he further observes—

"Mais je n'ai pas emprunté mes caractères à l'anatomie (état statique): couleur, volume, densité, forme. * * * * * * J'ai pris des caractères plus constants dans la physiologie (état dynamique); la maladie n'est après tout qu'une grande fonction pathologique."

M. Gaillard's first division is separated into "Acute" and "Chronic;" the former into two, and the latter into three other divisions having special features; the whole being arranged as follows: A. Cause; *i.e.*, efficient cause. B. Evolution; *i. e.*, mode of progression. C. Origin; *i. e.,* the principal and initial phenomena being constant. D. Termination; *i. e.,* the solution or final act of the disease, this being favorable or the reverse. E. Treatment.

These five divisions furnish twenty-eight subdivisions, constituting "Les familles Pathologiques."

The following difference between the *cause* and the *origin* of a disease is particularly enforced:

"La différence entre les causes et les origines paraît futile au premier abord, mais le mot cause efficiente nous dit un rapport particulier entre l'effet et la cause; le mot origine nous indique seulement une coincidence. En définitive, la cause engendre un maladie de forme aiguë, et l'origine donne lieu à une maladie de forme chronique."

This tabulated arrangement shows at one view the scheme of classification proposed:

CAUSE OF DISEASES.	*(Regular) acute diseases.*	Cause.		Physical agents	1. Injuries.
				Chemical agents	2. Poisoning.
				Mixture of urine with blood	3. Uræmia.
				Extravasation of blood	4. Purpura.
				Mixture of bile with blood	5. Jaundice.
				Mixture of pus with blood	6. Pyæmia.
		Evolution.		By successive crises	7. Venereal diseases.
				Towards the skin	8. Eruptive fevers.
				Towards the parenchyma	9. Fluxions.
				Leading to mortification of tissue	10. Gangrene.
	(Irregular) chronic diseases.	Origin.		Living organisms	11. Parasites.
				Action of cold moisture	12. Rheumatisms.
				Excess of supply over demand	13. Gout.
				Effusion of blood	14. Hæmorrhage.
				Decoloration of blood	15. Chlorosis.
				Excessive development of natural tissue	16. Hypergenesis.
				Production of new tissue	17. Neogenesis.
				Diminution, or arrest, of development	18. Atrophy.
				Fœtal life	19. Congenital maladies.
		Termination.	Ordinarily favorable	Without skin manifestation	20. Neuropathies.
				With skin manifestation	21. Herpetism.
			Ordinarily unfavorable	Suppression of urinary function	22. Anuria.
				Excess of urine	23. Polyuria.
				The presence of sugar in urine	24. Glycosuria.
				The presence of albumen in urine	25. Albuminuria.
				With alteration of pigment	26. Addison's disease.
		Treatment.		Tonics	27. Scrofula.
				Quinine	28. Marsh miasms.

Each of these twenty-eight subdivisions receives certain illustrative observations tending to make clear the meaning and intentions of the author, who concludes his "brochure" thus:

"Ami lecteur, faites-moi sans ménagement connaître vos critiquse, je les accueillerai et vous en ferai honneur; ce que je cherche avant tout, c'est la vérité."

That this systematic nosology of diseases is altogether free from objections—that it is sufficiently inclusive to prevent the necessity for "leaving out in the cold" some well-recognised clinical conditions — we do not, and we feel sure the talented author would not think of asserting; but looking at its usefulness as far as its capability of *practical application* is concerned, we are disposed to regard it in a very favorable light; and if a comparison be permitted between it and the methodical nomenclature of the College of Physicians, we are under the impression that, in the above respect, *i. e., practical use,* it deserves a very decided preference.

Nouveau Dictionnaire de Medecine et de Chirurgie.[1]—The production of such voluminous medical works as this is a feature of Continental enterprise not paralleled in England. That it is practicable and so often, particularly in France, speaks well for the medical profession of the country, as indicative of a widely prevailing desire to possess a complete system of instruction in the art they practise. And judging from the large production of special treatises, and monographs also, it would seem that this desire is not satiated by dictionaries of medicine, but calls for other supplies as ardently as though such *pièces de résistance* had no existence.

We have now before us the eleventh and twelfth volumes of this magnificent work, and as yet, judging from the letter reached, only about a fourth part of it has yet been produced. When complete it will consequently form a complete library of medicine of itself, of well-nigh fifty volumes, each containing from 700 to 800 pages of closely but very clearly printed matter.

The entire work is under the general direction of Dr. Jaccoud, who has established for himself by his various pathological writings a foremost place in the ranks of French physicians. Among the fifty-five contributors whose names appear on the title-page are the best known medical and surgical authorities of France. This statement may be accepted as a guarantee of the excellence of the articles produced. The various subjects appertaining to medicine and surgery are treated in alphabetical order; each is separately and fully dealt with, and bears the signature of the author. Each article is, therefore, complete in itself, and in the case of many of the subjects we are presented with most complete monographs.

Among the most important essays in the eleventh volume are those on saccharine diabetes, by Jaccoud; on diathesis, by Raynaud; on digestion, by Paul Bert; on digitalis and digitaline, by Tardieu and Hirtz; on diphtheria, by Lorain and R. Lépine; and on dysentery, by Barrallier, of Toulon.

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[1] *Nouveau Dictionnaire de Médecine et de Chirurgie Pratiques.* Illustré de figures intercalées dans le texte. Tomes xi, xii (Délir—Em). Pp. 795 and pp. 822. Paris, 1869-70.

In the twelfth volume are equally important articles, on dysmenorrhœa, by F. Siredey; on dyspepsia, by A. Luton; on dystocia, by Stoltz; on mineral waters, by Tardieu; on puerperal convulsions, by Emile Bailly; on eczema, by Hardy; on electricity, by Jaccoud; on embryotomy, by Tarnier; and on poisoning, by Tardieu and Roussin.

A bibliography is appended to the several articles; in the instance of some of them this is very copious, in others, again, it is scant. There is also a wide difference apparent among the several writers in the acquaintance they have with books on the subjects they take in hand; in the case of some among them, their reading seems to have extended little beyond the medical literature of their own country, whilst in others a commendable knowledge of the work done in other regions is displayed. For example, Jaccoud indicates a wide knowledge of the literature of his subjects; but, on the other hand, we are surprised at the small reference to British writers in the articles on dysmenorrhœa, dystocia, and embryotomy, and German writers fare little if any better.

To some of the most elaborate articles, which possess the character of complete monographs, we shall have from time to time to refer when reviewing the present doctrines prevailing concerning the subjects on which they treat. At present we must content ourselves with recording a high opinion of the value of this great dictionary of medicine and surgery, and with wishing it success and its editor health and courage to sustain him in carrying on to completion so gigantic a work. Its extent will preclude its admission into many private libraries, but it should have a place in every public medical library which pretends to completeness, as a most worthy representative of medical and surgical science and practice in the present day.

Norton on Infantile Diseases.[1]—The title of this little book is calculated to arouse great expectations, but the whole gist of it lies in a nutshell, and is conveyed in the first page, to wit, "1st, that at least 90 per cent. of infantile diseases arise from one cause—improper food; 2nd, that the poisonous agent is starch; 3rd, that the rejection of starch food from infant dietary and a strict adherence to natural food are all that we require to ensure a rapid diminution of that excessive mortality which is so great a blot on the medical skill and knowledge of the present day."

According to the author, the diseases of infancy are divisible into two groups—1, those resulting from local or direct irritation, viz. diarrhœa, inflammation of stomach and of bowels, infantile consumption, marasmus, and infantile jaundice; and 2, those depend-

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[1] *On the Causes, Prevention, and Treatment of Infantile Diseases: showing by what means the present Mortality may be greatly reduced.* By SELBY NORTON, M.D., &c. London, 1870. Pp. 75.

ing upon referred or distant irritation, viz. laryngismus stridulus, bronchitis, pneumonia, broncho-pneumonia, convulsions, teething. These several maladies he considers individually, and finds "no difficulty in tracing one and all to the same cause, viz. starch feeding."

As he desires his book to be read "generally, as well as by the profession," "he has done his best to make the subject so plain that every one who can read may understand." With this end in view, and, further, with wide-reaching philanthropic intentions, he deals out some very milk-and-water pathology calculated for the nutrition of sucklings in physic, and tells his general readers how to cure the infantile diseases he has enumerated, and of the principal features of which he kindly informs them. These remarks will suffice to show our readers how far they may hope to realise the promises of information held forth in the preface of this uncalled-for production.

Ashton on Diseases of the Rectum.[1]—It is enough to notice the third edition of this treatise, which has been long and fully brought under public notice, and met with considerable favour. Besides a general revision of its contents, chapters on fissure and on cancer of the rectum have been added.

E. Wilson on Ekzema.[2]—"The lectures which form the chief matter of the accompanying pages were published in the 'Journal of Cutaneous Medicine;' while the essays and cases constituting the last chapter were partly published in the same journal and partly in other journals." This history of the development of this bulky volume on eczema absolves us from the necessity of setting forth at large the teaching of the author—a necessity that his reputation, based on previous works, and his position as professor of dermatology in the Royal College of Surgeons, would otherwise have imposed upon us.

Although in Mr. Wilson's estimation eczema, regarded "in a scientific sense," may deserve to be recognised "as the keystone of dermatology," yet to others than dermatologists a volume containing three hundred and eighty-six octavo pages will, we apprehend, seem a disproportionately large keystone in one small section of the medical edifice.

At the same time it is but fair to acknowledge that more than a third of the treatise is taken up by a description of the general anatomy and pathology of the skin, and by a disquisition on the classification of cutaneous affections. Moreover, in the last eighty pages much space is occupied with a catalogue of cases, to which we can assign little or no value. For example, we may quote the

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[1] *Prolapsus, Fistula in Ano, and other Diseases of the Rectum: their Pathology and Treatment.* By T. J. Ashton. Third Edition. London, 1870. Pp. 175.

[2] *Lectures on Ekzema and Ekzematous Affections: with an Introduction on the General Pathology of the Skin, and an Appendix of Essays and Cases.* By Erasmus Wilson, F.R.S., &c. London, 1870. Pp. 386.

following from the record of cases of eczema with infantile disorders, dentition and rubeola:

"A child, aged nine months, has a severe attack of eczema; it has been in existence for ten weeks, and was excited by the cutting of his teeth. He was brought up by hand from the fifth month."

"In another case, presented on the same day, the mother remarked that several of her children 'had cut their teeth with eczema.'"

Notes of this kind may have their worth in an observer's note-book, by way of affording numerical statistics in etiology, but they certainly are not worth printing.

As many as six varieties and ten subvarieties of eczema are recognised by the author, and are invested with names as difficult to remember as the varieties are to distinguish and appreciate. But besides these varieties, we are introduced also to a class of "eczematous affections," regarded as allies or modifications of the ordinary forms of eczema. To this class are referred lichen, impetigo, and scabies, the two former including several varieties.

The language of the book is marred by much pedantry. A fondness is displayed for big-sounding words, or, to borrow a phrase from the author himself, for "magniloquent nomenclature." Another illustration of the fact is furnished by the crude attempts at reforming the accepted orthography of words. Thus, ekzema replaces eczema, and following in the same wake we have skleroma, ekthyma, anthrakoid, and phlyktenodes. But the substitution of leichen for lichen and of clinicle for clinical is even more pedantic and censurable. Mr. Wilson should know that, in the transference of Greek words bodily into English, and also in words of Greek derivation, no constant law is observed in assigning the representatives of several letters, particularly such as have no exact parallel in our tongue, and thus it happens that more frequently than not the Greek κ is represented by the English *c*, and this substitution is sanctioned by such lengthened usage that it is vain nowadays to attempt to set it aside. Indeed, Mr. Wilson is not consistent in his attempt at reform, for he sanctions words such as hemicrania, in which the objectionable *c* usurps the place of a *k*. Moreover, with respect to the proposed word leichen, it is not, as represented, more correct Greek than lichen, though it is certainly more clumsy and un-English in character.

The author has challenged criticism on this matter of orthography by a paragraph of his preface and elsewhere, and we have consequently thus briefly referred to it, but the reader must not assume from our animadversions on the manner that the matter is equally open to them. Mr. Wilson has too profound a knowledge of his subject, and so wide an experience as a dermatologist, that a work by him of the kind under review must well repay study. Indeed, its contents are of undoubted value, though we could well wish they had been presented in a more concise and less pretentious manner.

Liveing on Skin Diseases.[1]—As the title imports, the subject-matter of this little book is the treatment of skin diseases; but as it was well-nigh impossible to do otherwise, in order to make clear what precise morbid conditions the author intended by the nomenclature adopted, a short notice of the symptoms and general charcter and course of the several maladies enumerated precedes the account of the treatment recommended.

The classification of Hebra is preferred and principally followed. The general indications and directions for treatment are those approved by the best dermatologists, and a useful collection of formulæ is appended which may prove suggestive, if not novel, to the medical practitioner who refers to them. A further appendix is added of definitions and of the derivations of words specially used in the description of cutaneous diseases.

The volume is of a handy size for carrying about, and maybe well recommended as a convenient memorandum book on skin diseases, replete with excellent information.

Russell's Tables of Air Humidity.[2]—This ingenious and useful diagram for ascertaining the humidity of the atmosphere, is printed on a sheet of card-board. Observations are first made by the hygrometer, or dry- and wet-bulb thermometer, and the temperatures of the air and of evaporation thus found, being employed according to the directions in the tables, the degree of humidity is discovered at a glance.

The mode of using the table is so simple and obvious, that we have no need to take the trouble of further describing it. But to test its correctness, we have compared it in ten places with the "Hygrometrical Tables" of Mr. Glaisher, which are chiefly relied on by practical meteorologists, and find the results nearly identical. Taking complete saturation as 100, the average differences do not quite amount to one-fifth of one in a hundred, exactly ·018, a difference of no importance, more especially as Mr. Glaisher's Tables do not go beyond the second place of decimals.

To all whose observations of the atmosphere are confined chiefly to its bearing on health and comfort, we can recommend this publication as a very compendious and easy way of arriving at the desired information respecting the amount of humidity in the air.

The edition of Glaisher's Tables we have used is the third, which differs very materially from the first edition.

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[1] *Notes on the Treatment of Skin Diseases.* By ROBERT LIVEING, A.M., M.D., &c. London, 1870. Pp. 90.

[2] *Table to facilitate Finding the Humidity of the Air.* By H. C. RUSSELL, B.A., Government Observatory, Sydney, N.S.W. Published by W. D. THOMSON, Upper Street, Islington, 1870.

Original Communications.

I.—Miscellaneous Contributions to the Study of Pathology.—By John W. Ogle, M.D., F.R.C.P., Physician and Lecturer on Pathology at St. George's Hospital.

CHAPTER III.

Cases in which morbid changes in the arteries of the heart were met with upon dissection, adduced with special reference to sudden death and symptoms of so-called angina pectoris. Observations on the pathogeny of angina pectoris.

In the 'St. George's Hospital Reports for 1867' (vol. ii, p. 285) I detailed a recent case of aneurysm of one of the branches of the coronary arteries of the heart, citing, in connection with the subject, other cases of aneurysm and dilatation of these vessels, and took occasion to draw attention generally to certain other pathological conditions which they, as well as the coronary veins, presented. Chiefly influenced by the opinion which on all sides I find still entertained by medical men, that angina pectoris is almost certainly associated with, or caused by, diseased coronary arteries, I was led to pursue the subject and to examine into the question of other diseases of these vessels, with special reference to what is termed angina pectoris and to sudden death, as illustrated by the post-mortem records which we possess at St. George's, as also by various cases selected from the 'Transactions of the Pathological Society.' The results of this inquiry, stimulated in great part by the increased consideration which has of late in various directions been given to the pathology of angina pectoris, I propose to incorporate in the present communication.

I proceed, therefore, in the first place, to give the particulars of such cases as I find we have in our records; and in connection with them shall allude to others of a kindred nature, contributed by friends and gathered from other, chiefly recent, sources. It will be seen that among these cases (excluding the case of aneurysm of the coronary artery above alluded to) are instances in which the coronary vessels were found plugged with masses of fibrine, or more or less occluded by thickening of their walls, either by what is termed atheromatous or calcareous matter (often conjoined), their calibre being thus encroached upon; or instances in which these vessels have been pressed upon by aneurysm of the aorta, or in which their orifices in the aorta have been obliterated by layers of fibrine overlapping them; or cases in which the elasticity of their walls have suffered by reason of their being rendered rigid owing to calcareous deposit, even when their calibre has not been much or at all diminished.[1] Of these, in some we have also, as might be expected, a calcareous and rigid condition of other vessels of the body.[2] I can find no cases in which mention is made of enlargement or dilatation of these vessels (a much more infrequent condition than a narrowed or coarcted state of the coronary arteries[3]), nor of rupture of these vessels with or without aneurysm.[4]

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[1] In the 'Transactions of the Pathological Society,' to which I shall allude later on, may be found cases recorded of "atheroma," of "ossification," of "fatty degeneration" of the walls of the coronary arteries, and also of "obstruction" by coagulum, as in a case by Dr. Baly (vol. iii, p. 267), and in a case of my own (vol. xv, p. 15), the vessel at the part plugged up by coagulum being *dilated*, an interesting fact in connection with the supposed production of aneurysm by embolism (see 'Med. Times and Gazette,' 1866; also 'St. George's Hospital Reports,' vol. ii, p. 285). There is also a case related of suppuration of the valves of the heart by Dr. Habershon (vol. vi, p. 151), "some of the branches of the left coronary artery extending to the softened part being filled with a yellowish-white thin purulent fluid."

[2] Hasse quotes from Goodisson (see page 72 of his work published by the Sydenham Society) a remarkable case, in which the aorta itself, the left iliac throughout, and the right iliac arteries for half of their course were completely blocked up, and rendered impervious, owing to calcareous degeneration of their walls.

[3] Of general dilatation of these vessels, the most decided case I know of is related at page 57 of the work above mentioned by Hasse. In this case, simultaneously with dilatation of the right cavities of the heart and of the vena cava, the coronary arteries were found to be widened to the *calibre of the subclavian vein.*

[4] I exclude, of course, as before said, the case of the aneurysm of the coronary artery above mentioned as being described in the 'St. George's Hospital Reports,' and which burst into the pericardium. The history of that case did not show that any symptoms, which might be termed those of angina, had ever existed, and the rapid death was the result of hæmorrhage into the pericardium; but in another case, which I mentioned in connection with the above one related by Bougon and quoted by Dr. Peacock at the Pathological Society (see 'Transactions,' vol. i, p. 227), the patient died in an attack of acute pain passing along the course of the spine to the back of the head. He had for four years been subject to pains in the chest, with a sense of suffocation, and inability to sleep; and these attacks (curiously enough) were somewhat relieved by WALKING RAPIDLY OR SUSPENDING HIMSELF BY THE ARMS. Death was caused by the bursting of an aneurysm of the right coronary artery. Cases of rupture of branches of the coronary artery do exist, apart from aneurysm. Thus in the 'Medical Times and Gazette' for March 29th, 1862, Dr. Osborne relates a case of angina in connection with rupture of a branch of the coronary artery. The patient, a woman, aged 60, had suffered long from dyspepsia and dyspnœa, and had had much family trouble. Three weeks

CASE 1.—*Sudden death; orifice of one coronary artery completely occluded; aneurysmal pouch of the aorta; aortic valves diseased.*

James B—, æt. 33, was admitted into St. George's Hospital, June 12th, 1844, with symptoms of disease of the heart, of dropsy, orthopnœa, &c. On the morning of the 15th he was much the same as usual, but in the evening he said he felt very ill, and was sitting up drinking, when he suddenly fell back and died.

Post-mortem examination.—Old pleural and pericardial adhesions existed; there was hypertrophy and dilatation of the heart. The mitral valve was healthy; the aortic one thickened. The root of the aorta was thickly studded with atheroma, and immediately above the aortic valve was an aneurysmal pouch of the size of a small egg, containing but little coagulum; the interior was *smooth* and lined by a polished membrane. A distinct thick ridge existed at the part where the pouch communicated with the aorta. *The opening of the posterior coronary artery* was completely blocked up by atheroma, that of the anterior one remaining pervious. [138.]

CASE 2.—*Sudden death after angina-like symptoms; orifice of one coronary artery obliterated by a layer of fibrine.*

Robert W—, æt. 43, was admitted into the hospital, August 25th, 1847. He said he had been suffering from palpitation of the heart and dyspnœa for some time, but otherwise had been pretty well, until dropsy came on suddenly, five days before admission, in the legs and abdomen. He had never had rheumatic fever. He was obviously suffering from hypertrophy and dilatation of the heart, with considerable disease of the aortic valve. Cough became

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before she had been seen by Dr. Osborne she had "felt something give way" in the chest on going up stairs. She died suddenly, and after death the pericardium was found distended with blood. The orifice of one of the coronary arteries in the aorta was almost invisible owing to atheroma; another coronary artery branch could be traced to a part of the wall of the left ventricle, which was fissured at the source of the hæmorrhage, which appeared partly to have taken place in the small coronary vessel, which was atheromatous and marked with blackish specks. The aorta was dilated and atheromatous.

In our Hospital Museum we have a specimen which I removed from the body, showing a cyst beneath the visceral layer of pericardium filled with blood coagulum, and which appeared to be the result of rupture of a branch of the coronary artery (see series vi, No. 41). It does not appear that any aneurysm existed, or that the case had presented any angina-like symptoms. Rupture of the coronary vessels is described by Feigneaux and Buys in Schmidt's 'Jahrb.' for 1859, vol. civ, p. 168. Here I would refer to an interesting case, terminating fatally by rupture of the right coronary artery, related by Dr. Galliardi. It is quoted in the 'British and Foreign Med.-Chir. Rev.' for Jan., 1841, p. 239, from the 'Annali Univ. di Med.,' Dec., 1839. There was what was called "arteriasis" of the aorta and large vessels given off from it, but the remainder of the ruptured coronary artery was natural.

excessive and paroxysmal, and severe pain in the chest and suffocative feelings, presenting the character of angina pectoris, came on, along with lividity and cold sweats. On recovery from this attack he vomited. He was about to have another attack when, in raising himself, he suddenly fell and died.

Post-mortem examination.—Both lungs, otherwise healthy, were compressed by a large quantity of blood-tinged serum. There was a large quantity of turbid fluid in the pericardial sac. In addition to the disease of the heart and its valves before noticed, on the surface of the aorta was deposited much atheromatous and calcareous matter; and over these in several parts were placed *layers of soft fibrine.* Owing to the deposit of this fibrine, the orifice of the *left* coronary artery was entirely obliterated. On tracing the artery back to its commencement, the continuation of the artery was of natural size. Both coronary arteries were the seat of atheromatous deposits. The liver was rounded at its edges and granular. Other organs were natural. [208.]

I will dwell for a short time on the above case, for the sake of referring to the fact of the opening of the coronary artery in the aorta being obliterated by a layer of fibrine. I am inclined to think that if search were carefully made, it would at times be found that what appears to be atheromatous blocking up of the mouth of a coronary or other small vessel was, in fact, the result of the deposition of fibrine from the blood on to the lining membrane of the larger artery (whose walls may, in addition, be occupied or not by atheroma or calcareous material). I suppose it possible this may be at times the explanation of those so-called atheromatous specks so often found at the origin of small vessels, as, for example, of the intercostal arteries of even young subjects, the tendency of the blood being to be delayed at the giving off from larger vessels of subordinate branches. It is certain that fibrine will become deposited on the surface of an artery, and become, as it were, organically welded with the lining membrane, and I have frequently peeled off such a layer of fibrine from an arterial surface or the lining of a heart's cavity.[1] Such deposit would naturally tend to thicken, and this at times unequally, especially if the arterial walls were previously diseased, so as to assume a generally atheroma-like character. Moreover, such a deposit or layer would tend to assume a smooth and polished surface, and even, no doubt, become provided by epithelium, just as the

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[1] The aorta, having "patches of lymph" adherent to its *inner surface*, is described at fol. 62 of our Post-mortem Book for 1846, and at fol. 105 of Post-mortem Book for 1851. In one such case, after maceration in spirit for many years, this deposit, examined microscopically, merely exhibited a quantity of semi-opaque yellowish fibroïd material. (See Museum Catalogue, No. 25, Series VI.)

lining of a cyst,[1] or even pleural adhesions,[2] may acquire an epithelial investment. I have notes of an instance of absorption of bone in a case of fractured spine, in which a covering of synovial membrane was eventually formed (No. 304 in our Post-mortem Book, 1866). Blood-clot lining the arachnoïd membrane of the brain will at times afford an instance of this tendency to become smooth and polished; and I have seen the inner surface of the clot of an aneurysm present similar appearances.[3] I have also seen effused lymph on the surface of the pleura, cevered by a thin but distinct membrane.[4]

The smooth white patches sometimes called "milk spots" often met with on the visceral pericardium appear often to be the result of patches of fibrine which have undergone similar change.[5] I can direct you to a case in our post-mortem book for 1862, p. 42, where the entire heart was covered by a thin false membrane; and another, 1858, p. 114, where the auricle was lined by a false membrane. In such cases, the membrane whether exo- or endo-cardial, might easily become smooth and polished like the more limited white patches.[6] Here it is right to allude to the description given by Bizot, and quoted by Hasse (p. 88, loc. cit.), of the false or so-called 'analogous membrane' which he has often found lining an artery and extending into an aneurysmal sac, sometimes covering contained coagula, but appearing at first sight like the internal true membrane of the aorta. The delineation of such a membrane is made by Carswell ('Fasc. Anat. Tissues,' Pl. II, fig. 8), and is mentioned by Hasse, A very curious case is also quoted by this writer from Wardrop's work on aneurysm, 1828, in which an aneurysm of the innominate artery existed, the entrance into the right carotid artery being closed by a thin membrane, apparently a continuation of the spurious one lining the sac. May we not compare such patches and opacities of blood-vessels and inner surfaces of the heart's cavities, presumably thus occasioned, to the patches and thickening of the capsules of various organs, such as the liver and spleen, resulting from peritoneal inflammatory processes (these patches being often like cartilaginous plates in appearance and toughness, and at times containing calcareous material); and to certain cases of thickening and stiffening of

1 In the "Pathological Society Transactions," vol. vii, p. 280, the case of an ovarian cyst lined by ciliated epithelium is described.

2 See Hasse's work, p. 203. (Sydenham Society.)

3 This was so in the previous case, No. I, and at page 250 of our P.M. Book for 1848 is described a case of spontaneously cured aneurysm, in which the clot is covered by a membrane exactly similar to the lining membrane of an artery.

4 See Fol. 189 of our Post-mortem Book for 1845.

5 At page 143 of our Hospital Post-mortem Book for 1857, a case is described of a "white patch" of the heart, covered by a *cyst*.

6 At fol. 205 of the Post-mortem Book for 1855, is a description of the auricle of the heart lined by a thickened patch, doubtless resulting from some deposition of fibrine from the blood.

the valve-segments of the heart, the results frequently of fibrinous deposits from the blood?

Case 3.—*Sudden death; narrowing of coronary arteries by atheromatous material; aneurysmal pouch of aorta.*

Mary P—, æt. 32, was brought into hospital, March 7th, 1863, quite dead. It was stated that she had never shown signs of any disease of the heart. On the morning of her death she had left home to see what she could on the occasion of the Princess Alexandra entering London. At about 11 a.m., when standing among other people, she fell down, and was picked up dead.

Post-mortem examination.—The cranium and its contents were natural, also the lungs, excepting some emphysema. A thick patch of old lymph covered the anterior surface of the heart near the apex. The walls of the ventricles were very thick. The heart weighed eighteen ounces. The left ventricle was rather contracted, containing much coagulum. The flaps of the aortic valve were slightly thickened. Over the ascending part of the aorta was an extensive deposit of soft atheroma, beneath the lining membrane. The CORONARY ARTERIES WERE VERY NARROWED AT THEIR COMMENCEMENTS by the encroachment of the deposited material. ONE ARTERY WOULD HARDLY ADMIT THE SMALLEST PROBE. The first two inches of the ascending aorta were very atheromatous, and here was a hemispherical aneurysmal pouch, which would have held a marble. The lining of the uterus was covered with pus. Otherwise the various organs were natural. [57.]

Case 4.—*Sudden death, described as from Angina pectoris; highly atheromatous state of the coronary arteries; heart hypertrophied.*

John W—, æt. 71, admitted February 18th, 1863. Had enjoyed good general health until subsequent to his return from Australia some weeks ago, when, after much mental trouble, he became subject to palpitation of the heart and a choking sensation, not accompanied by pain, but by a throbbing feeling, extending from the cardiac region to the epigastrium, and thence to the shoulder blades. These attacks occurred at variable intervals, and by night as well as by day. In less than an hour the effects passed off, and he was as well as usual. On admission he appeared to be robust; his face was, however, pale. He had the arcus senilis well marked, and he complained rather of giddiness, and pointed to the heart as the seat of his trouble. The brachial artery could be seen pulsating heavily through most of its course, and the pulse at the wrist was like the passage of shot under the fingers. The heart's sounds were distant and divided *in a triple manner,* a heavy rushing first sound being followed by a feeble sound, to which succeeded the diastolic sound, prolonged and devoid of the natural click of health. A double sound was heard over the arch of the aorta. There was no dyspnœa or œdema; iron and calumba were prescribed. On the 17th he had slept little, and had much præcordial oppression, but he was cheerful and walking about. Whilst in bed in the evening he was seized with severe dyspnœa; he gasped heavily and turned pale. Some bloody mucus was expectorated, and he died in ten minutes.

Post-mortem examination.—The cerebral arteries were atheromatous, otherwise the cranial contents were natural. The lungs were natural. The left ventricle of the heart was much hypertrophied and quite uncontracted; its fibre was natural. The coronary arteries were very atheromatous, as was also the aorta throughout; and the cartilages of the larynx were ossified. The abdominal organs were natural. The brachial arteries were very rigid, but not atheromatous. [223.]

Case 5.—*Somewhat sudden death; complete occlusion of one coronary artery by atheroma, and partial occlusion of the other.*

William S—, æt. 52, was admitted, July 17, 1866, in a moribund state, and shortly died, no history having been obtainable.

Post-mortem examination.—The fornix of the brain was rather softened, otherwise the cranial contents were natural.

There were indications of bronchitis having occurred, and the lungs were emphysematous. The pericardial sac was destroyed by adhesions at its anterior part, and the right ventricle of the heart contained a partially decolorised clot, the tricuspid valve being much thickened; the mitral valve was natural. There was great atheroma of the aorta, small transparent plates of it being detached along one edge, thus projecting into the vessel; and on their free edges an almost decolorised clot was found, extending about two inches up the vessel and commencing about one inch above the aortic valve. The ORIFICES OF THE CORONARY ARTERY were greatly ENCROACHED UPON by the atheroma, and in this way the CORONARY ARTERY OF THE RIGHT SIDE WAS ALMOST OCCLUDED, whilst that on the LEFT WAS QUITE BLOCKED UP. The left ventricle was hypertrophied.

The liver and kidneys were very congested. [205.]

Case 6.—*Sudden death; orifices of both coronary arteries much narrowed by atheroma.*

Henry McC—, æt. 39, was admitted August 16th, 1866, who had latterly been subject to dyspnœa, and came in with anasarca of the legs and abdomen. When admitted the heart's sounds were weak and its rhythm variable, but there was no bruit. The cardiac dulness on percussion was increased, and there was slight albumen in the urine. Under treatment he did well for some time, and the dropsy decreased, but it was noticed that he became very purple on exertion. On the 20th of August he suddenly fell off the bed, and was found dead.

Post-mortem examination.—The lungs were congested, and contained patches of hæmorrhage. The heart, weighing 18¾ ounces, was quite contracted, and both sides were full of black coagula; its coats were thin and very friable, and, *microscopically* examined, showed much fatty matter in its fibre. The valves were healthy, and around the origin of the coronary vessels so much atheroma existed THAT THEIR ORIGIN WAS GREATLY OCCLUDED.

The liver and kidneys were very congested. [241.]

CASE 7.—*Sudden death; orifice of the coronary arteries occluded by atheroma; firm old clot in the carotid artery.*

A man, æt. about 30, a groom, had always enjoyed good health until about a week or ten days before his death. He then began to complain of a feeling of "being out of sorts," accompanied by aching pain in the loins and slight cough. He continued at his work until the day of his death; on that day he became excited in consequence of a dispute with another person, complained of faintness, and fell back suddenly, and died before assistance could be obtained. As far as is known he never suffered from angina, but as he was unmarried and lived in lodgings by himself, very little was ascertained about him. No account of his previous mode of life could be obtained.

Post-mortem examination five days after.—The body was found to be very much decomposed.

There was considerable congestion of the lungs; the anterior parts were emphysematous, the air-cells being in many cases ruptured, large vesicles containing air being found.

The heart was closely contracted and empty; the muscular substance was firm and natural in appearance; there were a few specks of atheroma on the mitral valve. The aorta was extensively affected by soft atheroma, especially near its origin; some of this atheroma had almost entrely OCCLUDED THE ORIFICES OF THE CORONARY ARTERIES, so that only a small probe would pass through them; these arteries were throughout the rest of their course natural.

The liver was slightly fatty. The kidneys were in a state of granular degeneration; they were smaller than natural, rough, and cystic on their surface; their capsules were adherent, and on section the secreting structure was seen to be diminished. The other abdominal viscera were healthy.

The brain was so soft and decomposed that very little could be made out of it. The only morbid lesion in this organ that could be found was a firm decolorised clot in the left internal carotid artery, just at its bifurcation into the anterior and middle cerebral; this clot was not adherent to the wall of the vessel.

The details of the above case, which was not a hospital one, were kindly given me by Mr. Pick, now one of my colleagues at the hospital.

CASE 8.—*Obliteration of one coronary artery; contraction of the left carotid artery.*

W. B— was admitted into St. George's Hospital December 30th, 1840, and died January 20th, 1841. Unfortunately we have no history of his life symptoms on record.

Post-mortem examination.—General adhesions of the pleura on both sides; lungs œdematous, otherwise healthy. A small quantity of blood existed in the pericardium, and a white spot on the surface of the left ventricle. Heart flaccid and filled with blood coagula. The mitral valve was healthy, the semilunar valves slightly thickened. *The opening of the anterior coronary artery was obliterated,* that of the posterior one being also very much contracted by atheroma. The ascending aorta and the arch were very much dilated, the dilatation being general, and in its cavity were contained coagula of blood of recent formation. The opening of the *left carotid artery* was so contracted as scarcely to admit of the passage of a probe; the trunk of the artery was, however, of natural size; the brachio-cephalic branch was slightly dilated. The whole of the aorta, from the heart to the iliac arteries, was very rough internally, owing to large patches of atheromatous deposits. At one spot only was there any calcareous deposits, viz. at the opening of the left carotid artery. The internal membrane was, in many places, easily separable from the middle coat of the aorta.

Excepting thickening and condensation of the interlobular areolar tissue of the liver and of its capsule, the various abdominal organs were natural. [20.]

CASE 9.—*Sudden death; considerable occlusion of the orifice of one coronary artery; valvular disease of the heart.*

Mary H—, æt. 30, was brought into the hospital September 11th, 1866, having suddenly died; but no details of her history could be obtained.

Post-mortem examination.—The body appeared externally to be in all respects in a natural state. The lungs were very congested, and the bronchial tubes full of frothy fluid. The heart was contracted and empty. The aortic valve-flaps were much thickened and opaque, and quite insufficient. One of them was so much contracted that the end of the little finger could not be introduced. The mitral valve curtains were also thickened: the aorta, for half an inch was much thickened, and covered with "soft atheroma." Around the orifice of the coronary artery of the left side much of this deposit existed, so that *the opening of the vessel was materially contracted, barely admitting a fine needle*: below this the coronary artery was natural. The muscular striæ of the heart were found (*microscopically*) to be very indistinct. The capsules of the kidneys were adherent, and easily split, and their secreting structure diminished. Other organs of the body natural.

I will here add a case, kindly put into my possession by Mr. Warrington Hayward, our surgical registrar at St. George's Hospital. Unfortunately the medical man who made the post-mortem examination is now dead, and the details are not given with the fulness which otherwise would have been obtained. It was as follows:

Case 10.—The patient was a clergyman, æt. 52, of active habits, in whom, for about a year, breath had been rather short. This he attributed to his having become much stouter. Three weeks before death he first experienced an attack of angina pectoris, which was severe. He had only three or four more attacks before the fatal one. He died very suddenly; not after any particular exertion. "Fatty disease of the heart, and obstruction of the arteries of the heart," were found after death. No statement exists as to the actual cause of the obstruction; and it may be supposed that it was owing to atheromatous deposit.

Case 11.—*Death, unexpected, following pain at the heart. Coagulum filling a portion of one coronary artery, and also other vessels.*

The patient was a woman æt. 56, who died in St. George's Hospital, and who had been the subject of so-called *hysteria*,[1] and who had complained greatly of severe pain at the region of the heart shortly before death. After death, softened fibrinous clots were found in the heart's cavities; in addition to other morbid changes, it was found that a portion of the wall of the left ventricle had became converted into a yellowish-white material, and that a branch of a coronary artery passing through this had become blocked up by a mass of fibrine. The left corpus striatum was softened, and the left internal carotid artery at one part contained a mass of firm fibrine and clotted blood.

[This case I have already reported in vol. xiv of the 'Pathological Society's Transactions,' p. 3, and therefore I have not here given it *in extenso*. The preparation showing the heart is preserved in our Hospital Museum, as No. 48 in Series VI.]

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[1] The frequency with which symptoms from serious disease of the brain simulate so-called hysteria cannot be too often dwelt upon.

I will here quote two cases from our hospital records which have been brought before the notice of the Pathological Society of London (see 'Transactions,' vol. xvii, p. 54), by my colleague Dr. Dickinson, from whose reports I make my abstract. They were adduced by him, along with another interesting case, from private practice, in connection with the subject of angina pectoris.

CASE 12.—*Sudden death; angina pectoris; great narrowing of both coronary arteries by atheroma.*

James H—, æt. 42, had for more than a year had pain after food, about the epigastrium, and latterly had had severe attacks, chiefly also after food, which had all the acknowledged characters of angina pectoris. The general health was pretty good.

On the fifth day after he came into St. George's Hospital he had a sudden attack of the excessive pain; he quickly became blue, and had a short but distinct epileptic attack. After this, in spite of treatment by galvanism, artificial respiration, &c., he gave no further signs of life.

Post-mortem examination.—The left ventricle of the heart was quite uncontracted, and contained fluid blood. The valves were natural. There was much soft atheroma under the lining membrane of the aorta, and some of this had encroached upon the mouths of the coronary arteries, and so narrowed them that neither could admit the head of a common probe without pressure. [It is not stated whether the coronary arteries were otherwise natural or not.] The heart weighed 13½ oz.; its muscular structure was apparently natural. The other organs of the body were natural.

There was no indication showing that this patient had suffered from ordinary symptoms of cardiac disease.

CASE 13.—*Sudden death; ? angina pectoris; complete closure of one coronary artery and slight narrowing of the other by atheroma.*

A soldier, æt. 35, was brought into the hospital dead. It was supposed from circumstances that he had had sexual intercourse shortly before date. It appeared also that he had been discharged from the army owing to heart disease; but nothing more was known of him.

Post-mortem examination.—The heart weighed 14 oz.; its left ventricle was quite uncontracted; the right partly so; and both contained fluid blood. Under the lining membrane of the arch of the aorta was much soft atheroma, which had so encroached on the opening of the right coronary artery as completely to close it, and the lining membrane of the aorta appeared to be continued over the mouth of the vessel. The opening of the left coronary artery was also much narrowed. The coronary arteries in their course were natural. The aortic valves were slightly thickened, but all the valves were effective. The muscular fibres of the heart were somewhat dotted with oil.

To the interesting observation and comments on these cases, and the supposed relation between the symptoms and the closure of the coronary arteries, by Dr. Dickinson, I shall refer later on.[1]

In all the above-mentioned cases of disease of the coronary arteries, either sudden death occurred, or symptoms akin to angina pectoris were occasioned during life.[2]

I will now quote two cases from our records, in which much disease of these vessels was found, but in which *no* angina pectoris or sudden death had occurred, which are sufficiently interesting, I think, to be placed on record.

CASE 14.—*Highly atheromatous state of the coronary arteries.*

William W—, æt. 54, was admitted November 8th 1854. About one month previously he had been taken ill with a "bad cold," followed by anasarca of the legs, and pain in the limbs generally. On admission his pulse was full and quickish, and the tongue dirty, and bronchitic sounds were heard over the chest. He improved under treatment, but subsequently dyspnœa in urgent paroxysms came on. The urine was highly albumenous. Listlessness and partial coma came on, and after an attack of dyspnœa the patient died.

Post-mortem examination.—The only parts examined were the heart and kidneys. The kidneys were found cysted and otherwise far advanced in disease, containing also a great number of yellow calcareous deposits, especially near their surfaces.

The pericardium contained several ounces of yellow fluid and one or two "white patches" existed on the surface of the heart. Both ventricles were very thickened, and their cavities dilated. The left one was almost but not quite contracted. One of the

[1] The third case which Dr. Dickinson mentions, is one of a man, æt. 45, who was a patient of Dr. Dudfield, of Kensington, and who for eight years had had symptoms of heart disease, and for three years that of much marked angina. In one attack which Dr. Dudfield attended the patient was *pale*. He died rather suddenly.

Post-mortem examination.—The heart was enlarged; its ventricles both contracted, and the root of the aorta and aortic valves very atheromatous, and by the encroachment of atheroma the orifice of the left coronary artery had become completely closed. The right artery was also much narrowed. The coats of these vessels were natural. [The state of the valves is not mentioned.] The heart's substance was somewhat fatty.

[2] Among recently published cases of angina pectoris, in which, after-death, affections of the coronary arteries were found, I may mention one described by Dr. Morehead, in the 'Lancet' for July 30, 1859. The case was of fifteen years' standing, and death occurred suddenly by syncope. The heart's valves were quite healthy, but the coronary arteries for one and a half inches from their origin were rigid tubes, owing to calcareous deposits, and the heart was very fatty.

aortic flaps had a tuft of old fibrous tissue attached to the corpus Arantii. The coronary arteries were, in places, very highly atheromatous, and in parts very rigid and brittle. [15.]

Case 15.—*Complete obliteration of one coronary artery.*

John S—, æt. 64, a wheelwright, was admitted February 18th, 1863. He stated that he had had a "convulsive attack" one month before admission, in which he lost consciousness for ten minutes. A weakness of the left hip and knee, which only lasted a few days, was the only apparent effect of the attack; but shortly afterwards he became affected with dyspnœa, cough, and expectoration. Anasarca came on. He had evidently hydrothorax, and the heart's action was irregular and weak, and the sounds distant; no murmur was audible. Two attacks of lividity and exhaustion with dyspnœa preceded death, which occurred March 26th. There is no mention of symptoms which would be termed neuralgic, or those of angina.

Post-mortem examination.—Indications of both old and recent pericarditis existed; the heart was greatly dilated, and the right auricular appendix was occupied by a ragged coagulum of old standing. *One coronary artery only could be discovered.* This was excessively atheromatous, so that it would not admit the smallest probe. There was *certainly no orifice for the other vessel* upon the aorta, and the great accumulations of false membrane upon the heart made it difficult to detect it in its course. The heart and pericardium weighed twenty-eight ounces. The left pleural sac was full of serum, much compressing the lungs. The right lung was infiltrated with serous fluid, and the smaller branches of the pulmonary artery, in its substance, were packed with coagulum of a brownish colour. The kidneys were slightly granular and cysted.

Upon the upper surface of the right hemisphere of the brain was a small cavity, which was covered only by the arachnoïd, probably the result of a superficial extravasation of a former period. This was just over the fissure of Sylvius vertically; within the white matter, above the left ventricle, was a similar but smaller cavity. *The white matter generally was full of pores or little round depressions.*[1] [82.]

Were it desirable I might of course multiply from our hospital records cases of simple atheroma or calcification of the cardiac coronary arteries, in which no symptoms severe enough to be denominated those of angina pectoris, existed, although in some cases there had been a certain degree of pain in the heart; and also I could refer to several specimens which our Pathological Museum contains, showing the same

[1] This pitted or porous condition of the brain I have seen in two or three other cases. My colleague, Dr. Dickinson, associates it with diabetes, I believe; but it is certainly found where no diabetes was known to have existed.

condition. The readers of Dr. Bright's 'Reports' may remember a case which he describes of apoplexy, in whom after death the coronary arteries of the heart were converted into bone-like canals, and in whom no angina and no cardiac symptoms of any kind had existed. Dr. Tweedie, in vol. iv of his 'System of Practical Medicine,' quotes a case described by Mr. Adams in the 'Dublin Hospital Reports,' in which both the coronary arteries were *obliterated* for nearly an inch at their origin. In this case there had been no pulse at all during life *in any part*, and even at the heart no indication of motion beyond a very feeble undulating sound. These symptoms are attributed to the condition of the coronary vessels, together with ossification of the aortic valve which was found.

I will subjoin a brief notice of a case, in which a branch of the coronary artery was blocked up by a mass of fibrine, but in which no symptoms referrible to angina were produced.

Case 16.—*Atheromatous state of the coronary arteries. Obstruction of one by coagulum.*

The symptoms were those of weak heart; a peculiar nervous condition, and frequent transitory attacks of partial loss of consciousness and muscular power also showed themselves. Anasarca and ascites came on, and the patient lingered long before death. A fatty state of the heart's muscle was found on post-mortem examination. The coronary arteries were converted into rigid tubes of atheroma, and one was quite occluded at its orifice by an old standing yellow and partially softened coagulum.

The specimen showing the condition of the heart was presented to our Pathological museum by Dr. A. W. Bell, and is described in the catalogue as No. 239 in the appendix to series 6.

The following case is an instance of more or less occlusion of the coronary arteries, without, however, any positive symptoms of true angina pectoris occurring.

Case 17.—*Sudden death; somewhat fatty heart; atheroma contracting the orifices of the coronary arteries.*

Samuel W—, æt. 64, had been subject to paroxysmal attacks of dyspnœa, and was known to have heart disease, was stated to have been in Hyde Park when he had one of his attacks, attended by painful sensation of tightness about the throat; staggered and fell. He was brought into the hospital dead.

Post-mortem examination.—The veins and sinuses within the cranium were much congested, as also the brain substance. The lungs were somewhat congested. The heart was large, weighing 15 ounces, and was covered by much fat. Both ventricles were in a state of semi-contraction, and contained a little fluid; its muscular substance was rather soft and fatty, and paler in colour than natural. The valves of the heart were blood stained. The arch of the aorta was covered over by patches of softening atheroma, and one of these patches was immediately above the left coronary artery, the orifice of which it overlapped, and had contracted so much that it was impossible to introduce a fine probe. The orifice of the right coronary artery was also much narrowed, though to a less extent, from the same cause.

The coronary arteries themselves were natural, except one or two slight patches of atheroma. The muscular substance of the heart contained an increased amount of fat globules, but when examined *microscopically* the "striæ" were well seen.[1] [?]

In contrast and in addition to the above-mentioned cases of positive and well-marked or even extensive disease of the coronary heart-vessels, in some of which sudden death or angina-like symptoms had occurred, in others of which they had been absent, it is interesting to notice the following case, in which the symptoms were described in our books as being most likely those of angina, but in which no affection of the coronary vessels is noted.

CASE 18.—*? Angina Pectoris.*

John S—, æt. 29, was brought into the hospital dead, August 6th, 1862. He had come to London from Newcastle-on-Tyne, to visit the great Exhibition, and during his stay in London had paid little attention to diet, getting his meals very irregularly. On the day of his death he went to the International Exhibition at 10 a.m., and complained of some pain in the region of the heart. He moved about the building until half-past 11, when he again complained of the pain at the heart. He put his hand to his side, and the next moment fell forward dead.

Post-mortem examination.—The body was well nourished, and the rigor mortis decided.

Cranium.—The large intra-cranial veins were full of blood.

Thorax.—The pericardium contained a small quantity of blood-stained fluid. Both ventricles of the heart were uncontracted, the left especially, and contained quite fluid blood, the lining membrane being blood-stained. The muscular substance was generally flabby, the cut surface mottled with yellow. The muscle was found, under the microscope, to be more or less fatty.

The lungs and bronchial membranes were congested.

Abdomen.—The stomach was distended with gas. The various organs were natural. [213.]

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[1] This case has been reported in the 'Lancet' for October 24th, 1868.

As I before observed, it by no means follows that symptoms of angina pectoris should have existed, or sudden death should have occurred in cases where the coronary vessels of the heart have been found very extensively diseased. Very frequently, indeed, whilst curator of the Pathological Museum, I found extensive atheromatous rigidity and brittleness of the coronary arteries in cases where no heart symptoms of any kind had previously existed.

For the following case of sudden death with angina-like symptoms, in which, nevertheless (as in the preceding case), no affection of the coronary vessels was noted, I have to thank the late Sir John Davy. The heading to the case and the "Remarks" which follow it are his own.

CASE 19.—*Spasmus Cordis ?*

W. S—, 51st Royal Artillery, æt. 28, was admitted into Corfu Hospital, 12th May, 1825, died 13th May. He was a man of dissipated habits, given to vice, and had been several times in hospital on account of pain in right hypochondrium; robust, healthy in appearance; on previous day he came over from Vido, where he worked at the limekiln, and spent two or three hours with a woman of the town. In the afternoon he was admitted into the hospital of 36th, complaining of pain in the region of the heart and in the right side, along the margin of the ribs. An aperient was given: at 7 P.M. he felt relieved. He then felt some pain in walking, but was free from it when in the recumbent posture. He talked cheerfully with the men in the adjoining beds till between 10 and 11 P.M. About 1 A.M. the man in the next bed heard him moan twice or thrice. He rose and went to him: he was lying on his back; he turned on his side and expired. It is worthy of notice that when Mr. Bouchan examined his pulse in the evening, at the *left* wrist it was *small, intermitting,* and only 40; at the right 70, and *regular and natural.*

Dissection eleven hours after death.—Limbs slightly rigid; neck and shoulders livid, apparently from gravitation of blood.

The vessels of the brain generally very turgid; slight serous effusion between the membranes and in the convolutions, and pretty much fluid in all the ventricles.

About two ounces serum in the pericardium. The heart rather large; the cavities of the heart merely moistened with blood; the valves sound; the aorta, including the A. innominata, and the arch, rather larger than usual: inner surface rough, becoming opaque in patches, and in a few points ossified.

The lungs sound, without adhesions and without tubercles; a little frothy mucus in the trachea. The tongue small and retracted so as to cover apparently the rima glottidis.

The liver weighed 4 lbs.: rather firmer than natural: so the spleen. Some adhesions of the gall bladder to the omentum.

No marked disease of the abdominal viscera; bladder empty; prostate and vesiculæ seminales large, the latter distended with an opaque milky fluid; the lining membrane of stomach and duodenum rather red.

"*Remarks.*—What was the cause of death in this instance? Was it connected with empty state of heart and turgescence of brain? As the head was low after death the unusual turgescence might have been partly owing to the posture. Was it owing to spasm of the heart? This organ being empty is favorable to such an idea. Was it owing to suffocation from retraction of the tongue over the epiglottis?

"The irregular state of pulse, the pain in the region of the heart support the idea, that spasm of this viscus was the principal cause.

"It is curious that the vesiculæ seminales were distended, as there is reason to suppose he had sexual intercourse the preceding afternoon. Perhaps this was owing to large size of the prostate."

I must observe, in passing, upon the remarkable phenomenon in the above case of the pulse differing so much at the two wrists, as well in frequency as in rhythm. In illustration of this, though not quite analogous, I would refer the reader to the description of a case of poisoning from the effects of chloral, related by Dr. Reynolds in the 'Practitioner' for March last, in which it was noticed that whilst the heart was acting with increased power but regularly, the action of the distal arteries was rapid, weak, *irregular*, and intermittent.

Thinking it would be useful to ascertain what we could learn from the 'Transactions of the Pathological Society' regarding the supposed connection of angina pectoris and disease of the coronary vessels, I find that in them are recorded several supposed cases of this affection, in which the condition of the heart and its nutrient vessels was very variable. [In some cases, however, the coronary arteries are unfortunately not mentioned.] And again, numbers of cases of disease of these vessels without any history of angina having existed. I observe *nineteen* cases in which angina pectoris, or angina-like symptoms occurred. They are as follows:—in the first case, dyspnœa and other heart symptoms having existed (vol. i, p. 238), related by Dr. Crisp, we have extreme calcification of the coronary arteries, a very large flabby heart and dilated left ventricle, and atheromatous and calcareous state of the aorta, the valves of the heart being natural. In the second case (vol. ii, p. 48), related by Dr. Peacock, and attended by dyspnœa and other heart symptoms, was found bone-like deposit in the right sinus of Valsalva, surrounding and contracting the orifice of the right coronary artery, the left one being converted into a cylinder of bone and quite *occluded* by a plug of *old decolorised fibrin:* the heart being fatty and the aorta dilated, and slight thickening only of the aortic and mitral valve flaps existing. In case third, related by Dr. Quain (vol. ii, p. 188), there was fatty degeneration of the heart, the aorta being calcified and the left coronary artery much diseased and obstructed, and the left ventricle dilated. In the fourth case (vol. ii, p. 190), also related by Dr. Quain, was fatty degeneration of the heart, the coronary artery leading to a part where blood had been extravasated into the heart's substance, being narrowed and ossified. It does not seem that any heart symptoms had existed.[1] In the fifth case, related by Dr. Beith (vol. iii; p. 69), was rupture of the right ventricle and septum of the ventricles, ossification of the arch of the aorta and of part of the anterior branch of the coronary artery (with obstruction) leading to the seat of rupture, which was very fatty; the heart being very large. Dyspnœa and fainting fits had been experienced. In the sixth case, related by Dr. Quain (vol. iii, p. 262), was fatty degeneration and rupture of the heart in connection with a branch of a coronary artery, which was nearly occluded by calcareous degeneration. It does not appear that any cardiac symptoms had existed during life. In the seventh case (vol. iii, p. 276), related by Dr. J. Bird, was a fibrous degeneration of the heart, in connection with acute rheumatism (no mention made of the coronary vessels). In this case the physical signs during life, and state of the pulse, &c., pointed to cardiac disease, but there had been no extreme cardiac symptoms. In the eighth case (vol. iii, p. 314), related by Mr. Obrè, there was dilatation of most of the arch of the aorta, producing pressure on and diminishing the calibre of the left bronchus, and also thinning of the heart's walls. Symptoms of heart disease had long been suffered (no mention made of coronary arteries). In the ninth case (vol. iv, p. 99), also related by Mr. Obrè, one in which very slight symptoms had ever existed, there was a broken down state of the aortic valve-flaps, with hypertrophied heart (no mention of coronary vessels). In the tenth case (vol. v, p. 73) related by Dr. Brinton, and one in which considerable heart symptoms had existed, was atheroma and dilatation of the aorta, with a pouched state of one aortic valve-flap, the coronary arteries being very cartilaginous, though scarcely calcified. In the eleventh case (vol. vi, p. 133), related by Dr. Wilks, was fatty degeneration of the heart, the whole course of the coronary arteries being converted into bone-like channels, and only one existing to supply the entire heart; the left ventricle was dilated. No cardiac symptoms appear to have existed beyond the angina. In the twelfth case (vol. vii, p. 102), related by Mr. Hutchinson, there was aneurysm of the aorta, compressing the pulmonary artery, the coro-

[1] When I speak of ordinary "heart" or "cardiac" symptoms, I of course mean palpitation, dyspnœa, anasarca, &c., as distinguished from breast-pain.

nary arteries being found to be healthy. In the thirteenth case (vol. viii, p. 164), related by Dr. Markham, and one in which much dyspnœa had been suffered, there was ulceration and other disease of the aortic valve-flaps (no mention of coronary arteries). In case fourteen (vol. ix, p. 180), described by Dr. Hare, there was dilatation of the arch of the aorta, and there had been well-marked symptoms of heart disease. In case fifteen (vol. xi, p. 67), related by Dr. F. W. Davis, and one in which dropsy and orthopnœa in addition to angina had existed (and there had also existed in this case a visible pulsation, besides a double bruit under the third costal cartilage, and one inch to the right of the sternum, so that aneurysm was at one time suspected), was fatty degeneration and rupture of the right ventricle, extensive atheroma of the aorta and calcification of the coronary arteries, which were patent, the right one being converted into a hard bone-like tube, and the right auricle dilated and thinned, being in one part "almost perfectly transparent;" the aortic valves were incompetent, the others being natural. In case sixteen (vol. xii, p. 72), related by Dr. Quain, was fatty degeneration and rupture of the left ventricle, the coronary arteries being atheromatous. In this case the only symptoms referable to the heart had been "short breath" on going up a hill. His death followed attacks of "gastrodynia" and "dyspepsia," and was quite sudden and attended by great pain at the heart. In case seventeen (vol. xiii, p. 54), related by Dr. Crisp for Dr. Williams, a supposed case of aortitis, there was fatty degeneration of the heart, very great narrowing and atheroma of the aorta, which was of a dark livid colour, and extreme smallness of the orifices of the coronary arteries. In this case the pain at the heart had been so great as often to render the patient almost maniacal. No symptom of heart disease had existed beyond this acute pain, and previous uneasiness at the breast for eight or ten minutes.[1] In case eighteen (vol. xiv, p. 133) there was obstructive mitral disease (no mention of coronary vessels); and in case nineteen, described by Mr. S. Watson, and one in which during life heart disease had been recognised by irregularity of its action and by physical signs (vol. xix, p. 170) there was, in addition to atheroma and extensive ulceration of the aorta, complete occlusion of the left subclavian and left coronary arteries at their origin, and almost complete closure of the other coronary artery at its origin. The coronary arteries were otherwise natural.[2]

[1] Some authors speak of aortitis as if it existed much more commonly than is generally thought. My own experience of it is very scanty. I may note here that Dr. Corrigan describes, as among the symptoms of "acute aortitis," tearing or constrictory pains at the præcordia, with a disposition to faint, and an impression of impending death; in fact, the symptoms of angina pectoris. (See 'Guy's Hospital Rep.,' Vol. 6, 1841, p. 324.

[2] It is specially mentioned in this case that the muscular structure of the heart was natural, being "of good colour and firm consistence."

Such are the examples I find in the records of the Pathological Society of symptoms of angina pectoris, in which obvious changes in the nutrient vessels of the heart, or in the substance of this organ were met with after death.

On the other hand, I find in these volumes descriptions of thirteen cases of atheroma of the coronary arteries, in only one of which were angina-like symptoms said to have occurred. In several of these there was fatty degeneration, and in one or two rupture of the heart. In one case the coronary artery was closed by coagulum, and in one the artery, which was atheromatous, was *ulcerated*. I also find one case in which fatty degeneration of the coronary arteries existed, four in which obstruction is said to have existed, and eight in which calcification is described as having been found, in all of which save one (and in that there was fibrous and fatty degeneration of the heart's muscle), no mention of angina-like symptoms existed. In three of the cases of obstruction of the coronary artery, rupture of the heart, and fatty degeneration existed. In one of these cases (vol. v, p. 336) we have the remarkable history of a man living in good health to the age of ninety-three, in whom after death the heart was found to be natural, but the right coronary artery was thickened though not bone-like, the left one bone-like almost throughout its entire length. The aortic and mitral valves, &c., were also much diseased. After a dangerous attack of syncope this patient had been kept two years in bed before death. In several of the above cases of ossification and obstruction of the coronary vessels sudden death had however occurred.

To anticipate somewhat, perhaps, I would here quote the remarks of Dr. Crisp, who observes that many morbid changes about the valves and aortic orifice may occasion the same symptoms as an ossified state of the coronary arteries, and it is impossible to discriminate between them. To this opinion I shall have to allude later on. He says, and truly, that the coronary arteries are seldom ossified without the same deposits taking place in the aorta or its valves, and that he has only met with two cases in which these vessels were *alone* implicated. In both cases angina pectoris existed.

II.—Epidemiological Conclusions and Suggestions. By Gavin Milroy, M.D., F.R.C.P., Vice-President of the Epidemiological Society, &c.

I. All who have attended to the history of epidemic diseases must be surprised to find how little we have yet really learned of their attributes or qualities as objects of physiographical research, and how small and uncertain has been the advance from one age to another of anything like sure knowledge respecting them. No other branch of natural science has been so stationary and unprogressive, and in none can its votaries point to fewer established truths or demonstrated general principles. The accumulations of experience do not seem to have rendered the conclusions of epidemiology at all more stable or steadfast. What has been confidently asserted one year is often contradicted a few years later, and doctrines accepted at one time are rejected at another, to be again, perhaps, brought forward with favour, and to be again repudiated or neglected.

> "Our little systems have their day,
> They have their day and cease to be."

Whence all this fluctuation and uncertainty? and how comes it there should be a ceaseless conflict of opinions and opposition of views in respect of most epidemiological questions? Is it because the subject matter is intrinsically so very intricate and obscure, and the phenomena with which it has to deal are so fleeting and variable as to defy all attempts to systematically note or to classify and arrange them, and thus to frustrate any reasonable hope of ever explaining their nature, and of determining their relations either to each other, or to other cosmical conditions or events? Or, is it because the mode hitherto of investigating this branch of natural knowledge has been itself at fault, and that the chief cause of failure in the past lies less in the essential difficulty of the problems to be solved than in the defective or improper method of our seeking to effect their solution?

Both reasons have had to do with the result. The general subject of epidemiology is indubitably mysterious and complex from the very nature of the phenomena and relations to be investigated; and then the plan ordinarily followed in endeavouring to interpret them has certainly not been the one which is recognised to be indispensable to the successful study of every other branch of physical inquiry. All will admit that the investigation of the attributes and actions of living bodies is necessarily more difficult than that of the qualities and properties of inanimate matter; and to this must be added that the investigation of the phenomena of life when disordered from disease is more difficult and complex than when it is healthy and normal. The difficulty becomes greater still when the disease is of only occasional occurrence, and its invasions are of merely temporary duration. It is only now and then that epidemics appear and can be watched. Moreover, different epidemics of the same distemper exhibit, under what seem to be similar conditions, signal differences in their general manifestations in various ways; and even in respect of its ordinary natural attributes or qualities—as, for example, of the period of its incubation, the activity of its power of propagation and diffusion, &c.—there is often much variableness and a remarkable want of congruity. The difficulties thence arising are not a little increased by the circumstance that many epidemiological phenomena and events are the products not of a single determinate cause, but rather of the combined operation of several concurrent elements or factors, each of which is liable to vary in potency at different times, and under different conditions and combinations.

If then epidemiological inquiries are from their very nature the very reverse of being simple and easy, it is obviously the more incumbent that the method adopted in exploring and interpreting the occurrences and questions with which they deal be such as invariably to insure the greatest amount of probability at least, if complete certitude is not always attainable. There is but one method of scientific research that can be successfully used in our endeavours to explain the phenomena taking place around us in all other departments of natural knowledge, and that, it is well known, is the slow but certain one of inductive investigation. What, therefore, has been the instrument of achieving such remarkable and often unlooked-for successes in their case, may reasonably be expected to lead to equally valuable results in respect of epidemiology, if we will but be content to submit to its requirements and to obey its precepts. A good deal of misconception often exists as to the exact meaning of the term 'induction' and of what it exacts at the hand of the student. As used by Bacon and his disciples, it is not equivalent to 'inference,' as is sometimes imagined, and as, indeed, the French employ the word; and it must be carefully distinguished from mere 'deduction.' In the inductive process, to be rightly pursued, three acts or steps have to be gone through. There is *first* the collection of a number of facts or instances, which have been carefully observed and accurately recorded. All data before being accepted must be thoroughly authenticated, and free from doubt; they must, moreover, be duly circumstantial and detailed. The bare register of phenomena or events without any notice of the attendant conditions is rarely of much use; while uncertified, or partly conjectural, statements respecting them are generally worthless, and oftener mislead than otherwise, when we come to reason about their causation or connection. Induction refuses to admit any datum, or to accept any statement, on hearsay or mere authority; and, in short, declines all evidence whatever except that of direct experiment or observation. It never rejects or seeks to set aside any evidence because this seems to be opposed to a doctrine, or theory, we happen to have in hand at the time; for it is quite as important to collect the negative as the affirmative evidence in the elucidation of the truth. It is only when we have gathered a due store of authentic facts, that we can proceed to the *second* step, that of collating and comparing our data for the purpose of discovering wherein, and to what extent, they agree or disagree in respect of certain features or characters which we are endeavouring to determine. To this end, they must be scrutinised and sifted, eliminating whatever is irrelevant and extraneous; and then be grouped in order, according to their points of agreement or disagreement, of resemblance and apparent connection. The result or results thus arrived at have then (and this is the *third* act in the process) to be expressed in a simple formula or proposition, declarative of the conclusion or outcome obtained from the evidence which has been subjected to the foregoing examination. Such a proposition, provided the evidence on which it rests and from which it has been derived is sufficiently ample and complete, will then stand as an axiom or established principle, serving to point and pave the way to the discovery of larger and more comprehensive general axioms or truths, which, almost invariably, prove to be "fructiferous" as well as "luciferous," by suggesting useful applications in practice. A single illustration derived from epidemiology itself will suffice to show this. That diarrhœal ailments are very generally prevalent among a community in which epidemic cholera has appeared, and that, in the vast majority of instances, individual attacks of cholera are preceded by signs of disordered bowels of longer or shorter continuance, are inductive truths that serve not only to illustrate the physiology of the disease, but also to suggest an important precaution in the way of its preventive treatment.

Very different from the inductive method is the deductive or pre-Baconian method, which was generally followed in physical investigations prior to the publication of the 'Novum Organon.' The latter first lays down as truths or axioms one or more propositions, and then proceeds to explain or account for the phenomena or facts which may be observed, showing that their occurrence is in accordance with the formulæ propounded. The propositions are always more or less hypothetical; sometimes purely so, and in other cases only partially so, being derived in part from observed facts, and in part from speculation or conjecture. In deduction, we reason *down* from generals to particulars, from accepted principles to the interpretation of individual facts or phenomena; in induction we reason *up* from the examination of numerous ascertained or established facts to the discovery of a general principle or law which serves to link the facts together. Induction has been defined to be the process of discovering laws from facts and causes from effects; and deduction that of deriving facts from laws and effects from their causes. Let it be ever borne in mind that to endeavour to establish, or to enunciate, a general proposition from the consideration of uncertified data is to sin against the fundamental rules of the inductive method. The data must first be proved to be facts or certainties before we proceed a step further. Not that hypotheses can be altogether foregone or rejected; as suggestions and incentives to further and more accurate observation, they are often most useful; but they can never form a safe basis or scaffolding for a superstructure. "Our varying hypotheses," said Faraday, "are simply the confessions of our ignorance in a hidden form; and so it ought to be, only the ignorance should be openly acknowledged." Again, however much the inductive method of investigation may differ in its procedure from the deductive method, there is one form or act of deduction or reasoning downwards, which it accepts and recognises as most valuable. What Bacon calls 'deductio ad praxim,' is the application of established physical truths to the uses and needs of human life. "When we have attained," says an able writer, "to propositions of any degree of generality in science, we can often by pure reasoning *deduce* from them consequences recommended by their curiosity or utility. And the more general are the truths which we have reached by our inductive ascent, the more copious and varied will be the inferences which we may obtain by reasoning downwards from them. This mutual dependence and contrast of induction and deduction, this successive reasoning up to principles and down to consequences, is one of the most important characteristics of true science."[1]

Just in proportion as the study of epidemiology is pursued according to the inductive or the deductive method, will its future course be either steady and progressive, or wavering and insecure. In other departments of natural science in their infancy the latter method, being the more prompt and easy, was long the one adopted in the schools: and so it has hitherto been with this branch of medical research. At the present time, epidemiology is little better than a maze of vague opinions and speculations, of *dicta* rather than of *data*, of surmises and hasty inferences rather than of thoughtful and logical conclusions. The character of the disputations respecting most of its problems generally savours much more of that of theological discussions than of philosophical reasonings, being efforts

[1] 'Quarterly Review,' No. 90, 1830. The article is a review of Sir John Herschel's 'Preliminary Discourse on the Study of Natural Philosophy,' then recently published, and which affords so admirable an exposition and illustration of inductive investigation.

rather to establish a favourite proposition than to demonstrate a truth. A total change must take place, if we hope ever to raise our branch of research to the same level with that already reached by other and kindred fields of scientific investigation.

II. A cursory retrospect of the last twenty-five or thirty years will show how wide and diversified is the field (rather only a part of the field) of inquiry which epidemiology presents to the scientific physician, while it serves also to attest the magnitude and variety of the interests, social and national, involved in a right understanding of the questions which appertain to it. Most of my illustrations will be taken from the history of those pestilential diseases whose career, since the beginning of the present century, I have endeavoured to sketch in previous numbers of this journal.[1] 1846 saw the publication of the 'Report of the French Academy on the Plague.' This most valuable document, the fruit of long and earnest labours, undertaken at the instance of their government, on the part of some of the most eminent professional and scientific men in France, scattered to the winds most of the doctrines long dominant respecting this dreaded disease, shed light where all had previously been the darkness of ignorant prejudice, calmed exaggerated alarms, and shook the foundations of the whole quarantine system throughout Europe—a system which had, for more than a century, acted as an oppressive burden on commerce and bar to international intercourse, without affording all the time any reliable protection to public health.

The disappearance of the plague for now nearly thirty years from the Turkish dominions, which had for ages been its principal seat and stronghold, is a very noteworthy event in epidemiological history; and not less interesting, in a scientific point of view, is the occurrence of, at least, two isolated and local outbreaks of the disease of distinctly spontaneous growth, in districts far apart from each other, and with an interval of several years between them, viz., on the north coast of Africa in 1858, and in the valley of the Euphrates in 1867.

As to yellow fever, never has this pestilence been known to be more widely and more fatally prevalent in the New World than during the last twenty years or so. It was in 1848-49 that Brazil, after a lengthened immunity from the scourge, again became the seat of its ravages. Prior to that period, the Brazilian station was one of the most healthy of our naval stations; since then, it has been far otherwise. But it has not been the western coast only of South America which has suffered so much of late years from this deadly fever; Peru and Chili appear, from the valuable observations of the late Dr. Archibald Smith, to have been the scene of frequent visitations of fever of

[1] Nos. for April, 1864; July, 1864; Oct. 1865; January, 1868; July, 1868; and July, 1869.

a kindred type, if not identically of the same nature. Throughout most of the Mexican gulf, there have been in different years a great amount of deadly sickness from this cause, in several of the West India islands, and also at various points on the mainland of North and South America. The great island of Cuba seems to have been never free for a single year from the fever; and St. Thomas, the entrepot of so much commercial activity of recent years, has become a fatal nest of disease and death. Jamaica, which had for many years prior to 1852 remained comparatively unscathed, has, again been the seat of frequent visitations; and the fever there has on more than one occasion, appeared at higher elevations above sea level than it was formerly known to do, a fact of no small interest as respects the natural history of the disease. The outbreak in New Orleans in 1853 exceeded perhaps in fatality that of any previous invasion; and our own colony of Bermuda has suffered more frequently since that year than it ever did before. Moreover, what adds not a little to the interest of this subject, there is the distressing fact that at no former period of our navy's history have our ships of war sustained more disastrous losses from yellow fever than during the last twenty or twenty-five years. The terrible outbreak in the "Eclair" on the West African coast in 1845, leading to the destruction of a third of her crew, and the prostration of all on board, was followed by the sad histories of the "Dauntless" in 1853, of the "Malacca" in 1856, and of the "Icarus" in 1860, not to specify other instances that might be cited in numbers. Naval medicine seems to have made no progress yet in preventing such disasters.

Nor has Europe quite escaped. Lisbon in 1856 experienced a visitation of no common virulence; and Oporto has more than once in late years been touched by the breath of the pestilence. The occurrences at St. Nazaire, at the mouth of the Loire, in 1861, and on our own shores at Cardiff, in 1865, are also not without importance.

But it is the recent history of epidemic cholera which presents the most striking illustrations of the great national importance of epidemiological inquiries. So far from decreasing in activity in its 'home,' it has recently manifested greater power and virulence in India than ever. Notwithstanding all that has been written and done about the disease for the last fifty years, we are still nearly, if not utterly, powerless as respects alike its prevention and cure. At the present moment, the Government of India is preparing a new scheme for the more thorough and profitable investigation of this mysterious foe, which has hitherto so signally baffled our researches. Thrice within the last twenty years, has it passed over Europe and extended to America, spreading dismay everywhere, and showing how futile are the attempts of man to stop the progress of the "pestilence which walketh in darkness." On two different occasions during that time, did the principal nations of Europe convoke an assembly of professional savans to enlighten them, if possible, respecting the nature and attributes of the disease which had menaced or assailed them, and to advise them as to the best measures for its prevention or arrest. On both occasions the results were discouragingly fruitless. Nor can it be now predicted in respect of the next European invasion that it will find greater unanimity of opinion among different nations as to what should be done to ward off the pestilence, or to mitigate its diffusibility and destructive power, than there was when it first loomed like a dark cloud on the eastern frontiers of Europe in 1830-31.

It is not only in reference to epidemic diseases abroad that the last quarter of a century has been noteworthy; the period has been one of no common interest in respect of our domestic epidemiology also. To mention only one or two events. There has been the outbreak of diphtheria, and its first recognition by us as a distinct specific disease, coupled with the curious fact of its speedy development, about nearly the same time (as far as we know), in different parts of the world, so remote from each other as Nova Scotia and Australasia. Whether there has been any connection or relation between this event and the marked increase, in this country at least, in the diffusion and virulence of scarlatina is an important question that awaits solution. The unusual prevalence of carbuncular disease generally, during a great portion of the time, is another fact deserving record. But it is more particularly with respect to the epizootics and epiphytics—the murrains and blights—that the last quarter of a century has been remarkable. It was about the beginning of that period that the great blight, known as the "potatoe disease," first infected that important article of human food in this country. Although from that time to the present it has never ceased to manifest itself among us to a greater or less extent, no real progress seems to have been made by botanists or agriculturists in the discovery of its leading attributes, in respect of its predisposing or exciting causes, of what favours its persistence or recurrence, or as to any reliable means for its prevention. There has been no geographical or chronological record, I believe, of its early history; so that we are quite unable to determine when and where this mysterious distemper was first observed, or to give a trustworthy account of its subsequent course and distribution over different regions of the world. The same thing may be said as to the defective state of our knowledge in regard of the epizootics, which have of recent years attracted so much the attention of most European countries, and are of so great economic interest to all peoples. Extremely little is known of the natural history of these murrains, of their climatic and seasonal relations, or of their geographical extension; nor has any systematic investigation of these phenomena been set on foot.

It may be reasonably presumed that whatever advance is made in elucidating the genesis and spread of murrains and blights, will pretty surely react advantageously on the science of epidemiology; and *vice versâ;* for it seems far from improbable that the same or, at least, similar cosmical agencies influence the production of disordered phenomena in the different families of organic existence.

If the number and variety of the topics demanding investigation may seem, from what has been said, to have increased of recent years, equally so have our means of intelligence, and our opportunities for acquiring information respecting them, if these means and opportunities are all turned to the most profitable account. The annual reports, now regularly issued, of the health of our army and our navy, of the different presidencies of our great Indian empire, of our colonial possessions, as well as of our domestic hygienics in the reports of the Medical Officer of the Privy Council, and of the registrars of England, Scotland, and Ireland, are valuable stores of authentic instruction respecting epidemiological occurrences which our predecessors did not enjoy, and the like of which no other country can produce. It behoves us to profit by such advantages.

III. The general characteristic attribute of epidemics is their tendency to alternating periods of development and disappearance, or of irregular recurrence and decline or extinction. For years, the disease may be entirely absent, or it may be seen only sporadically and partially; the cases being few, scattered and occasional, occurring singly, or in small detached groups of two, three, or so. These individual isolated attacks may be, at times, of a severe and malignant type; but the malady nevertheless shows no tendency to spread or multiply, even when the more obvious surrounding circumstances and conditions are notably favorable to its development and activity. In other years, it manifests from the first a marked disposition to increase and be diffused, and, ere long, it becomes rampant and widely disseminated. This signal difference in different years is to us a mystery, towards the physical elucidation of which no step has yet been made in advance. Many phenomena, it may be observed, in biology and in meteorology appear to be subject to cyclical changes of increase and diminution in point of activity, frequency of recurrence, &c., which it is equally beyond our power to explain. Dr. McDonald, F.R.S., remarks, in reference to the irregularly periodic occurrence of yellow fever in the tropics with intervals of immunity, that this circumstance "has its parallel in a fact well known to students of the diatomaceæ and desmidiaceæ, viz., that particular species, which are known to exist in a definite pond or pool one season, may be at another replaced by forms never before detected in the same spot; while again the original species, under favorable and often unaccountable circumstances, reappear after the lapse of a certain time."[1] Analogous occurrences are not unfrequent both in the animal and in the vegetable world.

It will be shown in the sequel that certain external agencies relating to the state of the weather and other meteorological as well as terrestrial conditions, to the household accommodation of human beings, the supply of the necessary articles of their food and drink, together with other matters connected with their physical constitution and general status, have much to do with the extension and persistence, if not with the primary development, of some epidemic diseases in certain years. But beyond the truths discoverable from the consideration of these external adjuvant circumstances, all is dark and mysterious about the genesis of epidemics, or the primary cause of their upspringing in one season and not in another. Plagues and pestilences are doubtless to be regarded as judgments of the Almighty Ruler, which play their appointed part in the scheme of Providence; but the recognition of this revealed truth does not of course exclude the rightfulness and duty of seeking to discover the links in the chain of material causes which lead to their development and extension. Each epidemic disease requires to be investigated for and by itself, and its natural history—in other words, its attributes in relation to other phenomena or events in the world of nature—needs to be ascertained by examination of the leading distinctive signs and properties which it exhibits in successive outbreaks; and, when practicable, not in one region or country only, but in diverse parts of the world. Fallacies and errors will almost inevitably be committed in seeking to establish any general propositions as regard either the physiography of the distemper, or its prophylactic and preventive treatment, from the experience of any single visitation, and particularly when the area of observation has been of limited extent. The inductive investigation of an epidemic is a much slower and more toilsome process. Conclusions, which seemed just and reasonable on the occasion of one visitation, will often be found to be scarcely tenable upon ulterior experience, and to require modification, if not total abandonment. The careful comparison of successive outbreaks, in respect of all their features and characteristics, is indispensable to our true knowledge of the natural history of the disease. If once we were in possession of such accurate knowledge of epidemic diseases derived from, and based on, such a full investigation of each of them, we should then have the necessary elements for the foundation of Comparative Epidemiology; a branch of scientific research hitherto quite unexplored; but which, if rightly pursued, might lead to many curious and important results, just as a similar method of inquiry has proved so prolific of good in other departments of natural knowledge. However much the attributes and general career of

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[1] 'Report on the Health of the Navy,' 1860, p. 72.

certain epidemic diseases may differ the one from the other, as, *e.g.*, influenza and the plague, still they will be found to present some points of not unuseful comparison; and the history of the one may possibly serve to cast some light on that of the other. The course of the 'black death' in the fourteenth century seems to have been not dissimilar to that of influenza in the last and the present centuries.

On comparing different epidemic diseases, we are soon led to recognise a marked distinction, in several respects, between the order of the proper Exanthemata (to which hooping-cough and diphtheria must be added), and that of most other forms of zymotic febrile disease. The persistence and, often, all but permanence of their presence (subject however to notable exacerbation and abatement from time to time) in most countries which they infest; the lengthened duration of their epidemic visitations, continuing for many months, and even for one, two, or three years; their special tendency to prevail chiefly in infancy and early childhood; their greater and more indisputable contagiousness; and their more marked independence of the ordinary local unsanitary influences in respect of their development and general dissemination, suffice to indicate peculiarities of character, which seem to stamp them as a special order or sub-class by themselves. It will be prudent, therefore, to exercise considerable reserve in interpreting the phenomena of other epidemic diseases by analogical comparison with the exanthemata, as is not unfrequently done. But here, as with every other topic of epidemiological inquiry, the grand desideratum is, first of all, to obtain thoroughly accurate accounts of the natural history of each disease, so as to enable us then to compare and contrast the records with each other, and thus discover the points wherein they mainly agree or differ. In the sequel, most of the data used in the way of illustration will be taken, as already intimated, from the history of the three epidemic diseases whose geographical and chronological distribution since the beginning of the present century has been sketched in former numbers of this journal. They are not all given as certified or indisputable facts, but only as the best that I know of; it is the want of trustworthy "pièces justificatives" that ever has been, and still is, the great obstacle to the progress of sound epidemiological knowledge. The use, moreover, of shaded maps to indicate the areas affected by epidemic outbreaks will be found to aid greatly in giving precision as well as interest to statistical details, and thus serve to engage and fix the reader's attention. They were employed, with much advantage, in the reports of Drs. Baly and Farr in illustration of the spread of the cholera epidemic of 1849 over England and Wales; and it is much to be regretted that similar charts have not accompanied the official narratives of subsequent visitations. No description however vivid, or statistical narrative however exact and detailed, will ever fix the leading facts as to the topographical course and extension of an epidemic like a pictorial illustration. In following the career of wide-spread physical phenomena, the student will invariably find it to be most useful to have a map of the country or region continually before him; many suggestions will thus occur to his mind which otherwise he would not have dreamed of.

Besides the diseases usually regarded as strictly epidemic from the circumstance of their generally being more or less widely prevalent among a community, it is to be remembered that various maladies, which ordinarily occur only sporadically, are liable in certain years and seasons to become so frequent in districts of varying extent, as to manifest there a decidedly epidemic character. Erysipelas, ophthalmia, metria, and several cutaneous affections may be cited as examples in point. The carbuncular epidemic of late years in this country is also a notable instance. Even such a disease as hydrophobia is known, in some countries, to have been so unusually common in certain years compared with others, as to have been described as prevailing almost epidemically. The systematic registration of such events, together with a brief notice of synchronous biological and other cosmical phenomena, if continued over a long series of years, might probably lead to the detection of many curious and instructive inter-connections. When we read of meteorologists giving the averages of half a century's daily observations, and of other physicists keeping regular records of solar spots for upwards of forty years, we forcibly feel how much in arrear has been hitherto our branch of physical inquiry in respect of its very A B C.

IV. Are epidemic outbreaks usually sudden and unheralded events? or are they generally preceded by discoverable nosological indications and premonitions of their advent? To answer this most interesting question, it is obvious that the exact health-state of the place or district for weeks or months previous to the earliest case of the incipient distemper must, among other data, of course be known. Such information is unhappily seldom to be had in respect of epidemics in former times, and it is far too rare even in the present day. No value should ever be attached to the common vague statements that the health of the community was quite good, or was as good as usual, prior to the first case of the epidemic: unless reliable statistics of the antecedent death-rate at least, if not of the prevalent sickness as well, can be referred to, so as to ascertain the facts relating not only to the actual season but also as compared with those of other years, they are all but utterly worthless. In the large majority of instances where this point has been rigorously inquired into, traces and signs of a pre-existing *dyscrasia*, or sickly diathesis, seem to have been detected. The French commissioners have given in their report many examples, which show that epidemic outbreaks of plague in the present century were very generally preceded, sometimes for many months, by the greater prevalence than usual of bad forms of fever, periodic or continued, accompanied occasionally with bubos or other pestoid symptoms, so that often it was scarcely possible to determine when the earliest attacks of the true plague really occurred. Sydenham and other physicians in the 17th century expressly refer to this very point; and in the recent outbreaks at Benghazi, and near to Bagdad, we knew that typhus had been prevailing among the affected communities for a considerable time before any mention was made of the existence of plague in either district. In the case of the plague at Malta in 1813, Hennen has told us that the season was remarkable for the extraordinary prevalence of carbuncular disease in Valetta, and the general tendency of wounds and ulcers to take on an unhealthy character, prior to the first cases of the pestilence.

With respect to epidemics of yellow fever, the existence of a precursory sickliness was very conspicuous in several recent outbreaks, of which we have fortunately detailed narratives. For at least one or two years prior to the appearance (after an absence of more than half a century) of this pestilence at Rio Janeiro and other seaports of Brazil in 1849, a notable change in the character of diseases had been observed; "and the ordinary endemic fever had been becoming less and less remittent, and more decidedly continued in type, so much so that many cases were regarded as of a new form of fever, to which the appellation of 'insolation fever' was given, and which resembled, it is said, in many respects, the milder cases of yellow fever. These occasional cases became from year to year of a more aggravated and fatal character, and in several instances the matters vomited had much of the appearance of the true black vomit." ('Brit. and For. Med.-Chir. Rev. for July, 1864,' p. 182). The outbreak of the severe epidemic at Bermuda, in 1853, the earliest cases of which occurred among convicts on board the hulks, was preceded by a season of great sickliness from flux, enteric fever, and 'dengue;' and Dr. Lyons tells us that, before the earliest cases of the Lisbon epidemic of 1856, together with the great prevalence of gastro-intestinal affections and cholera, fevers, both continued and periodic, had been extremely prevalent, and often manifested a marked hæmorrhagic tendency. Nowhere do we meet with more striking manifestations of a similar character than in the recent history of the disease in our ships of war. In almost every severe outbreak recorded in the reports of the navy, the health of the crew had been, for some time previously, suffering from a large amount of fever and diarrhœa. This ætiological point has been worked out at considerable length, and illustrated by numerous examples, in a recent number of this journal (April, 1869). Important practical lessons are obviously deducible from the position there laid down, if it be confirmed by subsequent experiencc. Certain it is that the extension and virulence of yellow fever on board ship seem to be invariably in proportion to the unsanitary condition of the vessel and the unhygienic condition of the crew. Part of a healthy ship's company, who have caught the disease by visiting another ship or a locality infected with it, may on returning to their own ship sicken and die; but then the fever will often not spread beyond those primarily affected, and ceases with them, whether the cases have or have not proved fatal. A striking instance of this sort occurred two or three years ago in the "Bristol" frigate soon after arrival at Sierra Leone, when a boat's crew contracted the fever by having gone on board a very sickly ship, and most of the men attacked died soon after their return to the "Bristol," without a single case occurring among any other persons of the crew. How different from the usual career of the disease when it imperceptibly, so to speak, becomes developed among a crew who have been suffering from flux and fever, especially if crowded together, and the vessel be at the same time unwholesome. Under such circumstances, it is frequently impossible to determine when the first unmistakeable instance occurs, as many of the previous cases may have, for some time before, begun to exhibit symptoms which awakened the suspicions of the medical officers.

In the history of cholera, particularly in that of its extra-Indian career, no feature in respect of its epidemic development, in different places and seasons, has been more conspicuous than that of its invasion having been very generally found (whenever full and accurate intelligence of the event has been procurable) to have been preceded by an increased amount of intestinal disorders among the affected population. As regards Great Britain, where the detailed records of the three last visitations have been far more complete than in any other country, this fact stands out in strong relief. For several years prior to the visitation in 1848-49, the mortality from alvine flux (diarrhœa and summer cholera) had risen in a very notable degree over the metropolitan area. Again, before that in 1853-54, there was a marked increase; and the same phenomenon was still more striking on the last occasion. From the ampler information we have of that visitation over the continent of Europe, no former one furnishes so many illustrations of the point in foreign localities as the epidemic of 1865-66. At Malta, the health-state of the island had been bad, and the mortality from fevers, &c., had been in excess for several months prior to the arrival of any infected vessels from Egypt; diarrhœa had also been prevalent, for some time previously, in that very part of Valetta where the earliest cases of cholera are known to have occurred. At Algiers, where the earliest cases of cholera occurred in the first week of September, alvine flux had been unusually prevalent and severe throughout the summer. The information respecting the public health at Marseilles prior to the earliest cases in June is conflicting; some medical men affirming, while others denied, the existence of antecedent sickliness among the population generally. That several deaths among adults from diarrhœa, gastro-enterite, and 'miserere,' occurred in the months of May and June, is admitted; but unfortunately, the mortuary registration had been so loosely and imperfectly kept, that no comparison could be made between the number of such casualties in 1865 and that in previous years. At Trieste, another Mediterranean port which was in constant intercourse with Alexandria, bowel disorders were unusually frequent and severe for between two and three months before the pestilence distinctly manifested itself there, about the end of September. The public health at Ancona, also, it may be fairly presumed from the observations of Dr. Ghinozzi at the time, seems to have been anything but satisfactory, previous to the arrival of any infected vessels in June; and we know that at Lisbon, and other places in Portugal, there was a veritable epidemic of "cholerine" during the summer and early autumn months before the earliest cases of cholera were observed. The same thing was notably the case at Lisbon in the former epidemic in 1856. The remarkable outbreak which took place at Altenburg in Saxony, the history of which has been so frequently and emphatically dwelt upon by writers on the epidemic of 1865, affords another striking illustration. The general death-rate had been greatly in excess for many months, particularly during the summer (apparently from the extreme prevalence of intestinal affections), before the earliest case of cholera occurred in a stranger recently arrived from Russia, and who, it has been confidently maintained by the Constantinople Conference, as well as by most writers on the history of the visitation, imported the pestilence from that country. In Russia itself, as we learn from the official report of the epidemic in that country, "in almost every place before the appearance of the cholera (in 1865), the prodromata of the disease, especially diarrhœa, were observed."

These data seem to afford reasonable indications that a greater sickliness than usual, manifested mostly by the prevalence of a "diarrhœal diathesis" so to speak, existed during the early summer of 1865 over southern and central Europe, prior to the appearance of the epidemic that year. A similar state of things was present in northern Europe at the same time, as I learn from some highly interesting communications which I have received from Dr. J. W. Moore of Dublin, and from which it appears that both in Copenhagen and in Christiania, and in Stockholm, bowel disorders were much more frequent and severe that season than is ordinary.[1] It was not

[1] The data communicated to me by Dr. Moore have since been published in the 'Medical Times,' for May 14th, 1870.

till the following year (1866) that the cholera appeared in any part of Scandinavia.

In respect of that puzzling incident in the history of the epidemic of 1865, the outbreak of the disease in the French West India island of Guadaloupe in the last quarter of the year, it appears from the report of M. Cuzent that, for several months previously, there had been numerous deaths at Point-a-Pitre by what is designated "une fievre algide choleriforme," from the resemblance of the attacks to those of genuine cholera. "L'ouragan electrique du 6 Sept. a eté la cause determinante de l'evolution spontanée du fleau, en donnant alors le caractere infectieux et epidemique du cholera Indien à l'affection endemique et localisée jusqu'à ce moment dans les faubourgs."[1]

It has been often asserted that outbreaks of cholera have occurred among perfectly healthy populations, and without any indications whatever of precursory sickliness. In the cholera epidemic of 1865, the two most conspicuous instances of this sort that I know of are those of Constantinople and of Gibraltar. In respect of the outbreak in the Turkish capital, it has been stated that, prior to the arrival of the infected frigate from Alexandria at the end of June, nothing in the condition of the public health had indicated the approach of any epidemic sickness. It may be so; but in the want of all mortuary registration, and of the means of comparing the mortality of one season with that in former years, it is obviously impossible to determine the point in question with any degree of accuracy. With regard to the case of Gibraltar, the source of difficulty is of another kind. A regiment of apparently healthy men, arriving by sea from a place where cholera existed, was landed and camped out on the neutral ground between Gibraltar and the Spanish frontier in the second and third weeks of July, when the general health of the garrison and civil population was, and had been throughout the previous season, unusually good. Three fatal cases of cholera occurred among the corps before the end of the month. The regiment, being on its way to Mauritius, was immediately embarked, and reached its destination without another case of the disease having appeared among the men. But within a week of their departure, it began to manifest itself in the garrison, and it became epidemically diffused during the next two months. The question comes to be whether the disease, or its heralds, existed in that part of Spain adjoining to Gibraltar at the time of the landing of the troops. Unfortunately, no reliable information on this point has ever been given by the Spanish authorities; and all that can be positively asserted is that the pestilence had appeared on the west

[1] 'Epidemie de la Guadaloupe' (1865-66). Par Gilbert Cuzent, Chevalier de la Legion d'Honneur. Paris, 1867.

coast of the peninsula in the beginning of July, and therefore prior to the earliest case on the neutral ground. Whether any towns on the south coast of Spain had also begun to suffer about the same time, it is impossible from want of intelligence to determine.

On the whole, it may be fairly concluded, I think, that epidemic outbreaks are very generally not sudden events, but are preceded by an unusual amount of sickliness, and that this sickliness often partakes of the nature, or at least exhibits some of the features, of the coming disease. The true history of an epidemic begins, it may be said, with that of its antecedents. Many illustrations of the truth of this remark might be drawn from the history of other epidemic diseases abroad besides those to which reference has been made in the previous observations. The case of the fatal fever which has been decimating our once flourishing colony of Mauritius is much to the point. For two years, at least, before it acquired the force of a disastrous epidemic, there had been a notable increase of general sickliness, and of fevers in particular; and prior to that time, the frequent return of cholera visitations, in an island which had long enjoyed a remarkable immunity, was a feature in its recent medical history of significant interest.

It would be highly instructive to be able to follow up the foregoing remarks on outbreaks of foreign epidemic diseases with similar notices respecting analogous outbreaks at home; but I have neither the necessary space nor materials to do this. There is much reason to believe that the same general law would be found to hold in both alike, and that the upspringing and development of most epidemics among ourselves are by no means such sudden events, unpreceded by any discoverable signs, as they have been often imagined to be. To quote the most recent instance, that of relapsing fever in the metropolis, it appears to be unquestionable that cases of the disease had been observed many months previous to the commencement of its epidemic prevalence.

V. What is the manner in which a *new* epidemic outbreak—*i.e.*, one quite unconnected with the recent existence of the disease in the locality—usually appears and becomes diffused? It has been so often confidently asserted that it has passed into a general belief, in respect more particularly of the distempers deemed pestilential, that the disease radiates from one or more spots where the earliest cases have occurred, as from a centre or centres, and that it gradually spreads thence from place to place and from district to district, according to their contiguity and to the amount of their intercourse with each other, until perhaps the whole area has come gradually to be more or less deeply infected. When, however, a systematic step-by-step inquiry has been instituted, this mode or explanation has generally been found to break down, the evidence as to a traceable connection between many of the early attacks in different localities being discovered to be quite uncertain and only conjectural. The first case or cases almost always occur among the poor, and in unhealthy localities. Within a short time, other cases, single or in small groups, appear successively, or it may be synchronously, in other similar spots more or less distant. Attacks multiply near to or around these several foci, while isolated cases occur elsewhere, perhaps in places remote from the early "centres of infection" (to use a common but doubtfully correct phrase), and milder and less distinct forms of the malady may be becoming more or less prevalent among the community generally. If represented pictorially, the affected area would exhibit a mottled or dappled appearance, the coloured spots varying in shade and size according to the virulence and extent of the disease in different localities. Many facts seem to indicate that morbific miasms are diffused in the atmosphere not uniformly or equably as gases or vapours are diffused, but rather in, at first, scattered (*eparpillés*) points or patches, which afterwards become larger and more numerous, and eventually coalesce into broad and wide nebulosities, somewhat as we see take place with clouds before a thunderstorm. Various cosmical phenomena appear to be developed after this fashion; and so it may be, perhaps, with those of epidemic disease. This is, however, as yet only a conjecture; and we want exact data respecting the chronology and topography of all the earliest manifestations of the nascent malady to test its soundness. Every one will perceive what important bearing all trustworthy facts relating to the *synchronism* of the early cases, in different localities, must have on our true knowledge of the natural history of an epidemic. Exact intelligence as to dates and localities is the foundation of all sound epidemiological research; without it, no real progress can be made.

The rise and early spread of epidemic cholera in London in 1848-9, and again in the visitations of 1853-4, and of 1865-6, seem to have taken place in the way indicated above. Nothing like radiation or step-by-step advance, as from a centre or centres to more distant parts, could be traced, although every genuine case of the disease as it occurred was at once known and recorded. Attacks took place nearly contemporaneously in different districts, remote from each other, and evidently independently of inter-communication. The same may be said as to the mode of the upspringing of the disease in other large towns of England, where its origin and cause were followed. Notwithstanding the most vigilant search, it was generally found to be impossible to discover any connection between very many of the early attacks. This was, for example, conspicuously the case in the late epidemic at Bristol, where, from the completeness of the supervision exercised, every circumstance relating to the outbreak was known with the greatest exactitude.

In two thirds of the twenty-five separate localities where the disease made its appearance in and around the city (between July 21 and Nov. 12) its source could "neither be made out nor guessed." In six of the remaining instances, the disease was directly imported by strangers arriving with it upon them. In two instances only, could the attack be distinctly traced to communication with the sick. All the while, diarrhœa was very prevalent throughout the city. And as with towns or small districts in respect of the mode of the spreading of the disease, so it was with the whole area of the United Kingdom; in which, thanks to our excellent statistical machinery, the death-progress of the epidemic can be easily followed out. The diffusion appeared to be not by radiation, but rather by the independent up-springing or evolution of the morbific element in numerous different localities. What was observed to take place in this country seems to have occurred, in like manner, in cities on the Continent; as well as can be judged of from the imperfect statistical accounts that we have of the inception and early diffusion of the pestilence in their midst.

We have no such detailed and circumstantial narrations of the rise and spread of epidemic yellow fever in towns as we have of malignant cholera; but from what information we have, it would certainly seem that the outbreak of this pestilence also is usually after the same fashion. In the epidemic at Bermuda in 1853, "the fever broke out," according to the official report, "in different and distant parts of the colony nearly simultaneously," p. 12. In that of 1856, the Commission of Inquiry came to the conclusion that "it made its first appearance not at one, but at several points, distinct and widely separated, and simultaneously at two such points," p. 9; and in the last visitation in 1864, we find that the Commissioners on that occasion failed to discover any connection between the early cases, which appear to have occurred in different localities. Moreover, the first attacks among the military could not be traced to communication with the previous cases in the town; nor did the one seem to be connected with the other. Again, Dr. Lyons, in his history of the Lisbon epidemic of 1856, states his "conviction, based on the results of inquiries made amongst medical men of all shades of opinion, that, prior to the declared and fully recognised existence of the epidemic, isolated cases presented themselves in various parts of the city." To the same effect are the remarks in most of the narratives of severe outbreaks of the fever elsewhere, as at New Orleans, and other cities in the Southern States, and in Demerara, &c.

If our information respecting the authentic early history of yellow fever epidemics be so imperfect, far more so is our knowledge respecting that of the plague. But from incidental remarks we gather that, at a very early period of most outbreaks, the disease did begin to manifest itself in several distinct localities, between which it was impossible to trace direct intercommunication. Thus we learn from Sir B. Faulkner's narrative of the plague at Malta in 1813, that soon "the contagion began to diverge in so many directions, that it would have been extremely difficult, if not impracticable, to follow up the direct line of contaminations;" and we also know, that in the outbreak at Corfu in 1815, the attempt to trace connection between the earliest cases quite failed, and, moreover, that the disease appeared about the same time in several villages not far from the district where its existence was first recognised.

And is it not the case that the history of most of our domestic fevers and other zymotic diseases reveals a similar mode of rise and development, viz., by the occurrence at first of sporadic cases, single or in small groups, in various localities of a town, village, or district, and by the subsequent multiplication and irregular extension of these "foci," until the whole or greater part of the area, it may be, becomes more or less sensibly affected? Is not this the usual course to be observed in respect of epidemics of typhus, of enteric and of relapsing fevers, of puerperal fever and of diphtheria, not to mention other kindred maladies, which from time to time appear in the land? The question, however, needs scrutiny.

VI. A fact, which the epidemiological student has often occasion to observe, is the marked uncertainty that very generally will be found to exist as to the exact date of the earliest case or cases of many epidemic outbreaks. And yet, without accurate knowledge on this point, it is obviously vain to speculate with any advantage as to the probable origin and cause of the disease. Various circumstances serve to bring about this uncertainty. Zymotic distempers, when they do not prove fatal, often fail to manifest some of their pathognomonic phenomena. Thus, the milder attacks of plague may not exhibit any carbuncles and petechiæ; the cases of yellow fever which recover are almost always without black vomit; and those of cholera are seldom accompanied with total suppression of urine and complete pulselessness. In short, the attack of plague is then often not distinguishable from one of typhus; that of yellow fever from one of tropical remittent or continued fever; and that of cholera from cholerine or of summer cholera. Yet these milder forms are as capable, it is generally maintained, of giving rise to attacks of a formidable type in other persons as those cases where the symptoms are more developed and more grave. In narratives of epidemics, the commencement of the outbreak is generally dated from the earliest deaths from the disease, whereas the morbific poison may have been at work in the locality for some time previously, indicated by the greater prevalence than usual of the milder forms of its manifestations. Much of the ambiguity respecting the origin of an epidemic outbreak, and many hot disputes on the question whether it was due to foreign importation or to indigenous development, may be traced to this very circumstance. It is, moreover, to be remembered that there is always an unwillingness on the part not only of the general public, but often of many medical men also in a community, to admit the existence of a pestilential disease in their midst, if there be a doubt upon the matter, and there be a mode of explaining the phenomena which have given rise to the suspicion in any way short of admitting the unwelcome truth of its indigenous development. If the imposal of any penalties or of restrictions, affecting the freedom of personal or commercial intercourse, be dependent on the decision, affirmatively or negatively, of the point in question, the difficulty of arriving at the truth will be increased tenfold. No reliance whatever can then be placed on most of the statements either one way or the other; they will be infallibly warped by the exigencies of a misleading partisanship. And this result will be the more marked just in proportion to the stringency of the prohibitions resorted to, in the way of prevention. Hence the invariable impossibility of obtaining accurate information about the rise and early progress of such diseases as pestoid and yellow fevers, or of spasmodic cholera in any city, especially maritime, of Spain or Portugal; and hence too the common practice in these countries, and in their dependencies, of their seeking to explain away, or of directly denying, the occurrence of cases of sickness which are of a suspicious character, until the fact becomes so notorious as to compel their tardy avowal of the truth.

It not unfrequently happens that among the first to be attacked by an incipient epidemic are persons recently arrived, it may be from a distance, in the locality. The suspicion of its having been thus introduced is a natural one; and if the stranger or strangers have come from a place where the disease exists, the suspicion usually becomes a conviction, on the part of the general public, that the disease has been imported by them. It is, however, well known to medical men that none are so susceptible to the poison of zymotic diseases generally as strangers and occasional visitors to a place where they either actually exist or are impending. The extreme liability of new comers to be attacked by yellow fever in the West Indies, and by cholera in lower Bengal, is notorious; and the fact is often little less conspicuous in respect of our own indigenous fevers, when persons from the country or from shipboard resort to localities in our large towns which are extremely unhealthy, but which may be, or seem to be, at the moment to be free from overt infection. The history of the last three visitations of yellow fever in Bermuda may be cited to illustrate the occasional ambiguity that there is in determining the primary source of an epidemic outbreak. In 1853, and again in 1856, the disease was considered by the Commission of Inquiry to have originated indigenously, the first cases having occurred in residents, and without any reasonable ground for believing that they had had previous communication with either infected persons or places. In 1864, the earliest *fatal* case (two or three suspicious milder cases had previously taken place) occurred in a man who had left Nassau, where the disease existed, a fortnight before, and had been in Bermuda for ten days. Within the next week or so, two other deaths occurred in persons who had not had any communication with the former patient; and the fever thereafter speedily appeared in different localities, separate from each other. It would obviously be most unwise to argue from such an instance as this that the disease must have been imported into the island from the West Indies; and the more so as in a subsequent year several well-marked cases of the fever occurred about the same season, as on the former occasion, and when there was no suspicion whatever of introduction *ab extra*. Among other incidental topics suggested by the above instance is that of the incubative period of the disease, a question to be hereafter considered.

No medical men have such favorable opportunities of ascertaining with exactitude the precise dates of the earliest attacks of epidemic sickness as the medical officers of the army and navy. It is to them we may reasonably look for most valuable aid in determining many points about which there is still no small uncertainty, and often much discrepancy of opinion. I would therefore urgently appeal to our *confrères* in the public service to scrutinize with the utmost vigilance, and faithfully to record, all details relating to the history of the first few cases of epidemic outbreaks which come under their observation, without regard to hypothesis or theory. They have much in their power; their labours, if conducted according to the strict rules of induction, might soon render stable and defined what is now fluctuating and ambiguous, and shed light on much that is hazy or obscure.

My intention was to proceed now to expound some of the most remarkable features and relations of epidemics in respect of their geographical distribution and chronological recurrence; but this subject must be reserved for another occasion; and I shall close this present paper with briefly discussing a problem of State medicine which is at once very interesting and important.

VII. Have epidemic diseases been kept out from countries by any external means of protection or defence? This question, viewed historically, and tested simply by the results of past experience, has never, as far as I am aware, been examined on a broad basis by medical writers; yet it is one of primary moment both scientifically and practically. It is impossible to discuss it as fully as it deserves to be, from the want of sufficiently ample and accurate evidence; but the mere attempt to open it up for future inquiry may perhaps be useful. No oné has ever dreamed of trying to stop the progress of influenza from one district or region to another; for it seems to be carried on the wings of the wind, and to defy all artificial arrest or control. And yet, if we may trust many narratives, the disease seems to have been distinctly introduced into small islands, far distant from other land, and rarely visited, as St. Kilda, Tristan d'Acunha, &c., by foreign vessels, after whose arrival it speedily followed.

It is more than doubtful whether the immunity of certain countries from some of the exanthemata can fairly be ascribed to the quarantine precautions resorted to for their exclusion. The extent to which this family of epidemic diseases is diffused over different sections of the world's surface seems to differ a good deal. I do not remember having ever heard or read of any of our colonies, or other distant lands to which our countrymen have gone, being exempt from measles, for example, or from its common accompaniment, hooping-cough. Scarlatina, although very generally present also at times, seems to be not so universally diffused. In India it is little known; and probably it is comparatively rare in some other regions in the East; but information is wanting. Smallpox appears to have penetrated almost everywhere, whether attempts are made to exclude it by quarantine or not. Wherever samllpox occurs, there varicella is, it is believed, also occasionally seen. The only notable exemption on a considerable scale appears to be the case of our Australian colonies, and of the adjacent islands of Tasmania and New Zealand. Their immunity however from an epidemic visitation of smallpox can scarcely be attributed entirely to the successful operation of their extrinsic defences; for it is well-known that, on several occasions, a few scattered cases have occurred both at Sydney and at Melbourne, but fortunately without manifesting any decided tendency to spread, and the disease happily ceased to exist in a short time, although a large proportion of the population is unprotected by vaccination.

It would be difficult to prove if any, and what, countries were preserved from invasion of the plague, when the disease was endemic in Egypt and the Levant, by the prophylactic measures employed for their protection. On the one hand, the disease exhibited at times no tendency to spread beyond a limited area around the seat of its original uprise; and, on the other hand, the quarantine measures employed were often so notoriously lax and irregular that no reasonable reliance could be placed on their efficiency. The mere circumstance of the exemption of a place or of a country from visitations of the disease was no proof that this was due to the precautions used for its exclusion. For half a century and more before the abolition of quarantine against the plague in our own country, Holland had continued to be quite as free from any traces of the distemper as Great Britain, although there had been unimpeded inter-communication on the part of the Dutch with the Levant all the time: and so it was in many other places. It had been long notorious that it was often next to impossible to prevent the clandestine arrival of smuggling and other coasting vessels from the Barbary coast at Malta for many years prior to the outbreak of the disease there in 1813—an outbreak which has never been explained, although at the time it was attributed, on merely conjectural evidence, to direct importation by a smuggler from Egypt. With regard to the subsidence and subsequent disappearance of the pestilence from Syria and Lower Egypt during the last five-and-twenty years, this phenomenon cannot, it is obvious, have been owing to *external* measures of defence, viz., against its introduction *ab extra*, as these were the very regions of its home growth; whatever credit may be due to the *internal* sanitary precautions which were being adopted throughout these countries, about the same time, under the surveillance of the able French physicians who were located in different parts of the Levant.

The whole history of yellow fever during the present century affords continuous evidences that no system of artificial restrictions and extrinsic precautions hitherto employed will suffice to exclude that disease. No example, as far as I know, can be cited of any country which is known to have been once visited by the pestilence being entirely protected from its recurrence. It may be needless to refer to the experience of any West India island or of any place on the mainland of the Caribbean Gulf, where the fever is believed to be indigenous, but where nevertheless measures of stringent exclusion are still from time to time adopted. The same want of success has attended their use in places at a distance from this great central region of infection. Within the last thirty years, our colony of Bermuda has been four times the seat of epidemic invasion, notwithstanding the vigilance of an officer of health appointed expressly to prevent the apprehended evil. Nor have the colonies of other countries, which adopt more rigorous measures of exclusion, fared better. The groups of the Cape de Verde and Canary islands afford a proof of this. When the outbreak of yellow fever occurred in 1845 at Boa Vista, one of the former, after the visit of H.M.S. *Eclair*, it was maintained at the time, by one party of the disputants in respect of that disastrous event, that the Cape de Verdes were not liable to invasions of the disease, and that the fevers which are endemic there are only of the tropical remittent type. Very little was then known of the nosological history of the island. In the following year, and at the same season, Teneriffe, one of the Canary group, ten degrees further northward, was attacked, the epidemic lasting for three or four months; and again, in 1847, Palmas, another of the same group, was the seat of a similar invasion (*Parliamentary Papers relating to Quarantine*, 1860, p. 16). In

1861 the Cape de Verdes were suspected of being infected; and in 1862 both these islands, and also Teneriffe and Palmas, are known to have suffered from the genuine "vomito" for several months at the end of the year and the beginning of the following one, as stated in this Journal for July, 1869, p. 219. If such facts as these can be gathered from the meagre scraps of intelligence which find their way into the public prints (for no systematic information is ever given by the authorities of Spain and Portugal on such matters), it would seem that yellow fever is far from being so infrequent in these island groups as has been imagined. Even the mother countries of these colonies, Spain and Portugal, have not escaped, notwithstanding their extraordinary vigilance and the extreme rigour of their precautions. After repeated outbreaks in several of their maritime towns during the first ten or fifteen years of the present century, a more stringent quarantine system was adopted, and with successful results, it was for a time believed. But in 1819, the pestilence reappeared in Cadiz, Seville, and other places in Andalusia, and again in 1820 and 1821. In the latter year, Malaga also suffered, and a fatal outbreak happened at Barcelona, Tortosa, and in Catalonia, at the opposite end of the peninsula. In 1823, a partial outbreak occurred at Passages on the coast of Biscay, connected with the arrival of a vessel from Havanna, after a ten days' quarantine; a case very similar to that at St. Nazaire, at the mouth of the Loire, in 1861. In 1828, the disease reappeared at Gibraltar after an absence of fourteen years. In the autumn of 1851, there was a partial outbreak at Oporto, and again in 1856, when Lisbon also became affected. Next year, the pestilence was fatally epidemic in the Portuguese capital, besides appearing partially in other towns of the kingdom. About the same time, Ferrol, Corunna, and other places on the coast of Gallicia in the north-west of Spain, were also affected. These repeated occurrences, which are known to have taken place in spite of the endeavours by the authorities to stifle all information about the appearance of any of the diseases which they seek to exclude, suffice to show how inefficacious have been the prophylactic measures hitherto chiefly relied on in the Iberian peninsula to guard their country from the occasional appearance of yellow fever.

Still more conspicuous has been the failure of all external defences against the incursions of the cholerific poison. It would be tedious and quite unnecessary to revert to the history of the earlier European visitations in connection with this subject further than to say that, at the first International Quarantine Conference held in Paris in 1851, the strongest evidence that was adduced by any of the delegates in proof of the credited efficacy of restrictive measures as a means of defence was the example of the small island of Elba, which escaped in 1836-37, and also in 1848-49, while the plague was widely spread over the Italian peninsula. Our own Isle of Man was equally fortunate on both occasions, although Liverpool and other places in Lancashire and Cheshire with which it continued freely to communicate were severely smitten. In the last epidemic, the immunity of Greece, when the disease prevailed more or less extensively in the countries around, was regarded by many persons, and among others by the Constantinople Conference, as a strong proof of the efficacy of quarantine as a means of protection in epidemic seasons. Greece appears to have enjoyed a remarkable exemption on former occasions also as well as in 1865; and indeed, with the exception of 1854, when the French and English troops quartered in the Piræus and (two or three months subsequently) Athens, were attacked, we have no knowledge of the disease having visited the country previously, even during the first European visitation in 1831-37, before any regular quarantine system was established in the kingdom. Almost all the islands of the Ægean Archipelago seem to have been equally fortunate; nor do even the large islands of Crete and Cyprus appear to have ever encountered an epidemic invasion of cholera. The hilly or mountainous character of their geography, the small size of the towns, the dispersed and scanty population, and their comparatively inconsiderable intercourse or traffic with other lands, may all have had something to do with the immunity they seem to have enjoyed, although certainly these circumstances alone do not suffice in themselves to account satisfactorily for the fact. But Greece and Greek islands are not the only places which have more or less completely escaped. Other Mediterranean islands have been seldom and sparingly visited by cholera, from the influence of causes which are very imperfectly understood. Corsica has suffered very little, and Sardinia was not, I believe, invaded till 1854. It is much to be regretted that (to the best of my knowledge) no authentic or complete narrative has been published in Italy of the true history of the disease as it appeared in Sicily in 1865-66. It has been confidently asserted that the island continued to be quite free from any traces of the pestilence throughout the former and during the first half of the second of these years, until indeed the landing at Palermo in the autumn (1866) of a body of troops from Naples where the disease was then existing; and that, from that date, the outbreak and spreading of the disease commenced. If such was exactly the case, the inference is certainly very reasonable that the two events, viz., the invasion of the malady and the arrival of the military from the mainland were connected, the one with the other, as cause and effect. Unhappily for the sake of truth, no accurate information has been published, and we are left to unauthentic rumour rather than to sure evidence to form our opinions. Upwards of two years ago I wrote in the pages of this Journal (for July, 1868, p. 15), "It is not possible from the want of reliable evidence to determine whether the disease had not been in the island previous to the landing of troops from Naples at Palermo, at the end of September. More than one of its seaports had been quarantined by other Mediterranean ports in the course of the summer (1866); and the extreme rapidity with which the disease appeared, according to report, at Catania and other places far distant, at Palermo, after the landing of the troops which were accused of having imported the pestilence, is not to be overlooked. Very speedily, nearly the whole of the island seems to have become infected." No contradiction or explanation of these statements has been given. On more than one occasion, the towns of Messina and Reggio, on the opposite sides of the narrow straits separating Sicily from the mainland, were mentioned in the public journals of this country as places where cases of the disease had occurred; but we are still, as far as I know, without the means of affirming or denying the rumours. The total want, too, of any evidence respecting the state of the public health in Palermo, or in any other places in the island prior to the occurrence of the first cases, presents a grievous obstacle to any trustworthy conclusions. Unless such *lacunæ* be in future filled up, it will be in vain to look for the establishment of truth. However ignorant we are about the cholera history of Sicily in 1865-66, we have complete knowledge of that of Malta in those years, and also of what occurred in 1867 when, in spite of the unusual vigilancy of the authorities and the extraordinary rigor of the precautionary measures which had been adopted since the cessation of the disease in 1865, the pestilence again found its way into Valetta.

The scope of the foregoing illustrations has, it will be observed, been simply historical. How far the appearance and development of epidemic diseases in many countries *can be* traced to importation *ab extra* by the introduction of infected persons or things from infected countries, or can be prevented by quarantine, is another problem which must be treated by itself, and which I shall afterwards have occasion to consider. At present, I would merely invite the reader's attention to the question with which we started, "Have epidemic diseases been kept out from countries by any external means of protection or defence?"

III. — Observations and Experiments on the Use of Opium, Bromide of Potassium, and Cannabis Indica in Insanity, especially in regard to the effects of the two latter given together. By T. S. Clouston, M.D., Medical Superintendent of the Cumberland and Westmoreland Asylum, Carlisle.[1]

So many cases of insanity consist of simple brain excitement, and in so many more is excitement the most distressing symptom, that if we could discover any agent which would subdue this excitement, and at the same time not interfere with the improved nutrition of the brain which rest, tonics, and good diet will effect, and on which complete recovery of its normal functions depends, such an agent would be a most incalculable blessing. There are many cases in which a physician knows that if he could tide over his patient for a few weeks of excitement, that recovery would come as the natural termination of the attack. Much distrust of strong narcotics prevails among the profession since Dr. Anstie's work on 'Stimulants and Narcotics' appeared. And yet how is such a case of maniacal excitement to be managed without them out of a lunatic asylum? The exact condition of the brain cells in mania being as yet quite unknown, we cannot apply a direct antidote. At best we can only work very empirically. But our empiricism may be founded on a rational and scientific examination of the effects of the drugs we use, and the natural history of the disease we treat, or it may be a mere haphazard employment of some agent recommended by some one who had no rational ground for his recommendation at all. Maniacal excitement is so essentially in many cases what has been hitherto called a functional disease, that it offers more hopes of benefit from drugs than most other complaints. A patient is rational and coherent in mind one hour and is furiously maniacal the next, and the excitement passes off as quickly. Surely such a condition may be reached and remedied by some therapeutic agent. We have many drugs more or less "narcotic" and "sedative," but hitherto the effects of those drugs have been far more carefully studied when given to persons previously free from excitement, than when given to those acutely maniacal. I do not mean to undervalue the observations which have been made on this subject, but all physicians know and strongly feel the want of accuracy and definiteness which prevails in this department of medicine. The following observations were undertaken almost entirely with the view of obtaining a little more accuracy in my own knowledge of the effects of certain medi-

[1] *The Essay for which the Fothergillian Gold Medal for* 1870 *was awarded by the Medical Society of London.*

cines on maniacally excited patients. They consist of two parts: the first, experiments made chiefly on incurable patients in whom simple brain excitement existed; the second, clinical observations on the effects of the same medicines on recent and curable cases of excitement. The experiments were undertaken to show, first, the effects of single doses, and second, the effects of long continued courses of the medicines.

The action of the bromide of potassium in cases of maniacal excitement especially deserves careful study. It acts differently in many respects from most of the vegetable narcotics. Given along with at least one of them, it seems to intensify and prolong its effect on the nervous system, without at the same time affecting injuriously the digestion and the nutrition of the body. The investigation of this point will be one special object of this paper.

The action of opium on disordered function of the cerebrum is better known than that of any other drug, though very much remains to be accurately ascertained. So much confusion exists as to its physiological action on a healthy subject, and its therapeutic action on a diseased one, so little is really known as to the tolerance of the remedy in certain disorders of the brain, there has been such a tendency to apply Dr. Anstie's theory as to the identity of narcosis and brain paralysis, where it is quite inapplicable, that any carefully recorded facts bearing on any part of this subject must be of value. Then the action on the healthy brain of a pure stimulant, such as whisky, has been carefully studied, but observations on the tolerance of large doses of such a stimulant in disordered brain functions are much needed. Any effect which food has in such cases we are accustomed to regard, and I believe truly, as beyond question directly towards health. To compare the effects of these various drugs with the effects of a highly concentrated food, therefore, on a given number of cases of brain disorder, can scarcely fail to be instructive.

Effects of single doses.—To ascertain the effects of single doses I at first selected eight patients (four men and four women), all labouring under great excitement, and from two hours and a half to three hours after breakfast, after taking their pulse and temperature and noting their mental state, I gave to each of them the dose of the drug or stimulant I was experimenting with. They were then sent out in the open air, from which they had been taken in, except the day was very cold, and in that case they were kept in the house, and in an hour I again took their pulse and temperature, and noted their mental state. Their condition during the afternoon was also observed. The next day I gave another drug, and this was continued till all had been gone over, when I began again, repeating the experiment four times, with most of the substances used, and twice with the others. I gave these patients in this way drachm doses of tincture of opium, drachm and two drachm doses of bromide of potassium, drachm doses of tincture of cannabis Indica ('British Pharmacopœia'), and a mixture containing one drachm of bromide of potassium, and one drachm of tincture of cannabis. I performed the same experiments, only instead of the medicine giving each patient four ounces of good Scotch whisky one day, and a pint of beef tea made from a pound of good beef another. I made experiments on myself and my assistant, using smaller doses, and not repeating them so often.

The reason I did not keep the patients in the house in a room of a uniform temperature was, that I wished to see the effect of the various substances on them in their ordinary circumstances at that hour. Two of them I did keep in a bedroom of uniform temperature during most of the experiments, but I found that this did not materially alter the results. I was not able to continue the experiments on all the patients continuously, on account of some of them being free from excitement on certain days, and other causes. On such days I usually substituted other patients who were also excited. They laboured under the most various forms of mania, but the element common to all of them was great excitement and disorder of the functions of the brain. None of them laboured under any bodily disease.

My objects were to ascertain accurately the effect of single doses of each medicine on, 1, the maniacal excitement; 2, the appetite; 3, the temperature; 4, the pulse, and to compare them with each other, and with the effect of a pure stimulant in large doses, and the most concentrated and nourishing of food. It is not surprising that I found the results with each drug were not the same in each patient in the successive experiments. A maniacal patient is so changeable and uncertain in his state with or without medicine, he varies so much as to the amount of muscular exercise he takes, and his whole system is so affected by these variations, that one cannot wonder at anomalies in the experiments on particular days. It was to obviate these uncertainties as much as possible that I took so many patients labouring under various forms of excitement, and repeated the experiments so often. In that way, I think, the results may be re-

TABLE I.

Substances given.	No. of Patients.	No. of Experiments.	Excitement aggravated at first.	Excitement subdued.	Excitement not subdued.
Tinct. Opii	9	29	2	19	10
Potas. Bromid. and Tinct. Can. Ind.	8	29	5	26	3
Potas. Bromid.	7	13	0	7	6
Tinct. Can. Ind.	7	15	0	12	3
Whisky	10	21	13	14	7
Beef tea	9	15	0	1	14

garded as trustworthy as to the *general* indications they give. I shall endeavour now to summarise the daily observations which I made.[1]

Excitement.—The effect of any medicine on maniacal excitement cannot be at all so exactly measured or defined as its effect on the temperature or pulse. The general and decided effects of the drugs I gave I have shown in Table I. From this it is seen that the combination of bromide of potassium and tincture of cannabis subdued the excitement in the greater number of cases, and certainly its effects were more patent and lasting than any of the others. Of the twenty-nine times in which it was given it decidedly subdued the excitement on twenty-six occasions, or in 90 per cent. of them. Opium was the next drug in potency of effect, though it only subdued the excitement in nineteen of the twenty-nine experiments, being 66 per cent. The bromide of potassium alone allayed excitement in about one half the experiments in which it was used, but its effects were very much less decided in the extent to which it allayed the excitement. Its effects usually lasted, however, for the remainder of the day on which it was given. In one half of the experiments two drachms were given, and this dose it was which had the effect on the excitement in five of the seven experiments in which any effect was observed. The Indian hemp produced abatement of the mania in twelve out of the fifteen experiments, but in almost all these cases its effects were comparatively slight, and seldom lasted for more than three hours. The whisky was followed by marked cessation of the excitement in fourteen out of twenty-one experiments, and its effects, contrary to what might have been expected, lasted usually for seven or eight hours. The beef tea had no appreciable immediate effect on the maniacal excitement in most cases. In only one case did a patient become more free from excitement after getting it, and this was the weakest of the number.

In regard to the length of time each drug took to act, and the mode of action of each, I found that the sedative effect of the opium was got most speedily. Aggravation of the excitement previous to the sedative effect was observed five times in the case of the mixture of the bromide and cannabis Indica, twice in the case of opium, and thirteen times in the case of the whisky, and this aggravation was so great and troublesome in the case of the last as to put it out of the question as a sedative for maniacal excitement. The sedative effect usually began to appear in from half an hour to two hours after the mixture of bromide and cannabis Indica, though in some of the experiments this was delayed for three hours. The preliminary stage of aggravation, when it occurred, lasted for about two hours in the case of the drugs, and for about one hour and a half in the case of the whisky.

[1] The daily records themselves were appended to the original essay, but they are too long for insertion here.

It was only the milder cases which were affected by the bromide or the cannabis Indica separately; the opium and whisky affected some of the worst cases at times, while the combination of the former affected the most excited on the largest number of occasions.

A very striking fact is seen at a glance at the records of the observations themselves, and it is the extreme *uncertainty* of action of almost all the medicines on successive days on the same cases. One day the drachm of Tincture Opii subdued the excitement and caused no loss of appetite. Another day in the case of the same patient the same dose was followed by no such effect at all. It is this which renders any such therapeutical inquiry so apparently unsatisfactory, but which gives additional value to any drug whose effects are most free from this element of uncertainty. It shows how many things have to be taken into account, and how very many accurate observations will have to be made before anything like reliable generalisation can be attempted. We can only at present follow the *prevailing tendencies* of action of a drug.

In none of the experiments, even when the patient was most fully under the influence of any drugs, was there anything like a narcotic action. The nearest approach to this was the drowsiness and sleep that sometimes occurred. But it seemed quite natural sleep. It would surely be a misnomer to apply the word "paralysis" to any result of those drugs in these cases. If an acute maniac is talking incoherent nonsense and moving about incessantly, his reasoning powers and intelligence being in abeyance, and if after a dose of bromide of potassium he ceases to talk and move about so much—all the other functions of the nervous system being undisturbed—and he makes some nearer approach to reason or intelligence, we must find some other name than either "narcosis" or "paralysis" for such a result. Dr. Anstie implies that the effect of a large dose of opium on maniacal excitement must be "narcotic." In the cases above related, the action was truly "stimulant" in his sense, though the doses were "narcotic doses."

As regards the food action on the excitement, it was in all these cases so inappreciable at the time that no comparison can be made

TABLE II.

Substances given.	No. of Patients.	No. of Experiments.	No. of times appetite taken away.
Tinct. Opii	9	29	7
Potas. Bromid. and Tinct. Can. Ind.	8	29	1
Potas. Bromid.	7	13	0
Tinct. Can. Ind.	7	15	1
Whisky	10	21	1
Beef tea	9	15	0

between it and the drug action as regards immediate effect on the excitement.

Appetite.—No effect of a drug is more important on a maniacal patient than its effect on his appetite for food. If that is much interfered with, the good effects of the medicine will have to be great and manifest indeed, to counterbalance so indisputable an evil.

I have in Table II shown the number of times in which the patient's desire for food was clearly interfered with after each medicine given. Opium stands in bad pre-eminence at the head of the list as that which most frequently produced this result. In seven out of the twenty-nine experiments with opium, the patients could not be got to take the next meal. This was never the case after bromide of potassium at all, and only once after cannabis Indica and its combination with the bromide. This was one of acute excitement, with complete incoherence and absence of reason, being that most like the acute delirium of fever. In his case it caused on two occasions dryness of the tongue and lips as well as loss of appetite.

Temperature.—The temperature of the body in maniacal excitement has been far too little attended to. It often rises in a direct ratio to the brain excitement present, it is most important as a diagnostic of organic disease, and it affords most valuable indications for treatment in many cases. In Table III I have recorded the results of my observations in regard to temperature. The tendency of opium was to raise the temperature slightly; that of the bromide and cannabis Indica combined to depress it; of the bromide alone to raise it rather more than opium; of the cannabis Indica alone

TABLE III.

Substances given.	No. of Patients.	No. of Experiments.	Average Temperature.		Average gain in each experiment.	Average loss in each experiment.
			Before medicine.	After medicine.		
Tinct. Opii . . .	9	29	97·6°	97·8°	·2°	...
Tinct. Can. Ind., Potas., and Bromid.	8	29	98°	97·7°	...	·3°
Potas. Bromid. . .	7	13	98·1°	98·4°	·3°	...
Tinct. Can. Ind. . .	7	15	97·5°	97·6°	·1°	...
Whisky . . .	10	21	97·9°	97·3°	...	·6°
Beef tea . . .	9	15	98·16°	98·14°	...	·02°

to raise it very slightly; of the whisky to lower it most of all, and of the beef tea to lower it in the most trifling degree possible. It must be remembered that the temperature of the body in such maniacal patients is higher than in health. I think we must look on the action of opium, bromide of potassium, and tincture of cannabis, therefore, in this respect, as being away from the healthy standard, while that of a mixture of the bromide and tincture of cannabis is in the opposite direction. There can be no doubt about the lowering of temperature caused by the whisky being too great. Its effect in this direction was almost uniform in nearly all the experiments. Even when it aggravated the excitement at first, and there was much more muscular motion, the temperature was usually found lower. A loss of 2·3° in the temperature of the body in an hour (when that temperature had not been very abnormally high to begin with) means devitalization, and that was the case once after the whisky. We may fairly in this case, then, take the effect of the beef tea as our standard of what we might expect from a drug which most readily approached the reparative action of food. The effect of opium was in the wrong direction altogether; that of the mixture of the bromide and cannabis Indica in the right direction, but perhaps going too far; and of the whisky in the right direction, but going too far.

The different effect of the mixture of the bromide of potassium and the cannabis Indica from each of them given separately is worthy of notice, as it confirms my experience that in all respects this mixture acts differently from either of its constituents.

The effects of smaller doses on my assistant and myself differed from those above mentioned in the bromide lowering the temperature, the cannabis Indica raising it considerably, and the mixture of both raising it very slightly, while the beef tea also raised it slightly. The lowering of the temperature by whisky was very marked.

TABLE IV.

Substances given.	No. of Patients.	No. of Experiments.	Pulse before getting medicine.	Pulse after getting medicine.	Average gain in each experiment.	Average loss in each experiment.	No. of times. pulse decidedly irregular, or intermittent.
Tinct. Opii .	9	29	85	84	...	1	3
Potas. Bromid. and Tinct. Can. Ind.	8	29	84	95	11	...	7
Potas. Bromid.	7	13	83	79	...	4	1
Tinct. Can. Ind.	7	15	83	89	6	...	3
Whisky . .	10	21	81	82	1	...	0
Beef tea . .	9	15	80	79	...	1	0

Pulse.—In Table IV the effects of the different substances on the pulse are seen. That of opium was to lower it to a most trifling extent. The bromide did so in greater degree, and the beef tea about the same extent as the opium. The tincture of cannabis Indica, on the other hand, caused an average increase of eleven beats, and the tincture of cannabis alone of six beats. The tendency of the mixture was also slightly to lessen the force of the pulse; and as is seen from the table, to cause irregular action to a greater extent than opium. In seven of the twenty-nine experiments an irregular or intermittent pulse followed a dose of this mixture, while in only three of the twenty-nine was this the case after opium. It is known that cannabis Indica quickens the action of the heart; but why the bromide, which itself tends to reduce its action, should actually strengthen the accelerating action of the cannabis, is only explicable by the theory that all the effects of the latter are greatly added to by giving it with the former.

On myself and my assistant the effects of smaller doses on the pulse were the same as the results shown in the table.

Taking all the effects of these medicines into account, I think the balance of good is decidedly on the side of the mixture of bromide of potassium and cannabis Indica. The greater certainty and longer duration of its sedative effect on the excitement, and the absence of any bad effect on the appetite, are good results which are not materially interfered with by its action on the heart.

In regard to the dose of bromide of potassium, which is equivalent in sedative effect to opium or henbane, it is very difficult indeed to come to an exact conclusion. If there is much excitement, no single dose of the bromide up to two drachms will be at all equal to one drachm of the tincture of opium. To produce sleep in milder cases my experience is, that one drachm of the bromide will have the same soporific effect in most cases as half a fluid drachm of laudanum, or two drachms of the tincture of hyoscyamus. With regard to the dose of a mixture of the bromide and tincture of cannabis, which will be equivalent to laudanum, I have had more experience. The experiments as to single doses throw some light on this, and in many other cases I have given the two medicines alternately to see the effects; while in others, to whom I now give the bromide and tincture of cannabis to allay excitement, I used formerly to give opium, hyoscyamus, and tincture of cannabis alone. In only two out of thirteen cases did I find one drachm of the bromide with one drachm of the tincture of cannabis to have a less sedative effect than one drachm of tincture of opium. In two cases its effect was about the same, and in the other nine it was unequivocally more decided, while in two it was more decided than ninety minim doses of the laudanum. My experience has been, that about forty-five grains of the bromide, along with forty-five minims of the tincture of cannabis, will have the same sedative effect as a drachm of laudanum. In one violent case of periodic mania, with whom opium did not agree, I find that drachm doses of the bromide, and a drachm of tincture of cannabis, have rather more than the same effect as half-ounce doses of the tincture of hyoscyamus, and that by adding drachm doses of the bromide to one drachm of the tincture of hyoscyamus, the same sedative effects were produced, or by half-ounce doses of the latter alone. In another case drachm doses of each of the bromide and cannabis have as much effect as six drachms of the tincture of hyoscyamus. In another case half-drachm doses of each of the former have about the same sedative effect as two-drachm doses of the latter.

The experiments with single doses which I have related quite bear out my previous experience as to the decided increase in the effect of a combination of the bromide and the Indian hemp over the effect of either of them used separately. Over and over again I have found that half-drachm doses of each were more powerful than drachm doses of the tincture of cannabis, or than two-drachm doses of the bromide. This seems to me one of the peculiarities of the bromide of potassium, that, combined with a sedative drug, it powerfully increases the usual effect of such drug. I cannot speak so certainly of this effect when combined with opium, but with hyoscyamus and Indian hemp it is most decided. In my own case, the effect of fifteen minims of tincture of cannabis along with half a drachm of the bromide was very much stronger than half a drachm of the former alone, and incomparably stronger than a drachm of the latter. In very many cases I have given a combination of the two medicines where I had been giving sometimes the one and sometimes the other previously, and I have always found that half quantities of each combined were as powerful as double the amount of the cannabis, and more powerful than double the amount of the bromide given alone.

It was one of the effects of Indian hemp specially mentioned by O'Shaughnessy in his first experiments with the drug, that it caused an increase of appetite, and I found that when giving the bromide of potassium to my epileptic patients, it increased their appetites. Certainly no sedative or narcotine drug that I have ever used in large doses in the case of maniacal patients affected their appetites so little as the two given together. In doses under a drachm of each I never saw any diminution of appetite at all, and in large doses it was only after a long time that I found the appetite sometimes lessened.

My own experience of the kind of effect produced by mixture of the bromide and the Indian hemp as distinguished from the effect of each of those taken separately when I took them myself was this. Half a drachm of the bromide produced a slight drowsiness in about an hour, which lasted for about two hours, but the effect was almost imperceptible. A drachm of the bromide produced a more decided drowsiness, and after about two hours a feeling of coldness and slowness of the pulse. Half a drachm of the tincture of cannabis produced in an hour a feeling of confusion and fulness in the head, then a sort of preternatural acuteness of hearing, then the impression of a great length of time between acts performed within a minute of each other described by Christison, and in two hours great drowsiness, which lasted for four hours, leaving a feeling of confusion in the head and incapacity for continuous mental exertion. Fifteen minims of the same tincture along with half a drachm of the bromide, caused in an hour first a tingling in the calves of the legs, then numbness of the legs extending gradually all over the body, then confusion of ideas and the impression of lengthened time and sense of fear, then a tendency to jerking and unsteadiness of the muscles. In about two hours all these sensations became merged in a sensation of fulness in the head and great drowsiness, which lasted for six hours. Its effect was also strongly diuretic. It produced quickness and irregularity of the pulse for the first three hours. I do not attach very much importance to the experiments on myself and my assistant, as no true comparison can be made between a therapeutical and a purely physiological experiment. Curiosity alone prompted me to take the drugs myself.

Effect of the sedatives when given regularly for long periods.— Opium in large doses having been hitherto regarded as the most powerful, and in the majority of cases the most reliable sedative in uncomplicated maniacal excitement, I selected nine chronic maniacs, all labouring under great excitement of long duration, and on them tried the effect of opium given continuously for a length of time. I knew the natural history of the disease in them all, for they had all been under my observation for periods of from three to five years, with little or no medical treatment for long periods. I regarded them all as quite incurable. Whatever effects on the bodily health and organic functions the brain excitement could produce, had taken place, and they were nearly all, as it were, *in statu quo* as regards body and mind. I had no reason to suppose that any of them had such organic disease of the brain as softening, tumours, or any other progressively fatal lesion. In three of them there were remissions of the excitement at regular times, but the periodicity in each of them was regular and quite well known to me. My reasons for taking such cases were, first, that they were incurable, and therefore the experiment could not do any harm to them; second, that in them I had simple brain excitement in known amount, against which I could, as it were, match a sedative drug; and third, that in them I could observe the effects of that drug on the bodily functions, the temperature, the weight of the body, the pulse, &c., with the certainty that any changes that might occur were the effects of the drug, and were not happening in the natural course of the disease.

For a month I had these patients weighed every week, noting their weights, their morning and evening temperature, and their morning and evening pulse. Then for twelve weeks I gave them opium in the form of tincture opii of the 'British Pharmacopœia' in increasing doses, noting every week their mental state, their weight, temperature, and pulse. For the first two weeks I gave them twenty-

TABLE V.

Name.	Age.	Weight before taking opium.	Weight after taking opium.	Average morning temperature before medicine.	Average evening temperature before medicine.	Average morning temperature after medicine.	Average evening temperature after medicine.	Average morning pulse before medicine.	Average evening pulse before medicine.	Average morning pulse while taking medicine.	Average evening pulse while taking medicine.	Patients' mental state.
		lbs.	lbs.									
J. G. . .	61	133	123	97°	98°	97·8°	97·6°	77	89	93	71	Chronic mania; 4 years' duration; excitement severe; no remissions; intelligence not quite gone.
E. M. . .	42	128	122	98·2°	97·7°	97·2°	97·6°	78	93	81	77	Chronic mania; 4 years' duration; exacerbations, but not regular; mental powers very much impaired.
M. T. . .	34	148	140	98·6°	97·8°	98·5°	97·4°	74	74	82	73	Chronic mania; 10 years; regular periodic exacerbations; great excitement; mental powers not so much impaired as any of the others.
J. H. . .	26	134	127	97·8°	97·5°	97·5°	97·2°	100	77	91	74	Chronic mania; 10 years; mind much impaired.
C. M. . .	30	113	114	98·4°	97°	97·5°	97·5°	104	93	90	79	Chronic mania; 3 years; tendency to phthisis; great impairment of mental powers.
S. R. . .	35	116	103	96·9°	97·7°	96·6°	97·4°	81	78	80	71	Chronic mania; 4 years; great excitement and violence; mental powers considerably impaired.
E. S. . .	39	105	103	98·4°	97·7°	97·5°	97·6°	103	94	93	89	Chronic mania; 10 years; one lung tubercular; mind quite impaired.
Aggregate weights and average temperature and pulse		877	832	97·9°	97·6°	97·5°	97·5°	88	85	87	76	

five minims three times a day, for the next two weeks one fluid drachm three times a day, for the next eight weeks one fluid drachm and a half three times a day.[1] Of course the patients during all this time were in the same circumstances with regard to diet, clothing, and other conditions. The reason I kept up the drachm and a half doses so long was, that this is about the limit of the doses commonly used in mania, and I wished to ascertain the effects of such ordinarily employed doses. At the end of the twelve weeks the medicine was stopped. Of the nine I found that the opium caused such persistent sickness and total absence of appetite in two, that I could not continue it for more than a few days. There were only seven, therefore, in whom the experiment was continued to the end.

Excitement.—As regards the maniacal excitement, I found that in none of the cases did the twenty-five minim doses subdue it in any degree, and during the fortnight they took this dose there was no perceptible change in their appetites, weights, temperatures, or pulses.

During the fortnight they took drachm doses there was a very perceptible difference in the maniacal excitement in all of them but one, who at the time was passing through one of the regular exacerbations which characterised her case. In three of them there was a decided tendency to drowsiness through the day. In one case the partial subsidence of the excitement which characterised the first week did not last through the second, for by the end of it she was about as excited as ever. Five of them had begun to lose in weight, though the absolute loss was small, being only eleven pounds in the five. There was a slight fall in the temperatures of most of them.

When the dose was raised to a drachm and a half the excitement was very markedly lessened or altogether overcome in all of them. This effect was not lasting, however, in all the cases, for by the end of six weeks two of them were nearly as excited as ever, and the one who was subject to exacerbations had one of these during this period, and was almost as bad as when she was free from the influence of any drug. The most careful examination into the character of the cases did not show any reasons why one case should have been more and longer affected by the opium than another. To refer to Table V, J. G. and C. M. became almost as bad as ever when taking the medicine, and M. T., during one of the exacerbations of her malady, was but little affected by it. One of these patients was old, the other two young. In one case the mental powers were very much affected, in the other two not so much so. Two of them were robust, the third had a tendency to phthisis.

The results ofthe treatment in all of them are shown in Table V.

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[1] They got the first dose of the opium at seven o'clock in the morning, and the temperatures were taken at half-past ten. I wished to avoid the immediate effects of a dose of the drug.

Weight.—They all lost weight while taking the large doses of the drug. In some of them the loss was considerable, in others trifling. The greatest loss was thirteen pounds, the smallest one pound. The aggregate loss in all the cases was forty-five pounds. The patients whose excitement was most subdued did not lose most, nor did they lose least. It may be thought that the total amount lost in weight by them all is very small indeed, but it must be remembered that all those patients had no doubt lost greatly in weight when they first became excited, and were mostly at the minimum consistent with such health as they enjoyed. The opposing tendencies of the excitement of the brain and the reparative powers of the food they ate had found their balance, as it were, in each of them, and the effect of the opium was to give some more strength than had previously existed to the exhaustive forces.

Temperature,—On comparing the average temperature of each of those patients when taking no medicine, and during the whole time they were taking the opium, we find (Table V) that in every one of them but one it was lower while taking the medicine. The actual fall in each case is seen to vary from 1° to ·1° in the morning temperature, and from ·5° to ·1 in the evening temperature. The total amount of the loss of temperature in the seven was in the morning 2·7°, and in the evening 1·1°. This seems small and unimportant, but it must be remembered how much the permanent lowering of one degree of temperature represents of loss in vital energy and reparative action if it is already below the normal amount, and how much of good it may represent if it is above the natural standard.

The average temperature of those patients was higher than most other classes of patients in the asylum, and higher than the average temperature of forty sane persons who were employés in the asylum, which I found to be 97·5° in the morning, and 96·7° at night. Was it not, therefore, a health tendency, this reduction of the temperature caused by the drug? To answer this we must take into account certain facts in regard to the temperature of the insane. I found from an examination of the temperature of patients labouring under all forms of insanity, that a high evening temperature, as compared with the morning temperature, represents a large mortality in the class where this exists. Any drug that would have a curative tendency must reduce the evening temperature. In the cases experimented on, it is seen that the reduction of the evening temperature is very slight as compared with the morning temperature of the patients, and that, therefore, the average evening temperature is relatively higher while they were taking the opium than when they were not, the difference between the average morning and evening temperature of the same persons was ·8°, the difference in those seven patients taking no medicine was ·3°, while the difference after they began to take the opium was nil. It seems to me, therefore, that taking the effects of the opium on the weight and temperature of the patients together, we must conclude that it lowered the reparative power of the body below the point at which it could, as it were, cope with the destructive tendencies of the brain excitement.

It will be clearly observed that my experiments, though pointing to ill effects that may result from the use of opium in maniacal excitement for short periods or in single doses, yet tend to show that those ill effects will probably only be slight at first. The necessity or the supposed advantage of temporarily subduing the excitement may be so great or so urgent in any particular case that the physician will decide to do so by opium, notwithstanding those risks.

Pulse.—The average frequency of the morning pulse with opium and without it does not show any constant result in all the cases. But if we look at the evening pulse we see that in every case it was lower after taking the opium than before. The pulse of a chronic maniac is a most variable quantity, especially during the day, when there is much movement of the body, and the indications got from it are not much to be relied on. Taking all the cases there was a lowering of one beat in the general average of the morning pulse, and of nine beats in the evening pulse, the numbers being 88, 87, and 85, 76.

It will be observed that all those effects were the result of a continuous use of the opium in doses that were far from being narcotic in their effects. In no case did any comatose symptoms show themselves. Sickness was not caused in those patients who continued to take the medicine. In those whose excitement was allayed there was no torpor of mind or body produced so that they could not take their food or take their usual exercise. In regard to sleep there is no doubt they all slept very much better when taking the opium than before. The functions of the hemispheres of the brain were disordered, and this in all cases tends to impair or interfere with the healthy nutrition of the body, and opium when given continuously in the doses in which I gave it, whether as in some of the cases it seemed to allay the symptoms of the disordered cerebrum, or whether it did not do this, yet in all cases it still further interfered with the proper nutrition of the body, and pushed it one step further down hill in the direction of death.

The rise in temperature, which was the immediate effect of single doses of opium, was thus seen not to last when the drug was given continuously. The loss in weight among the patients is, no doubt, directly connected with the tendency observed in the experiments with single doses to interfere with the appetite for food. The element of uncertainty in regard to its effect on different cases was seen to exist when it was given continuously just as much as when single doses were given, and an explanation of this was as difficult in the one case as in the other.

In order to compare accurately the effects of a continuous course of a mixture of the bromide of potassium and cannabis Indica with that of opium, I discontinued the use of the latter in the cases of those seven patients, and waited till the same time of year came round as that in which I had made the preceding observations. I then, after having observed their mental state and weighing them, put them on half-drachm doses of each thrice a day, and continued this for a fortnight, but finding that the medicine was having an effect, and showing no signs of losing that effect, I continued its use for a fortnight longer. I then increased the doses to forty-five grains of the bromide and forty-five minims of the tincture of cannabis, and continued this for a fortnight, but as this was having a very decided and continuous sedative effect I could not safely increase the doses any more, except in one or two of the cases, to whom I gave a few doses of a drachm of each, and in whom the effects were decidedly too strongly narcotic to be long continued. I then reduced the doses to a half drachm of each. I found this treatment so beneficial to the patients that I have continued it now for about eight months, with a few days' intermission occasionally in all the patients. In the case of those whose excitement was periodic I gave it during the excited periods only. I have noted some of the results that could be tabulated in Table VI. They are shortly these:—

Excitement.—The half-drachm doses had the effect of allaying the

TABLE VI.

Name.	Weight.				Morning Temperature.		Evening Temperature.		Morning Pulse.		Evening Pulse.	
	Before taking medicine.	After four weeks.	After six weeks.	After eight months.	Before taking medicine.	After six weeks.	Before taking medicine.	After six weeks.	Before taking medicine.	After six weeks.	Before taking medicine.	After six weeks
	lbs.	lbs.	lbs.	lbs.								
J. G. . .	128	125	130	123	97°	96·1°	98°	97°	77	87	89	60
E. M. . .	130	128¼	131	134	98·2°	96·5°	97·7°	96·7°	78	86	93	82
M. T. . .	134	139	138	135½	98·6°	98·7°	97·8°	98·2°	74	83	74	68
J. H. . .	126	122	122	124	97·8°	97·2°	97·5°	97·4°	100	110	77	80
C. M. . .	109	107½	105	110	98·4°	98°	97°	97·5°	104	83	93	81
S. R. . .	106	106¼	107	108	96·9°	97·3°	97·7°	96·9°	81	77	78	67
E. S. . .	102	103¾	101	106	98·4°	98·2°	97°	98·4°	103	98	94	96
Aggregate weights and average temperature and pulse	835	831¾	834	840½	97·9°	97·4°	97·6°	97·4°	88	89	85	76

excitement in all the cases but two. This was quite as strongly marked as the first effect of the drachm-doses of laudanum, and during the nine months it has been given there seems to be no perceptible tendency to lose its effect. This is in most marked contrast to the manner in which the sedative effect of the opium was lessened or lost in a week or two. The effect of each dose is not so soon observed, but lasts longer than each dose of the opium, and is not so apt to cause an approach to narcotic drowsiness in any case. The patients look better, less as if they were under the influence of a narcotic drug, and more as if their maniacal excitement was naturally abated. The forty-five grain and forty-five minim doses were in one of the patients followed by a decided drowsiness and sluggishness, with coldness and paleness of the skin and weak pulse, and this was also the case to a greater extent with the drachm doses. But when the excitement was very intense indeed, even these doses were not followed by any such effects.

Weight.—For the first four weeks, and with the half-drachm doses, the aggregate weight of the patients diminished about four pounds, four having lost in weight and three having gained. It will be remembered that during the same period under the opium treatment they lost eleven pounds. Curiously enough nearly all the patients who lost under the opium gained weight under the other treatment. At the end of seven weeks the patients were beginning to gain in weight, so their aggregate weight was only one pound less than when they began the treatment, and now at the end of eight months their aggregate weight is five and a half pounds more than it was to begin with, five having increased, two diminished in weight, and one remained stationary.

Appetite, &c.—In no case was the appetite interfered with. At very rare intervals the mixture produced sickness in one of the patients. Their tongues all remain clean, and no constipating or purging effect on the bowels has been produced.

Temperature.—As seen in Table VI, the average morning temperature of the patients fell ·5°, and the average evening temperature ·2°. The latter result I consider as more favorable than the results of the opium treatment. Taking each patient separately, five of them fell in morning temperature, and four of them in evening temperature. There was no fall in the evening temperature relatively to the morning temperature, in this respect not being different from the opium.

Pulse.—The pulse was slightly increased in frequency in the morning and diminished in frequency at night (see Table VI), the diminution not being nearly as great as that caused by opium. In three of the cases the pulse was slightly weakened.

If we compare the general results of those two modes of treatment it is seen that the maximum of good effects and the minimum of those that are ill in their tendency were obtained by the use of the combination of half-drachm doses of bromide of potassium with thirty minim doses of the tincture of cannabis Indica.

It is interesting to compare the results of the two modes of treatment I have described with the effects of bromide of potassium alone in large doses on the weight and pulse and temperature.[1] I had been giving it to twenty epileptic patients for thirty-eight weeks in doses rising from five grains three times a day up to fifty grains three times a day, with the following results. They gained steadily in weight for twenty-eight weeks, and their aggregate weight was then fifty-six pounds more than when they began to take the medicine. During the last ten weeks, when they were all taking 150 grains of the medicine per diem, they lost forty pounds. At the end of the time they were still sixteen pounds heavier in the aggregate than they had been to begin with. During the same time the effect of the medicine on their temperature was to lower it steadily until forty grain doses were reached. After that it rose, until at the end of the ten weeks of fifty grain doses it was above what it had been to begin with. Still the difference between the morning and evening temperature was greater at the end of the time than it had been to begin with. The effect on the pulse was to lower its average frequency steadily up to forty grain doses, and then to raise it slightly, but at the end of the time its average frequency was not so great as it had been to begin with. The patients who took the bromide of potassium being epileptics, the effects of the medicines above described cannot be compared with it with perfect precision, but still the general result holds good, that the tendency of the bromide of potassium was at least up to 120 grains per diem, in the direction of health, while from the beginning the effects of the opium were as regards the bodily state in a direction away from health, and the addition of the cannabis Indica to the bromide in the maniacal patients, while it had a good effect on the maniacal excitement, did not seem materially to interfere with the nutritive process.

It has been the result of my experience with bromide of potassium given either in epilepsy or in insanity, given alone or in combination with cannabis Indica, that there is a certain dose which may be given with perfect impunity as regards the general health, and with great benefit to the disease for long periods, and that if this dose is increased cumulative effects will show themselves, and all the symptoms of poisoning will come on. The safe and beneficial dose differs in different cases. It would seem as though the kidneys (through which the salt is principally eliminated from the system) can only carry away a certain amount in each individual. I have given twenty-five grain doses three times a day to seventeen epileptics for two years, with a break of only one week, and in only two cases did any constitutional symptoms show themselves, and that was not till the end of a year. The others are all much the better for the medicine, having gained in weight, and improved in general health.

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[1] 'Journal of Mental Science,' Oct., 1868.

And when I put twenty-nine epileptics on the medicine in graduated doses, beginning with 15 grains per diem and ending with 150, only the twenty I have referred to stood this treatment for the thirty-eight weeks it was persevered in. At the end of eleven weeks, when they were all taking 75 grains per diem, a boy of fifteen showed signs of being "bromidized;" two men, at the end of seventeen weeks, when taking 105 grains, began to feel bad effects, and two others, at the end of eighteen weeks, with the same doses as the last, showed that the medicine was accumulating in the system. The same phenomenon we shall observe in the clinical observations which follow where the bromide was combined with Indian hemp. In some cases, drachm doses of each were given three times a day for weeks with good effects; in other cases, the same doses could not be continued for more than a few days. This I regard as one of the most important facts yet discovered in regard to the bromides. I have as yet been able to discover no fact by which we can predict beforehand that any particular case will stand large doses for a long period, or that another case will not do so with impunity. Until such a test is discovered we can only give them tentatively in gradually increasing doses in each case until we get up to the maximum of good result without any danger of the cumulative effects of the drug. My experience has been that a strong, vigorous patient, with all the functions of the body performed actively, will generally stand large doses for a longer time than a weaker patient: but to this I have seen exceptions. The salt being eliminated by the kidneys would point to giving a diuretic along with it in cases where it is considered of importance to give large doses, and yet not cause cumulative effects. Indian hemp has been much used as a diuretic. I have given some Spt. Eth. Nitrosi with each dose in such cases, but my experience on this point as yet has not been sufficient to enable me to express any conclusion as to the result.

To test the effects of bromide of potassium alone on acute maniacal excitement, I selected by far the most violent case of periodic mania in the asylum, a woman in whom the attacks of excitement had come on about every month or six weeks for two years, and lasted from a fortnight up to a month. She was a young strong woman in good bodily health. I had tried opium in this case, but it aggravated all the symptoms, and tincture of hyoscyamus in half-ounce doses thrice a day produced but slight abatement of the symptoms. On the occasion on which I made the experiment she became suddenly excited, on the 13th of March. On the 14th she was furiously maniacal, shouting, restless, sleepless, most violent and destructive, with a strong pulse of 90, flushed face, suffused eyes, muscular system in constant activity, and temperature 99·6°. I put her on two-drachm doses every three hours, on this the second day of her excitement. In forty-eight hours, after she had taken four ounces, she was as excited as ever, but her tongue was beginning to get furred in the centre with a raw line down each side, and there seemed slight muscular unsteadiness. Her pulse too was somewhat weaker. The medicine was given for another day and a half, until she had got seven ounces of it. The maniacal excitement then quite suddenly abated, and her mental state became one of torpid depression. She became pale and pinched-looking, the pupils sluggish, the pulse 108 and very weak, and the temperature fell to 96°. These symptoms increased after the medicine was discontinued, until the co-ordinating power of the muscles was so completely lost that she reeled like a most intoxicated man when she attempted to walk. She slept, almost continuously, for three days. She only took food when given to her, and seemed not to care for it. Her bowels were regular. Her tongue was at first furred with a thick white fur, and then got quite raw-looking. Her breath smelt of bromide of potassium. Her articulation was much affected. She remained in this state for about five days, her temperature remaining at 96°, and it was quite a fortnight from the time it was discontinued before she got over the effects of the drug. As she got better there was slight feverishness with dry tongue and want of appetite. The excitement did not return when the effects of the drug passed off; but the next attack began on the 10th of April, which was sooner than it ought to have come on by a week at least.

This experiment is instructive, as showing, 1st, that the most acute excitement can be subdued by bromide of potassium; 2nd, that this cannot be done without pushing the medicine far beyond what is safe; indeed, almost up to complete paralysis of the cerebrum and sympathetic ganglia of the heart; 3rd, that the action of the drug in such a case is strongly cumulative, increasing in intensity for days after it has been stopped, and lasting for a long time; and 4th, that no permanent improvement is necessarily produced in the morbid cerebral action. I do not regard this a case of cutting short the excitement. It was rather one of half poisoning the patient. If any justification of such an experiment is needed, I must plead the importance of knowing exactly the effects of the bromide on maniacal excitement, the hope that benefit might possibly accrue to the patient, and the absolute want of any precedent to guide me. It seems to me highly instructive, clinically as well as physiologically. Such a case should show the necessity for stopping the bromide at once when its bad effects are first observed, on account of this cumulative action. It illustrates the prolongation of the effects of this as compared with any other drug that has the power of producing the same narcotic or paralytic action on the nervous system. It proves that maniacal excitement gives the same tolerance to the system in resisting the ordinary physiological effects of this drug as in the case of the ordinary vegetable narcotics.

(*To be continued.*)

Chronicle of Medical Science.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.,
Member of the Royal College of Physicians, Physician to the Bloomsbury Dispensary, London.

On the Employment of Alcohol in the Treatment of Pneumonia and Broncho-pneumonia. By Dr. L. Gros.—Dr. Gros, after relating the particulars of a case in which a child was successfully treated for broncho-pneumonia by alcohol, remarks that, in his opinion, the treatment supported life, and gave to the local inflammation time to accomplish its evolution. Without the treatment, he argues that the child would not have been able to live, inasmuch as it would have suffered from asphyxia, the result of paralysis of the lungs. It may be admitted, he thinks, that alcohol nourishes indirectly, not by increasing the bulk of the body, but by diminishing its waste, at the same time that it raises the nervous force. He insists upon the fact that it wards off death and gives the disease time to run its course. Dr. Gros also gives the particulars of the case of an old woman, suffering from pneumonia of an adynamic character, who was cured by the alcoholic treatment, and he remarks upon the *regulating* power of the alcohol, which appeared plain and obvious. Under its influence delirium, sleeplessness, cardiac disturbance, and excessive frequency of the pulse were relieved as if by enchantment. But Dr. Gros does not assert that the alcoholic treatment of pneumonia and broncho-pneumonia in children or in old persons is always successful, and indeed he gives instances where it has failed, but he considers that when adopted according to the plan proposed by English physicians, it is likely to render great service in the treatment of pulmonary and bronchial inflammations, and probably in other inflammatory affections.—*L'Union Médicale*, August 8, 1869.

On the Treatment of Pneumonia by Digitalis. By Professor Hertz, of Strasburg.—The inflammatory diseases in which digitalis has been principally employed are pneumonia, pleurisy, acute articular rheumatism, erysipelas, pericarditis, general bronchitis, &c. The drug appears to act by reducing the fever, on which the danger of the pneumonia chiefly depends, and the exact effects it produces in lowering the pulse and diminishing the temperature have been lately determined with accuracy. The dose in which Dr. Hertz administers digitalis in pneumonia is generally from three-fourths of a gramme (a gramme is about 15 grains) in 100 grammes of water and 20 grammes of syrup, administered in spoonfuls every hour. The pulse is affected first, becoming irregular, intermittent, and at last slow, sometimes in extreme cases being reduced to 40 or even 29 in a minute. Together with the fall of the pulse, but rather more slowly, comes the diminution of the temperature. This is at first shown by the absence of the febrile exacerbation in the evening, and then the next day it is found that the temperature has fallen about two or three degrees. The temperature artificially reduced remains for a shorter time than the pulse under the influence of the digitalis, and after two or three days it resumes its normal rate even when the pulse remains a long time below it. The heat, when it has once resumed its normal character, no longer exceeds it and the fever is definitively subdued, which is not the case with bleeding, the latter measure having only a transient effect. But although the fever is abated, the local malady in general still remains, although the patient experiences a decided sensation of relief. Starting from this moment the hepatization is arrested in its progress, and the *rhonchus crepitans redux* announces the approaching resolution of the disease. In pneumonia digitalis is particularly indicated when the fever is clearly inflammatory, the temperature elevated, the pulse frequent, and the patient young and strong. Still, children, women, and even old persons, when they are not very much weakened, bear the treatment very well. Dr. Hertz does not employ it in typhoid pneumonia, or in cases marked by coldness of the extremities, or by smallness and frequency of the pulse, but he would use it even in these cases if he did not fear to endanger the credit of the drug by employing it in cases which are almost necessarily fatal.—*Gazette des Hôpitaux*, July 31, 1869.

On the Employment of Digitalis in Typhoid Fever. By M. Hankel.—M. Hankel has collected the particulars of eighty cases of typhoid fever, treated in the hospital at Leipsic with digitalis. The general results of the treatment appear not to have been favorable, as it is recorded that only forty-five were cured; but M. Hankel has analysed the effects produced on each organ by the drug. In reference to the *brain*, it was found that delirium was relieved in the slight cases, but that the more serious forms of cerebral disturbance were not affected by the use of the digitalis. As to the *vascular system*, it was found that the frequency of the pulse was distinctly lowered by digitalis in certain cases, but whenever it was found that no such effect was produced by the medicine, the cases were always fatal. When the pulse was small and feeble, it became fuller under the influence of the treatment whenever its frequency diminished in a notable degree. The effect of digitalis was particularly favorable in the cases complicated with heart disease; and if the pulse had been irregular, it became more regular. In reference to the *digestive system*, M. Hankel found, in the cases treated by digitalis, that there was a more marked moisture of the tongue, and that vomiting often followed immediately after the first dose of the medicine, but did not continue, even although the doses were repeated. The *urine* was increased in seven cases treated by digitalis, but its volume was never diminished. Moisture of the *skin* was sometimes excited by digitalis, and sometimes even sudamina were developed. The duration of the disease was not abridged by this treatment. The herb was employed in infusion in the dose of $1\frac{1}{4}$ gramme to 2 grammes (a gramme is about 15 grains) in a day.—*Archiv der Heilkunde*, 1869.

On the Employment of Chloral in Hooping-cough. By Dr. A. Ferrand.—After some preliminary remarks on the efficacy of chloral in painful and neuralgic affections, Dr. Ferrand relates some cases in which he administered this drug successfully in hooping-cough. In the case of three children attacked with hooping-cough he had employed some of the ordinary remedies without any good effect, when he determined to make a trial of chloral. He gave at first only very small doses, namely, one fourth of a gramme (a gramme is about fifteen grains) in syrup every evening at bed-time, but, as the medicine was given irregularly, no result ensued. The dose, however, was increased, and given more frequently and regularly, and a very manifest improvement was obtained. The nights, which had before been disturbed, now became quiet; and, instead of three or four paroxysms of cough, with vomiting, there was complete and refreshing sleep. In the morning there was a paroxysm every day for some days, but it gradually ceased to return, and the cure was soon accomplished. The treatment had lasted ten days, and the disease about twenty, in one of the cases; in another case, in which the disease had lasted longer and was more severe, the cure was effected in a few days more; but in the third case the disease had lasted a month, and the treatment occupied twenty days. Dr. Ferrand does not undertake to explain the action of chloral in hooping-cough, but he regards its effect as being chiefly hypnotic, and does not agree with other authorities in considering its operation to be due to the liberation of chloroform in the system.—*Bulletin Général de Thérapeutique*, Jan. 30, 1870.

On the Use of Hydrate of Chloral in Affections of the Nervous System. By Dr. Clouston, Medical Superintendent of the Cumberland and Westmoreland Asylum.—All the patients to whom Dr. Clouston administered chloral were suffering from affections of the nervous system, complicated with mental symptoms. He has given the drug in forty cases, in doses varying from ten grains up to eighty, for the purpose of procuring sleep, of calming nervous irritability, of subduing maniacal fury, of allaying extreme depression of mind, of arresting an attack of mania, and in neuralgia and chorea. The principal cases in which it was given were acute and chronic mania and in sleeplessness. In seven cases of acute mania uncomplicated with any apparent organic affection of the brain the chloral was administered for the purpose of procuring sleep, and by means of forty-grain doses this object was effected in every instance. In the excitement of chronic mania, and the still greater excitement of epilepsy and general paralysis, a drachm is often required. In cases of mere sleeplessness, without any particular excitement, twenty grains were usually sufficient, and generally thirty grains produced sound sleep, and no bad consequences supervened on waking, except slight giddiness and confusion in two cases out of eight. The doses given were twenty grains at the beginning when the symptoms were slight, and thirty grains when they were very severe. In the former class of cases Dr. Clouston often increased the dose to thirty grains, and in the latter nearly always to forty and often to sixty grains. With respect to the effect of chloral on the temperature of the body —a point on which much diversity of opinion exists—Dr. Clouston found in the cases where the temperature was noted that in some the temperature remained as high as before the drug was given, but in others it was decidedly lowered. This latter effect of chloral Dr. Clouston considers to be a strong point in favour of its use in acute mania, for this disease, by causing increased heat of the body, burns away the tissues, and any substance which diminishes this tendency must do good. In one case of puerperal mania, the patient's temperature was 100 degrees when she went to bed; but she took forty grains of chloral, slept eight hours, and the next morning the temperature was 98·2, and this was only a single example. Comparing the effects on temperature of chloral, opium, bromide of potassium, and alcohol respectively, Dr. Clouston found that chloral lowered the average temperature, taking all the cases together, but not to any great extent, and not nearly so much as alcohol. Dr. Clouston himself took a dose of fifteen grains, which raised the temperature half a degree, and, on the whole, he is not yet quite able to determine the conditions under which chloral affects the temperature of the body. Dr. Clouston's conclusions as to the effects of chloral are that it has proved a most safe and certain sleep-producer; that by this property attacks of insanity may probably be warded off in some cases; that its action in abating and soothing excitement is more uncertain than its sleep-producing powers, and that it is, therefore, only adapted for the treatment of short or recent cases of insanity, or to subdue temporary excitement. Dr. Clouston states, moreover, that, whether it does good or not, it never does harm, and in this respect it is the very king of all narcotics.—*British Medical Journal*, May 7, 1870.

On the Physiological and Therapeutical Effects of Cold Affusions. By Dr. de Lambert.—The object of Dr. de Lambert in this paper is to remove the prejudices which have hitherto opposed the general adoption of cold affusions as a therapeutical agent in certain cases. The physiological effects of cold affusion may be classed under two heads, namely, the lowering of the temperature and the excitement of the nervous system: and such are the results obtained, from all the observations hitherto made, especially in clinical medicine. The impression of cold gives rise to complex effects, local and general. Seated at once in the skin, the capillary vessels, the muscles of the animal and organic life, in the glands, and in the central and peripheral nervous systems, the phenomena produced are of the reflex kind, and consist of spasm of the contractile structures of the skin, contraction of the capillary cutaneous vessels, paleness of the skin, and retrocession of the blood towards the internal organs. After the spasm first induced, there comes a more or less rapid reaction, and the circulation of the surface is restored, the skin again becomes coloured, the heat returns with the blood, the pulse again becomes full and strong, and a general feeling of comfort is induced. Dr. de Lambert attributes to Dr. Wright, of the British navy, and to Currie, of Liverpool, the merit of having methodically employed cold affusions in the treatment of disease. Dr. de Lambert describes the method which he considers most efficacious for employing the cold affusion, and which consists in placing the patient in an empty bath, or one containing a very little cold water, and then by the help of an assistant, pouring from a pail, at some height above the head, a copious stream having a considerable force of propulsion. If the water be not poured from a sufficient height, it does not produce in a sufficient degree the shock which is required. The time occupied by the affusion should be short, and not exceeding two or three minutes at the most. This method may be adopted in certain cases of eruptive and other fevers, as typhoid, variola, and scarlatina.

Dr. Lambert considers that cold affusions act especially by their antipyretic effects in typhoid fever, and he chooses the word "antipyretic" instead of "antiphlogistic" in order to avoid any possible confusion. These affusions are, in fact, directly applied to the heat distributed through the system, and they moderate and diminish it in a marked manner. The augmentation of the heat in typhoid fever, when it becomes excessive, constitutes one of the worst symptoms of the disease, because it brings with it all the unfavorable train of intense nervous symptoms, of general prostration and rapid weakening, and it alters or completely abolishes the series of molecular changes and of nutrition of the organs and tissues. It is proved that the heat in typhoid fever produces these effects, for when the disease assumes a benignant type the heat is not excessive, whereas in the most serious cases the heat is very considerable. It is, therefore, principally with the object of diminishing the temperature that many authors have recommended cold affusions in typhoid fever and in the eruptive fevers; but Dr. de Lambert does not recommend this treatment indiscriminately in all such cases. When typhoid fever proceeds in a normal course, or when eruptive fevers occupy their usual period, the general tendency in them all is towards a cure, and he therefore considers that the employment of the cold affusion is especially indicated in serious, abnormal, or unusual cases. Dr. de Lambert draws certain conclusions from his researches on this subject, and they may be summarised as follows:—Cold affusions diminish the temperature from five to three degrees (Centigrade); they promote the return of full and deep respiration by exciting the activity of the muscular fibres entering into the composition of the pulmonary tissues; they promote the contraction of the capillary vessels, and thus restore the important functions of the skin; they also facilitate the phenomena of change in the muscular tissue in general; they prevent the formation of retrograde products, observed so often in different tissues, such as purulent collections, molecular waste of muscles, and fatty degeneration; they promote in general all the secretions, and render the skin supple, moist, and cool; they tranquillise the nervous excitement, and arouse the activity of the brain; they procure sleep, diminish the frequency of the pulse, and relieve cephalalgia. Dr. de Lambert then points out what he conceives to be the indications and counter-indications for the employment of the cold affusion. Among the former are an excessive temperature, severe nervous phenomena, such as delirium, *subsultus tendinum*, coma, or marked stupor; insufficient, superficial, or irregular respiration; small and rapid pulse; and a hot and very dry skin. Among the counter-indications are intestinal hæmorrhages and perforations. But the following are not counter-indications, namely, the existence of bronchitis, even of the capillary kind; nor pneumonia, if proper precautions be taken; nor diarrhœa, for the discharges have been diminished after the application of cold affusions; nor does the presence of the menstrual discharge present any obstacle to their use, although it must be noted that the menses do not usually appear during typhoid fever; nor the occurrence of epistaxis.—*Bulletin Général de Thérapeutique*, July 15 and 30, 1870.

On the Treatment of Epilepsy by the preparations of Copper and Zinc. By Dr. Auguste Voisin, Physician to the Salpêtrière.—Dr. Voisin, in this paper, relates some of the cases of epilepsy treated by the late Dr. Herpin, of Geneva, whose experience on the subject has been recorded in memoirs which have fallen into Dr. Voisin's hands. The cases have long ago been cured, and since Dr. Herpin's treatment has been frequently called in question, both during his life and since his death, Dr. Voisin thinks it his duty to publish the details of some of them, concerning whose present health he is either himself well-informed, or has received very trustworthy reports. He has selected the individuals who have presented no epileptic symptoms for ten years or more, as he thinks that such a period is indispensable before a cure can be positively affirmed. He thinks it the more necessary to publish these cases of Dr. Herpin, because in the present rage for treating epilepsy by the bromide of potassium, there is too great a tendency to condemn all other kinds of treatment as useless, and especially those which Herpin prescribed. Dr. Voisin argues, however, that clinical experience will compel every one to admit that all forms of epilepsy are not cured by the bromide, and some are not even relieved by the use of that salt, while some cases are even made worse. He has therefore employed, in some cases which have resisted the bromide, lactate of zinc, nitrate of silver, ammoniated sulphate of copper, and he has found these salts produce sometimes more satisfactory results than the bromide of potassium. Dr. Voisin then gives the details of the cases treated by Dr. Herpin, which certainly seem to confirm the opinions expressed as to the efficacy of the so-called metallic preparations, to which however henbane and mugwort were sometimes added. Besides the cases recorded, Dr. Voisin states that he has thirty more in which Dr. Herpin effected a cure, and he hopes to add them at some future time to those which have been successfully treated by himself by means of the bromide of potassium, and thus to prove the truth of Herpin's proposition, which that physician maintained all his life, namely, the curability of epilepsy.—*Bulletin Général de Thérapeutique*, March 15, 1870.

On the Therapeutical Employment of the Sodio-chloride of Gold (aurum chloratum natronatum). By Dr. Ludwig Martini, of Biberach.—Dr. Martini employed the sodio-chloride of gold in the cases of hysterical females, whether attended or not by deviations from the normal conditions of the genital organs; but he obtained the best results in those instances where the uterus manifested palpable changes, which were the sources of the hysterical affections. In several cases Dr. Martini observed that in proportion to the diminution of the local suffering the nervous phenomena improved *pari passu;* and, on the other hand, he recollects no case of hysteria complicated with local uterine disease in which the former continued after the latter had been relieved; and again, several cases of uncomplicated hysteria remained unimproved in spite of large doses of the sodio-chloride of gold. He relates the case of an hysterical female, who had been treated without success by the usual anti-hysterical remedies, and who was suffering from an enlargement of the uterus (resulting from a forceps delivery in former times), accompanied by partial induration of the os uteri. The administration of the sodio-chloride relieved the patient of the severe hysterical attacks, and its continuance seemed to diminish the volume of the enlarged uterus. Calomel was, however, given during the treatment, but was discontinued owing to the supervention of ptyalism, and the sodio-chloride of gold was then resumed, and eventually the uterus returned to its natural size. In other cases where there was a morbid degeneration of the uterus, or induration, &c., Dr. Martini obtained successful results from the use of gold, and he recommends it as a real tonic, especially in relaxation of the uterine tissues. He mentions, besides, several cases of true congenital atrophy of the vaginal portion of the uterus, which condition was the demonstrable cause of sterility. In five of such cases Dr. Martini found that, during the administration of the gold, the cervix uteri, which had been arrested in its development, increased in size and consistency, subsequently became normal, and eventually conception occurred in three instances. He found the sodio-chloride very useful also in habitual abortion and premature labour depending on induration of the womb, and in degenerations and ovarian dropsies of an aggravated character. He relates a case of cure of ovarian degeneration on the right side of a female aged fifty, after she had taken two and a half drachms of the sodio-chloride; also the cure of an enormous ovarian dropsy in a girl aged fourteen; and he used it successfully in a case of dropsy during pregnancy. The sodio-chloride of gold, on account of its styptic taste, can scarcely be given in solution or powder. Dr. Martini, therefore, recommends it to be given in the form of pills, in the dose of one tenth of a grain about an hour before meals, which should contain no acids. The number of the pills may be gradually increased till the patient takes five a day.—*Schmidt's Jahrbücher der Gesammten Medicin,* June 23, 1870.

On the Treatment of Diabetes by Arsenic. By Drs. Devergie and Foville, junr.—After alluding to the difficulty of treating diabetes with success by any methods hitherto in use, the writers of this paper adduce their reasons for believing that arsenic may be found useful in this disease, and they support their view both by clinical experience and by physiological data.

I. *Clinical facts.*—A dozen years ago M. Devergie was led by chance to apply arsenic to the treatment of diabetes, for he was attending a lady affected with a painful attack of prurigo of the vulva, for which he was prescribing arsenic. In the meantime some of the symptoms exhibited by the patient caused a suspicion that she was suffering from diabetes, and this turned out to be the case. Thence the two maladies, prurigo and diabetes, were both watched together and were both treated successfully by the same remedy, and since that time M. Devergie has employed the same treatment in diabetes and with the same result, even when it was not accompanied by prurigo. Dr. Foville, the elder, employed arsenic in 1857 in the case of a diabetic patient, and the proportion of sugar in the urine was notably diminished by the treatment; and encouraged by this result he afterwards treated a great number of cases in the same manner, and very often with great success. Dr. Foville, the younger, having observed the effects of arsenic in his father's diabetic patients and in some analogous cases, wrote in 1868 that arsenic is one of the best agents for arresting the symptoms of diabetes, and diminishing considerably, if not altogether banishing the presence of sugar in the urine. Other practitioners in France have also reported favorably of this mode of treatment, although it is not pretended that the results have been invariably favorable. The mode of administration recommended by Drs. Devergie and Foville is Fowler's solution, beginning with 1 drop daily, and gradually augmenting the dose to 3 and 4 and even to 12 or 14 drops a-day.

II. *Physiological considerations.*—Taking as their basis the physiological experiments of Claude Bernard on the glycogenic function of the liver, and supposing that diabetes is caused by a paralysis of this function, Drs. Devergie and Foville argue that the indication to be fulfilled in the treatment of diabetes ought to consist in opposing the vascular paralysis and in awakening the energy of the vaso-motor filaments of the great sympathetic, or in a word in regulating the capillary circulation of the liver. This indication, they say, arsenic appears especially adapted to fulfil, for it is by its action on the great sympathetic and especially on the vaso-motor nerves that some authors explain its efficacy in intermittent fevers, exophthalmic goitre, &c., and that they themselves (Drs. D. and F.) consider it very efficacious in certain congestive conditions of the encephalon.

Drs. Devergie and Foville, admit, however, that the pathology of diabetes is still involved in some obscurity, although they believe that Claude Bernard's theory is the most plausible, and that it supports their views as to the good effects of arsenic in this disease. —*Bulletin Général de Thérapeutique*, June 30, 1870.

On some Peculiarities in the Action of Bromide of Potassium.—Dr. J. Lockhart Clarke relates a case in which some very extraordinary effects were produced from two half drachm doses of the bromide of potassium. The patient called on Dr. Clarke the morning after he had taken them, and surprised that gentleman by the peculiar expression of his countenance. He was perfectly rational, but declared himself wholly unfit for any kind of occupation. At Dr. Clarke's request he wrote an account of the feelings he experienced, and the following abstract describes some of the more remarkable of them. It appears that the patient took both the doses in one day, and in the evening, although he drank nothing after tea except a little beer, he seemed to his friends as if he was intoxicated. The next morning he found that he could recollect nothing of what had happened the night before, and forgot even the day of the week or the month of the year. To his surprise he found in his pocket a note he had himself written on the preceding night, and which contained a short account of his feelings. He then called on Dr. Clarke, and subsequently went down to his office, but found his head so confused that he was obliged to leave his duties, and after calling on some friends, who were astonished at his altered appearance, he went with them to the Crystal Palace, and after dining there the anomalous sensations disappeared entirely. Dr. Clarke states that the patient was not epileptic. In another case to which Dr. Clarke refers, and in which the narrative is also given by the patient himself, some very similar phenomena are recorded. The patient was the subject of the *petit mal*, and was taking the bromide in large daily doses, the amount being eventually raised to eighty grains in the twenty-four hours. The symptoms first noticed were deep but disturbed sleep and confusion of mind on waking. The mental powers were enfeebled, so that it was difficult for him to prepare and balance an ordinary account, and there was a tendency, both in speaking and writing, to mistake one word for another. He wrote for instance "contraction" for "subscription." On leaving off the medicine, all these anomalous symptoms disappeared, and as long as the dose of the bromide did not exceed forty grains a day, he was able afterwards to take it safely, but when the slightest indication presented itself of mental confusion he reduced the dose. It should be mentioned that the *petit mal* was arrested by the use of the bromide.—*British Medical Journal.*

On the Treatment of Syphilis by the Hypodermic Injection of the Salts of Mercury. By Dr. T. J. Walker, Surgeon to the Peterborough Infirmary.—Dr. Walker, in spite of the opinion often expressed against the use of mercury in syphilis, believes in its efficacy in the treatment of that disease, but he thinks that the plan of giving it till the gums are touched is an erroneous practice. On the contrary, he considers salivation as an untoward accident in the mercurial treatment, and one which ought to be avoided if possible, and he himself adopts the practice of Professor Sigmund, of Vienna, in preventing salivation by diligent attention to the cleanliness of the mouth and the constant use of alum. Assuming, then, that mercury is a proper remedy for syphilis, Dr. Walker proceeds to show that the method of administering the drug in the smallest quantity, and with the fewest complications, ought to be preferred. Hitherto he has employed the mercurial inunction, guarded by alum, as above mentioned; but he now suggests the use of the hypodermic injection of corrosive sublimate, as recommended by Dr. Lewin, of Berlin. The preparation which Dr. Walker has used is a solution of 5 grains of the sublimate in 250 minims of water with 250 minims of glycerine. The mixture is of such a strength that 100 minims contain 1 grain of the sublimate, and of this he injects 10 minims, or $\frac{1}{10}$th of a grain of the salt. The injection almost always causes some pain and irritation, which, however, are only transient, the seat of the injection being usually over the abdomen, or into the areolar tissue of the arms or thigh.—*British Medical Journal.*

On the Use of Opium in Diabetes, and on the Use of Carbonate of Ammonia in large Doses, in the same Disease. By Dr. F. W. Pavy.—Dr. Pavy knows no medical agent that is capable of exerting such a controlling influence over diabetes as opium. He has employed alkalies and ammonia largely, and thinks that they produce a slowly beneficial action in the complaint, but with opium the effect is so striking, that there can be no doubt of its reality. In three cases treated by Dr. Pavy the sugar was removed, in two of them by opium, and in one by morphia. In these cases no other medicine was administered. They were instances of the disease in middle-aged subjects. The sugar was reduced as far as it could be by a restricted diet, and then, under the influence of the morphia and opium, it was entirely removed. In another case, in Guy's Hospital, Dr. Pavy administered ozonic ether without beneficial effect; but, under the use of morphia, the quantity of urine and sugar declined from day to day, and in about three weeks the sugar disappeared altogether. When the morphia was discontinued the sugar reappeared, but when opium was afterwards administered the sugar again disappeared. Carbonate of ammonia was employed in another case by Dr. Pavy, and it certainly seemed to exert a controlling influence over the disease, but such an immediate and decided an effect was not produced as with opium. The patient was ordered to take 100 grains of carbonate of ammonia in the twenty-four hours; the salt was dissolved in a pint of water, and administered in small and frequently repeated portions. Under this treatment, accompanied by a restricted diet, the sugar disappeared, but it sometimes reappeared.—*British Medical Journal.*

CHRONICLE OF MICROLOGY.

By J. F. Streatfeild, F.R.C.S.

Part I.—Physiological Micrology.

On the Question of the Connective Substance of the Cortical Cerebral Matter, by Dr. M. Roth.—If one endeavours to isolate the vascular twigs from the cortical matter of the brain of a calf, which has been placed some days in a very much diluted solution of chromate of potash, there remain considerable fragments of brain substance imprisoned with them, partly larger flakes of the *spongiosa*, partly pretty regularly scattered over the whole surface, mostly the finest filaments standing perpendicularly to the vessel's axis. The latter originate from funnel-shaped thickenings of the vessel's wall, are very pale, finely granular, and may measure over 0·03 mm. Sometimes they divide outwards, rarely do they unite.

In the ordinary preparations in chromic acid and alcohol one finds the statement of His corroborated, that the spaces to be seen around the vessels of the central nervous system are lymphatic spaces that may be injected, and exhibit tubes devoid of independent walling, in which the blood-vessels centrally or excentrically float freely.

Roth having removed with a razor small pieces, at most 2 or 3 mm. thick, from the convolutions next the Rolando furrow, put them for half-an-hour to two hours in super-osmic acid of 0·2—0·5 per cent., then sometimes for a few hours in 40—80 per cent. of absolute alcohol, lastly in oil of lavender. The separation of the parts was effected by solution in paraffin. The sections were cleaned with chloroform and examined in Canada balsam.

In cross-sections of the medium sized arteries of 0·03 or 0·04 mm. diameter, there appeared the surrounding lymphatic space from 5 to 10 and more extremely delicate interwoven radiate fibrils, which springing from various points of the periphery, became attached to the coats of the blood-vessels. The fibrils which were rectilineal or somewhat bent to one side, remained single, or entered into scanty communications with their neighbours, or subdivided forkedly in proximity to the blood-vessels. Both offshoots were for the most part expanded in a rather funnel-like way. The length of the fibrils differed more or less, according as the blood-vessel concerned was lying centrally or excentrically. Nowhere was there a membranous or endothelial covering of the lymphatic spaces; the spongiosa throughout appearing with simple flat contour upon the hollow space. By some modifications of the magnifying power in use there very plainly came into view a condensation zone (His) as a dull bright halo around the lymphatic space—limited by compacter grouping of the granules or by more closeness of the meshwork. Concentric streaking of the halo originated not unfrequently in this, that the meshes are extended somewhat parallely to the contour of the hollow space. Towards without there appeared transition into the common spongiosa without clearly defined limitation. In the latter besides capillary vessels the familiar nucleated vesicles were embedded.

In all parts of the cortical cerebral matter, and by cross-section of the vascular system altogether, the radiate fibres of the lymphatic spaces appeared. In the human brain Roth never saw any combination of the radiate fibres within the lymphatic spaces, with nuclei or cells; whereas many times a homogeneous rigid fibre of the character of a so-called axis-cylinder group traversed the lumen of a small lymphatic space.

Also in the longitudinal and oblique branching blood-vessels the radiate fibres are easily to be discovered, especially in vessels with adventitia standing afar off.

Roth interprets the perivascular radiate fibres as a very fine lymphatic network, in which the vessels are suspended; the morphological elements of the lymph proceed by this with no sort of difficulty, contrary to the repeatedly quoted assertion of the clearly nervous nature of the brain cortex, the results of Roth confirm, that also near the blood-vessels a certain portion of genuine connective substance is present in it.

The epicerebral lymphatic spaces are also near the exhausted blood-vessels traversed by numerous vertical fibres (0·006—0·008 mm. in length). The fine non-nuclated fibres proceed from the undermost parallel-fibred layer of pia and advance with the superficial net-work of the brain cortex. Where vessels penetrate into the brain these pia processes are accompanied by similar processes of the adventitia, which within the lymphatic space more or less obliquely, pass over horizontally into the spongiosa after the entrance of the vessel into the brain. One gets the best sections in brains with very delicate pia.

The same parts of the calf's brain furnished, with osmium, on the whole, similar results. However the condensation zone around the lymphatic spaces is less pronounced, the radiate fibres more compact, here and there, also, supplied with stellate nucleated cells. The nuclei of the stellate elements are in nothing distinguished by many granules (flattened, round, smooth, homogeneous) interspersed in the spongiosa.—*Schmidt's Jahrbücher*, vol. cxliv, p. 20.

Distal Communication of Blood-vessels and Lymphatics, and on a Diaplasmatic Vascular System.—Dr. T. A. Carter recommends an injection of carmine precipitated in gelatine from its ammoniacal solution by acetic acid, for its brilliant colour, generally *without risk of staining the textures with which it is in contact*, besides its valuable penetrating qualities. When the portal and hepatic systems of the liver have been injected and the pigments have distended the superficial lymphatic network, it is rightly supposed that some arterial or venous twig has become ruptured, *when finely divided* *mineral matters*, such as vermilion or chromate of lead, have been used. For they attach themselves to the parietes of the vessels and finally choke them. Since 1863 the author has been collecting corroborative evidence of his formerly published observations and additional facts. He affirms that direct communication exists between the lymph vessels and those of the blood at their distal as well as at their proximal extremities; and in the former position through tubes of dimensions so small as to preclude the possibility of the blood corpuscles entering them (admitting "filtered blood" only). He discovered, also, another system of vessels not connected apparently with the lymphatics, but commencing and terminating in the capillaries, and resembling those above mentioned in being so minute as to admit of the passage only of the *liquor sanguinis.*—*Journal of Anatomy and Physiology*, pp. 97—118, Nov. 1869.

Optical Peculiarities of Streaked Muscular Fibre.—The *singly refracting*, in ordinary illumination *darker streaks*, correspond to Krause's cross lines, and Hensen's central disk. The *bright bands* lying at the side of them, which, by an altered position of the reflector, may vary in their breadth and vanish altogether, Heppner declares to be *the expression for total light-reflexion*, in that they disappear when the angle of incidence of the light excludes them, and return and increase in breadth with an increase of the angle of incidence. Then when a substance of less refraction's index (here the darker streaks) is on each side bordered by a substance of greater refraction's index, so at the limit of both substances, rays of light coming here from below with a sufficiently large angle of incidence, become totally reflected. Therefore on either side of the substance of the less index of refraction there is situated a zone of the substance of greater index of refraction, the brighter is as the remaining portion of it; then this zone becomes many times illuminated, to wit, by the rays simply refracted by itself, by the two substances at the border totally reflected, and by the rays refracted there towards it from the weaker refracting substance. The disappearing and the changing breadth of the brighter zone is connected with the change in the direction of the rays of light. If the difference of the indices of refraction of the two muscular substances were brought about by a reagent, *e.g.*, alcohol, which infiltrated uniformly the muscular fibre, then the appearance is seen to vary. The brighter bands vanish entirely, or pale very considerably. They are only present as broad feebly-bright zones between dark narrow finely-granular (?) streaks. The refraction's indices of the two substances are to that of alcohol there changed; their difference is consequently diminished, wherefore the total reflexion is not more marked in the earlier stage. The differences of brightness in the doubly-refracting parts, which are also found in polarized light, may be explained likewise by the total reflexion.—*Schmidt's Jahrbücher*, vol. cxliv, p. 7.

Protoplasma-bodies in the Embryonic Liver.—Dr. S. L. Schenk, of the Vienna university, says that "being occupied in the history of the development of the liver, his attention was called to cell-formations in the mammalian (rabbit's, pig's, dog's) embryonic liver, which, in the first place, by their size, were distinguished from the surrounding elementary organic structures. Whilst most of the elementary organic structures which go to make up the embryonic liver are of the size of 0·01 mm. in diameter, one finds the cell-formation in question of from 0·02 to 0·04 mm. In sections of the livers hardened in spirits of wine or diluted chromic acid, their appearance is very diverse. They are roundish, oval, furnished sometimes with numerous processes, which are roundish or pointed. The protoplasm of these structures is granular, having one or many nuclei, which not unfrequently appear polygonal, particularly when in the same protoplasm several are included, which, as far as possible, proceeds from mutual flattening of the walls of the nucleus. If one seeks such structures (by addition of iodine-serum) in fresh embryonic livers (rabbits', 2 or 3 centimetres long), they always seem to abound.

"In the fresh state they are symmetrically rounded, very often without a nucleus, which latter also, in some, is visible in the fresh state.

"If one puts these structures into a 1 per cent. solution of common salt, or into iodine-serum, they then, by elevated temperature (28° to 32° C.) exhibit signs of movements, which consist of this, that they send out and draw in roundish knob-like processes. During the projection, the round processes seem to be engaged in continual changes of shape, with which also a local change is an accompaniment.

"Here he would remark that the structures he has met with in the embryonic liver somewhat resemble some of Peremeschko's protoplasma-bodies referred to. But whether the nuclei in the protoplasma-corpuscles in the protoplasma of the liver—as in those of the spleen—first come to light at death, or whether these structures receive the nuclei by their movements, he can not yet determine."—*Centralblatt für die Medicinischen Wissenschaften*, 11th December, 1869, p. 865.

Improved Valentin's Knife.—The new double knife invented by Mr. W. R. Gowers, of University College Hospital, and made by Hawksley, of Blenheim-street, Bond-street, ensures accuracy, in the parallelism and self-adjusting of the two blades, at all distances, by means of equal springs, and also two pins projecting from one blade and passing through holes in the other. The springs and pins are attached to that part of the blades which is next the handle, and are equidistant on either side of *a single screw*, by which alone the two blades are connected. The broad head of this screw is graduated so as to show the distance to which the blades are apart at any time.

Part II.—Pathological Micrology.

Granular Cells in softening of Nerve-tissue.— W. Koster, of Utrecht, in a first paper, relates a case supposed to be of apoplexy of the spinal cord. The patient died, and the author "found a condition of the inferior part of the spinal cord—not to be expected"—merely atrophy of the *cauda equina*, and of the lower third of the spinal cord, without any externally visible change. No effused blood or any *sequelæ* were found. There were in the fresh spinal marrow, the usual constituents which occur in "softening"—granular cells, attenuated and thickened nerve-fibres, and much granular mass. And also, brownish or yellowish, irregularly scattered lumps and very small crystals of hæmatoidine. Using chromic acid, in longitudinal and transverse sections, in the most degenerated places, the nerve-tissue had in good part disappeared, and was replaced by the well-known thick filaments of connective tissue. Elsewhere, in transverse sections, granular degeneration seemed to have begun or further advanced. In the former this granular metamorphosis appeared distinctly to commence around the axis cylinder. In many places, the vessels of the pia mater were loaded by natural injection. And many capillaries, congested with blood, were perceptible in the atrophied spinal cord. Moreover, in almost every preparation, among the granular mass and the granular cells, lumps of brown or yellowish pigment of irregular form were met with, and only here and there rhombic crystals were found. The ganglionic cells of the grey substance were often tolerably normal. Other times they were large granular heaps. The author ascribes particular importance to the irregularly scattered masses of pigment and the small crystals of hæmatoidine. He believes that *in apoplexy of the medulla spinalis hæmorrhage, in the ordinary sense of the word, seldom or never takes place, but only intense hyperæmia, with capillary hæmorrhage or merely transudation of blood-cells.*

In a second paper, the author investigates the origin of the above granular cells, concerning which the examination of the above case led him to a definite conclusion. They are well known, large, and constitute, in great part, the softened nerve mass. In some transverse sections, he says, in addition to normal fasciculi of nerve-fibres, I met with others where the granular metamorphosis had apparently just begun, and where the first accumulation appeared around the axis cylinder. At the same time nerve-fibres in a state of more advanced degeneration were visible, quite dark and granular; while free granular cells were not wanting, and here and there, as it were, appeared projecting from the transverse sections of the nerve-fibres. In such spots the neuroglia had undergone no appreciable change. The nerve-fibres lay still close to one another, as in a normal transverse section. Only in the grey substance were further changes met with, as has already been stated in the preceding paper; dilated blood-vessels, pigmentary and granular change of the ganglionic cells.

In longitudinal sections the granular contents of the nerve-fibres were visible, sometimes still as a continuous column, but sometimes also distinctly divided into groups, though as yet comprised within the limit of the nerve-fibres. When the degeneration was further advanced, where the boundaries of the nerve-fibres were scarcely any longer recognisable and the interstitial connective tissue was greatly thickened, the granular groups (granular cells) were still often seen lying regularly in rows, quite in the direction of the nerve-fibres.

For this investigation into the origin of the granular cells, the ordinary method of making the spinal marrow transparent by means of turpentine or creosote, is not advisable. Especially in very thin sections the proper boundaries of the granular columns or groups vanish, and the whole section appears uniformly granular. I attained my object best by examining very thin sections with weak soda ley and glycerine, or with solution of chloride of calcium, or by allowing somewhat thicker sections to become only partially transparent in creosote. Opaque spots then remain, in which the granular degeneration is still well seen.

"I do not wish to assert," the author concludes, "that the origin of the granular cells positively demonstrated by me, in the spinal cord, from the contents of the nerve-fibres (and probably also from ganglionic cells), proves that a different course of things can now here take place; but I may even now provisionally say, that an accurate study of the morbid processes in the nervous system will probably everywhere exhibit these granular cells as products of degeneration of the nerve elements. I should in that case, prefer to call them granular heaps or groups. *Dublin Quarterly Journal of Medical Science.*—November, 1869, pp. 669-75.

On the Tissue Changes in the Inflamed Liver.—A. v. Hüttenbrenner, in opposition to the assertion of Joseph,—that a granular degeneration of the liver-cells begins about a needle thrust into the parenchyma of the liver, writes in confirmation of the earlier statements of Holm. He also found in the neighbourhood of the pin a concentric disposition of spindle-shaped elements, and indeed, even after twelve hours, altogether in such manner as after one or more days. The proof of this state in the extirpated liver, which, after the introduction of a needle, at once had been placed in chromic acid, justifies the supposition that those spindle-like forms in layers are liver-cells, and shows at the same time that the change described is essentially brought about by mechanical subdivision. Accordingly one misses also the spindle-shaped layering around the needle in cases in which, in the penetration, accidentally the connective tissue has been met with.

In acute suppuration, which has been brought about by irritation of the surface of the liver-section by means of ammonia, one finds in the neighbourhood, as also in the interior of the individual lobules, massive cells appearing like pus-corpuscles placed around the vessels as in a circle. Then in the inflamed liver, both between the liver cells and within them, even indeed in their nuclei, one finds vermilion, so the amœboid cells containing vermilion and the vermilion test generally will prove nothing here, and one may thence conclude it to have originated only in the local arrangement.

In conclusion the author yet briefly describes the relations of the liver-cells, and of the fibrous tissue in abscesses of the liver, cirrhotic thickenings and syphilitic nodes. In all these one perceives, in the ground layer by the side of each of the liver-cells in succession, at last simple fibrous arrangements, intermediate stages, which point out a transformation from the former to the latter.—*Centralblatt für die Medicinischen Wissenschaften*, 4th September, 1869, p. 631.

Cirrhosis of the Liver.—Dr. Cryan exhibited (Dublin Pathological Society) a specimen of this disease and other complications. He considered it a well-marked instance of true or fibrous cirrhosis. Microscopic examination showed that many of the hepatic cells constituting the granulations were surrounded by dark pigment granules; a few of the cells were loaded with oil, and many were destroyed, and their place occupied by little masses of dark-brown pigment, mixed with molecules of fatty matter; the fibrillated connective tissue was absolutely—not merely relatively—increased, and was traversed by minute bile-ducts and blood-vessels; many of the capillaries of the vena portæ were compressed and destroyed, but the trunk and primary branches were quite pervious.—*Dublin Quarterly Journal of Medical Science*, 1 November, 1869, p. 651.

On the Origin and Increase of Bacteria.—Dr. A. Polotebnow has been led to the following conclusions:—

1. That a perfect genetic connection exists between *Bacterium*, *Vibrio*, and *Spirillum*, and that these organisms present no other differences but those of size and direction.

2. None of the vibriones (*Vibrio*, *Bacterium*, and *Spirillum*), are independent organisms, but only derivations (delicate mycelia) from the spores of fungi, especially those of *Penicillium glaucum*.

3. The development of the vibriones from the spores of *Penicillium* may be best followed when the spores are exposed to the action of a high temperature (140° to 212° F.).

4. The notion that vibriones are developed in the filaments of mycelium from the granules occurring in the cells proves to be quite erroneous, as also that of the conversion of vibriones into other higher forms (yeast, &c.).—*Anzeiger der k. k. Akad. der Wiss. in Wien*, 29th April, 1869, pp. 87-88.

Pathology of the Vitreous Body.—Dr. Hermann Pagenstecher, of Wiesbaden, writes: "Within a short time I have communicated the results of a large series of experiments, which I conducted in the spring of the present year during my stay in Marburg. The design thereof was to follow up, ophthalmoscopically, the changes in their evolution which were brought about by the most diverse traumatic interferences, in the vitreous of the rabbit, and at the same time microscopically to inquire into the various stages. Especially for the clearing up of the question of an independent inflammation of the vitreous body would be put at the head as a leading principle by these investigations, by an interference as small as possible with the surrounding membranes, a stirring up as considerable as might be was by it brought about in the midst of the vitreous itself. Moreover, injections into the vitreous body of irritating substances in solution prove to me, after very many trials, inconclusive. The method of which I availed myself was briefly the following:

"An exceedingly sharp, fine cannula of a Pravaz syringe, previously charged with some irritating agency (wire, pieces of wood and glass, &c.) was then thrust as far as into the centre of the vitreous body, and its contents pushed out by means of a fine wire. In order ophthalmoscopically to observe the behaviour of the vitreous body in presence of a powerful irritating agency and afterwards to investigate microscopically, without any change in their relative position, the alterations thereby superinduced, I modified my proceedings in the following manner:

"A fine lymphatic tube was almost entirely filled with croton oil, and, lest the contents should be emptied in its introduction, stopped with wax at its upper end; then, by means of the canula, as above described, it was inserted into the interior of the vitreous body. The changes brought about by these experiments lead to the conclusions which certainly are very contrary to the views now prevailing as to the nature of the vitreous body. They are the following:

"1. Neither the gelatinous substance of the vitreous nor the elements contained in it are able, in consequence of the influence of the irritation, which in other tissues produce the phenomena of inflammation, to produce lymphoid corpuscles by morphological changes. Hence it follows—

"2. That these must enter from the neighbouring organs.

"3. The vitreous body appears to behave quite indifferently, even towards very violent irritations or more precisely expressed, it is not by them induced to call forth an accumulation of lymphoid corpuscles at the part influenced by the irritation—a phenomenon which we in all organs susceptible of inflammation see occur, even in shorter time, in consequence of the effect of similar irritation.

"4. The vitreous body cannot, therefore, be considered as capable of inflammation, as we understand it of every other organ; but every so-called inflammation affecting it can be only looked upon as secondary in consequence of the changes brought about in the neighbouring organs: also an irritation existing in the vitreous body only thereby can lead to inflammatory manifestations in, that has an influence exciting inflammation on, the vascular surrounding parts of the vitreous body. Simultaneously with these experiments I had abundant occasion to investigate extravasations of blood and their changes in the vitreous body. In this I am able to corroborate the opinion adduced by Langhaus on the blood extravasations in general, namely, that the red blood-corpuscles are taken up by the contractile round cells, whilst it happened to me directly to observe this event in a fresh preparation of the vitreous of a rabbit.

"The more accurate proof of the view above expressed, together with the experiments belonging thereto, such as the fuller description of the last mentioned event, shall appear forthwith in a larger work."—*Centralblatt für die Medicinischen Wissenschaften*, 25th September, 1869, pp. 676-7.

Parasitic Sycosis.—Its existence as a form of the disease was demonstrated in Paris, in 1861, by H. Köbner. The parasitic formation is one and the same in Herpes tonsurans and in the parasitic sycosis. The two diseases may simultaneously exist in the same patient. P. Michelson now describes a confirmatory case investigated by him at Tübingen. Examined microscopically, in glycerine, there were found in the diseased hairs :—

1. Clearly defined, little branching threads of 0·004 mm. in breadth, which mount from the root of the hair to the under part of its shaft, and in its interior show four-cornered corpuscles of 0·002—0·003 mm. in size square, which by transmitted light appear dark. Sometimes the thread vanishes superficially, the four-cornered corpuscles then lie free in the same disposition, but are somewhat larger, and contain a distinct nucleus.

2. Between the threads in the radical part of the hair, as in the hair follicle—roundish corpuscles (conidia) of 0·006 mm. in size—with distinct nuclei of 0·002 mm. in size.

3. Strongly reflecting, roundish or four-cornered, distinctly defined bodies exhibiting homogeneous contents, often in a series as to 5-10 (gonidia series).

4. Very thin (0·0009 mm. broad) short fibres sometimes cleft as a fork, in their interior fine little dots.—*Centralblatt für die Medicinischen Wissenschaften*, 9th October, 1869, p. 718.

Leucocythemia.—In the Parisian Hospitals' Medical Society there has been an anatomical pathological discussion, originated by a case related to the society by M. Bourdon—a woman, æt. 58, who had had insufficient food and bad lodging. Her blood showed very many white corpuscles, sixty perhaps, in the field—twenty times the normal number. The patient had had but some slight epistaxis, and at last a simple purpura of the lower limbs. She died of thoracic accidental causes. The spleen was of enormous size; microscopically there were found a rather considerable hypertrophy of the fibrous framework and Malpighian granules, and great quantity of white globules. These alone almost made up the infarctus below mentioned, for the red corpuscles in them were very few. The liver was only less hypertrophied. The capillaries were distended by corpuscles, almost all white, compacted, the red corpuscles being very rare. The glands generally were immensely developed, some as large as a pigeon's egg. On their surface were blood-spots, and in their section some small infarctus; centrally, especially, were found great quantities of white globules, with no trace of any framework, and here and there, superficially, vessels filled with globules, which, by their reddish colouring, encroach upon a pale-yellow foundation, corresponding with the central part of the organ. The hypertrophied heart was considerably overloaded with fat about the right ventricle. Its cavity was occupied by a yellowish, friable clot, prolonged into the pulmonary artery. It was made up almost exclusively of white globules, with a very few dispersed red globules and a small proportion of fibrine. Similar clots filled the left ventricle, the aorta, &c. The author remarks on the very characteristic features of this case.

M. Dumontpallier was of opinion that these clots must be in great part composed of fibrine; and an examination of them had also confirmed him in this opinion. Preparations made at once simply by scraping with the scalpel the incised surface of the clot immediately led him to believe that they were almost exclusively composed of white globules of molecular fibrin and fatty matter, and that they did not contain fibrillary fibrin; but, by removing fine slices of the clot by incisions made on the level surface he became convinced that the white globules were surrounded by fibrillary fibrine as a framework, in which the globules remained accumulated.

M. Peter said that, according to his examination, these clots were almost exclusively made up of blood-globules, the red ones being much the least numerous and deprived of colour, and of a very scanty amount of fibrine.

M. Isambert remarked that the proportionate quantity of the molecular state of the fibrin in bones of cythemic subjects was a point undecided hitherto. There is found, besides the abnormal proportion of white globules a lessened quantity of fibrine, and such a change in its molecular state, that it will hardly coagulate by beating it in long elastic filaments; but, instead, it falls to the bottom of the vessel in infinitely small lumps, in which alone the microscope showed the fibrillary structure of the fibrin. M. Isambert then inquired if, in the instance now in question, the fatty elements were not found in considerable proportion; and this being affirmatively answered, he said that their predominance seems to be a necessary condition to the formation of white clot.

Another case of the same nature was, at a subsequent sitting, brought before the same society by M. Desnos. The spleen was found to be enormously hypertrophied, and in the left ventricle of the heart there was, besides some dark blood, a whitish clot of weak consistence, prolonged some distance into the arch of the aorta. These were examined by M. Hayem. He said that the splenic tissue, besides hypertrophied Malpighian corpuscles, showed cells and free nuclei and minute crystals, octahedral, elongated, colourless, of 0·04 to 0·0026 mm. diameter, soluble in acetic acid. Of the clots the white part was, he said, composed of a considerable quantity of fibrin, partly granular or amorphous, partly fibrillary, involving a number of nuclear and cellular elements analogous to those of the spleen.—*Archives Générales de Médecine*, Oct., 1868, pp. 503-7.

Lymph of Vaccinia and Variola.—Dr. F. Keber has found in the vaccinal lymph, besides the elements of the epidermis, of pus and blood, special cellular productions having the following characteristics:—there are granular cells measuring 1-150th to 1-300th of a line; free nuclei of 1-800th to 1-3000th of a line; as well as punctiform molecules. These elements, more or less numerous, are never missing. The cellular elements show, especially by addition of water, an enveloping membrane. Acetic acid makes the membrane transparent, and, on the other hand, makes the granulations more distinct; the latter are of from 3 to 20 in the cells. They must not be confounded with pus corpuscles, and they show different appearances, proving thereby an active cellular process, namely, multiplication by scission. These elements again are found in the vaccinal pustules from the fourth or fifth day. Even from the time when the lymph of the vaccinal pustules has been filtrated one may observe them. Finally, in the dried vaccinal lymph one may point out specially the molecular granulations. These elements should be distinguished from different productions seen in vaccinal lymph, such as crystals, needles, the tufts due to the crystallization of urates, and the vegetations which are found in changed vaccinal lymph. These formations, moreover, exist in the exudations, variolic pustules, and even in the scabs. Besides all this, in the pustules of varicella, the author has observed analogous, if not identical, productions.—*Journal de Médecine*, Aug., 1868, p. 135.

Syphilitic Induration.—In consideration of that which is taught on this subject, E. Verson has been examining a recently excised syphilitic induration. He found the interstices filled with a basis connective tissue of roundish cells. In other places the cells were indented—spindlelike or furnished with processes—here and there also was found, instead of the network of connective tissue, a reticulation with nuclei at the knot-points. The vessels were compressed; the adventitia filled with cells. In one preparation the author thought he had convinced himself that a nerve epithelial cell had taken to itself two round exudation cells, or, as the author said, had devoured them. The nerve sheaths were thickened.—*Virch. Arch.*, xlv, 117-8.

Acute Atrophy of the Liver and Phosphorus Poisoning.—Dr. Bollinger, in the present work, describes two cases of phosphorus poisoning, and two others of acute yellow atrophy of the liver, according to the results exhibited in their investigation, both macro- and microscopically, and gives, thereupon, a summary of these diseases, and especially of the opinions hitherto brought forward of the origin of the so generally concomitant icterus. Contrary to numerous assertions the author notes the absence of the over-growth of small cells of the interstitial tissue of the liver. In regard to the icterus the author declares for himself that it is in both diseases a *resorption-icterus*, and certainly not, as Virchow has in most cases made it to be received, as being set up by swelling of the mucous membrane at the outlet of the ductus choledochus, but by parenchymatous inflammatory fatty degeneration of the liver cells, and throwing off of the fatty degenerated epithelium of the fine biliary canaliculi. The theory of the hæmatogenous icterus appears very doubtful, as in the liver the effect was always sufficient to demonstrate the origin of the icterus.—*Centralblatt für die Medicinischen Wissenschaften*, 3rd April, 1869, p. 246.

Development of Epithelial Cells in Chronic Skin Diseases and in Epithelial Carcinoma.—F. Pagenstecher has, especially in preparations which, taken from the living and (as much as possible even whilst warm) hardened in very weak chromic acid solution, found spindle-shaped cells, which he identified with the "wandering cells," in greater abundance, as Biesiadecki had before described them in the normal skin where an increased epithelial development had preceded (granulating cicatrization surfaces, psoriasis, chronic eczema, in the parts around an epithelial carcinoma). Whether they have their origin in the cells of the connective tissue, or emigrate from out of the vessels, he leaves uncertain, but he is inclined to the latter view. Pagenstecher supposes that the epithelium does not originate in a direct multiplication of the epithelial cells, but from these wandering cells, which by their entry into the region of the epithelial formations become in some measure infected by the latter, and so changed into epithelial cells. He has throughout, where increase of the epithelial cells appears, met with those wandering cells in greater number; there he now has not been able to prove, that these wandering cells disappear, that they further are transformed into other tissue elements, for instance, connective-tissue cells, and there finally appeared to him every origin of the epithelial cells, otherwise inexplicable, so he believes the acceptation of their origin from the above-named wandering cells so much the more authenticated as it has appeared to him comformably to his statement, by means of Hartnack's immersion lens, to have observed the positive indication of the transformation forms between the wandering and epithelial cells (accumulation growth of the wandering cells, both in relation to the nucleus and the protoplasma, clearing of the latter, and appearances of the nucleus, previously, with a power of 450 diameters, invisible, and finally rounding of outline.)—*Ibid.*, Jan., 1869, p. 59.

REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

By Francis C. Webb, M.D., F.L.S.,

Member of the Royal College of Physicians, Physician to the Great Northern Hospital.

The Origin of Pus-globules.—M. Vulpian has recently communicated to the Academy of Medicine the result of observations and experiments conducted by himself and Dr. Hayem with a view to elucidate the question of the origin of pus-globules. M. Vulpian had previously presented to the Academy a note by Dr. Hayem on the mechanism of suppuration, which confirmed generally the facts published by Cohnheim. If pus-globules are nothing else than the leucocytes of extravasated blood, it must be possible, in all cases of suppuration in the human subject, where the necessary observation can be made, to establish the different phases of the emigration of the leucocytes. Vulpian has confirmed the observations made by Volkmann and Studener on erysipelas, and he stated that his observations on skin irritated by vesicatories had conducted him to conclusions in every respect similar to those of Waller and Cohnheim. The following are the new facts observed by himself and Dr. Hayem. *Pericarditis.*—Hayem, when examining the false membranes uniting the two layers of the pericardium in a case in which there was adhesion at the level of an aneurism of the ventricular wall, has seen in the false membranes globules of pus accumulated round the vessels. These latter were filled with white and red globules. *Encephalitis.*—In spots of encephalitis produced by experimental irritation in rabbits, guinea-pigs, and dogs, he has found the vessels filled with red globules, whilst the perivascular sheaths were distended with a greater or less quantity of white globules. *Myositis.*—He has also observed leucocytes along the veins in the neighbourhood of small abscesses formed in the muscle after typhoid fever; a similar observation has been made in the case of muscles artificially inflamed in dogs and guinea-pigs. *Anal fistula.*—Lastly, Hayem has examined incisions made in the wall of a fistula in ano, and has found beneath a somewhat thick epithelial layer irregular papillæ, containing vessels surrounded with a large number of white globules. The connective tissue in the neighbourhood was rich in anastomosed spaces, which contained bodies analogous to those which surrounded the vessels. Vulpian has examined a variety of suppurating tissues, but principally the mucous membranes and skin. In every case in which it has been possible to make a sufficiently distinct preparation he has met with a similar disposition of leucocytes to that observed by Hayem. In a case of subacute cystitis in a dog following transverse section of the spinal cord, a mass of leucocytes was observed in the immediate neighbourhood of vessels filled with red globules, intermixed with a small number of white. In a case of erysipelas of the face, accompanied by erysipelas of the mucous membrane of the nasal fossæ, this mucous membrane contained a large number of leucocytes, of which some were disseminated, but the larger number were collected around the veins. These vessels—and this disposition was also seen in the skin—contained a large number of leucocytes, and some were also found in the thickness of the vascular walls. In a case of inflammation of the membrane lining the frontal sinuses in a syphilitic patient vessels were seen enclosing a large number of leucocytes in the midst of red globules, but the mass of leucocytes intercalated in the intervascular spaces was so great that it was not possible to distinguish if there were any relation of distribution between the situation of the vessels and the accumulation of the white globules. The bronchial mucous membrane also, in cases of chronic bronchitis, exhibited leucocytes in large numbers in the neighbourhood of the superficial vessels. and the latter contained a large number of leucocytes. Similar appearances were presented by sections taken from the edges of a suppurating sore in a dog. Lastly, as regards the skin, in addition to the observations before noticed, M. Vulpian directs attention to the conditions produced by croton oil and the variolous poison. His observations tend to show that the pus of smallpox-pustules is constituted, as far as its leucocytes are concerned, by the white globules of the blood extravasated, which have made their way across the dermic tissue from the outer surface of the vessels into the areolæ found in the Malpighian layer. The leucocytes found joined, in variable number, into large cells are doubtless white globules which have penetrated, by a kind of invagination and subsequent incorporation, into the protoplasm of the epidermic cells. This presumption is supported by the observations of Volkmann. The theory of Waller and Cohnheim explains, better than the received theory, the rapidity with which pus may be formed under certain conditions. It would also assume greater importance if subsequent researches support the hypothesis that the leucocytes of the blood, after emigration, possess the power of proliferation, and may thus contribute to the development of morbid and normal tissues.—*Archives Générales de Médecine*, Mars, 1870.

The Origin of Diabetes.—In an elaborate paper Prof. W. T. Lusk, M.D., after giving a summary of the views and experiments of Bernard and Pavy, and of the opposite doctrines enunciated by them with regard to the production of sugar—as to whether it is produced in the liver during life, or whether it is a post-mortem production—relates a series of experiments performed by himself, "undertaken to determine the precise truth regarding these opposing statements." These observations consisted in the estimation of the quantity of sugar contained (*a*) in the blood of the right heart, obtained from a living dog by catheterisation through the jugular vein; (*b*) in the blood of the jugular vein, obtained from a living dog; (*c*) by determining the relative quantities of sugar in the blood of the same animal when drawn from the right side of the heart, and when taken from other parts of the body. From these experiments the author draws the following conclusions:—I. That the blood of the general system, in carnivorous animals confined to a nitrogenous diet, contains appreciable quantities of glucose, not only during the period of digestion, as admitted by Bernard, but even in cases where animals have been deprived of food for a considerable period of time. 2. That the blood of the right side of the heart contains from a quarter to half a grain of glucose per fluid ounce, under strictly physiological conditions. 3. That the quantity of glucose in the right side of the heart is from two to four times greater than that found under corresponding circumstances at the jugular vein. 4. That this excess argues a by no means insignificant amount of sugar in the pure hepatic blood before it has become largely diluted with the comparatively non-saccharine fluids of the venæ cavæ. 5. That we are forced to admit the fact of sugar-formation by the liver, though we fail to detect the presence of sugar in the liver-tissue, when after death the fermentation of the glycogenic matter is prevented. The author, however, does not believe that the liver is the sole source of the sugar found in the economy. During fœtal life glycogenic matter exists in the muscles and lungs. At birth the glycogenic matter disappears from the muscles, while it continues to form in the liver throughout the entire period of existence; but the glycogenic matter may reappear at any time in the muscles, when reparative matter accumulates, and the contractile elements are not exercised, as in hybernating animals, and limbs paralysed by division of the motor nerves. There are instances of diabetes where some other mechanism than that of the liver seems requisite to account for its origin. Tcherinow recently reported a case of diabetes in which there was atrophy of the liver-cells, with destruction, consequently, of all that could have made the liver a secreting organ. Tcherinow attributed the diabetes to the condition of the liver, which no longer acted as a barrier, storing up in its substance the saccharine matters brought to it by the portal vein, and converting them into glycogen, but allowed them to pass through into the general current unchanged. Bouchardat thinks that diabetes may be caused by the abuse of starchy articles of food and prolonged stomach digestion. In such case starch is converted into sugar by the altered gastric juice, instead of by the pancreatic juice. As absorption takes place in the stomach, glucose is at once transmitted to the blood in great quantities, because the liver through which it passes is already saturated with glycogenic matter, as a consequence of a prolonged amylaceous diet. A small atrophied pancreas is not uncommon in diabetes. Dr. Lusk met with a case of diabetes in which the only lesion discoverable after death was total calcareous degeneration of the pancreas. In a large class of cases, also, diabetes is connected with disorder of the nervous centres. It may be produced by violent emotion, injuries of the head, sexual excesses. The causes, therefore, of diabetes are various, and the source of sugar not confined to a single organ: but the liver is to be regarded as the most active agent in sugar production. It may also be the indirect cause of diabetes by its failure to fulfil its function of arresting the passage of saccharine principles through it.—*New York Medical Journal*, July, 1870.

Microscopical Characters of the Blood in Relapsing Fever.—Dr. H. C. Hand has studied the condition of the blood in thirty cases of relapsing fever, in the Philadelphia Hospital. He notes a granular condition of the red corpuscles, as if the colouring matter were breaking up its uniform distribution, and becoming collected in patches. This granulation is often most marked round the circumference of the corpuscles, giving them the appearance of crenation. Crenation, also, is observed, both in connection with the granular condition, and independently of it in various degrees from mere waviness of edge to complete deformity. Another change is increase in the white corpuscles. Dr. Cormack and Prof. Allan Thompson, in 1843, made this observation. Dr. Hand says that he found the white corpuscles increased in only three of his cases; but the red corpuscles, in their granular condition, bear a resemblance to the white, and may be mistaken for them. The alteration in the blood occurs very early. The author relates a case in which the blood was examined within three or four hours of the first seizure, and found to be in an average state of degeneration. The case proved a typical one of relapsing fever. The author believes that by living in a contaminated atmosphere the blood may become changed without the fever necessarily following. This was noted in the case of four of the resident medical attendants of the hospital. Once developed, the characters of the blood are constant, abating but slightly in the remissions. The abnormalities do not disappear for some time after the subsidence of the last relapse, when only debility and anæmia remain. The coagulability of the blood seems but little impaired. "Chicken-fat clots" are quite common in the post-mortem examinations; and in most cases the tendency of the red corpuscles to form rouleaus has been decided.—*New York Medical Journal,* August, 1870.

Intermittent Fever latent for several months. By Prof. Wilh. Braune.—The professor relates that of twelve persons who, in the autumn of 1868, had resorted to Borkum, an East Friedland island, for the purpose of taking sea-baths, eleven were attacked with intermittent fever. But while two of the eleven had the fever whilst on the island, the other nine did not suffer from it till the following spring and summer, *i.e.* from six to nine months after their stay at Borkum. The patients were adults, all of high social position, and most of them females, and belonged to various localities, as Leipzig, Halle, Liegnitz, Atteburg, &c.; and in these places when the patients severally residing in them were attacked, intermittent fever did not prevail, or was present in only a few sporadic cases. None of the patients had ever before had intermittent fever. It seemed, therefore, that in all of them the malady must have had its cause in malarious poison received during their autumnal stay in Borkum. The summer had been there, as elsewhere, very hot, and there was a considerable scarcity of pure water fit for drinking. The professor remarks that a prolonged period of incubation or latency after malarious poisoning is not so rare as some authorities, as Griesinger, have supposed; and in support of this he appeals to the observations made by Pfeiffer in the army of Weimar, published in a 'Jeniaschen Zeitschrift,' 1868. Twice—in 1849 and 1867—certain divisions of the troops suffered an epidemic of intermittent fever, which must have been due in each instance to exposure to malarious poison in the autumn of the preceding year.—*Archiv. f. Heilkunde,* December, 1869, pp. 68—74.

Pylephlebitis.—Dr. Fraentzel reports a case under the care of Dr. Traube. The patient had suffered from habitual constipation, followed by griping and some diarrhœa. After violent exertion he was seized with violent shivering and vomiting of greenish matters. His skin and conjunctivæ became jaundiced. Violent shiverings, followed by a sense of heat, were among the most prominent symptoms. When examined by Professor Traube the skin was noticed to be dry, hot, of a clear light yellow colour; the conjunctivæ were also coloured. Temperature 39·9° Cent.; the pulse 104; respirations 24. The tongue was whitish, a little dry; great thirst. For two days there had been no vomiting; there is constipation and slight tympanitis; but the sound on percussion duller in the right iliac region than in the left. The liver dulness 6½ inches; the border of the liver is easily felt; above it there is tenderness on pressure. The area of splenic dulness is considerably enlarged; it is 6 inches in length, and 4⅜ inches in breadth. The urine red; no albumen, but nitric acid indicates the colouring matters of the bile. Circulation, respiration, and nervous system unaffected. The jaundice, tenderness on pressure, and meteorisation of the abdomen became more pronounced. There were frequent shiverings; blood was discovered in the stools; there was severe pain in abdomen. Symptoms of peritonitis were developed before death, which took place in twenty-five days from the first seizure. A fortnight before death Professor Traube diagnosed abscess of the liver following pylephlebitis. According to Traube violent shiverings, accompanied by a considerable rise in temperature, are only met with in two affections of the liver. 1. In blenorrhœa of the biliary canal, due to the existence of calculi. 2. In abscess of the liver. The history and rapid course of the case, and the enlargement of the spleen negatived the former. Abscess of the liver may be developed in six different ways: *a*, by traumatism; *b*, by rupture of hydatids; *c*, by emboli (purulent infection, ulcerous endocarditis); *d*, by emboli from the branches of the portal vein in cases of pylephlebitis. Circumscribed peritonitis is one of the most frequent causes of pylephlebitis. *e*. By calculi in the biliary passages; *f*, under the influence of special conditions in the tropics. With the exception of abscess produced by pyohæmia, ulcerous endocarditis, and pylephlebitis, all purulent collections developed in the liver are, according to Traube, characterised by a fever of a regular type, like that of intermittents, whereas in abscess depending on pyohæmia, ulcerous endocarditis, and pylephlebitis the fever is irregular, that is the rigors occur three or four times in the twenty-four hours.

Autopsy.—Abundant purulent exudation in the peritoneum. The intestines were matted together by masses of exudation. On the spleen and the right lobe of the liver there was a thick fibrinous deposit of a greenish tint. In the right iliac region there was a large purulent collection, limited behind by the psoas and iliacus, and in front by the matted intestines. There were two perforations near the appendix vermiformis; the mucous membrane of the cæcum and colon were œdematous. The portal and splenic veins were filled with mixed liquid and coagulated blood. The mesenteric vein contained purulent fluid, and near the trunk of the portal vein a thrombus two inches in length. The wall of the vein was thick, and its inner surface unequal. The greater part of the veins from the ileum were filled with pus. At the bifurcation of the vena portæ there was a very adherent clot, which extended an inch and a half into the left branch, and an inch into the right. The ramifications of the left branch of the vena portæ were filled with purulent fluid; the walls of the veins were partly destroyed, and the hepatic tissue had undergone purulent transformation. The liver was augumented in size. On cutting it purulent foci, having the form of oak leaves, corresponding to the ramifications of the portal vein, were found. The acini between the abscesses were not enlarged; the cells of the liver were normal. The intact portion of the parenchyma was larger than the suppurated portion. The spleen was trebled in size, its parenchyma was of a cherry-red tint. The diagnosis of pylephlebitis and consecutive abscess of the liver rests on five points. 1. The patient presents a recent increase of the volume of the liver, which has taken place simultaneously in all parts of the organ. 2. There is also considerable enlargement of the volume of the spleen. 3. The patient has violent rigors, frequently repeated at irregular intervals. These rigors are accompanied by considerable elevation of temperature; in the interval of the rigors the temperature is elevated or normal. 4. Pyæmia, in the ordinary sense of the word, must be excluded, and endocarditis of the left heart. 5. On the other hand, there must be a purulent focus or ulceration with which the ramuscules of the portal system communicate. Or we must suppose that the patient has swallowed a rough foreign body.—*Berliner Klinische Wochenschrift, Nos.* 1 *and* 2, 1869, and *Archives Gén. de Médecine, Fevrier*, 1870.

Suppurative Pylephlebitis.—M. Fr. Chvostek relates the case of a soldier, æt. 23, who had suffered from intermittent fever, and whose health was consequently enfeebled. Since 1866 he had suffered from constipation, loss of appetite, and pain in the head and spine. From the middle of January, 1868, he had from one to four shivering fits daily, which came on by night or day, never at a fixed hour. There was almost always fever between the rigors. From the commencement of the shiverings the spleen increased considerably in size, but it was not painful. The liver augmented at the same time, the left lobe especially becoming rapidly hypertrophied. The liver was painful, especially on pressure. Some days before death the spleen and liver became somewhat smaller. There was jaundice, but it was never very pronounced. The urine contained the colouring matters of the bile, and the stools were slightly yellow. General peritonitis set in on the 29th January, and the patient succumbed twenty-five days after the first rigors. The author during life had diagnosed suppurative pylephlebitis from an unknown cause. The irregular rigors with the rapid and painless development of the spleen led him to suppose that there existed metastatic inflammation of the liver with blood stasis in the system of the vena portæ. After death there was found pylephlebitis, with secondary abscess of the liver and general peritonitis. There was thrombosis of the portal vein, with puriform degeneration of the clot. There was, besides, in the smaller curve of the stomach near the pylorus a circular ulceration, from three to four lines in diameter, with perpendicular edges and an even base. The surrounding mucous membrane was thickened, and of a greyish-white colour. The author thinks this ulceration was the cause of the pylephlebitis.—*Archives Génerales de Médecine Fevrier*, 1870, and *Œsterr. Zeitschr. f. pract. Heilk.*, 1868, N. 47, 45-47.

Ulcero-Membranous Angina.—At a meeting of the College of Physicians of Philadelphia in March last, Dr. J. M. Da Costa described a form of sore throat then prevalent in that city. The complaint begins with a chill, followed by fever, and the ordinary manifestations of angina. Within twenty-four hours of the outbreak, on the tonsils are seen small spots covered with a yellowish exudation, which on close inspection is found to be limited to the follicles, one tonsil being more affected than the other, but both sharing the disorder. The tonsils and the palatine arches are red and swollen, as also may be the back wall of the pharynx. Painful enlargement of the submaxillary and cervical glands follows, and there is great prostration. The fever is not high, but there is loss of appetite, nausea, vomiting, coated tongue, and sometimes diarrhœa. After three or four days the yellow spots disappear, and leave raw spots, as if from superficial ulceration. The glands subside, and the patient, though convalescent, is left very weak, and is specially liable to relapses. Dr. Da Costa draws a line of distinction between this epidemic affection and diphtheria. The disease seemed to him contagious. It especially attacked children and young adults. It might be akin to diphtheria, but it was the result of a special and milder poison. He thinks the cases he describes were perhaps identical with some of follicular diphtheritis lately reported, but more certainly with the malady named by Gubler "herpes guttural," but which Trousseau has delineated better under the term "common membranous sore-throat."—*American Journal of Medical Sciences, July*, 1870.

Rupture of the Heart. By Dr. J. de Bary, of Frankfort.—Mrs. J—, 72 years old, who had suffered for a long time from pain in the region of the liver, which, from its continued severity and its paroxysmal character, was judged to be caused by gall-stones (and rightly, as the post-mortem examination proved), was on the 28th of September, 1869, suddenly attacked with pain on the left side under the angle of the shoulder-blade. She became breathless and covered with a cold sweat, and her face and extremities became cold. Dr. de Bary saw her soon after the attack came on, and could not detect anything abnormal except an irregularity in the cardiac sounds and a remarkable weakness of the radial pulse. On the 29th and 30th of September and the 1st of October the attack of pain suddenly recurred each day, with difficulty of breathing, violent eructations, and inclination to vomit. The attacks were mostly of short duration. On the 2nd of October the patient felt comparatively well; she complained only of a difficulty in swallowing, even with fluids; the pulse was somewhat open and soft, yet hardly weak; there was no longer any pain; the heart's sounds were regular. This improvement lasted during the 3rd and the 4th, deglutition having improved, but in the evening the patient, while sitting up in bed and in the act of drinking some water, suddenly fell back dead. On examination of the body the pericardium was found greatly distended, and filled by about a pint of coagulated blood; in the middle of the posterior wall of the left ventricle there was an almost transverse rupture, about an inch long, with ragged edges; its internal opening was rather smaller than the external. Around the rupture the muscular tissue had undergone complete fatty degeneration, but everywhere else it appeared to be all but normal. The aorta was slightly atheromatous. The valves were quite healthy. In the gall-bladder were two gall-stones, each as large as a walnut, firmly embraced by the greatly hypertrophied walls of the gall bladder. At the left edge of the liver were two small nut-sized cysts, containing a thin uniform fluid. The author remarks that there can be little doubt that the rupture of the heart occurred on the 28th of September, so that the patient survived such an accident six days.—*Deutsch. Archiv. f. Klin. Med.* Feb., 1870, p. 152.

Croupous Bronchitis, terminating in recovery. By Dr. Roth, of Einersheim.—A small, weakly, anæmic woman, who between her frequent pregnancies menstruated regularly, and who suffered from chronic bronchial catarrh, was seized at the end of July, 1869, with violent fever, loss of appetite, headache, and much cough. Thinking the attack was only an exacerbation of her catarrh, she did not take to her bed, but, though seized at times with intense dyspnœa, continued to attend to her work in the house, and even in the fields. Soon after the beginning of the illness, and for the three weeks following, she was seized every two or three days with convulsive cough, a sense of strangulation, and extreme dyspnœa, so severe as to compel her to leave her work, and only relieved by the discharge, by cough, of rounded white masses, often blood-stained, and tasting as sweet as sugar. These masses, when floated in water, unfolded as very elegant fibrinous bronchial casts, which from stems about the thickness of a goose-quill dwindled down, by repeated dichotomous branches, to ramifications as fine as the finest sewing-silk; only in the largest branches could any lumen be discovered. In the second week of the illness this fibrinous discharge occurred, to a greater or less extent, two or three times a day for three consecutive days; then only every second or third day, as in the beginning; and at the end of three weeks it ceased entirely, without the patient's having taken any medicine except a glassful of infusion of ipecacuanha at the commencement. The patient recollected that two years previously she had had a similar attack, which also, after lasting about eight days, had ceased spontaneously. Physical exploration during the illness, and soon after the discharge of the fibrinous masses, gave the usual signs of dry bronchial catarrh, with marked râles, particularly on the left side, while on the right side the respiratory murmur was greatly diminished. The apices of the lungs were fully expanded. The patient was anæmic in a high degree, as was evidenced by the wan pallor of the skin and a bellows murmur at the apex of the heart.—*Deutsch. Archiv. f. Klin. Med.*, Feb., 1870.

Tumour of the Bones of the Skull. By L. R. Thomson, M.D., Dalkeith; and A. G. Miller, M.D., F.R.C.S.E.—The following case is interesting from the order in which the symptoms occurred, as well as from the pathological changes with which they were associated. J. G—, æt. 21, was first seen in May, 1868. He was then ruddy, and had enjoyed good health up to the commencement of his illness. In September, 1867, whilst at work as a gardener, he suddenly felt a disagreeable sensation in his left ear, as if a wasp had got into it and had produced a buzzing sound. This was followed, a month afterwards, by pain in the left ear at the back of the auricle. In December he had become somewhat deaf on the same side, and had the ordinary symptoms of coryza. In the spring of 1868 he complained of increasing pain at the back of the ear, extending down to the angle of the lower jaw, where at this time were observed several well-defined glandular swellings. He then also began to sleep much when he came in from his work, felt weak, lost his usual appetite, and frequently vomited his food. In May of the same year dysphagia came on, and he could only swallow soft and fluid materials. There was then also a good deal of diffused swelling behind the left angle of the lower jaw. Deafness had increased very much, and pain had begun to extend through both temples and to the right ear. On rising in the morning the eyelids were œdematous. In June Dr. Watson diagnosed a tumour affecting the sphenoid bone, extending downwards to the soft palate and upwards to the base of the brain. His sight then became dim, and was first lost in the right eye. He ultimately became stone-blind, and before death sloughing of the cornea took place. At an early period there was ptosis on the left side; the right eye was afterwards closed by œdema. In July his mental faculties became intermittingly disordered, and he was seized with fits of violent delirium. Deglutition became more difficult, but in August a severe bleeding from the throat took place, which relieved this symptom. The tumour seemed now to stop growing downwards, but to extend rapidly upwards and on each side, as manifested by impairment of mental faculties and fulness in the temples. During August and September he gradually got worse, but at the end of the latter month delirium ceased. He was mentally clear and collected except when under the attacks of delirium. In October an ulcerated cavity became visible in the tumour; this extended to the palate, and threw the nares and mouth into one cavity. At the end of this month the tumour showed itself in the left cheek, extending from the antrum. The patient died from asthenia on November 9th.

Sectio cadaveris, twenty-six hours after death.—Membranes of the brain, superiorly, and the substance of the brain were healthy, except on the inferior aspect of the right anterior lobe, where the brain was softened, of the consistence of cream. The dura mater was here firmly adherent to the substance of a tumour, which projected in the anterior and middle fossæ of the base of the skull. The olfactory nerves were softened; all the other nerves were healthy at their origin. By sections through the median line and through the parietal and temporal bones a portion of the tumour, occupying the greater part of the left half of the face, was removed, and the limits of the growth were traced. "The internal and part of the anterior and superior walls of the superior maxilla, the palatal, spongy, ethmoid and sphenoid bones, and the basilar process of the occipital bone, were entirely gone, and replaced by the tumour, which also bulged into the mouth, almost filled the antrum, and extended both outwards and downwards into the region of the neck." A careful description is given by Dr. Miller of the dissection of the cerebral nerves and vessels in connection with the tumour. It is worthy of remark that the auditory nerve on the left side, where deafness first began, was healthy and in no way interfered with by the tumour. The first symptoms—singing in the ears and obstruction of the nares—indicated that the disease had advanced sufficiently far to interfere with the functions of the Eustachian tube and to partially fill the posterior nares. The disease evidently originated in the osseous textures, and showed special affinity for the bones. Some portions of the tumour were softer than others. The microscope showed a number of cells, filled with nuclei and nucleoli, distributed amongst a fibrous tissue, the fibrous tissue being most marked in the portions nearest to healthy bone. In the remarks appended to the case by Dr. A. G. Miller the symptoms are seriatim referred to the pathological conditions revealed by the post-mortem.—*Ed. Monthly Journal*, July, 1869.

Epileptiform convulsions; left hemiplegia; tumour in the right anterior lobe of the cerebrum.—A specimen was exhibited by Dr. C. B. Nancrede for Dr. J. R. Bell, at the Pathological Society of Philadelphia, taken from a woman, æt. 27, who had been the subject of epileptiform convulsions, unattended by loss of consciousness. There was more or less headache in the intervals, but it was increased after each epileptiform attack. The pupils were normal, excepting when under the influence of conium, when the left pupil was largely dilated. Some time in November, 1869, after a more severe attack than usual, which was attended with unconsciousness, entire loss of power over the left side, with the exception of the fingers, as well as impaired sensation, was found to have occurred. In the second week of December another convulsion was followed by total paralysis of the left side. Ten days afterwards she died from lung trouble but slightly comatose.

Post-mortem.—Both lungs were extensively diseased, with a cavity in one. There was inflammation of the membranes of the brain, with easily separated adhesions along the longitudinal fissure and base.

On the upper surface of the right lobe was an old adhesion. On making a section beneath this there appeared to be a cicatrix, upon dividing which a hardened mass was found which could be turned out of the softened brain substance. Immediately surrounding this mass the brain substance was softened and reddened. The rest of the organ seemed healthy, though more softened than could be accounted for by post-mortem change. A committee appointed to examine the tumour reported that it was irregularly lobulated, and measured about 1½ inches across its base; it was closely adherent to the under surface of the dura mater, the mass being imbedded in the brain substance. It occupied a portion of the upper and outer part of the right hemisphere, corresponding to the middle portion of the corpus striatum. Its section presented two round nuclei of cheesy matter, ⅓rd inch in diameter each, surrounded by firm greyish tissue. Microscopically examined, the central cheesy portions were composed of oval or irregularly triangular cells, containing much granular matter, but no true nuclei. A certain number of spindle-shaped cells were mixed with these, but no true stroma, nor any vessels could be detected in the cheesy nodules. There was also a large amount of free granular fat. The dense layers surrounding the nodules consisted of highly vascular fibro-cellular tissue, the vessels of which were large, tortuous, with thin walls and very imperfectly developed peri-vascular sheaths, and the cells chiefly large spindle-shaped connective-tissue cells.—*American Journal of Medical Sciences, July*, 1870.

Disseminated, Diffuse, or Multiloculer Sclerosis of the Brain and Spinal Cord.—Dr. Meredith Clymer, the author of this paper, defines sclerosis as overgrowth (proliferation) and transformation of connective tissue, with consequent wasting of the proper functional elements of the part. The following is his definition of disseminated sclerosis of the brain and cord: "A disease of the cerebro-spinal centres, of gradual invasion; beginning with muscular weakness of one or both lower limbs, subsequently extending to the upper, and sooner or later passing into complete paralysis, which may in time affect, in some degree, the muscles of the head, neck, face, pharynx, and tongue. No constant derangement of cutaneous sensibility; the univocal symptom, tremor in the implicated muscles, which happens only when any voluntary movement is attempted, and ceases in a state of rest; frequently nystagmus; attended in the later stages with cramps, and permanent rigidity and contraction of the palsied members; of probably diathetic origin; slowly and surely progressive in its course, and constantly of fatal termination; the anatomical characters being patches or corns of sclerosis, irregularly disseminated in the brain and spinal cord." The author has collected and tabulated sixteen cases of this disease from various authors, and analysed them with reference to age, sex, anatomical characters, disorders of motility, sensibility, special senses, intellect, and the cause of death. The paper is of too great length to analyse, but we subjoin the following abstract of the section on differential diagnosis.

Paralysis agitans is the disease with which disseminated sclerosis of the nervous centres has been most frequently confounded. Paralysis agitans is a disease of declining years. Diffuse sclerosis of the brain and cord is a disease of adult life. The invasive stage is somewhat alike in both. It is often so imperceptible that the patient cannot fix the exact period of its commencement. In both there are crawling sensations and numbness, but in the neurosis these are felt in the arms, in the organic disease in the legs, in the initial stage. In paralysis agitans a tendency to trembling is an initial symptom; in sclerosis tremor invariably follows paralysis limb by limb. The muscular weakness of paralysis agitans begins in one arm or both, and then extends to the legs, and only passes into true paralysis, and that rarely in the final stage; in sclerosis one or both lower limbs are first attacked with paresis, and ultimately the patient becomes perfectly paraplegic. The gait in the two diseases is diacritic. In paralysis agitans the patient, after balancing and oscillating, starts with the head and trunk bent forwards on the toes and forepart of the feet, with short, quick steps, and to maintain the centre of gravity thus displaced goes trotting and hopping along at almost running speed, with one or both arms and wrists semiflexed and closely pressed to the sides. In sclerosis the gait is that of paraplegia. In paralysis agitans tremor is the earliest phenomenon; it is incessant, and but little modified by rest or motion, or in the developed stage by sleep. In sclerosis it is consecutive to the motorial troubles, and it is never spontaneous, but is always provoked by or follows upon muscular movement. Nystagmus is constantly present in sclerosis, never in paralysis agitans. The articulation in a sclerotic is slow and scanning; in paralysis agitans it is embarassed and indistinct. In paralysis agitans intellect is unaffected until near the close. In diffuse cerebro-spinal sclerosis it is mostly weakened from an early period. The characteristic deformities of shaking palsy described by Parkinson and Charcot cannot be confounded with the permanent contraction of late muscular rigidity, which is constant towards the end of diffuse sclerosis. There are some symptoms common to multilocular sclerosis of the anterior, or antero-lateral columns of the cord and locomotor ataxy (posterior sclerosis), in the forming stages of both disorders. In both there are tinglings, occasional numbness, and ready fatigue after slight exertion. In locomotor ataxy these are accompanied by ocular troubles, as weakness of sight, defective accommodation, strabismus, ptosis, double vision, &c. In the special form of disseminated sclerosis these are wanting, and if they occur in the cerebral or cerebro-spinal form they are persistent; in locomotor ataxy they are generally temporary. The pains of the ataxic are rare in disseminated sclerosis. The course and physiognomy of the two diseases when fully developed are sufficiently distinct. But if the posterior columns are invaded by the sclerotic patches as well as the anterior and antero-posterior, the signs of the two diseases will coexist, although by careful examination they may be separated.—*New York Medical Journal*, May and June, 1870.

Tubercular Spinal Meningitis occurring in Generalised Miliary Tuberculosis.—M. G. Hayem in October last brought before the attention of the Société de Biologie of Paris two cases in which meningeal spinal tuberculosis occurred in adults suffering from generalised miliary tubercle. Both patients were females; one was twenty-five, the other twenty-nine years of age. In the former there was a cavity in the right lung, and the parenchyma of both lungs was filled with miliary tubercular granulations. Miliary tubercles were found in the liver, spleen, and kidneys. There were tubercular granulations in the membranes of the brain. In the spinal canal there were feeble adhesions between the spinal dura mater and the arachnoid; the sub-arachnoidean tissue and the meshes of the pia mater were infiltrated with fibrinous exudations, containing numberless tubercular granulations, which formed a considerable mass on the posterior surface of the cord. In the second case in which there were miliary granulations in the pleuræ, the lungs, on the convex surface of the liver, in the kidneys, in the spleen, and in the cerebral membranes, there were numerous miliary tubercular granulations in the spinal meninges, particularly on the posterior surface of the spine in the dorso-lumbar region. The author believes that rachidian tubercular meningitis is not a rare lesion in generalised tuberculosis. And that tubercular granulations in the spinal membranes would be found in the adult, and still more frequently in children, if they were looked for in cases of tuberculosis.—*Gazette Medicale de Paris*, *April* 16*th*, 1870.

On a Form of Partial Atrophy of the Face.—In this paper, Dr. Louis Lande has collected records of several cases of a rare affection which was first described by Parry in 1825, and has since been noticed by Romberg, Stilling, and others. The following case was recorded by Guttman:—A. R—, 18, of a healthy family; up to the age of 11 in perfect health. At this time the left cheek assumed a pale tint, and began to diminish slowly. At the end of three years medical advice was obtained, and stimulating applications and electricity were tried, but without effect. The disease made progress for five years and then stopped. At the end of that time he presented the following condition:—The muscular system is well developed, and, except in the face, the body presents no want of symmetry. There, on the contrary, whilst the right cheek is full, fresh, and rosy, the left cheek is pale and shrunken, like that of an old woman; the skin of the left cheek is thin, corrugated, and may be easily raised; the subcutaneous fatty tissue has almost completely disappeared, so that at several points the integument adheres to the bone; through its thickness the teeth may be felt; the muscles are atrophied and the bones are thinner than on the healthy side. The buccinator, the zygomatics, the masseter, and the frontal portion of the temporal, have especially suffered. The muscles of the forehead, the lips, and of the chin, are free; the muscles of the nose are only feebly attacked; the ears do not present any difference; a deep furrow ascends from the left commissure of the mouth towards the zygomatic arch and extends as far as the insertion of the temporal muscle. Near to the left commissure of the mouth, under the white and thin skin, a branch of the facial is seen beating. Some rather large veins descend from the suborbital border towards the middle of the face. The skin excoriates easily, but is never covered with sweat, even under the influence of violent exercise. The growth of the eyebrows, the eyelashes, and of the hairs, also the secretion of tears, present no difference on the two sides. Sensibility and motricity are not in any way behind those of the healthy side. Temperature is the same in the two ears (36·8° Centigr). There is no want of symmetry of the different organs contained in the buccal cavity; no difference of coloration of the gums or of the buccal mucous membrane; lastly, on each side the salivary secretion is the same and temperature at 37·5° Cent. The following measurements give an idea of the changes of size produced by the atrophy:

	Right.		Left.	
	Inches.	Lines.	Inches.	Lines
From the extremity of the nose to the lobe of the ear ...	4	10	4	5
From the same to the outer angle of the eye	3	1	.2	9
From the middle line of the part to the lobe of the ear .	4	½	4	⅜
Thickness of the face	1	½	0	4
Height of the superior maxilla	1	⅜	1	0
„ inferior „	1	½	1	⅜

The body of the patient presents otherwise no anomaly. Of eleven cases noted by the author, in five the disease began by a white spot, which by degrees extended and became the seat of depression. In five there was simple pallor of a portion of the integument. In three the affected region subsequently became yellow, turning to brown as is seen in the cicatrices of certain burns. The skin becomes depressed, not only by the disappearance of the subcutaneous adipose tissue, but by the modification of certain of its elements. To the touch it presents the sensation produced by cicatricial tissue. The muscles become diminished, but continue to contract, after a time the cartilage and the bones are attacked. In consequence of lesion of the cartilage, in one instance the temporo-maxillary articulation became dry and loose. The author relates the case of a young woman in whom a somewhat analogous form of progressive atrophy of the skin attacked the right infra-mammary region. It began as a yellowish spot the size of an almond, which extended in the antero-posterior direction. The skin became hard, depressed, and adherent, of a brownish-yellow colour, bluish in certain points, the epidermis shining like that of a cicatrix. Romberg gave this affection the name of "Trophonévrose;" Moore, Virchow, and Hasse, consider it a form of progressive muscular atrophy. Drs. Bitot and Lande point out that the muscles retain their contractility for years. They consider it a special autopathic affection, and propose to designate it "Aplasie lamineuse progressive."—*Archives Générales de Médecine*, March, 1870.

CONTRIBUTIONS TO MEDICAL LITERARY HISTORY.

Adversaria Medico-Philologica.

BY W. A. GREENHILL, M.D. OXON.

Part IX.

(*Continued from* vol. xlv, p. 554.)

δαίμων, in later Greek meant *an evil spirit, a devil;* hence used as a popular name for *epilepsy*.[1] Σεληνιασμός was used in the same sense,[2] whence it is probable that both δαιμονίζομαι and σεληνιάζομαι sometimes signified merely *to be afflicted with epilepsy*. The two words were not, however, always used synonymously, and in one place they are distinguished from each other.[3]

δακτύλιος,[4] or δακτύλιον,[5] *the anus*, so called, either because it can be enlarged by inserting the *finger*, (δάκτυλος,)[6] or (as is more probable) simply from its resemblance to a *ring* (δακτύλιος). Albucasis,[7] in his Arabic translation of Paulus Ægineta,[8] renders the word by مقعدة *mak'adat.* Probably the earliest writer who uses the word in this sense is Dioscorides.[9]

δάκτυλος, the name for the *fingers* (including the *thumb*) and the *toes* (like the Latin *digitus*), the sense of each passage where the word occurs sufficiently indicating whether the *fingers* or the *toes* are intended. The derivations of the word proposed by Meletius[10] may be mentioned in a note,[11] but do not require refutation. The names of the different fingers were as follows:[12]

The thumb, ἀντίχειρ, μέγας,
The fore-finger, λιχανός,
The middle finger, μέσος,
The ring finger, παράμεσος,
The little finger, μικρός.

[1] Leo, 'Conspect. Medic.,' ii, 6, in Ermerins, 'Anecd. Med. Gr.,' p. 115.

[2] Id., ibid.

[3] St. Matthew speaks of δαιμονιζομένους καὶ σεληνιαζομένους καὶ παραλυτικούς, iv, 24.

[4] Joann. Alex., 'Comment. in Hippocr. *De Nat. Pueri*,' in Dietz, 'Schol. in Hippocr. and Gal.,' tom. ii, p. 220, l. 28; Meletius, 'De Nat. Hom.,' cap. 25, in Cramer's 'Anecd. Græca Oxon.,' vol. iii, p. 112, l. 4.

[5] Soranus, 'De Arte Obstetr.,' p. 164, l. 6, ed. Dietz.

[6] Joann., Melet., *ibid.*

[7] 'De Chirurg.,' p. 350, ed. Channing.

[8] Lib. vi, c. 81, p. 330, ed. Briau.

[9] 'De Mat. Med.,' i, 89, tom. i, p. 92, l. 15, ed. Sprengel.

[10] 'De Nat. Hom.,' in Cramer's 'Anecd. Gr.,' vol. iii, p. 121.

[11] *Quasi* δράκτυλος, from δράττεσθαι, *to grasp;* or δέκτυλος, as *receiving* (δεκτικός) things given; or δείκτυλος, because of their use in *pointing out* (δεῖξις) objects.

[12] Rufus Ephes., 'De Part. Hom.,' pp. 29, 30, 50, ed. Clinch; Pseudo-Galen, 'Introd.,' c. 10, tom. xiv, p. 704, l. 9 seq.; Pollux, 'Onomast.,' ii, 4, § 145; Meletius, *loco cit.*

δάρσις, a word first used by Herophilus to express the separation of parts united simply by cellular tissue, which can be effected by tearing with the fingers, without the use of the scalpel.[1] In this sense the word is always used by Galen.[2] Perhaps it is not to be found in any other ancient author, but it is used and explained by Vesalius.[3] As the word is derived from δέρω, *to flay*, no doubt its etymological meaning would be "*the removing the skin;*" but it was probably never really used in this sense, still less in that of "*an excoriation.*"

δαρτός, properly an adjective, derived from δέρω, and signifying *flayed;* but used as a substantive, and probably applied (as now) to the *dartos*, or second covering of the testicle.[4] It is sometimes found in the plural,[5] and is applied by Galen to the capsule of the ovary.[6] Adams explains the word to signify the *cremaster* muscle;[7] but the meaning given above is the more probable.

δειρή, *the neck*, a word as old as Homer,[8] and found in the Hippocratic Collection,[9] but not in common use as a medical term.[10]

δελτοειδής, *shaped like a* Δ, *triangular*, applied to the muscle still called *deltoid*.[11] The name was not universally adopted in Galen's time,[12] and he himself sometimes calls the muscle ὁ τῆς ἐπωμίδος μῦς,[13] or ὁ τὴν ἐπωμίδα κατειληφὼς μῦς,[14] or ὁ κατὰ τὴν ἐπωμίδα μῦς;[15] and this perhaps led Vesalius[16] to say that the muscle itself was sometimes called ἐπωμίς.

[1] Galen, 'De Anat. Admin.,' iii, 2, v. 1, tom. ii, p. 349, ll. 9, 14, 17; p. 350, ll. 13, 16; p. 476, l. 2.

[2] Ibid., v, l. 2; viii, 10, p. 483, l. 14; p. 484, l. 3; p. 487, l. 8; p. 700, l. 7.

[3] 'De Hum. Corp. Fabr.,' ii, 6. Castelli (in his Lexicon) refers to Lyser, 'Culter Anat.,' and C. F. H. Marx ('De Herophili Vita,' p. 27) refers to P. J. Hartmann, 'De Orig. Anat.'

[4] Celsus, 'De Medic.,' vii, 18, p. 295, ed. Daremb.; Pseudo-Galen, 'Introd.,' c. 11, tom. xiv, p. 719, l. 10.

[5] Rufus Ephes., 'De Appell. Part.,' p. 41, l. 4, ed. Clinch; Paulus Ægin., vi, 61, p. 260, l. 6, ed. Briau; Meletius, 'De Hom. Fabr.,' c. 26, in Cramer's 'Anecd. Gr.,' vol. iii, p. 113, l. 13; p. 115, l. 14.

[6] 'De Uteri Dissect.,' c. 9, tom. ii, p. 899, l. ult.

[7] Paulus Ægineta, vol. ii, p. 364.

[8] See Daremberg, 'La Méd. dans Homère,' p. 15.

[9] 'De Morb. Mul.,' i, 38; ii, 169; tom. viii, p. 92, l. penult.; p. 348, l. 16, ed. Littré.

[10] Rufus Ephesius mentions the word as synonymous with τράχηλος and αὐχήν ('De Appell. Part.,' p. 28, l. 14, ed. Clinch), as does also Georgius Sanguinatius (in Daremberg's 'MSS. Méd. Grecs,' p. 126).

[11] Galen, 'De Anat. Admin.,' iii, 3, 5, tom. ii, p. 356, ll. 5, 12; p. 373, l. penult.; 'Comment. in Hippocr. *De Artic.*,' i, 4, tom. xviii A, p. 314, l. penult.; p. 315, l. 7.

[12] 'De Anat. Admin.,' iii, 3, tom. ii, p. 354, l. 10; p. 359, l. 10; 'Comment. in Hippocr. *De Artic.*,' i, 2, tom. xviii A, p. 306, l. 8.

[13] 'De Anat. Admin.,' iii, 3, tom. ii, p. 359, l. 10.

[14] Ibid., i, 2; iii, 3, p. 273, l. 4; p. 354, l. 5; 'De Musc. Dissect.,' tom. xviii B, p. 972, l. 8, quoted by Oribasius, 'Collect. Med.,' xxv, 41, tome iii, p. 452, l. 8, ed. Daremb.

[15] 'De Usu Part.,' xiii, 13, tom. iv, p. 134, l. 8.

[16] 'De Hum. Corp. Fabr.,' lib. ii, cap. 23, tom. i, pt. 2, p. 171, ed. 1552.

δελφύς, an old name for the *uterus*, found in the Hippocratic Collection.[1] Hence is probably derived ἀδελφός, *a brother*, as coming from the same *womb*;[2] though some have derived δελφύς from ἀδελφός, as making all that it produces to be *brethren*.[3]

δέρμα, the common name for the *skin*, rendered in Latin by *cutis*,[4] and in Arabic by جلد *jild*.[5] It was used both for men and for beasts, either living or dead. It differs from χρώς (which also signifies *the skin*, but is much less frequently found in medical writers), inasmuch as χρώς, in the Hippocratic Collection,[6] includes the fleshy parts of the body in general. The use of the two words in Homer is given at length by Dr. Daremberg,[7] but is not of special interest. The term δέρμα σαρκῶδες, *skin resembling flesh*, is applied to the *sphincter ani*;[8] and δέρμα μυῶδες, *skin resembling muscle*, to the skin of the forehead,[9] and also of the lips.[10]

δερματικός, *of* or *like skin*. The term δερματικὸς ὑμήν, *a membrane resembling skin*, is applied by Aristotle both to the *dura mater*,[11] and also to the wings of insects.[12]

δερματώδης, *like skin*.[13] The term δερματώδεις ἐπιφύσεις, *ongrowths resembling skin*, is applied to *the auricles of the heart*;[14] δερματῶδες κάλυμμα,[15] *a covering resembling skin*, to the *operculum* of the young frog; δερματώδης μῦς, *a muscle resembling skin*, to the *sphincter ani*,[16] and also to the lips.[17] The name δερματώδης μῆνιγξ, *the membrane resembling skin*, is given by Galen as one of the names of the *dura mater*,[18] and is rendered by Honain[19] in Arabic by غشآء الجلدي *gisháu-l-jildí*.

[1] 'De Mul. Steril.,' § 222, tome viii, p. 428, l. antep., ed. Littré.

[2] Aristotle, 'Hist. Anim.,' iii, 1, p. 53, l. 14, ed. Bekker. So ἀκοίτης, ἄλοχος, *a spouse*, as using the same *bed*, κοίτη, λέχος; see Julius Pollux, 'Onomast.,' ii, 4, § 221.

[3] Soranus, 'Obstetr.,' c. 4, p. 6, l. 7, ed. Dietz; Moschion, 'Morb. Mul.,' c. 2, p. 2, ed. Dewez.

[4] Celsus, 'De Medic.,' i, 9, p. 25, l. 21, ed. Daremb., copied from Hippocrates, 'Aphor.,' v, 20, tome iii, p. 538, ed. Littré.

[5] Honain's translation of the 'Aphorisms' of Hippocrates, v, 20, 68 (69), p. 43, l. 2; p. 52, l. 3.

[6] Galen, 'Comment. in Hippocr. *De Fract.*,' ii, 9; iii, 6, tom. xviii B, p. 435, l. 5; p. 543, l. 3.

[7] 'La Méd. dans Homère,' p. 24.

[8] Galen, 'De Musc. Dissect.,' c. 32, tom. xviii B, p. 999, l. 10; unless we should read *μυῶδες*, *resembling muscle*, as in the corresponding passage of Oribasius, 'Coll. Med.,' xxv, 53, tome iii, p. 469, l. 9, ed. Daremb.

[9] Galen, 'De Anat. Admin.,' iv, 6, tom. ii, p. 444, l. 15.

[10] Theophilus, 'De Corp. Hum. Fabr.,' iv, 29, § 7, p. 179, l. 12, ed. Oxon.

[11] 'Hist. Anim.,' i, 16, p. 19, l. 26, ed. Bekker.

[12] 'De Part. Anim.,' iv, 6, p. 98, l. 25.

[13] Galen, 'De Anat. Admin.,' vii, 9, p. 615, l. penult.; p. 616, ll. 7, 8; Aristotle, 'De Part. Anim.,' iv, 3, p. 87, l. 15, ed. Bekker.

[14] Galen, ibid., p. 616, l. 5.

[15] Aristotle, 'Hist. Anim.,' ii, 13, p. 41, l. 7.

[16] Galen, 'De Musc. Dissect.,' c. 32, tom. xviii B, p. 999, l. 9, copied by Oribasius, 'Coll. Med.,' xxv, 53, tome iii, p. 469, l. 8, ed. Daremb.

[17] Galen, 'De Anat. Admin.,' iv, 3, tom. ii, p. 434, l. ult.

[18] Ibid., ix, 1, p. 708, l. 8.

[19] Translation of Galen, still in MS.

δευτέριον[1] and δεύτεροι[2] (or, in the plural, δεύτερα[3]), one of the names given to the *after-birth*, on account of its coming away after the birth of the child; for which reason it was also called ὕστερα,[4] and in Latin *secundæ*.[5] The Arabic name مشيمة[6] *mashímat*, has no such etymological meaning.

διαβήτης, *a siphon;* hence the disease *diabetes*, because "the fluid does not remain in the body, but uses the man's body as [a *siphon*] whereby to leave it."[7] The disease received various other names, all more or less referring to its most prominent symptoms, as far as they had been observed by the old physicians; *e.g.* from the excessive thirst of the patient it was called δίψακος,[8] and from the great flow of urine ὕδερος (or ὕδρωψ) εἰς ἀμίδα, *urinal dropsy*, and διάρροια εἰς οὖρα, *urine diarrhœa.*[9] The word was adopted by the Arabic physicians, and written ديابيطس *díábítis.*[10] The disease was probably rare in old times, for it is not noticed at all in the Hippocratic Collection, and Galen says that he had only met with two cases in his own practice.[11] It is not therefore remarkable that its special peculiarity, viz. the saccharine quality of the urine, should have escaped detection (at least in Europe) until the time of Willis, who says that in the seventeenth century, when there was much drinking of undiluted wine (*vinum meracius*), cases of diabetes were of very frequent occurrence.[12] He then goes on to mention (as if he were the first, or almost the first, to make the observation),[13] that the urine of diabetic patients had a wonderfully sweet taste, as if imbued with honey or sugar. This fact has never since been lost sight of; and it was no doubt in consequence of its having been first prominently brought before the public by Willis that this species of

[1] Paulus Ægineta, vi, 75, p. 308, l. 2, ed. Briau; Dietz, 'Schol. in Hippocr. et Gal.,' tom. ii, p. 463, note, l. 2.

[2] Soranus, 'De Arte Obstetr.,' c. 46, p. 94, l. 17, ed. Dietz.

[3] Moschion, 'De Morb. Mul.,' cc. 53, 54, ed. Dewez.

[4] Galen, 'Comment. in Hippocr. *Aphor.*,' v, 35, tom. xvii B, p. 824, l. 6.

[5] Celsus, 'De Medic.,' vii, 29, p. 318, l. 33, ed. Daremb.

[6] Avicenna, vol. i, p. 580, l. 4; Albucasis, 'De Chir.,' ii, 78, p. 346, ed. Channing.

[7] Aretæus, 'Caus. Morb. Chron.,' ii, 2, p. 97, l. 7, ed. Adams. In the text Adams has retained the common reading διαβάθρῃ, *a ship's ladder*, which hardly makes sense; but in the notes to his translation (p. 339) he proposes to read διαβήτῃ, *a siphon*, a conjecture which has been adopted above.

[8] Galen, 'De Locis Affect.,' vi, 3, tom. viii, p. 394, l. 12; Alexander Trall., ix, 8, p. 552, l. 29, ed. Basil.; Paulus Ægin., iii, 45, p. 47, l. 46, ed. Ald.

[9] Alexander Trall., ibid.; Galen, ibid., and 'De Sympt. Differ.,' c. 6, tom. vii, p. 81, l. 2; and 'De Cris.,' i, 12, tom. ix, p. 597, l. 4.

[10] Avicenna, iii, 19, § 2, vol. i, p. 549, l. 18.

[11] 'De Locis Affect.,' vi, 3, tom. viii, p. 394, l. 14.

[12] 'Pharmaceutice Rationalis,' pt. i, sect. 4, cap. 3, first published in 1674.

[13] His words are, "Quod autem *plerique authores* potum aut parum aut nihil immutatum reddi asserunt, a vero longissime distat; quoniam urina in omnibus (quos unquam me novisse contigit, et credo ita in universis habere), tum a potu ingesto, tum a quovis humore in corpore nostro gigni solito, plurimum differens, quasi melle aut saccharo imbuta, mire dulcescebat."

diabetes was at first called "diabetes Anglicus." Probably, however, it is not generally known that among the Hindus, several hundred years before Willis's time, it had been distinctly mentioned (and not at all as if there were anything new in the statement), that the urine occasionally assumed a saccharine character, and the serious nature of the disease in which this occurs had also been pointed out. The words of Susruta (who cannot have lived later than the ninth or tenth century of our era, and who probably lived in the fifth or sixth[1]) are as follows:—"Mellita urina laborantem quem medicus indicat, ille etiam incurabilis dictus est. . . . . Omnes urinales affectiones tempore incurabiles fiunt; ad mellitum urinæ statum perveniunt, et tunc insanabiles fiunt."[2] This passage is, of course, very fragmentary and incomplete, but it is worthy of being brought forward (almost for the first time), and perhaps of being noticed in all future historical accounts of the disease.

διάβρωσις, *erosion*, one of the species of hæmorrhage recognised by the old writers, occurring when the coats of a vessel were eaten through.[3] The word *ἀνάβρωσις* was used synonymously,[4] and also, according to some editions of Aretæus, *βρῶσις*.[5] The verb *διαβιβρώσκω*, used in the same sense, is found several times in Galen.[6] The two other species of hæmorrhage mentioned by Celsus[7] are *ῥῆξις* and *ἀναστόμωσις*, but this classification was not universally adopted. [See *ἀναστόμωσις*, *διαπήδησις* in this Glossary.]

διαγινώσκω, simply *to distinguish* one thing from another,[8] but especially (in medicine) to understand the symptoms by which one disease is distinguished from another, that is, *to form a diagnosis*.[9]

διάγνωσις, the distinguishing one thing from another, especially diseases by means of their characteristic symptoms, *diagnosis*.[10] In

[1] See the 'Imperial Dict. of Universal Biography,' art. *Susruta*.

[2] Taken from Hessler's translation, tom. i, p. 184. His note on the passage is as follows (Fasc. ii, p. 39):—"Mellitus urinæ status (*mad'hu—méhatva*) hoc loco nihil aliud esse videtur, nisi urinæ dulcedo, quam in *diabete mellito* animadvertimus; ad quem statum morbosum omnes affectiones urinarias suo tempore pervenire Suśrutas sua auctoritate affirmat." (See 'Suśrutas, Áyurvédas, id est Medicinæ Systema,' &c. Erlang. 1844, &c.) See also Wise's 'Hist. of Medicine,' vol. ii, pp. 328, 330.

[3] Celsus, 'De Medic.,' iv, 11 (4, § 5), p. 134, l. 7, ed. Daremb.; Aretæus, 'Caus. Acut.,' ii, 2, p. 25, l. 21; p. 26, l. 22, ed. Adams; Galen, 'De Sympt. Differ.,' c. 6; 'De Sympt. Caus.,' i, 5, tom. vii, p. 79, l. 16; p. 234, l. 7.

[4] Galen, 'De Locis Affect.,' v, 5, tom, viii, p. 338, l. 3; 'De Meth. Med.,' v, 2, tom. x, p. 312, l. 1, compared with p. 311, l. 11.

[5] *Loco cit.*, p. 32, l. 6, ed. Kühn, p. 25, l. 25, ed. Adams; but Ermerins reads (p. 29, l. ult.) *διάβρωσις*.

[6] 'De Sympt. Differ.,' c. 6; 'De Sympt. Caus.,' i, 5, tom. vii, p. 79, l. 15; p. 233, l. 12; p. 234, l. 6.

[7] *Loco cit.*

[8] Aretæus, 'Caus. Acut.,' ii, 2, p. 29, l. 2, ed. Kühn; Galen, 'De Anat. Admin.,' vii, 4, tom. ii, p. 600, l. 6, and often elsewhere.

[9] Erasistratus, in Galen, 'De Locis Affect.,' i, 1, tom. viii, p. 14, l. 16; Alexander Trallianus lays down the maxim, *ὁ ἄριστα διαγνοὺς, ἄριστα θεραπεύει*, "he that is best in diagnosis, is best in therapeusis" (viii, 9, p. 454, l. 3, ed. Basil.)

[10] Galen, 'De Anat. Admin.,' i, 2, tom. ii, p. 225, l. 5; 'Comment. in Hippocr.

Galen's time it would seem that σημείωσις was used as synonymous with διάγνωσις. (See διαγνωστικός.)

διαγνωστικός, *relating to diagnosis, able to distinguish.*[1] Διαγνωστικὴ θεωρία, *a knowledge of diagnosis.*[2] Διαγνωστικὰ σημεῖα, *signs indicating a person's present state of health*, as distinguished from ἀναμνηστικά, *those relating to the past*, and προγνωστικά, *those relating to the future.*[3] Τὸ διαγνωστικὸν[4] μέρος τῆς τέχνης, *that branch of medicine which relates to diagnosis*, which Galen says[5] was called by the more recent writers τὸ σημειωτικὸν μέρος. Ἡ Διαγνωστική, "*The Diagnostics*," was the common title given by later writers to Galen's treatise Περὶ Πεπονθότων Τόπων,[6] *De Locis Affectis.*

διάζωμα, a name applied to *the diaphragm*, perhaps peculiar to Aristotle,[7] who also calls it ὑπόζωμα.[8]

διάθεσις, *a state or condition of the body, diathesis;* not a predisposition to this or that particular disease, as the term διάθεσις ὑγιεινή,[9] *a healthy diathesis*, is found, as well as διάθεσις νοσώδης,[10] *a morbid diathesis.* It is used as equivalent to σχέσις in the sense of *a temporary condition*, and opposed to ἕξις, which is *a permanent condition*, and is said to be διάθεσις μόνιμος, *a permanent diathesis.*[11] Galen appears to derive the word from διακεῖσθαι, *to be in a certain state or condition;*[12] but (if we understand the passage aright) this is quite a mistake, as it evidently comes from διατίθεσθαι, *to be disposed or arranged.* In Aretæus it is applied to disease, διάθεσις τῆς νούσου,[13] and sometimes seems to signify the disease itself.[14]

διαΐσσω, a word used in the Hippocratic Collection for shivering or pain *darting* or *shooting through* the body.[15] It was the name

Prognost.,' i, 5, tom. xviii B, p. 24, l. 2; and frequently. The title of the chief work of Joannes Actuarius is Περὶ Διαγνώσεως Παθῶν, "On the Diagnosis of Diseases."

[1] Galen, 'De Usu Part.,' v, 10, tom. iii, p. 380, l. 12; Theophilus, 'De Corp. Hum. Fabr.,' v, 11, § 3, p. 202, l. 5, ed. Oxon.

[2] Galen, 'De Constit. Artis Med.,' c. 13, tom. i, p. 271, l. 16.

[3] Id., 'Ars Med.,' c. 3, tom. i, p. 313, ll. 7, 9, 12.

[4] Id., 'Comment. in Hippocr. *De Med. Offic.*,' i, 1, 2, tom. xviii B, p. 634, l. 6; p. 648, l. 2.

[5] Ibid., p. 633, l. 10.

[6] Theophilus, *loco cit.*, iv, *in fine*, p. 186, l. 3, and note, p. 322, ed. Oxon.

[7] 'Hist. Anim.,' i, 16, § 16; 17, § 8; ii, 17, § 8; p. 21, l. 10; p. 23, l. 4; p. 46, ll. 2, 7, 14; 'De Part. Anim.,' iii, 10, § 1, p. 76, l. 1, ed. Bekker. The word is found also in the 'Anonymi Introd. Anat.,' published by Bernard, but this work is taken from Aristotle.

[8] 'Hist. Anim.,' iii, 1, §§ 26, 29, p. 51, l. 12; p. 53, ll. 17, 22; 'De Part. Anim.,' ii, 16, § 11, p. 47, l. 16.

[9] Galen, 'Ad Thrasyb.,' c. 12, tom. v, p. 826, l. 7.

[10] Id., 'Comment. in Hippocr. *Epid. VI*,' v, 3, tom. xvii B, p. 238, ll. 7, 11, 13.

[11] Id., *loco cit.*, tom. v, p. 824, ll. 11. 17. Compare Joannes Actuarius, 'De Diagn.,' ii, 1, in Ideler's 'Phys. et Med. Gr. Min.,' vol. ii, p. 424, l. 24.

[12] 'De Sympt. Differ.,' c. 1, tom. vii, p. 43, l. 11, *παρὰ δὲ τὸ διακεῖσθαί πως τὸ τῆς διαθέσεως ὄνομα γέγονεν.*

[13] 'Caus. Chron.,' i, 4, p. 55, l. 20, ed. Adams.

[14] Id., ibid., ii, 8, p. 108, l. 21; Galen, 'De Anat. Admin.,' iii, 1, tom. ii, p. 343, l. 14.

[15] 'De Morb.,' i, 22, tome vi, p. 186, l. 21, ed. Littré; 'De Morb. Mul.,' i, 35; ii, 133, tome viii, p. 82, l. 17; p. 282, l. 17.

given by Archigenes to one of the recognised species of pain, which commences in the part primarily affected, and from thence, as from a root, spreads quickly to the neighbouring parts.[1] It is called in Latin, *emicans, persultans, pertundens;* Adams renders it *darting.*[2]

δίαιτα, *a mode of living, regimen,* used sometimes in its widest sense to signify all that relates to the preservation of health; sometimes in the more restricted sense of eating and drinking (*diet*);[3] sometimes as synonymous with διαιτητική, *dietetics.*[4] It answers to the Arabic تدبير *tadbír.*[5]

διαίτημα, a word used in the plural number to signify *articles of food,*[6] and also in a larger sense *all things relating to regimen.*[7]

διαιτητικός, *relating to regimen.* Ἡ διαιτητικὴ τέχνη,[8] or ἡ διαιτητική,[9] one of the three divisions of medical science generally recognised by the ancients, the two others being φαρμακευτική and χειρούργική. This division was not, however, always observed.[10]

διακινέω, *to move slightly;*[11] hence διακίνημα[12] and διακίνησις,[13] applied especially to bones connected in such a manner as to admit of only a slight degree of motion.

διακοπή, *a cutting through,* was used sometimes to signify a deep-seated wound;[14] sometimes as synonymous with ἐγκοπή and ἕδρα,[15] in

[1] Galen, 'De Locis Aff.,' ii, 8, tom. viii, p. 90, l. pen.; p. 94, l. 1.

[2] Paulus Ægineta (who copies Galen), ii, 40, vol. i, p. 296, l. 6.

[3] Galen, 'Ad Thrasyb.,' c. 35, tom. v, p. 872, l. 6; 'Comment. in Hippocr. *Epid. III,*' iii, 9, tom. xvii A, p. 660, l. 7.

[4] Id., 'Comment. in Hippocr. *De Vict. Acut.,*' i, 5, tom. xv, p. 425, l. 7; 'Introd.,' c. 7, tom. xiv, p. 690, l. 15.

[5] Honain's translation of Hippocrates, 'Aphor.,' i, 4, 5, p. 3, ll. 1, 2, 4; Rhazes, 'De Var.,' p. 26, ll. penult., ult., Greek translation, p. 245, ll. 36, 37, ed. Steph.

[6] Hippocrates, 'De Vet. Medic.,' cc. 3, 13, tome i, p. 576, l. 8; p. 598, l. 18, ed. Littré.

[7] Galen, 'Ad Thrasyb.,' c. 28, tom. v, p. 857, l. 9, φάρμακα σύμπαντα καὶ διαιτήματα; he had just before mentioned ἐδέσματα καὶ πόματα, so that διαιτήματα would appear to signify more than *articles of food.*

[8] Galen, 'Comment. in Hippocr. *De Vict. Acut.,*' iv, 96, tom. xv, p. 899, l. 5.

[9] Celsus, 'De Medic.,' i, præf., p. 2, l. 22, ed. Daremb.

[10] Galen, 'Defin. Med.,' c. 11, tom. xix, p. 351, l. 12.

[11] Hippocr., 'De Artic.,' § 9, tome iv, p. 102, l. 5, ed. Littré; Galen, 'De Anat. Admin.,' i, 10, tom. ii, p. 270, l. 11; 'Comment. in Hippocr. *De Artic.,*' iv, 44, tom. xviii A, p. 742, l. 6.

[12] Hippocr., 'De Fract.,' § 37, tome iii, p. 540, l. 13; Galen, 'Comment. in Hippocr. *De Fract.,*' iii, 51, tom. xviii B, p. 611, l. 5; 'Defin. Med.,' c. 474, tom. xix, p. 461, l. 1.

[13] Galen, 'Comment. in Hippocr. *De Artic.,*' iv, 44, tom. xviii A, p. 742, l. 6.

[14] Galen, 'Comment. in Hippocr. *Aphor.,*' vi, 18, tom. xviii A, p. 27, l. ult.; p. 28, l. penult.; p. 29, l. 13.

[15] Hippocr., 'De Cap. Vuln.,' §§ 9, 12, tome iii, p. 212, l. 4; p. 228, l. 7, ed. Littré; Galen, 'De Caus. Morb.,' c. 11, tom. vii, p. 38, ll. 4, 5.

the sense of a cut through the outer plate of the skull produced by a sharp instrument; sometimes as synonymous with ῥῆξις, one of the species of hæmorrhage arising from the rupture of a vessel.[1]

διάλειμμα, *an interval of time*,[2] generally; or between the attacks of a disease;[3] or specially synonymous with ἀπυρεξία, *the interval between the paroxysms of fever*.[4] It was sometimes used to signify the whole period of decline in fever, whether terminating in apyrexia or not.[5]

διαλείπω, *to leave off*, or *intermit;* applied sometimes to the *pulse*, when one pulsation was from time to time either much weaker than the rest, or wholly wanting;[6] sometimes to *fevers*, either when there was complete apyrexia, as in *intermittent fevers*, or when there was a less decided intermission in the case of *continued fevers*.[7] *Intermittent fevers* (called sometimes περιοδικοί)[8] were generally divided into three kinds, πυρετὸς ἀμφημερινὸς, τριταῖος, τεταρταῖος, *febris quotidiana, tertiana, quartana;*[9] to which were sometimes added πεμπταῖος, ἑβδομαῖος, ἐναταῖος,[10] *quintana, septimana, nonana*, but these were so rarely observed[11] that Galen had never met with any cases of the last two, and no well-marked cases of the *quintana*.

διαλεκτικός, *fitted for holding articulate converse;* as διαλεκτικὸν ζῷον, *a conversing animal*, applied to man; διαλεκτικὸν ὄργανον, *the organ of conversation*, applied to the tongue.[12] The verb διαλέγομαι is used in the sense of *holding articulate converse* in the Hippocratic Collection.[13]

διαπήδησις, literally *a leaping through*, applied to *the transudation of blood through the coats of vessels.* It probably corresponds with

[1] Galen, 'Defin. Med.,' c. 461, tom. xix, p. 457, l. 4.

[2] Galen, 'De Anat. Admin.,' vii, 15, tom. ii, p. 641, l. 7; 'De Sanit. Tu.,' vi, 7, tom. vi, p. 414, l. 13.

[3] Aretæus, 'Caus. Chron.,' i, 4, p. 55, l. 14, ed. Adams; Theoph. Nonnus, c. 33, tom. i, p. 130, l. 3.

[4] Galen, 'De Morb. Temp.,' cc. 3, 5, tom. vii, p. 414, ll. 10, 11; p. 420, l. penult.

[5] Id., ibid., c. 6, p. 427, ll. 3, 9.

[6] Paulus Ægineta, ii, 11 (12), p. 16, l. 48, ed. Ald. This chapter contains a full account of the pulse, taken from Galen, and is fully illustrated by Adams in his Commentary, vol. i, p. 202, &c., p. 213, &c.

[7] Galen, 'Comment. in Hippocr. *Epid. I*,' iii, 2, tom. xvii A, p. 220, l. 1, &c.

[8] Pliny, 'Hist. Nat.,' xx, 8, ed. Tauchn.

[9] Celsus, 'De Med.,' iii, 3, p. 77, l. 8, ed. Daremb.; Galen, 'De Febr. Differ.,' ii, 2, tom. vii, p. 336, l. 9; 'Comment, in Hippocr. *Epid. III*,' iii, 64, tom. xvii A, p. 715, l. 1.

[10] Hippocrates, 'Epid.,' i, 3, § 11, tome ii, p. 672, ed. Littré; Pseudo-Galen, 'Introd.,' c. 13, tom. xiv, p, 730, l. 2.

[11] Galen, 'Comment. in Hippocr. *Epid. I*,' iii, 2, tom. xvii A, p. 222, l. 12, &c.

[12] Theophilus, 'De Corp. Hum. Fabr.,' iv, 2, 27, p. 128, l. 2; p. 175, l. 12.

[13] 'Epid.,' iii, tome iii, p. 82, l. 6; p. 112, l. 8, ed. Littré.

"expressio sive sudatio" in Cælius Aurelianus;[1] and if so, it was a term invented by Bacchius in the third century B.C., or at least first used by him to signify a fourth species of hæmorrhage.[2] Galen[3] does not recognise it as a distinct species of hæmorrhage, but as being nearly synonymous with *ἀναστόμωσις* when applied to the smaller vessels, or to the exudation of bloody serum.[4] The word occurs in the Hippocratic Collection in one passage as given in the old editions, but both Littré and Ermerins read *διαπίδυσις*.[5]

διαπνευστικός, *calculated to dissipate or expel by exhalation* (*διαπνοή*), especially vapours or flatus.[6]

διαπνέω, literally *to blow through*, or *to breathe through*, when used transitively signifies *to disperse or dissipate by exhalation* (*διαπνοή*);[7] when intransitively, *to promote exhalation*,[8] or *to be dissipated by exhalation*.[9] In the passive voice it signifies *to exhale*, as plants, opposed to *ἀναπνεῖν*,[10] *to breathe*, as animals; *to be subject to διαπνοή*, *to perspire*;[11] *to be expelled or dissipated by perspiration*.[12]

διαπνοή, literally *a breathing through*; a word that is found in the Hippocratic Collection,[13] and is explained by Galen[14] to signify the drawing the external air through the skin into the arteries, as distinguished from *ἀναπνοή*, which was the drawing the air through the mouth into the lungs. Hence it was rendered in Latin by *difflatio*,[15] or *perspiratio*,[16] which latter word is still in constant use, though it is no longer connected with the theory expressed by its etymology. The epithet *ἄδηλος*, *imperceptible*,[17] or *ἄδηλος αἰσθήσει*, *imperceptible*

[1] 'Morb. Chron.,' ii, 10, p. 390, l. 21; p. 391, l. 2, ed. Amman.

[2] The other three were *ἀναστόμωσις*, *διάβρωσις*, and *ῥῆξις*.

[3] 'De Caus. Symp.,' iii, 5, tom. vii, p. 234, l. 13, &c. See also 'De Meth. Med.,' v, 2, tom. x, p. 311, l. 15, &c.

[4] In another passage, however (if the reading be correct), he mentions *διαπήδησις* as one of the *three* species of hæmorrhage, in the place of *διάβρωσις*. 'Defin. Med.,' c. 461, tom. xix, p. 457, l. 4.

[5] 'De Nat. Pueri,' tom. i, p. 402, l. 5, ed. Kühn; tome vii, p. 512, l. 13, ed. Littré; vol. ii, p. 506, l. 4, ed. Ermerins.

[6] Aretæus, 'Cur. Acut.,' i, 1, 2, p. 192, l. 3; p. 204, l. 8, ed. Kühn.

[7] Aretæus, 'Caus. Acut.,' i, 7; 'Cur. Acut.,' ii, 3; 'Cur. Chron.,' i, 2, p. 14, l. 12; p. 264, l. penult.; p. 299, l. ult., ed. Kühn.

[8] Id., 'Cur. Chron.,' i, 4, p. 311, l. 10.

[9] Id., 'Cur. Acut.,' i, 10, p. 241, l. ult.

[10] Marcus Antoninus, 'Comment.,' vi, 16, p. 57, l. 15, ed. Tauchn.

[11] Pseudo-Hippocr., 'De Alim.,' § 28, tome ix, p. 108, ll. 10, 11, ed. Littré; Aretæus, 'Cur. Acut.,' i, 1; 'Cur. Chron.,' ii, 3, p. 200, l. 7; p. 302, l. 10; Galen, 'Comment. in Hippocr. *De Alim.*,' iv, 2, tom. xv, p. 377, l. 6. In this last passage *διαπνεῖσθαι* should probably be read for *διαπνοεῖσθαι*, a word which is not recognised in Liddell and Scott's Lexicon.

[12] Galen, ibid., p. 377, l. 13; Aretæus, 'Caus. Acut.,' ii, 9, p. 57, l. 6.

[13] 'De Alim.,' §§ 28, 30, tome ix, p. 108, ll. 8, 9, antep., ed. Littré.

[14] 'De Hipp. et Plat. Decr.,' viii, 8, tom. v, p. 710, l. 11. See also 'Comment. in Hippocr. *De Sal. Vict. Rat.*,' § 2, tom. xv, p. 180, l. penult.; 'Defin. Med.,' cc. 108, 109, tom. xix, p. 375.

[15] J. P. Crassus, translation of Theophilus, 'De Corp. Hum. Fabr.,' iii, 11, § 11, p. 104, ed. Oxon.

[16] The verb *perspiro* was applied to the *veins* as early as the time of Cato. See 'De Re Rust.,' c. 157 (quoted by Facciolati).

[17] 'Galen, 'Comment. in Hippocr. *Aph.*,' i, 15; ii, 28, tom. xvii B, p. 421, l. 3; p. 521, l. 7.

by sense,[1] is sometimes added, and the words then signify the exhalation going on at the surface of the body,[2] viz. *insensible perspiration*, as distinguished from ἱδρώς,[3] *sweat* (or *sensible perspiration*), and from ῥύπος,[4] *sordes*. The word occurs several times in Aretæus, and in different senses: as, *the expulsion of flatus*, both upwards and downwards;[5] *cutaneous perspiration*;[6] *the organs or channels for the passage of fluids or vapours*;[7] *the expulsion or dissipation of vapours or humours*.[8] Both διαπνοή and ἀναπνοή are used with reference to the perfume of odoriferous plants, the former word to express its entrance into the system through the cutaneous surface of the body, the latter its entrance by the mouth and nostrils;[9] and both words signify *the passing in*, and *the passing out*, indiscriminately (both εἰσπνοή and ἐκπνοή[10]).

[1] Id., 'Comment. in Hippocr. *Epid. VI*,' iv, 21, tom. xvii B, p. 193, l. 14, where it is stated that some of the later physicians used the phrase λόγῳ θεωρητή, *perceptible by reason*, instead of αἰσθήσει ἄδηλος, *imperceptible by sense*.

[2] διὰ τῶν ἀδήλων αἰσθήσει πόρων, *through the pores that are imperceptible by sense*. Galen, 'Comment. in Hippocr. *De Alim.*,' ii, 4, tom. xv, p. 240, l. 13. Theophilus, 'De Corp. Hum. Fabr.,' iii, 11, § 11, p. 104, l. 8.

[3] Galen, 'De Sanit. Tu.,' i, 12, tom. vi, p. 66, l. ult.; p. 67, l. 2; 'Comment. in Hippocr. *De Alim.*,' iii, 17, tom. xv, p. 323, ll. 5, 6; 'Comment. in Hippocr. *De Humor.*,' i, 12, tom. xvi, p. 121, l. 5.

[4] Id., 'De Meth. Med.,' iii, 3, tom. x, p. 175, l. 15; p. 176, l. 1.

[5] 'Caus. Chron.,' ii, 8, p. 152, l. pen., ed. Kühn.

[6] 'Caus. Acut.,' i, 10, p. 23, l. pen.

[7] Ibid., i, 5; ii, 2, p. 5, l. 13; p. 30, l. 12; 'Cur. Acut.,' ii, 7, p. 279, l. 14. See the note in Ermerins' edition, p. 6.

[8] 'Cur. Acut.,' i, 1, p. 196, l. 3; 'Cur. Chron.,' ii, 13, p. 345, l. 2.

[9] 'Cur. Chron.,' ii, 11, p. 339, l. 1.

[10] The whole subject is discussed and illustrated at great length by Kaau, in his 'Perspiratio dicta Hippocrati per universum corpus anatomice illustrata,' Lugd. Bat., 1738.

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